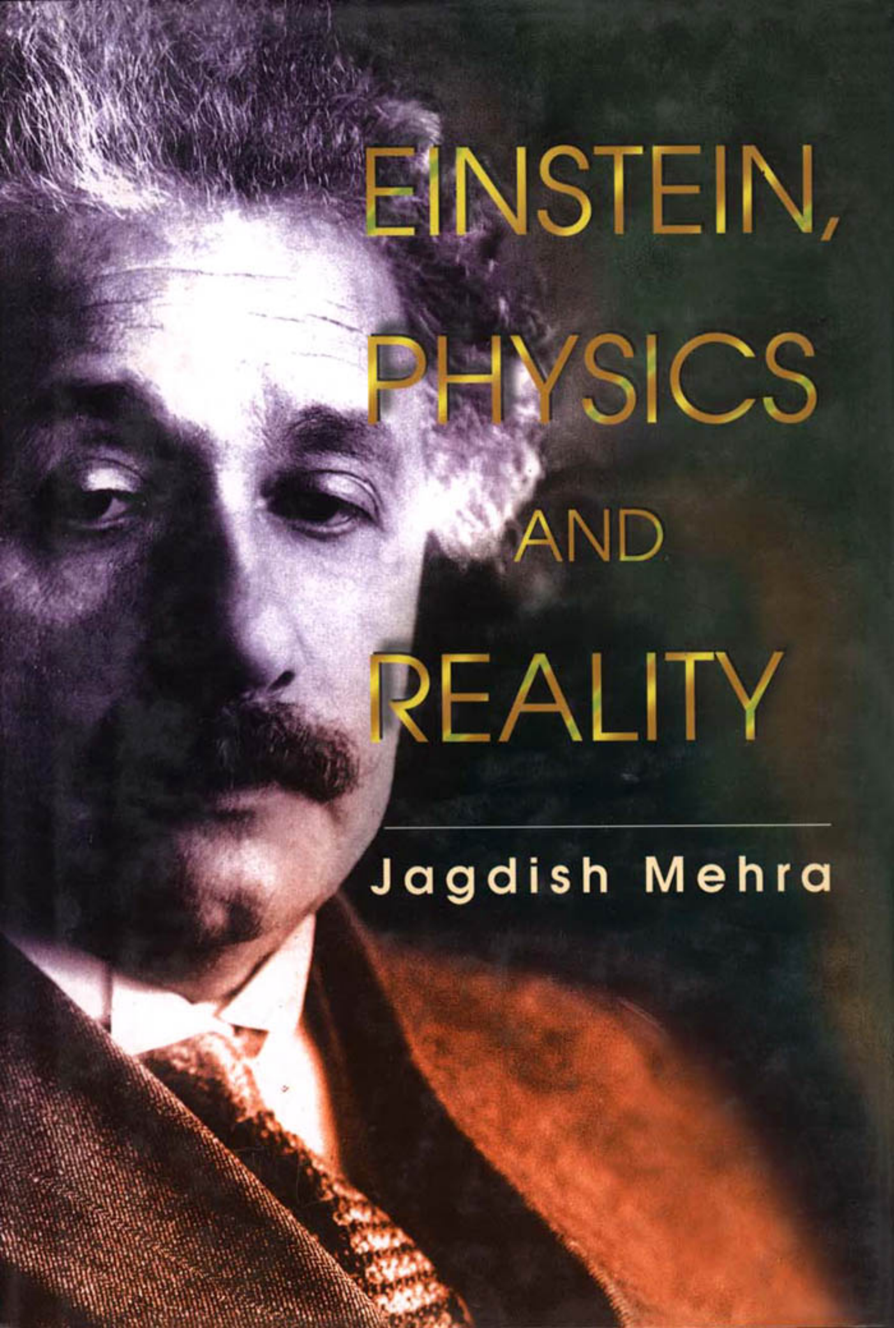
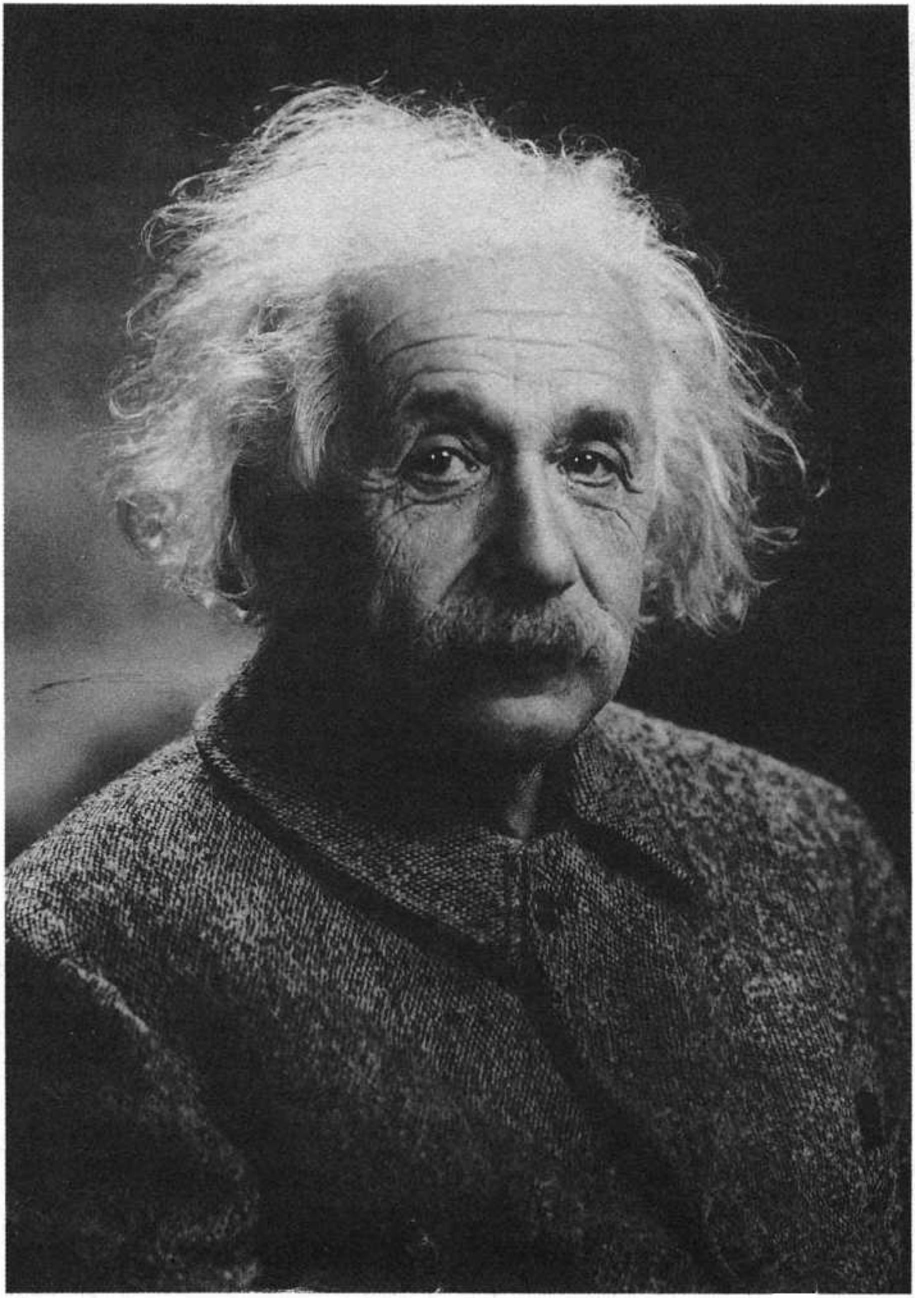

EINSTEIN,
PHYSICS
AND
REALITY
Jagdish Mehra

About the Author

Trained as a theoretical physicist in the schools of Heisenberg and Pauli, Jagdish Mehra is a distinguished historian of modern physics. His major work (with Helmut Rechenberg, six volumes, nine books) is *The Historical Development of Quantum Theory, 1900–1942* (Springer-Verlag New York, 1982, 1987, 2000). In 1994 Professor Mehra published *The Beat of a Different Drum: The Life and Science of Richard Feynman* (Oxford University Press), and has just completed (with Kimball A. Milton) a companion volume, *Climbing the Mountain: The Scientific Biography of Julian Schwinger* (Oxford, 2000). With Arthur Wightman of Princeton University, he has coedited *The Collected Works of Eugene Paul Wigner* in eight volumes (Springer-Verlag, 1990–2000).

Professor Mehra has held prestigious academic appointments in the USA and Europe, including the Regents' Professorship at the University of California at Irvine and the UNESCO – Sir Julian Huxley Distinguished Professorship of History of Science in Trieste, Italy, and Paris, France. He lives in Houston, Texas, USA, where he is associated with the University of Houston.



"Reason, of course, is weak, when measured against its never-ending task."

— ***Albert Einstein,*** *14 March 1879 – 18 April 1955*

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Preface

At a rather young age I wrote an essay with the pretentious title 'Albert Einstein's Philosophy of Science and Life' for an open essay competition of the International Council of YMCA's. I gave a copy of it to Paul Arthur Schilpp (Editor of *Albert Einstein: Philosopher–Scientist,* Einstein's 70th birthday volume), who was visiting my university to give a lecture; he forwarded it to Einstein. One fine morning I received an aerogram, marked '112 Mercer Street, Princeton, N.J.'; it contained a one-line message: 'Dear Sir: Apart from too unwarranted praise I find your characterization of my convictions and personal traits quite veracious and showing psychological understanding. With kind greetings and wishes, sincerely yours, Albert Einstein [signed].' (*Einstein Archive.*) Much more than the prize which I won for my essay, Einstein's letter greatly excited and inspired me for a long time. In the course of time and my later work I met all of my scientific heroes, but Einstein had died on 18 April 1955, before I came to America; however, when I did so about a couple of years later, my first pilgrimage was to his house in Princeton, where Helen Dukas, his loyal secretary, received me and remained very kind and helpful during the following years.

Preface

In my scientific-historical work over the years I published a great deal on Einstein — on his life and his work on the quantum, statistical, and relativity theories — but I always regretted that I did not have a chance to meet him. There were some questions I would have liked to ask him! My work (with Helmut Rechenberg) *The Historical Development of Quantum Theory* (Springer-Verlag, six volumes) and my essay *Einstein, Hilbert, and the Theory of Gravitation* contain much about the various aspects of Einstein's work and views on most topics dealing with physics and the nature of physical reality. This slim volume, based on two lectures I gave in February 1991 at CERN (European Organization of Nuclear Research) and the University of Geneva in Switzerland, and again at the International Centre for Theoretical Physics, Trieste, Italy, and at UNESCO in Paris, France, in May 1991, touches upon certain aspects of Einstein's views on physics and reality.

Permission to publish the Einstein materials has been granted by the Albert Einstein Archives, the Jewish National & University Library, the Hebrew University of Jerusalem, Israel, for which I am grateful.

Houston, Texas
15 February 1999

Jagdish Mehra

Introduction

In *An Interview with Einstein,* made two weeks before Einstein died in April 1955, the interviewer noted: 'Einstein said that at the beginning of the century only a few scientists had been philosophically minded, but today physicists are almost all philosophers, although "they are apt to be bad philosophers." He pointed as an example to logical positivism, which he felt was a kind of philosophy that came out of physics.'[1] In his later years, in particular those following the creation of ideal gas statistics in 1924–25, Einstein did not work actively in the field of quantum theory. He concentrated on the generalization of the field theory of gravitation and on efforts to unify the theories of general relativity and Maxwell's electrodynamics. Moreover, he seemed to have taken a hostile point of view towards the developing and successful quantum mechanics. On many occasions Einstein acted as the principal opponent, in particular to the philosophical consequences that flowed from the new quantum theory. His epistemological discussions with Niels Bohr and Max Born might be counted among the greatest dialogues in the history of science, which raised some very fundamental questions. Yet Einstein could not agree with the answers he obtained. Not only did his later work on general relativity and unified field theory alienate him from most of the contemporary, especially the younger, physicists, but their criticism also concentrated on points which appeared to be secondary to Einstein — such as the questions of statistics and detailed determinacy. Thus he finally resigned himself to his critics with the following statement: 'It is my opinion that the contemporary quantum theory, by means of certain definitely laid basic concepts, which on the whole have been taken over from classical mechanics, constitutes an optimum formulation of the conceptions. I believe, however, that this theory offers no useful point of departure for future development. This is the point at which my expectations depart most widely from that of contemporary physicists.'[2]

1

The 'Non-Einsteinian Quantum Theory'

Einstein, through his work on the 'light-quantum,' was one of the great founders of quantum theory. From the questions in which he became involved during his long association with the investigations of quantum phenomena, one notices that he never showed interest in detailed kinematical models — including the atomic models that had been fashionable — from the very beginning. Even in his very first papers, dealing with inferences drawn from the phenomena of capillarity, Einstein considered the forces between molecules and not their detailed structure.[3] The theory of atomic models, which had been pursued so vigorously by J. J. Thomson within the framework of classical theory and which had been initiated by Johannes Stark in an early quantum speculation and then pursued by Arthur Haas in his doctoral thesis, offered no attraction to Einstein, who was interested only in questions of principle. The existence of atoms and molecules was such a question of principle, as was the structure and geometry of space filled with gravitating matter, but not the detailed kinematics within atomic and molecular models.

The attitude among British physicists, like J. J. Thomson and Ernest Rutherford and many others, had been quite different. The structure of matter offered such a wide variety of phenomena and effects that were worth being investigated, especially for future applications. There were the phenomena of radioactivity, though discovered in France by Henri Becquerel and the Curies, but intensively studied in England in the laboratories of William Ramsay and later Rutherford. To explain these phenomena a detailed knowledge of the constitution of matter (and that meant the structure of atoms and molecules) was necessary, since the phenomena were connected with specific chemical elements. Thus, in 1911, Rutherford in Manchester had developed the planetary model of atoms on the basis of his experiments on the scattering of alpha particles by atoms.

Niels Bohr, who worked with Rutherford in Manchester from March 1912 to the end of July 1912, learned about Rutherford's atomic model and accepted it. But how could such a model work within the framework of classical theory? Already in 1912 Bohr had become convinced that the quantum hypothesis should ensure the stability of the Rutherford model of (neutral) atoms: 'This hypothesis is: that there, for any stable ring (any ring occurring in atoms), will be a definite ratio between the kinematic energy of an electron in the ring and the time of rotation. This hypothesis, for which there will be given no attempt at a mechanical foundation (as it seems hopeless), is chosen as the only one which seems to offer a possibility of an explanation of the whole group of experimental results, which gather about and seem to confirm concepts of the conceptions of the mechanics of the radiation as the ones proposed by Planck and Einstein.'[4]

1.1. The Bohr–Sommerfeld Atom

In early 1913 Niels Bohr developed the theory of atomic spectra.[5] He started with the simplest atom, that of hydrogen, which consists of a positively charged nucleus and an electron circulating in different but stable orbits in accordance with the quantum number. Otherwise the classical laws of mechanics and electrostatics (for electrical attraction) apply, but the rotation (or, in fact, the angular momentum) becomes 'quantized.' The laws of electrodynamics concerning, for instance the radiation, do not apply to these stable states. The radiation occurs only by transition between the states with a well-determined frequency given by the energy difference between the states and Planck's law.[6] Bohr's atomic model of the hydrogen atom could be generalized to hydrogen-like atoms (like the ionized helium) and at least qualitative consequences could be drawn also for multielectron molecules. Arnold Sommerfeld developed Bohr's model further by including elliptical (Kepler) orbits.[7] In particular, he tried to generalize the quantization condition, his phase integral

$$\int p\,dq = nh, \tag{1}$$

to several degrees of freedom. This fact did not play a role in the calculation of the hydrogen spectrum, for although we obtain two degrees of freedom in a Kepler ellipse (the motion of the electron occurs in a plane with variable distance from the atomic nucleus and the angle ϕ), the quantum numbers n and n' (due to the 'quantization' of the r and ϕ coordinate) appear only as a sum and the spectral lines do not depend on n and n' separately. On the other hand, Sommerfeld calculated the relativistic mass corrections to the motion of electrons on elliptic orbits and found a fine structure in the lines corresponding to a sum of quantum numbers $(n + n')$.

Further applications of the Bohr–Sommerfeld model were made to the Stark effect of spectral lines.[8] In this case, Paul Sophus Epstein showed that one could choose such quantization conditions as explain the empirically found splitting.[9] It was, however, necessary to restrict the possibility of transitions by 'selection (*Auswahl*) principles.'[10]

The calculations of the Zeeman splitting of lines in a magnetic field turned out to be less successful. One could explain the normal Zeeman effect, but not the 'anomalous' Zeeman effect and the so-called Paschen–Back effect.[11] Besides the difficulties which such a well-known phenomenon as the Zeeman effect posed to the Bohr–Sommerfeld atomic dynamics, further empirical facts could not be explained with the 'old quantum theory,' such as the properties of the hydrogen model.[12] In particular, one could not calculate the intensities of spectral lines. The first attempt at solving this problem was made by Niels Bohr in his 'correspondence principle,' to which we shall turn in the next section.

However, given the partial success of the atomic model of Bohr and Sommerfeld it was still difficult to decide which coordinates one should quantize. Epstein[13] and Karl Schwarzschild[14] solved this problem partially by referring to the Hamilton–Jacobi theory. And here Einstein entered the field with his only contribution to the 'old quantum theory.'[15] He modified the result of Schwarzschild and Epstein such that the quantization condition could be formulated independently of the coordinate system.

We should recall here the most important contribution that Paul Ehrenfest made to the quantum theory: his *adiabatic hypothesis*, which he first presented in 1913: 'If a system is affected in a reversible adiabatic manner, allowed motions are transformed into (other) allowed motions.'[16] Further 'Each application of the adiabatic hypothesis forces us to look for "adiabatic invariants" — that is, for quantities which retain their values during the transformation of a motion $\beta(a)$ into a motion $\beta(a')$ related automatically to the former.'[17] Adiabatic invariants are the quantities $\frac{\overline{2T}}{\nu}$ for periodic motions, where T is the period and ν the frequency of the motion, the cyclic momenta of systems which possess cyclic coordinates, etc. Now the adiabatic invariants can be related to the quantum conditions of Planck, Sommerfeld and others.[18] The advantage of the adiabatic hypothesis is also apparent in the fact that it applies likewise to quasiperiodic motions. Ehrenfest concluded by saying: 'The problem discussed in this paper shows, I hope, that the adiabatic hypothesis and the motion of adiabatic invariants are important for the extension of the theory of quanta to still more general classes of motions; furthermore, they throw some light on the question: What conditions are necessary that Boltzmann's relation between probability and entropy may remain valid? Hence it would be of great interest to develop a systematic method of finding adiabatic invariants for systems as generally as possible.'[19]

1.2. Physics and the Correspondence Principle

In his paper entitled 'On the Quantum Theory of Line Spectra,' Niels Bohr wrote: 'In spite of the great progress involved in these investigations [of Sommerfeld, Schwarzschild, Epstein, and Debye, cited above], many difficulties of fundamental nature remained unsolved, not only as regards the limited applicability of the methods used in calculating the frequencies of the spectrum of a given system, but especially as regards the question of the polarization and the intensity of the emitted spectral lines. These difficulties are ultimately connected with the radical departure from the ordinary ideas of mechanics and electrodynamics involved in the main principles of quantum theory, and with the fact that it has not been possible hitherto to replace these ideas by others forming an equally consistent and developed structure. Also, in this respect, however, great progress has recently been obtained by the work of Einstein[20] and Ehrenfest.[21] On this state of the theory it might therefore be of interest to make an attempt to discuss the different applications from a uniform point of view, and especially to consider the underlying assumptions in their relations to ordinary mechanics and electrodynamics.'[22]

In his paper 'On the Quantum Theory of Line Spectra,' whose first and second parts appeared in 1918 (the third was not published until 1922[23]), Niels Bohr tried to connect the results from the 'old quantum theory' of atomic structure with those obtained by applying the classical theories of mechanics and electrodynamics. The reason for this approach might be found in the fact that the classical theories allow one to calculate quantities like radiation intensities, etc.

However, if applied to atomic systems, the results turn out to be wrong. In the 'old' quantum-theoretical model of Bohr and Sommerfeld, one did not know how to compute these quantities. Now Bohr postulated a connection between the available classical results and not-yet-existent quantum-theoretical results for high quantum numbers. 'We shall show, however, that the conditions which will be used to determine the values of the energy in the stationary states are of such a type that the frequencies calculated by (1) [that is, Planck's energy–frequency relation], in the limit where the motions in successive stationary states comparatively differ very little from each other, will tend to coincide with the frequencies to be expected on the ordinary theory of radiation from the motion of the system in the stationary states. In order to obtain the necessary relation to the ordinary theory of radiation in the limit of slow vibrations, we are therefore led directly to certain conclusions about the probability of transition between two stationary states in this limit. This leads again to certain general considerations about the connection between the probability of a transition between any two stationary states and the motion of the system in these states, which will be shown to throw light on the question of polarization and intensity in the different lines of the spectrum of a given system.'[24]

Bohr then made use of Ehrenfest's adiabatic hypothesis, which he called the 'principle of mechanical transformability,' to prove his assertion that: 'Although, of course, we cannot without a detailed theory of the mechanism of transition obtain an exact calculation of the latter probabilities, unless n is large, we may expect that also for small values of n the amplitude of the harmonic vibrations corresponding to a given value of τ will in some way give a measure for the probability of a transition between two states for which $n' - n''$ is equal to τ. Thus in general there will be a certain probability of an atomic system in a stationary state to pass spontaneously to any other state of smaller energy, but if for all motions of a given system the coefficients C [the Fourier coefficients in the expression for the intensity] are zero for certain values of τ, we are led to expect that no transition will be possible, for which $n' - n''$ is equal to one of these values.'[25]

With these words Bohr first stated the 'principle of correspondence,' which would determine the application of quantum theory to atomic systems during the following seven years. It determined Bohr's work on atomic spectra as well as the systematic guessing of results by others. R. Ladenburg was the first to apply, in 1921, the quantum correspondence considerations to the theory of dispersion.[26] This theory was further developed by Hendrik Kramers.[27] In a very explicit paper, 'The Absorption of Radiation by Multiply Periodic Orbits, and Its Relation to the Correspondence Principle and the Rayleigh–Jeans Law,' J. H. Van Vleck extended Bohr's ideas.[28] In this paper one also finds the correspondence derivation of Einstein's 1917 *Ansatz* for induced emission. Niels Bohr had cast some doubt whether this *Ansatz* was compatible with correspondence considerations. Finally, Hendrik Kramers and Werner Heisenberg completed the theory of dispersion.[29]

Another paper which came close to establishing the new theory was W. Kuhn's article 'On the Total Intensity of Absorption Lines Emanating from a Given State'[30] and a paper by W. Thomas,[31] which contained the Thomas–Kuhn sum rule, which was used at a crucial point in Heisenberg's famous paper on the foundation of quantum mechanics.[32]

We conclude this section by making two remarks. First, the correspondence principle emerged in Bohr's mind *after* he had studied Einstein's 1916 paper on the absorption and emission coefficients[20]: 'Quite recently, however, Einstein has succeeded, on the basis of the assumptions I and II [that is, only stationary discrete states of an atomic system exist, and the energy of "unifrequentic" radiation is given by Planck's quantum], to give a consistent and instructive deduction of Planck's formula by introducing certain supplementary assumptions about the *probability of transition of a system between two stationary states* and about the manner in which this probability depends on the density of radiation of the corresponding frequency in the surrounding space, suggested from analogy with the ordinary theory of radiation. Einstein compares the emission and absorption of radiation of frequency *v* corresponding to a transition between two stationary states with the emission or absorption to be expected on ordinary electrodynamics for a system consisting of a particle executing harmonic vibrations of this frequency. In analogy with the fact that on the latter theory such a system will without external excitation emit a radiation of frequency *v* ... .'[33] Thus one might consider Einstein the father of the correspondence principle. In fact, the influence of his ideas on this paper of Bohr was rather large and Einstein's spirit pervaded it regarding the simplicity of the arguments and the kind of general conclusions that were drawn by Bohr. No detailed kinematics disturbed the Einsteinian spirit of Bohr's first correspondence considerations.

Our second remark might stress the fact that with the correspondence principle physicists were in a position to calculate the quantities for which there was no place in Bohr and Sommerfeld's original atomic model. Actually, in his famous *Handbuch der Physik* article (1926) on the old quantum theory, Pauli reported on (Heisenberg's) nearly 'always correct results from a completely wrong theory,' using the physical (correspondence) intuition.[34] When Pauli wrote his second review article (1933) on the new quantum mechanics, he stated that according to some unidentified sources 'this article would certainly not be as good as the first [1926] one, but still the best in the field.'[34]

1.3. Quantum Mechanics

In his famous paper in which he invented the new quantum mechanics, Werner Heisenberg wrote: 'It has become the practice to characterize this failure of the quantum-theoretical rules [given by the "old quantum theory"] as a deviation from classical mechanics.

This characterization has, however, little meaning when one realizes that the Einstein–Bohr frequency condition (which is valid in all cases) already represents such a complete departure from classical mechanics, or rather (using the viewpoint of wave theory) from the kinematics underlying this mechanics, that even for the simplest quantum-theoretical problems the validity of classical mechanics simply cannot be maintained. In this situation it seems sensible to discard all hope of observing hitherto unobservable quantities, such as the position and period of the electron, and to concede that the partial agreement of the quantum rules with experience is more or less fortuitous. Instead it seems more reasonable to try to establish a theoretical quantum mechanics, analogous to classical mechanics, but in which only relations between observable quantities occur.'[32]
In July 1925 Heisenberg submitted his fundamental paper on quantum mechanics to *Zeitschrift für Physik.* His great idea was to retain the equation of motion or even further the Hamiltonian equations but to reinterpret the kinematical quantities or dynamical variables, like position, momentum, etc.[35] The important question was which quantities are to be substituted as dynamical variables, and Heisenberg answered it by taking the Fourier coefficients q_n of a periodic motion. These Fourier coefficients have to be replaced in a quantum theory by quantities with two indices, $q_{n,\,n-\tau}$, which enter into the Fourier expansion, and the exponential function has the form $e^{i\omega_{n,n-\tau}t}$. This *Ansatz* satisfies the frequency condition of Bohr, Planck and Einstein, and Heisenberg could derive the sum rule of Thomas and Kuhn,

$$h = 4\pi m \sum_{\tau=0}^{\infty} \Big\{ |q(n, n+\tau)|^2 \omega(n, n+\tau) - |q(n, n-\tau)|^2 \omega(n, n-\tau) \Big\}. \qquad (2)$$

Applying Eq. (2) to the anharmonic oscillator, Heisenberg obtained the correct quantization rule, which is a half-integer in the case of zero anharmonicity.

In the same paper, Heisenberg 'derived' a multiplication rule for the Fourier coefficients q:

$$q_{n,n-\beta}e^{i\omega_{n,n-\beta}t} = \sum_{\alpha} q_{n,n-\alpha}q_{n-\alpha,n-\beta}e^{i\omega_{n,n-\beta}t}. \tag{3}$$

This step aroused Born's imagination deeply and, between 15 and 19 July, he arrived at the following conclusion: '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. I found this by just simplifying the notation a little: instead of $q(n, n+\tau)$ ... I wrote $q(n, m)$, and rewriting Heisenberg's form of Bohr's quantum conditions I recognized at once its formal significance. It meant that two matrix products pq and qp are not identical. I was familiar with the fact matrix multiplication is not commutative; therefore I was not too much puzzled by this result. Closer inspection showed that Heisenberg's formula gave only the value of the diagonal elements ($m = n$) of the matrix $\mathbf{pq} - \mathbf{qp}$: it said that they were all equal and had the value $h/2\pi i$. But what were the other elements when $m \neq n$?

'Here my own constructive work began. Repeating Heisenberg's calculation in matrix notation, I soon convinced myself that the only reasonable value of the nondiagonal elements should be zero, and I wrote down the strange equation

$$\mathbf{pq} - \mathbf{qp} = \frac{h}{2\pi i}\mathbf{1}, \tag{4}$$

where $\mathbf{1}$ is the unit matrix. But this was only a guess, and my attempts to prove it failed.'[36]

'Quantum mechanics' was completed in two papers from Born's institute in Göttingen, namely: M. Born and P. Jordan, 'On Quantum Mechanics'[37] and M. Born, W. Heisenberg and P. Jordan, 'On Quantum Mechanics II.'[38] In these papers the matrix formulation and the simplest applications to physical problems, in particular the calculation of eigenvalues, was presented. Independently P. A. M.

Dirac in Cambridge developed the ideas of quantum mechanis in two contributions: 'The Fundamental Equations of Quantum Mechanics'[39] and 'Quantum Mechanics and a Preliminary Investigation of the Hydrogen Atom.'[40] In the first paper Dirac developed the operator formalism. At that time he only knew about Heisenberg's first paper,[32] i.e. the fundamental idea of noncommutativity of the product of quantum variables. In the second paper he extended his 'algebraic laws' and applied them to solve the hydrogen spectrum. However, five days later, Wolfgang Pauli — who had been very critical with respect to Born's introduction of the matrix formalism[41] — submitted a paper, 'On the Hydrogen Spectrum from the Standpoint of the New Quantum Mechanics,' to *Zeitschrift für Physik,* on 17 January 1926.[42] In this paper the problem of the hydrogen atom was completely solved, though the calculations were very tedious. In a letter to Pauli on 3 November 1925, Heisenberg remarked about this work: 'I need not assure you how much I am pleased with the new theory of the hydrogen spectrum.'[43] And finally, in another letter, dated 16 November 1925, he concluded: 'How one really integrates you have demonstrated in your hydrogen paper and all the rest is formal nuisance [*Kram*].'[44]

The Heisenberg–Born–Jordan–Dirac approach to the new quantum theory, which we might call 'algebraic' according to Dirac, rested essentially on the fact that had been realized in Heisenberg's initial fundamental paper[32]: one can retain the fundamental equations of quantum mechanics (such as the equations of motion and the Hamiltonian equations) but one has to reinterpret the dynamical variables like position, momentum, etc. In the matrix scheme they became infinite quadratic matrices. One of the important properties which these quantities have (and which no quantity in classical theory exhibits) is that they do not commute with each other. Dirac could show that the quantum-mechanical commutation relations, like Eq. (4), followed from a generalization of the classical Poisson brackets,[45] rather than from the commutation of the classical quantities. The physical measurement of a quantity — say, the momentum of an electron — reproduces one of the eigenvalues of the corresponding matrix (according to Born, Heisenberg and Jordan) or operator (according to Dirac). These eigenvalues can be calculated by transformation to the 'principal axes.' The transformations, on the other hand, correspond to canonical transformations in classical mechanics. The Heisenberg–Born–Jordan–Dirac scheme presented a complete and consistent answer to all problems of microphysics. The physical understanding of the quantum-mechanical scheme, in early 1926, was still very much in the beginning stage, when a second independent approach to the same problems was developed by Erwin Schrödinger. This approach seemed to be rather complementary, if not contradictory, to the work of the 'quantum mechanicians.'

1.4. Wave Mechanics

On 27 January 1926, ten days after the *Zeitschrift für Physik* had received Pauli's matrix-mechanical solution of the hydrogen atom, there arrived an article entitled 'Quantization as a Problem of Proper Values (Part I)' at the *Annalen der Physik*. The author, Erwin Schrödinger, established in that paper that one could treat a quantum system starting from Louis de Broglie's wave theory.[46] His first attempts had been made by demonstrating that Einstein's new gas theory[47] 'can be based on the consideration of such stationary proper vibrations, to which the dispersion law of de Broglie's phase waves has been applied.'[48] Schrödinger represented the quantum systems and, as the first example, he chose the nonrelativistic and unperturbed hydrogen atom by an equation for the wave function ψ. This equation yields stationary states for the matter wave (here the electron wave) ψ, according to a calculation of its eigenvalues,

$$H\psi = E\psi. \quad (5)$$

Here H is the generalized Hamilton function which acts in Eq. (5) as a differential operator depending on the position variable q and a gradient with respect to this position, which replaces the momentum. Under suitable conditions for the wave function ψ,[49] Schrödinger calculated from Eq. (5) the eigenvalues of the hydrogen spectrum.[50]

Schrödinger's first paper appeared in *Annalen der Physik (Leipzig)* **79**, 361 (1926). The second part of his paper, received on 23 February 1926, was in the same volume of the *Annalen*. In Part II, he went into the interpretation of his formalism and pursued the analogy presented by the wave theory of optics. 'Undulatory' or 'wave' mechanics is an extension of 'geometrical' (classical) mechanics, and the wave equation (5), which can be reformulated as

$$\text{div grad}\, \psi + \frac{8\pi^2}{h^2}(E - V)\,\psi = 0, \tag{6}$$

arises naturally from this analogy. Schrödinger immediately applied Eq. (6) to the harmonic oscillator and calculated both of the energy states E_n $(n = 0, 1, \ldots)$ and the corresponding eigenfunctions, which, apart from a constant factor, turn out to be Hermite polynomials. Other examples treated in the second communication were the various rotators, which were done here consistently for the first time.

In the following volume of *Annalen der Physik*, there appeared Schödinger's third communication on 'Quantization as a Problem of Proper Values'[52] and, finally, in the next volume the fourth communication was published.[53] In the third communication, he developed the perturbation-theoretic approach to problems which are not *exactly* soluble, but are not far removed from them.[54] He applied his new method immediately to the Stark effect and made the first attempt to calculate the intensities and polarizations of the Stark effect patterns. In his fourth communication, he extended the perturbation theory to cases which contain the time explicitly.[55]

We must mention here another paper of Schrödinger's,[56] received on 18 March 1926, in which he developed the 'Relation Between the Quantum Mechanics of Heisenberg, Born and Jordan, and that of Mine.' Although there were 'extraordinary differences between the starting points and the concepts of Heisenberg's quantum mechanics and the theory which has been designated as "undulatory" or "physical" mechanics, and has lately been described here, it is very strange that these two new theories agree *with one another* with regard to the known facts, where they differ from the old quantum theory.'[57] And he proceeded: 'In what follows the very intimate *inner connection* between Heisenberg's quantum mechanics and my wave mechanics will be disclosed. From the formal mathematical standpoint one might well speak of the *identity* of the two theories.'[58] In particular, he proved: 'The solution of the natural *boundary-value problem* of this differential equation [the Schrödinger equation] is *completely equivalent* to the solution of Heisenberg's algebraic problem.'[59]

Schödinger's work presented two aspects. Though he started from the opposite point of view with respect to the algebraic 'quantum mechanicians,' namely from a *continuum theory,* he presented the complete equivalence of the results. This had two consequences. First, one could now use the much more workable system of Schödinger's differential equations to calculate actual eigenvalue problems, intensities, and so on. Second, and this aspect presented a great challenge to the Göttingen school: What was the meaning of the wave function in particular, and what did Schödinger's continuum approach mean in general? Max Born was to give the answer to this challenge. He 'interpreted' the wave function and Heisenberg completed the quantum-mechanical description of Nature.

1.5. The Interpretation of Microphysics

The understanding of microphysics was obtained in two distinct steps, each of which might today seem to us independently satisfactory. First, Max Born analyzed the scattering and collision processes in terms of wave mechanics and arrived at the interpretation of the wave amplitude as a probability amplitude. By this step he obtained, according to his own judgment, a complete description of microscopic phenomena, and one could deduce an interpretation in terms of suitably restricted classical concepts. In particular, a specific case of the more general uncertainty relations follows from it. The uncertainty relations were derived by Werner Heisenberg, who started from matrix mechanics and the transformation theory of Dirac. This form of quantum theory is less conducive to calculations than is Schödinger's wave mechanics, although in some sense it is more fundamental conceptually. Finally, Niels Bohr developed the philosophical language to talk about phenomena in microphysics, at the center of which stands the principle of complementarity. It seemed to Bohr that this language was applicable to a wider range of phenomena than those of atomic mechanics, namely all those in which natural contradictions arise when they are dealt with in the ordinary classical and macroscopic language.

1.5.1. *The Probability Interpretation of the Wave Function*

Max Born wrote: 'The matrix form of quantum mechanics founded by Heisenberg and developed by him together with Jordan and the author of this report, starts from the idea that an exact description of the phenomena in space and time is not possible at all and therefore is satisfied in obtaining relations between observable quantities, which can be interpreted only in the classical limit as properties of motions. Schödinger, on the other hand, seems to ascribe to the waves which he considers with de Broglie as the carriers of atomic processes a reality of the same kind as light waves do possess; he tries "to construct wave groups which have small extensions in all directions" and which should apparently represent the moving particle directly.

'None of these concepts seem to me to be satisfactory. I shall try at this place to give a third interpretation and to test its usefulness with scattering phenomena. For this purpose I shall start with a remark of Einstein's on the relation between wave fields and light-quanta; he said in effect that waves exist only to guide the path of the corpuscular light-quanta and he talked in that sense about a "ghost field." This determines the probability that a light-quantum which carries energy and momentum follows a certain path; to the field, however, no energy and no momentum belongs.'[60]

In four papers, Born developed in the year 1926 the quantum theory of scattering processes. In the first, which he wrote together with Norbert Wiener during his visit to the United States, he extended the formulation of quantum laws to nonperiodic phenomena.[61] Since this paper was submitted on 5 January 1926, i.e. before Schrödinger sent his first communication to *Annalen der Physik*, Born and Wiener did not know about the wave-mechanical formulation but extended the matrix representation of quantum mechanics to the more general representation by linear operators. This operator formalism, which was similar to the one developed in detail by Paul Dirac, could then also describe nonperiodic systems. Evidently, Born and Wiener were also not aware of the prior publications of P. A. M. Dirac.

By the time of his second paper, submitted on 25 June, Born had learned about Schrödinger's wave mechanics and he used it to formulate the scattering problem.[62] He wrote: 'Many people assume that the problem of transitions cannot be treated by the quantum mechanics in the form obtained thus far, and that one requires new concepts to do that. I myself arrived, impressed by the completeness of the logical structure of quantum mechanics, at the conjecture that the theory must be complete and should also be able to deal with the problem of transitions. I believe that I have now succeeded in giving a proof.'[63]

For all practical purposes Schrödinger's wave mechanics is appropriate. 'From the different forms of the theory, only the one due to Schrödinger is applicable and therefore I would like to consider it as the deepest formulation of quantum laws.[64] The reason was that far from the point of impact and far after the impact the wave description — say, by plane waves — is particularly simple. But if the incident wave is a plane wave, the outgoing wave is a superposition of plane waves with coefficients or amplitudes $\Phi_{\underset{\tau}{mn}}(\alpha, \beta, \gamma)$. Born noted that 'If one tries to interpret this result in the particle language then only one interpretation is possible: $\Phi_{\underset{\tau}{mn}}(\alpha, \beta, \gamma)$ determines the probability for the fact that the electron coming from the *z*-direction is scattered into the direction given by *a, b, g* (with the phase change given by *d*), and its energy *t* increases by a quantum $h\nu_{nm}^{a}$ obtained from the atomic energy.' At this point, Born appended an important footnote: 'Remark added in proof: A more accurate consideration demonstrates that the probability is proportional to the square of the quantity F.'[65]

Schödinger's wave mechanics therefore answers the question for the effect from an impact in a well-defined sense; but this answer does not consist in a causal relation. One does *not* obtain an answer to the question 'What is the state after collision?' but only to the question 'How probable is a given effect of the collision?' The whole question of determinacy, Born noted, followed from here. He denied the existence of determinacy in the microscopic world.

In his third paper, submitted on 21 July 1926, Born gave a full account of his new theory of scattering processes and the physical interpretation of the wave function.[66] In this paper, entitled 'The Quantum Mechanics of Scattering Processes,' Born treated aperiodic motions in general.[67] From the free motion of a wave packet, he derived the fact that

$$\int_{-\infty}^{+\infty} |\psi(x)|^2 dx = |\overline{C}|^2 \frac{\Delta p}{h}. \tag{7}$$

'Thus one obtains the result that a cell of linear dimension $\Delta x = 1$ and of extension in momentum of $\Delta p = h$ has the weight 1, in agreement with the *Ansatz* of Sackur and Tetrode, which proved to be true in many cases by experience, and that $|C(k)|^2$ is the frequency for a motion with momentum $p = h/2\pi\ k$.'[68] This remark already came very close to the uncetainty relation and, in fact, we shall observe a similar step in Heisenberg's considerations.

Born concluded his paper by noting: 'But it remains for anbody who is not content [with this indeterministic interpretation] to assume that further parameters exist which have not yet been introduced into the theory, which fully determine the individual result. In classical mechanics these parameters are the "phases" of the motion, e.g., the coordinates of the particles at a certain time. It seemed to me at first improbable that one can introduce quantities which correspond to these phases without forcing them into the new theory, but [Jakov] Frenkel has told me that this is perhaps possible. Be that as it may, this possibility would not change the practical indeterminacy of the scattering processes, since one cannot give the values for the phases, and the results from this theory would be expressed in the same formulae as given "without phases" proposed here.'[69]

In the last paper of 1926, Born finally generalized Ehrenfest's adiabatic hypothesis for scattering processes.[70]

1.5.2. *The Uncertainty Relations*

As Heisenberg noted: 'The quantum mechanics resulted from the attempt to abandon the usual kinematic concepts and replacing them by relations between concrete experimentally observable quantities. However, since we have succeeded, the mathematical scheme of quantum mechanics need not be revised. A revision of the space–time geometry for small distances and time intervals would also not be necessary since we may approximate the classical laws arbitrarily closely by choosing large enough masses in the quantum-mechanical laws. However, from the fundamental equations of quantum mechanics it seems apparent that the kinematical and mechanical concepts have to be revised. Given a definite mass *m*, we are used to talking about position and velocity of its center of mass. In quantum mechanics, however, the relation $pq - qp = h/2\pi i$ must hold between mass, position and velocity. Therefore, we have to be careful about the uncritical application of the words "position" and "velocity." '[71]

In spring 1927, Werner Heisenberg submitted a paper to *Zeitschrift für Physik* which, according to Wolfgang Pauli, finally 'brought daylight into quantum mechanics.'[72] Heisenberg described the origin of the ideas which completed the physical interpretation of quantum mechanics in his article 'Memories of the Time of Development of Quantum Mechanics' in the memorial volume of Wolfgang Pauli.[73] He wrote: 'At that time, in fall 1926 the uncertainty relations gained form in the exchange of letters between Pauli and myself. In a letter dated 28 October 1926 the sentence was contained: "In the wave picture the equation $pq - qp = -i\hbar$ always expresses the fact that it makes no sense to talk about a monochromatic wave at a definite instant of time (or in a very short time interval). It also does not make sense to talk about the position of a corpuscle of a definite velocity. If one does not take velocity and position too accurately, one can make good sense of it." '[74] In his reply, Pauli repeated the old argument about dividing the phase space into cells of magnitude h^3 for three degrees of freedom and that one cannot determine a state of a particle more accurately than by assigning the phase cell. However, this was not enough, and Heisenberg replied: 'If you are able to assume the exact position of the walls of the phase cells and can determine the number of particles in each cell, then could you not obtain the number of atoms in an arbitrarily small cell by choosing its walls close to the original position? Then, does it make sense physically to choose definite cell walls? Perhaps we may only assume the relative position of two cell walls, but not the position of a definite cell wall.'[75] Three months of intensive discussions between Heisenberg and Bohr passed before Heisenberg sent Pauli a 14-page letter, which almost held the content of his later paper 'On the Perceptual Content of Quantum Kinematics and Mechanics.' In this paper Heisenberg developed the uncertainty relations for specific examples, like the Compton effect, by using Dirac's transformation theory. He obtained the famous relation

$$\Delta p_x \Delta q_x \sim h, \tag{8}$$

for the accuracy Δp_x and Δq_x, with which one can determine simultaneously the momentum p_x and the position q_x of a microscopic particle.

Heisenberg recalled the following about this paper: 'This paper, a few days later, I then also sent to Pauli for his critique, so that I could show Bohr the paper already refereed by Pauli when he reurned [from his vacation in Norway]. However, Bohr did not completely agree with certain points of this paper; thus it was sent, not before some time had elapsed, with important improvements for publication. Meanwhile Bohr had also developed the concept of complementarity, conceived by himself, so that the physical content of quantum theory was clearly apparent in the same manner from different starting points. If differences in the concepts still existed, then they referred to different starting points or to a different language but not anymore to the physical interpretation of the theory. Concerning this interpretation one had now gained complete clarity, and Pauli was the first one outside the inner Copenhagen circle who agreed without reservation with the new interpretation of the formulation to which he had contributed so greatly.'[75]

The first public presentation of the new interpretation was due to Niels Bohr, who talked about 'The Quantum Postulate and the Recent Development of Atomic Theory' at the International Congress of Physicists in Como in September 1927.[77] In his talk, Bohr actually turned the physical interpretation into the philosphical language of complementarity. The particle and the wave descriptions of matter, according to him, formed two complementary but not contradictory aspects of the same microphysical object, and he chose as the first example the uncertainty relation between the energy and time of a wave motion,

$$\Delta t \, \Delta E \geq h, \tag{9}$$

which arises from the 'classical' equation

$$\Delta t \, \Delta \nu \geq 1 \quad (\nu = \text{frequency}). \tag{10}$$

Then he dealt with the measurement process in quantum theory. Heisenberg had expressed the impossibility of arbitrarily accurate simultaneous measurements of conjugate quantities like the position q_x and the momentum p_x of a microscopic particle. Bohr pointed out that the essence was that 'A closer investigation of the possibilties of definition would still seem necessary in order to bring out the general complementary character of the description. Indeed, a discontinuous change of energy and momentum during observation [e.g. of the position] could not prevent us from ascribing accurate values to the space–time coordinates, as well as to the momentum–energy components before and after the [measurement] process. The reciprocal uncertainty which always affects the values of these quantities is, as will be clear from the preceding analysis, essentially an outcome of limited accuracy with which changes in energy and momentum can be defined, when the wave-fields used for the determination of the space–time coordinates of the particle are sufficiently small.'[78] After considering several examples, Bohr concluded: 'The experimental devices — like opening and closing the apertures, etc. — seen to permit only conclusions regarding the space–time extension of the associated wave-fields.'[79] About observations, in general, Bohr remarked: 'Strictly speaking, the idea of observation belongs to the causal space–time way of description. Due to the general character of the [uncertainty] relation, however, this idea can be consistently utilized also in the quantum theory, if only the uncertainty expressed through this relation is taken into account... . Indeed, it follows from the above considerations that the measurement of the positional coordinates of a particle is accompanied not only by a finite change in the dynamical variables, but also the fixation of its position means a complete rupture in the causal description of its dynamical behavior, while the determination of its momentum always implies a gap in the knowledge of its spatial propagation. Just this situation brings out most strikingly the complementary character of atomic phenomena which appears as an inevitable consequence of the contrast between the quantum postulate and the distinction between object and the agency of measurement, inherent in our very idea of observation.'[80]

Bohr then turned to a consideration of matrix and wave mechanics. 'In fact, wave mechanics, just as the matrix theory, on this vew represents a symbolic transcription of the problem of motion of classical mechanics adapted to the requirements of quantum theory and only to be interpreted by an explicit use of the quantum postulate. Indeed, the two formulations of the interaction problem might be said to be complementary in the same sense as the wave and particle idea in the description of the free individual.[81] From this remark there arose Bohr's general complementary philosophy, which properly allowed one to deal with the phenomena in microphysics.

Though Bohr did not participate in the formulation of the new quantum theory, and especially did not apply it to treat any example or unsolved problem, because of his deep insight he became the representative of the young generation around Heisenberg. At the fifth Solvay Conference in Brussels in 1927, it was Bohr who defended the new quantum theory against the attacks of 'conservative' scientists, in particular against the vigorous and unceasing efforts of Albert Einstein, who constructed examples that should contradict the new theory and the philosophical consequences drawn from it.

2

'The Crisis in Theoretical Physics'

In a lecture, 'On the Present Crisis of Theoretical Physics,' delivered during a visit to Japan in 1922, Albert Einstein said: 'Many times one has remarked that in the present state of knowledge the representation of the laws of Nature by differential equations seems to be dubious... . To cope with the quantum relations a new mathematical language seems to be necessary; at any rate it seems to be without sense to express the laws by a combination of differential laws and integral conditions as we do today. Once more the foundations of theoretical physics are shaken and experience calls for a higher level to express the laws. When shall we receive the saving idea? Happy will be those who might live to see it.'[82]

In the early 1920s, Einstein talked on several occasions about a crisis in theoretical physics.[83] Clearly, there was the existence of the energy quantum and new quantum effects that needed to be explained. However, by that time all the available quantum effects had been verified, including the corpuscular nature of the light-quantum. Louis de Broglie had further successfully proposed the hypothesis that all material particles possess a wave nature, thus putting Einstein's 'heuristic viewpoint' on a general level. But, in principle, no theory was available that could claim to be complete and be able to describe the laws of the atoms and of radiation consistently.

Then, in 1924, S. N. Bose proposed a statistical method which could deal with both (corpuscular and wave) natures of the light-quantum (photon), and Einstein was able to extend this statistics to the quantum theory of ideal gases. With this method, for the first time, quantum effects could be described entirely correctly and quantitatively. Unfortunately, the only example besides the blackbody radiation, the case of ideal gases, did not offer at that time the possibility of verifying Einstein's theory quantitatively. But Einstein was certain that his ideal gas theory described real phenomena. Still, it did not provide an answer as to the nature and meaning of the quantum for which both Einstein and Planck had been looking.

The new theory had been developed on the basis of rather different ideas than the ones Einstein liked in those days. And though he sympathized with Schrödinger's approach in many respects because the wave seemed to represent the reality far better than the transformation matrices of the Göttingen–Cambridge school, he did not consider it as the final solution either. The Einstein of the late 1920s became for the first time an authority who was at variance with the progressive ideas of the younger generation. In his discussions with Niels Bohr and (later on) with Max Born, Einstein criticized the results and interpretation of the new quantum theory. At the same time he seemed to abandon his pragmatic position which had led him in earlier years to so many fruitful points of view and to form a dogmatic philosophy. It would seem to be worthwhile, as an introduction to the new period in Einstein's work concerning the quantum theory, to consider the development of his philosophical ideas in greater detail.

2.1. Einstein's Early Readings

As he recalled later in life, 'At the age of 12–16 I familiarized myself with the elements of mathematics together with the principles of differential and integral calculus. In doing so I had the good fortune of hitting upon books which were not too particular in their logical rigor, but which made up for this by permitting the main thoughts to stand out clearly and synoptically. This occupation was, on the whole, truly fascinating; climaxes were reached whose impression could easily compete with that of elementary geometry — the basic idea of analytical geometry, the infinite series, the concepts of differential and integral calculus. I also had the good fortune of getting to know the essential results and methods of the entire field of natural sciences in an excellent popular exposition, which limited itself almost throughout to qualitative aspects (Bernstein's *Popular Books on Natural Science*, a work of 5 or 6 volumes), a work which I read with breathless attention. I had also already studied some theoretical physics when, at the age of 17, I entered the Polytechnic Institute of Zurich [ETH] as a student of mathematics and physics.'[84]

The young Einstein used much of his time to study important books on physics and science in general. He read the works of Kirchhoff, Helmholtz, Maxwell, Boltzmann and Hertz.[85] In 1897, Michele Besso, a more advanced student at the ETH and a friend of Einstein's, had introduced him to Ernst Mach's book *The Science of Mechanics: A Critical and Historical Account of Its Development.*[86] Einstein wrote in his *Autobiographical Notes*: 'We must not be surprised, therefore, that, so to speak, all physicists of the last century saw in classical mechanics a firm and final foundation for all physics, yes, indeed, for all natural science, and that they never grew tired in their attempts to base Maxwell's theory of electromagnetism, which, in the meantime, was slowly beginning to win out, upon mechanics as well. Even Maxwell and H. Hertz, who in retrospect appear as those who demolished the faith in mechanics as the final basis of all physical thinking, in their conscious thinking adhered throughout to mechanics as the secure basis of physics. It was Ernst Mach who, in his *History of Mechanics*, shook this dogmatic faith; this book exercised a profound influence upon me in this regard while I was a student. I see Mach's greatness in his incorruptible skepticism and independence; in my younger years, however, Mach's epistemological position also influenced me very greatly, a position which today appears to me to be essentially untenable.'[87]

In this book Mach examined the historical development of mechanics. In it, the main role was played by a critical review of Newton's ideas, in particular the concepts of mass and absolute space and time. With respect to the concept of mass, Mach formulated a new definition starting from Newton's *third law of mechanics* which allowed one to measure masses. By this derivation he initiated a method which was later on elaborated by P. W. Bridgman in his theory of *operationalism.*[88]

With respect to the concepts of absolute space and time, Mach rejected them because they were not observable. In a theory, only those concepts should play a role that are observable, at least potentially. Thus Mach became one of the founders of *positivism.*

Further on, Mach stated another aspect of his philosophy of science in the following sentence: 'Science, itself, therefore, may be regarded as a minimal problem, consisting of the completest possible treatment of facts with *the least possible expenditure of thought.*'[89,90]

Not only did Mach influence Einstein, but his philosophy influenced the so-called Vienna Circle, of which, for example Philip Frank, was a member. Among the more important adherents were Ludwig Wittgenstein and R. Carnap. It is interesting that Mach influenced Einstein in a way which he did not accept himself in his later years. Following Mach's criticism of absolute space and time, Einstein developed in 1905 a theory which derived these concepts and served as a new basis of mechanics: the special theory of relativity. Mach had written: 'I do not consider the Newtonian principles as completed and perfect; yet, in my old age, I can accept the theory of relativity as little as I can accept the existence of atoms and other such dogma.'[91]

This brings us to another point — Mach's rejection of atomism. 'The atomic theory plays a part in physics similar to that of certain auxiliary concepts in mathematics; it is a mathematical *model* for facilitating the mental reproduction of facts.'[92] It might have been essentially these statements with respect to the useful and proved 'heuristic viewpoints' as expressed by the principle of relativity and the assumption of the molecular structure of matter which brought Einstein later to the recognition that Mach's philosophical points of view were 'essentially untenable.' In fact, in 1905, Einstein called his theories of special relativity and light-quanta 'heuristic viewpoints.' By the year 1910 the consequences from the special theory of relativity and from the atomistic structure of matter had been proved experimentally, and they had become accepted theories, on which basis one could speculate further. Mach denied that such a speculation would be useful; Einstein, on the other hand, based his work on such speculations and criticized Mach: 'For he did place in the correct light the essentially constructive and speculative nature of thought and more especially scientific thought; in consequence of which he condemned on precisely those points where its constructive–speculative character unconcealably comes to light, as for example in the kinetic atomic theory.'[93]

2.2. The Basic Principles in Einstein's Early Work

Einstein's first two papers were concerned with consequences from thermodynamics. The first published paper presented a specific application to the phenomena of capillarity, and in the second paper a study of electric potential differences between metals and solutions of their dissociated salts was dealt with. The aim of these two papers was, however, to obtain the law of molecular attraction, perhaps in a similar simple form to the law of gravitational attraction. We may consider these attempts the first indication of Einstein's search for a unified theory.

After these two 'worthless beginner's works' (as he described them), Einstein turned to other topics suggested to him by his reading of Boltzmann's *Lectures on Gas Theory.* He completed Boltzmann's foundations of statistical thermodynamics, in many respects similar to the results achieved by Josiah Willard Gibbs (with whose work in this field he became familiar only later on). The consequences from this work, the fluctuation phenomena as present in the Brownian motion and light-quanta, led to the proof of the molecular hypothesis, in particular through the experiemental work of Jean Perrin.

In his *Autobiographical Notes*, Einstein recalled: 'The most fascinating subject at the time that I was a student was Maxwell's theory. What made this theory appear revolutionary was the transition from forces at a distance to fields as fundamental variables... .

'What rendered the insight into the essence of electromagnetic theory so much more difficult at that time was the following peculiar situation. Electric or magnetic "field intensities" and "displacements" were treated as equally elementary variables, empty space as a special instance of a dielectric body. *Matter* appeared as the bearer of the field, not *space*. By this it was implied that the carrier of the field could have velocity, and this was naturally to apply to the "vacuum" (aether) also. Hertz's electrodynamics of moving bodies rests entirely upon this fundamental attitude.

'It was the great merit of [Hendrik Antoon] Lorentz that he brought about a change here in a convincing fashion. In principle a field exists, according to him, only in empty space. Matter — considered as atoms — is the only seat of electric charges; between the material particles there is empty space, the seat of electromagnetic field, which is created by the position and velocity of point charges which are located on the material particles... .

'If one views this phase of the development of theory critically, one is struck by the daulism which lies in the fact that the material point in Newton's sense and the field as continuum are used as elementary concepts side by side. Kinetic energy and field energy appear as essentially different things. This appears all the more unsatisfactory inasmuch as, according to Maxwell's theory, the magnetic field of a moving electric charge represents inertia. Why not then *total* inertia? Then only field-energy would be left, and the particle would be merely an area of special density of field-energy. In that case one could hope to deduce the concept of the mass-point together with the equations of motion of the particles from the field equations — the disturbing dualism would have been removed.

'H. A. Lorentz knew this very well. However, Maxwell's equations did not permit the derivations of the equilibrium of the electricity which constitutes a particle. Only other, nonlinear field equations could possibly accomplish such a thing. But no method existed by which this kind of field equations could be discovered without deteriorating into adventurous arbitrariness. In any case, one could believe that it would be possible by and by to find a new and secure foundation for all of physics upon the path which had been successfully begun by Faraday and Maxwell.'[94]

It was Einstein's specific contribution to connect this problem in the foundation of electrodynamics of matter to the other problem discovered by Max Planck in 1900 — the existence of elementary quanta of energy in the theory of radiation. In his first public lecture as a physicist, Einstein stated at Salzburg in 1909: 'The theory of relativity has changed our concepts about the nature of light insofar as it considers light not as the consequence of the states of hypothetical aether but as something existing by itself similar to matter. It [light] shares further, according to this theory, the property with a corpuscular theory of light to transmit inert mass from the absorbing to the emitting bodies. Concerning our concepts of the structure of radiation, in particular of the distribution of energy within the radiation space, the theory of relativity did not change anything. I hold, however, the opinion that we stand with respect to this aspect of the problem at the beginning of a development which cannot yet be overlooked but is most remarkable.'[95]

Einstein then proceeded by remarking: 'The constitution of radiation seems, therefore, to be different from that which follows from our undulatory theory.'[96] In particular the undulatory theory could not explain the elementary phenomena of the creation and transmutation of light. And he advanced the following prediction: 'Be that as it may, the concept seems to me to be most natural, that the existence of electromagnetic fields of light is connected as much to singular points as the existence of electrostatic fields to the electron theory. It cannot be excluded that in such a theory the total energy of the electromagnetic field might be regarded as localized in those singularities, exactly as in the old action-at-a-distance theory. For instance, I consider each such singular point as being surrounded by a field of force, which possesses essentially the character of a plane wave, whose amplitude decreases with the distance from the singular point. If many such singularities are present in distances which are small with respect to the range of the field of force of a singular point, then the fields of force will superpose and form in total an undulatory field of force which does deviate perhaps only a little from the undulatory field in the sense of the present electromagnetic theory of light. That one cannot consider such a picture as valuable as long as it does not lead to an exact theory need not be emphasized particularly. I just wanted to illustrate by it briefly the fact that both structural characteristics (undulatory structure and quantum structure) which are connected with the radiation theory according to Planck's law should not be considered as being incompatible.'[97]

2.3. The Discussion of the Light-Quantum with Niels Bohr

John Clarke Slater, a young physicist from Harvard — where he took his Ph.D., then traveled on a fellowship first to Cambridge, England, and from there to Copenhagen, where he worked on the theory of radiation, making an attempt to bridge the dual aspects of light (wave and particle pictures) — wrote in an article published in *Nature*: 'In the attempt to give a theoretical interpretation of the mechanism of interaction between radiation and matter, two apparently contradictory aspects of this mechanism have been disclosed. On the one hand, the phenomena of interference, on which the action of all optical instruments essentially depends, claim an aspect of continuity of the same character as that involved in the wave theory of light, especially developed on the basis of the laws of classical electrodynamics. On the other hand, the exchange of energy and momentum between matter and radiation, on which the observation of optical phenomena ultimately depends, claims discontinuous features. These have even led to the introduction of the theory of light-quanta, which, in its extreme form, denies the wave contribution of light. At the present state of science it does not seem possible to avoid the formal character of the quantum theory which is shown by the fact that the interpretation of atomic phenomena does not involve a description of the mechanism of the discontinuous processes, which in the quantum theory of spectra are designated as transitions between stationary states of the atom. On the correspondence principle it seems nevertheless possible, as it will be attempted to show in this paper, to arrive at a consistent description of optical phenomena by connecting the discontinuous effects occurring in atoms with the continuous radiation field in a somewhat different manner from what is usually done. The essentially new assumption introduced in §2 that the atom, even before a process of transition between two stationary states takes place, is capable of communication with distant atoms through a virtual radiation field, is due to Slater.'[98]

Originally, Slater's endeavor had been to obtain in this manner a harmony between the physical pictures of the electromagnetic theory of light and the theory of light-quanta by coupling transitions of emission and absorption of communicating atoms together in pairs. It was pointed out by Hendrik Kramers, however, 'that instead of suggesting an intimate coupling between these processes, the idea just mentioned leads rather to the assumption of a greater independence between transition processes in distant atoms than hitherto perceived. The present paper is the result of a mutual discussion between the authors [Bohr, Kramers and Slater] concerning the possible importance of these assumptions for the elaboration of the quantum theory, and may in various respects be considered as a supplement to the first part of a recent treatise by Bohr,[99] dealing with the principles of the quantum theory, in which several of the problems dealt with here are treated more fully.'[100]

In the paper 'The Quantum Theory of Radiation,' written jointly by Bohr, Kramers and Slater, the authors attempted to find a unique description of microscopic phenomena at the expense of renouncing the principle of conservation of energy and momentum: 'As regards the occurrence of transitions, which is the essential feature of the quantum theory, we abandon on the other hand any attempt at a causal connection between the transitions in distant atoms, and especially a direct application of the principles of conservation of energy and momentum, so characteristic for the classical theories.'[101] And they proceeded to claim that there is as yet no experimental evidence to test these ideas. Of course, the Compton effect had been discovered experimentally and described successfully by the concept of the light-quanta of Einstein, using strict energy and momentum conservation in elementary processes.[102] But Bohr and his collaborators claimed that one could represent these results also by means of a statistical conservation of energy and momenta in the elementary processes, contrary to the idea of Einstein, Ehrenfest and Pauli,[103] who retained complete conservation or — as Bohr called it — causality.

In fact, the Compton effect had brought with it the verification of Einstein's 1905 heuristic point of view: the light-quantum. It is rather remarkable that Einstein did not draw this consequence from his conception earlier, but Arthur Holly Compton and Peter Debye developed it independently in 1923. Einstein had struggled for many years with attempts at finding a crucial experimental test. In December 1921, he had proposed an experiment in a communication to the Prussian Academy.[104] Einstein developed a tricky experiment by means of which he thought one could test the question whether the frequency of light emitted from atoms (the canal rays) did change with the angle θ between the direction of observation and the propagation of the source. He concluded that the wave theory would predict a dependence (Doppler effect) on the angle, but the light-quantum theory did not. Bothe and Geiger, who carried out the experiment, seemed to prove according to Einstein the light-quantum nature of light.[105] Einstein's derivation and conclusion was attacked by his friends Paul Ehrenfest and Max von Laue.[106] In any case, it turned out that the whole experiment was not conclusive, or, more accurately, that Einstein's assumption contained an error, in that both the light-quantum and light-wave emission should show Doppler effect.[107]

Fortunately, a little later Compton came up with his experiment. And the common belief of most scientists was that now the light-quantum concept had been proved.[108] Still, Niels Bohr was not convinced and took up an idea which he had already expressed as early as 1921 in a lecture at the third Solvay Conference on Physics.[109] In this lecture, he first expressed doubts about the principles of conservation of energy and momentum in order to avoid the necessity of light-quanta.[110] He repeated his ideas in greater detail in the paper 'On the Application of the Quantum Theory to Atomic Structure, Part I':[111] he emphasized the fact that the concept of light-quanta was at variance with the problem of interference phenomena. 'A general description of the phenomena, in which the laws of conservation of energy and momentum retain in detail their validity in their classical formulation, cannot be carried through.'[112]

Niels Bohr was not isolated in his deviation from the idea of strict conservation of energy. Sommerfeld, in his 1922 edition of *Atmomic Structure and Spectral Lines*, had expressed similar ideas. In late 1923, John Slater arrived in Copenhagen and brought with him the idea of a 'virtual' field of radiation existing together with stationary states and causing the possible quantum transitions. This virtual field, by its very existence, could provide a statistical conservation of energy and momenta. But Bohr and Kramers thought differently: they preferred an altogether statistical conservation and actually formulated the paper.[113] Slater intended to use the light-quantum as the central concept, but Bohr and Kramers decided to eliminate it altogether.[114] Pascual Jordan, in his thesis, also tried to abandon Einstein's light-quantum theory.[115]

However, the experiments of Walther Bothe and Hans Geiger, in which they measured the coincidence between recoil electrons and the scattered X-rays, settled the question in favor of the Einstein–Compton–Debye theory.[116] A. H. Compton and A. W. Simon checked the relationship between the scattering angle of the X-rays and the recoil of the electrons. They found a strong correlation,[117] in contrast to the prediction of Bohr, Kramers and Slater.

Already in May 1924 Einstein wrote to Ehrenfest: 'I reviewed the Bohr, Kramers, Slater paper at our colloquium the other day. This idea is an old acquaintance of mine, but I don't consider it to be the real thing. Principal reasons:

(1) Nature seems to adhere to the conservation laws (Franck–Hertz experiments, Stokes's rule). Why should action-at-a-distance be an exception?
(2) A cavity with reflecting walls containing radiation, in empty space that is free of radiation, would have to carry out an ever-increasing Brownian motion.
(3) A final abandonment of strict causality is very hard for me to tolerate.
(4) One would also almost have to require the existence of a virtual acoustic (elastic) radiation field for solids. For it is not easy to believe that quantum *mechanics* necessarily requires an electrical theory of matter as its foundation.

(5) The occurrence of ordinary scattering (not at the proper frequency of the molecules), which is above all standard for the optical behavior of bodies, fits badly into this scheme.'[118]

The experiments of Bothe and Geiger, on the one hand, and of Compton and Simon, on the other, settled the question in favor of Einstein: 'The results do not appear to be reconcilable with the view of the statistical production of recoil and photoelectrons proposed by Bohr, Kramers and Slater. They are, on the other hand, in direct support of the view that energy and momentum are conserved during the interaction between radiation and individual electrons.'[119] In a letter to Ehrenfest on 18 August 1925, Einstein remarked: 'We both had no doubts about it.'[120] Bohr also had to agree that the Bohr–Kramers theory had failed.[121] A more drastic change in the foundation of the theory had to occur: a genuine quantum mechanics had to be developed.

2.4. Does Field Theory Present Possibilities for the Solution of the Quantum Problem?

Under the heading of this question, Albert Einstein communicated to the Prussian Academy of Sciences a summary of the current problems of the quantum theory of the day. In his mind, they concentrated on the central question of the dual nature of radiation, which Einstein himself had fully realized at least since 1909. But, since about the same time, he had put his main efforts into the development of a general field theory of gravitation. This was, on the other hand, a natural enlargement of his 1905 'heuristic point of view' concerning moving systems. On the other hand, it should provide a deeper understanding of the problem of matter. The molecular structure of matter had been fully established by that time. Similarly, the granular structure of light also appeared to be quite clear in Einstein's mind. The problem was how to incorporate the 'elementary quanta' or molecules of matter and light into a single theory, because according to Einstein's mass–energy equivalence there was no difference between matter in the form of material particles or radiation. This was the great question. Perhaps it could be resolved by a unified theory of gravitation? Thus he posed the quantum problem in the context of a field theory, as follows: 'The initial state of an electron circulating around a hydrogen nucleus cannot be chosen freely, for this choice has to correspond to the quantum conditions. In general: not only the evolution in time but also the initial state is subject to laws.

'Can this knowledge about the phenomena of Nature, which we ought to consider as quite general, be incorporated into a theory founded on partial differential equations? Of course, we just have to "over-determine" the field variables by equations. That means the number of differential equations has to be larger than the number of field variables determined by it.'[122]

Before 1910, Einstein had used most of his time to deal with the 'quantum problem': less than a dozen out of 35 contributions were devoted to the principle of relativity and, in most cases, they were smaller papers or notes. The center of his interest up to that time lay in the quantum domain. This ratio changed completely in the following years. Only seldom, and mostly in short notes, did he return to the quantum problems. Besides some applications such as the one to the laws of photochemical equivalence, only two contributions to the quantum problem were outstanding: these were the 1916/17 papers which introduced the 'Einstein A and B coefficients,' and the 1924–25 communications on the extension of Bose's new statistics to the quantum theory of ideal gases. His emphasis now was on the problems of gravitational field theory and its extensions.

From this fact one might draw two different conclusions. One is that Einstein got stuck with the quantum problem and wished to avoid it for some time; as he wrote to Sommerfeld in October 1912: 'But I assure you that I cannot tell you anything new about the quantum problem that is of interest... . I now work only on the problem of gravitation.'[123] On the other hand, his frequent return to the quantum problem, in particular in 1916 and later on, in 1924, does not support the opinion that he had abandoned the quantum problem altogether. Thus one might be inclined to believe that Einstein tacitly assumed the work in field theory of gravitation as a consequence of his endeavors in the fundamental question of 'elementary quanta.' From there he finally expected an answer to the great problems of fundamental physics. And the experience with this theory told him not to consider the finally achieved quantum mechanics as *the* real thing. It would be worthwhile to go over Einstein's paper on relativity and to examine the indications and ideas that might be of relevance for the quantum problem.

2.4.1. *A New Heuristic Viewpoint*

In his contribution to James Clerk Maxwell's commemoration volume, Einstein wrote: 'Since Maxwell's time, Physical Reality has been thought of as represented by continuous fields, governed by partial differential equations, and not capable of any mechanical interpretation. This change in the conception of Reality is the most fruitful that physics has experienced since the time of Newton; but it must be confessed that the complete realization of the programme contained in this idea has so far by no means been attained. The successful physical systems that have been set up since then represent rather a compromise between these two programmes [of Newton and of Maxwell], and it is precisely this character of compromise that stamps them as temporary and logically incomplete, even though in their separate domains they have led to great advances.

'Of these, Lorentz's Theory of Electrons must first be mentioned, in which the field and the electric corpuscles appear side by side as complementary elements in the comprehension of reality. Then followed the Special and General Theory of Relativity, which, although based entirely on field-theory considerations, have not yet been able to dispense with the independent introduction of material particles and total differential equations.'[124]

After his two epoch-making fundamental papers, one concerning the heuristic viewpoint in the creation and transformation of light,[125] and the other on the random (Brownian) motion of particles in fluids,[126] Einstein contributed another work of the greatest importance to the 'Einstein Volume' of *Annalen der Physik*. In this longest of all three papers, entitled 'The Electrodynamics of Moving Bodies,'[127] he united Maxwell's equations with the principle of relativity. Although this paper did not seem to have anything to do with the question of elementary quanta, in Einstein's mind it was linked with this topic, for it was in this paper that the first step had been taken to unite electron theory and the field theory of Maxwell's equations.

The two principles on which this work was based, i.e. the constancy of the velocity of light and the principle of relativity of coordinate systems moving with uniform velocity, had immediate consequences: 'If a body emits the energy L in the form of radiation, then its mass is decreased by L/V^2 [V being the velocity of light]. At this point it is obviously essential that the energy pulled out of the body is transformed into the energy of radiation, thus we led to the general conclusion: The mass of a body is a measure of its energy changed in the same sense by $L/9 \times 10^{20}$, where the energy is measured in ergs and the mass in grams.[128] In a later work he extended his considerations: 'In this paper I now wish to show that the law [namely that the mass of a body depends on its energy content] is the necessary and sufficient condition for the validity of the law of conservation of the motion of the center-of-mass (at least in the first approximation) also in those cases in which, besides mechanical, also electrodynamical processes occur.'[129] In fact, his prediction was tested very soon, and by around 1909 the validity of Einstein's law was more or less assured.[130] In 1906 Einstein discussed the fact that his law of the change of mass with energy had to be favored because it stemmed from a theoretical system which covers a wide range of phenomena. Nevertheless he did not call it a theory. In his paper 'Remarks on a Note by P. Ehrenfest,'[131] which he submitted on 14 April 1907, he stated: 'The principle of relativity or — more accurately expressed — the principle of relativity together with the principle of the constancy of the velocity of light has not been considered as a "closed system," in fact, not even as a system at all, but merely as a heuristic device, which looked at *per se* only includes statements concerning rigid bodies, clocks and signals. Any further results that the theory of relativity yields extend only insofar as it demands relations between physical laws which otherwise seem to be independent of each other.'[132]

In the years between 1906 and 1908 the special theory of relativity was completed by Einstein together with his first collaborator J. Laub,[133] Hermann Minkowski and Max Planck in particular. At the *Naturforscherversammlung* in Salzburg in September 1909, it could be regarded as an established theory, to which Arnold Sommerfeld and others made contributions.

It is remarkable that during the years between 1908 and 1911, Einstein mentioned the theory of relativity only marginally. This was the time in which he tried very hard to concentrate on the specific quantum problem, in which he addressed himself to the dualistic nature of light. But already in 1907, in his article 'On the Principle of Relativity and Its Consequences' for Johannes Starks's *Jahrbuch der Radioaktivität und der Elektronik,* he had prepared the ground for further research. In Section 5 of 'The Relativity Principle and Gravitation,' he remarked that one should state a general principle demanding the equivalence of a frame of reference which is uniformly accelerated and a homogeneous field of gravitation. He noted: 'From the above one concludes that the light coming from the surface of the sun ... has a wavelength which is larger by two parts in a million from the light produced by the same substance on earth.'[135] This effect, which was not measured before the Mössbauer effect

(1958) allowed one to obtain very sharp spectral lines, would, of course, serve to underline Einstein's concept of the light-quantum, and the consequence which Einstein drew at that time was as important as the construction of the framework of the theory of general relativity. In the same article, Einstein predicted a change of the velocity of light in a gravitational potential ϕ: it becomes larger, and a bending of light rays occurs in a strong gravitational field. Finally he remarked: The law ... that a quantity of energy E possesses a mass of the magnitude E/c^2 is therefore ... true not only for the *inertial* but also for the *gravitating* mass.'[136] Here, for the first time, Einstein took into account the principle of equivalence of inertial and gravitational masses, from which he was to build his general theory of gravitation.

After his arrival in Prague, to take up his first chair of physics in spring 1911, Einstein concentrated his efforts on research in the theory of relativity. And, upon returning to the ETH in Zurich, he wrote to Sommerfeld (who had asked him about his progress in the quantum problem): 'Your kind letter only adds to my feeling of uneasiness. But I assure you that I cannot say anything new with respect to the quantum problem that might interest you... . I am now completely occupied with the problem of gravitation.'[137]

2.4.2. *Foundations of the Theory of Gravitation*

In 1911, Albert Einstein restarted his work on the general theory of relativity by considering the influence of gravitation on the propagation of light. After a new calculation, he discovered already in his first paper that the bending of light in the gravitational field of the sun should be measurable.[138] In another paper, entitled 'On the Theory of the Static Gravitational Field,' he derived the consequences from a static gravitational field on the electromagnetic and thermal processes.[139] In 1913, together with his old friend and former fellow student, Marcel Grossmann, who was now his colleague at the

ETH, Einstein wrote the great paper 'On the Framework of a General Relativity Theory and a Theory of Gravitation.' In this paper, Einstein wrote the physical part, while Grossmann formulated the mathematical framework. Einstein introduced this valiant attempt (*Entwurf*) by saying: 'The theory presented in the following has originated from the conviction that the proportionality of inertial and gravitational masses of the bodies is an exactly valid law of nature, which has to be incorporated into the very foundations of theoretical physics. Already in some earlier papers I tried to express this conviction by the endeavor to reduce the *gravitational* to the *inertial* mass; this effort has led me to the hypothesis that an (infinitely small extended, homogeneous) field of gravitation can be replaced entirely by an accelerated state of the frame of reference.'[140]

Einstein gave a report on his joint work with Grossmann on the general theory of relativity at two conferences. On 9 September 1913, he discussed the 'Physical Foundations of the Theory of Gravitation' at the *Jahresversammlung* (annual assembly) of the *Schweizer Naturforschenden Gesellschaft* in Frauenfeld, and in the same year he dealt with 'The Present Status of the Problem of Gravitation' at *85. Naturforscherversammlung* in Vienna. At that time he had not yet moved to his position in Berlin, which would allow him to devote all of his time to research unencumbered by teaching and other obligations.

In his talk in Vienna, Einstein presented his 'scientific credo' concerning the principles of general relativity. Besides the equivalence of inertial and gravitational masses, he regarded as very probable and desirable the following points: the conservation of energy and momentum; the validity of special relativity at short distances; the fact that the laws of nature do not depend on the absolute value of the gravitational potential; and Mach's principle, according to which the inertia of bodies is caused by other bodies. This last principle was not satisfied by the competing (scalar) theory of Gunnar Nordström, but in Einstein's theory it became one of the guiding principles in his further efforts to construct the theory of gravitation. Einstein returned to this discussion in his later papers.[141] These presentations of his new theory may have been the final motivation for the Prussian authorities to offer him his extraordinary position in Berlin, and in April 1914 he moved there from Zurich.

Immediately after arrival in Berlin, Einstein plunged into his work on the theory of gravitation. The proceedings of the Prussian Academy of Sciences contain the fruits of those years: 'The Formal Foundations of the General Theory of Relativity';[142] 'Concerning the General Theory of Relativity';[143] 'Explanation of the Perihelion Motion of Mercury from the General Theory of Relativity.'[144] In the fourth communication of that most remarkable month of November 1915, on 25 November, Einstein finally remarked: 'With that the general theory of relativity has been completed as a logical system. The postulate of relativity, which reduces the space–time coordinates to physically meaningless parameters, leads with compelling necessity to a well-defined theory of gravitation, which explains the perihelion motion of Mercury. On the other hand, the general postulate of relativity cannot reveal to us anything about the nature of other physical phenomena, which has not already become clear in the special theory of relativity.'[145,146]

From this last statement, Einstein's ambition became clear. He did not only like the 'enchantment' of the 'method of absolute differential calculus' (tensor calculus) — he demanded physical constraints on other phenomena as well.[147] But 'any physical theory satisfying special relativity can be incorporated with the help of the absolute differential calculus into the system of general relativity theory; however, the latter does not give any criterion for the reliability of that theory.'[148] Nevertheless, the completed theory of general relativity allowed for important consequences. Einstein drew the first one in his communication of 22 June 1916, in which he introduced gravitational radiation. He said: 'The atoms should not only radiate ... electromagnetic but also gravitational radiation, though in a tiny amount. Since this might not be true in nature, it seems that the quantum theory will not only have to modify Maxwell's electrodynamics but also the new theory of gravitation.'[149] In his paper 'Cosmological Considerations Concerning the Theory of General Relativity,' he stated his belief in a closed universe.[150] Other problems included 'Hamilton's Principle and the Theory of General Relativity,'[151] and the formulation of energy conservation in the new theory.[152] In a short note to *Naturwissenschaften*, he could finally report the successful result obtained by Arthur Stanley Eddington's expedition to test the bending of light of fixed stars by the sun.[153]

2.4.3. *Towards the Unified Field Theory*

In his communication, entitled 'Do Gravitational Fields Play an Essential Role in the Structure of Elementary Material Particles?', Einstein said: 'Neither Newton's [law of gravitation] nor the relativistic gravitational theory has brought along progress in the theory of the constitution of matter up to now. In contrast to that it will be demonstrated in the following that indications exist that the elementary entities forming the bricks of atoms are held together by gravitational forces.'[154] With these remarks Einstein introduced his efforts that would concern him for the rest of his life: to unify all physical theories and explain the quantum nature of reality. Gustav Mie, David Hilbert and, especially, Hermann Weyl, had tried to construct a theory in which gravitation and electromagnetism were combined, and Einstein entered into this competition vigorously. In the above-mentioned paper,[154] he introduced a new set of field equations: the energy tensor contains three quarters of the electromagnetic field, and one quarter is due to gravitation. But in this theory not enough conditions were given to consider the problem of elementary quanta, which he regarded as spherical distributions of electric charge. In a later paper, he found further constraints.[155] And in 1923 he considered Eddington's generalization as a good starting point for his own efforts.[156] These efforts were connected with the so-called 'affine field theory,'[157] but in 1923 he remarked: 'A singularity-free electron is not given by these equations.'[158]

Two years later, in 1925, a new theory emerged; during these two years, Einstein had not only formulated quantum statistics and its application to ideal gases, but had made numerous forays to develop *his* field theory. In the 'Unified Field Theory of Gravitation and Electricity,' an overdetermination of the system of equations was possible, i.e. more field equations than field parameters existed.[159] Einstein regarded this overdetermination as a prerequisite for the existence of elementary quanta in field theory. But the question remained: Did a centrally symmetrical electrical charge exist which was not a singularity? Another problem was connected with the fact that in his theory the masses of positive and negative electrons had to be equal. This was a difficult problem, since only the proton existed at that time as a candidate for the positive electron. Einstein noted: 'The recognition seems to me essential that an explanation of the inequality of the two electricities is only possible if one attributes a direction of duration to the time and uses this for the definition of deciding physical quantities. In this the electrodynamics is essentially different from gravitation; therefore the goal of uniting electrodynamics with the laws of gravitation does not seem to be justified to me anymore.'[160]

This problem discouraged Einstein very considerably and his following papers on this topic did not appear before 1927.[161] However, he soon returned to the basic problem as to how to formulate the laws of motion within the field theory. He noted: 'All attempts of the last few years to describe the elementary particles of matter by continuous fields have failed. After many fruitless attempts, about which we do not wish to talk here, a strong suspicion has grown that this is not the correct way to explain the existence of material particles.[162]

'Thus one is forced to consider elementary particles as singular points or singular world lines. This is also suggested by the fact that the equation of the pure gravitational field as well as the equations supplemented by the Maxwellian electromagnetic field possess simple centrally symmetric solutions which demonstrate a singularity. *It now seems to be very probable that the law of motion of the singularities is determined fully by the field equations and the character of singularities,* and no additional assumptions are necessary.'[163]

Einstein's work proceeded further in 1927, and he stated on 24 November: 'Most physicists today are convinced that the fact of [the existence of] quanta excludes the validity of a field theory in the conventional sense of the word. But this conviction is not based on a sufficient knowledge of the consequences of the field theory. Therefore it seems to me to be necessary to pursue further consequences concerning the motion of the singularities for the present, although another path has opened by a far-reaching command of the numerical relations by quantum mechanics.'[164]

In 1928 Einstein proposed a 'New Possibility for a Unified Field Theory of Gravitation and Electricity'.[165] In this the concept of Riemannian geometry was generalized, and was connected by the so-called 'distant parallelism.'[166] As Einstein noted: 'The great attraction of the theory lies for me in its unity and in the high (allowed) overdetermination of the field variables. I have also been able to demonstrate that the field equations lead in the first approximation to equations which correspond to the Newton–Poisson theory of gravitation and to Maxwell's theory of the electromagnetic field. Nonetheless, I am far from claiming the physical validity of the equations thus derived. The reason is that I have not succeeded in deriving the laws of motion for corpuscles.'[167] And later: 'The most important question regarding the (exact) field equations is for the existence of singularity-free electrons which could describe the electrons and protons.'[168] However, soon thereafter he could solve, together with W. Mayer, the central electrical problem,[169] but the singular solution was not influenced by the field equations. Thus one had to look for regular solutions.[170]

By the end of the year, Einstein and Mayer had changed their theory. They succeeded formally (like Kaluza in his five-dimensional theory) to unite gravitation and electromagnetism.[171] They noted, however, 'that it does not provide at present an understanding of the structure of corpuscles as well as the facts summarized in quantum theory.'[172]

As the end of the year 1932 approached, Einstein and Mayer developed a new approach including 'semi-vectors.'[173] The semi-vectors were generalized four-component spinors and were preferred 'because apart from the possibility (shown in the previous paper) of building them into the edifice of general relativity, which is not known in a genuine spinor theory, from the point of view of spinors we cannot understand why there exist in nature exactly *two* different elementary inertial masses with (apart from the sign) equally large electric charge.'[174]

With the change of the political situation in Germany and in Europe at large, Albert Einstein settled in Princeton in 1933 and pursued field-theoretic problems with new collaborators: Nathan Rosen, Leopold Infeld, Banesh Hoffmann, Peter Bergmann and Valentine Bargmann. They treated the problem of motion of particles in the framework of the general relativity theory,[175] Kaluza's theory,[176] and finally Einstein changed to a new approach — bivectors,[177] because Einstein and Pauli had demonstrated in a paper that no regular stationary solutions existed in the so-far-known unified field theories.[178] In the new theory, a bivector $\underset{12}{g_{ik}}$ replaces the metric. But the search for the solutions of old problems — Does a regular solution exist? Is the particle's motion determined by field equations? Is its motion quantized? — continued.[179]

In Appendix II of his book *The Meaning of Relativity* (5th edition, Princeton, 1955), one finds the last expression of Einstein's opinion on these questions. He wrote: 'For the present edition I have completely revised the "Generalization of Gravitation Theory" under the title "Relativity Theory of the Non-symmetric Field." For I have succeeded — in part in collaboration with my assistant B[ruria] Kaufman — in simplifying the derivations as well as the form of the field equations. The whole theory becomes thereby more transparent, without changing its content.[180] There he expressed his opinion concerning a field theory: 'A field theory is not yet completely determined by the system of field equations. Should one admit the appearance of singularities? Should one postulate boundary conditions? As to the first question, it is my opinion that singularities must be excluded. It does not seem reasonable to me to introduce into a continuum theory points (or lines, etc.) for which the field equations do not hold. Moreover, the introduction of singularities is equivalent to postulating boundary conditions (which are arbitrary from the point of view of the field equations) on "surfaces" which closely surround singularities. Without such a postulate the theory is much too vague. In my opinion, the answer to the second question is that the postulation of boundary conditions is indispensable.'[181]

Having made the specific remarks concerning the field theory, Einstein turned to the most important question of his life: 'Is it conceivable that a field theory permits one to understand the atomic and quantum structure of reality? Almost everybody will answer this question with "no." But I believe that at the present time nobody knows anything reliable about it. This is so because we cannot judge in what manner and how strongly the exclusion of singularities reduces the manifold of solutions. We do not possess any method at all to derive systematically solutions that are free of singularities.'[182] 'Approximation methods are of no avail since one never knows whether or not there exists to a particular approximate solution an exact solution *free of singularities.* For this reason we cannot at present compare the content of a nonlinear field theory with experience. Only a significant progress in the mathematical methods can help here. At the present time the opinion prevails that a field theory must first, by "quantization," be transformed into a statistical theory of field probabilities according to more or less established rules. I see in this method only an attempt to describe relationships of an essentially nonlinear character by linear methods.'[182a]

'One can give good reasons why reality cannot at all be represented by a continuous field. From the quantum phenomena it appears to follow with certainty that a finite system of finite energy can be completely described by a finite set of numbers (quantum numbers). This does not seem to be in accordance with a continuum theory, and must lead to an attempt to find a purely algebraic theory for the description of reality. But nobody knows how to obtain the basis of such a theory.'[182b]

3

Letters on Wave Mechanics

On 16 April 1926, Albert Einstein wrote to Erwin Schrödinger: 'Dear Colleague: Professor Planck pointed your theory out to me with well-justified enthusiasm, and then I studied it, too, with the greatest interest.' And he immediately proceeded to point out an 'error' in Schrödinger's first communication to *Annalen der Physik*. He wanted to replace the fundamental equation in Schrödinger's work by another. But this was due to a misreading by Einstein. Still, his objection made Schrödinger 'happy.' Einstein concluded his letter by saying: 'The idea of your article shows real genius.'[183]

In fact, the response to Schrödinger's theory, in particular from renowned senior colleagues, was very favorable. Here seemed to be a theory which was clearly understandable and worked with partial differential equations. For instance, Max Planck remarked in a letter to Schrödinger on 2 April 1926: 'I read your article the way an inquisitive child listens in suspense to the solution of a puzzle that he has been bothered about for a long time, and I am delighted with the beauties that are evident to the eye, but I have to study it more closely in detail to be able to grasp it completely.'[184] And the old Hendrik Antoon Lorentz responded on 27 May 1926: 'I am finally getting around to answering your letter and thanking you very much for kindly sending me the proof-sheets of your three articles, all of which I have in fact received. Reading these has been a real pleasure to me.'[185]

3.1. The Real Schrödinger Equation

In his first letter to Schrödinger, mentioned above, Einstein wrote: 'In the process [of reading your article] one doubt has arisen which I hope you can dispel for me. If I have two systems that are not coupled to each other at all, and if E_1 is an allowed energy value of the first system and E_2 an allowed energy value of the second, then $E_1 + E_2 = E$ must be an allowed energy value of the total system consisting of both of them. I do not, however, understand how your equation

$$\operatorname{div}\operatorname{grad}\phi + \frac{E^2}{h^2(E - \phi)}\phi = 0$$

is to express this property. So that you can see what I mean, I put down another equation that would satisfy this condition:

$$\operatorname{div}\operatorname{grad}\phi + \frac{E - \phi}{h^2}\phi = 0.\text{'}$$ [186]

From this letter one immediately notices the typical reaction of Einstein. First, he did not study too carefully the papers of others. But he immediately understood the first tests of a new theory, and this enabled him to guess the correct formula at once. Moreover, this apparent 'mistake' of Schrödinger's did not change Einstein's appreciation for the work of his younger colleague.

Finally, Einstein at that time was used to playing formally with theories, but he needed some time to understand fully the physics behind them. As he wrote to Schrödinger: 'It also seems to me that the equation ought to have such a structure that the integration constant of the energy does not appear in it; this also holds for the equation I have constructed, but despite that I have not been able to assign a physical significance to it, a matter on which I have not reflected sufficiently.'[186] But Einstein thought more about Schrödinger's theory, which seemed to him to be very ingenious, and in a letter on 22 April 1926 he acknowledged that his proposed formula was actually contained in Schrödinger's paper. 'So my letter was superfluous,' he remarked.[187]

To Schrödinger, Einstein's letter meant a great deal: 'Your approval and Planck's mean more to me than that of half the world. Besides, the whole thing would certainly have not originated yet, and perhaps never would have (I mean, not from me), if I had not had the importance of de Broglie's idea really brought home to me by your second paper on gas degeneracy.'[188] And Einstein's guessing of his formula pleased him even more. 'The objection in your last letter makes me even happier. It is based on an error in memory. The equation

$$\text{div grad}\,\psi + \frac{E^2}{h^2(E - \phi)}\,\psi = 0$$

is not mine, as a matter of fact, but my equation really runs *exactly* like the one that you constructed free-hand from the two requirements of the "additivity" of the quantum levels and the nonappearance of the absolute value of the energy:

$$\text{div grad}\,\psi + 8\pi^2\,\frac{E - \phi}{h^2}\,\psi = 0.$$

'Your very basic requirements *are* therefore fulfilled. I am, moreover, very grateful for this error in memory because it was through your remark that I first became consciously aware of the formal apparatus. Besides, one's confidence in a formulation always increases if one — and especially you — constructs the same thing afresh from a few fundamental requirements.'[189]

Einstein's short reply to Schrödinger's effusive letter was: 'I am convinced that you have made a decisive advance with your formulation of the quantum condition, just as I am equally convinced that the Heisenberg–Born route is off the track. The same condition of the system additivity is *not* satisfied in their method.'[190]

3.2. On the Uncertainty Relation

About two years later, Einstein wrote to Schrödinger again, this time about the Heisenberg–Bohr interpretation of quantum mechanics. 'The Heisenberg–Bohr tranquillizing philosophy — or religion? — is so delicately contrived that, for the time being, it provides a gentle pillow for the true believer from which he cannot be very easily aroused.'[191]

In late 1927, at the fifth Solvay Conference on Physics in Brussels, Einstein and Bohr had been great opponents.[192] Planck was too old, and Lorentz had never taken a very strong stand against the quantum theory after his lecture in Rome in 1908 at the International Congress of Mathematicians. Thus the most prominent opponent of the statistical description of nature had been Einstein. On the other hand, a distinguished physicist of his own generation, Niels Bohr, was the great defender of the new quantum mechanics and its acausal interpretation, not so much the brilliant young people like Heisenberg, Dirac and Pauli; nor was the systematic, dogmatic and middle-aged Max Born, whose statistical interpretation had initiated the 'revolution' and who was a dogged and spirited fighter like Bohr. Schrödinger, once overrun by the powerful Bohr during his visit to Copenhagen at Bohr's invitation, had also not been a great vocal opponent of the 'Heisenberg–Bohr tranquillizing philosophy,' much as he disliked it, though he was most in a position to assist Einstein.

On 30 May 1928, Schrödinger wrote a letter to Einstein and informed him about a controversy he had had with Bohr. Schrödinger had claimed that Heisenberg's uncertainty relation would not allow one to determine neighboring states of a quantum system accurately: 'If we quantize a molecule that is reflected back and forth along the segment l, then we have

$$\oint p dx = p \oint dx = 2lp = nh, \text{ i.e } p_n = \frac{nh}{2\pi}.$$

Neighboring quantized values of the momentum therefore differ from each other by so little (namely, only by $h/2l$) that even with the largest possible uncertainty in the coordinate ($\Delta x = l$), I cannot gain enough accuracy in the *momentum* to allow me to distinguish between neighboring quantum states.'[193] Bohr had replied that he did not understand the application of the uncertainty principle to a gas molecule because there the momentum conjugate to the coordinate had no unique value. But Einstein understood Schrödinger's point and answered: 'I think you have hit the nail on the head.'[191] And he proceeded: 'But the uncertainty relation interpreted that way does not appear to be very illuminating. The thing was invented for free particles, and it fits only that case in a natural way. Your claim that the concepts p, q will have to be given up, if they can only claim such a "shaky" meaning, seems to me fully justified.'[191]

Einstein himself tried to make his way through the uncertainty relations. For instance, on 30 November 1931 he reported at the Physics Colloquium in Berlin a thought experiment, in which a finite ray or signal of light is sent from a box to a faraway mirror; it returns after the reflection, but the interesting fact is that one can determine either the time of flight accurately or the energy of the light.

3.3. Are There Quantum Jumps?

In an article entitled 'Are There Quantum Jumps?', Erwin Schrödinger wrote: 'But in every case, however complicated the actual motion is, it can be mathematically analyzed as being the *superposition* of a discrete series of comparatively simple "proper vibrations," each of which goes on with a quite definite frequency.'[194] For Schrödinger the great question always was to understand the meaning of 'discreteness.' After all, the quantum systems could be described with the continuous wave equation, and the quantization turned out to be a problem of proper [eigen-]values, of some resonance. Schrödinger was vigorously attacking the point of view of energy packets (quanta), because everything could be achieved by the wave equation. He was further criticizing the usual description of interacting systems by attributing a free energy to its parts and an interaction energy to both. 'Summarizing: the current view, which privileges the "sharp energy states," is self-contradictory, anyhow in the language it uses.'[195]

It is of interest to complement this view of the older Schrödinger with that of his colleague Louis de Broglie, whose equation he so often quoted. In his article 'Will Quantum Physics Remain Indeterministic?', de Broglie stated: 'To my knowledge, three possible interpretations of this dualism have been offered. One interpretation, which seems to be the one favored by Schrödinger, simply denies the reality of dualism, claiming that waves alone have a physical significance in the classical sense. While the propagation of waves may occasionally give rise to corpuscular appearances, these are, in fact, no more than appearances. ... Now, the other two interpretations to which I have alluded are, in fact, based on this duality but look at it from quite different points of view.

'The one to which I myself subscribed until 1928 attaches a concrete physical meaning in the traditional sense to the wave–particle dualism, and considers the particle as a sort of central singularity within a continuous wave phenomenon. The problem then arises why wave mechanics can successfully operate with *continuous* waves lacking the singularities of the continuous classical waves. I shall outline my attempted solution below.

'In the second interpretation of the wave–particle dualism, particles and continuous waves are considered as being complementary facets of reality, in Bohr's sense.'[196]

By 1927, de Broglie had proposed the double solution theory, in which there existed two objects: a singular wave solution *u* which represented the particle, and an accompanying continuous wave ψ whose square represented the probability of finding a particle at a given place, etc. In 1927, de Broglie had thought of obtaining his solutions from the linear Schrödinger equation. In the early 1950s 'closer contact with the general theory of relativity ... have since persuaded me that the real equation of propagation of a *u* wave must be nonlinear, like Einstein's gravitational equations.'[197] But the discussions at the fifth Solvay Conference turned de Broglie's thoughts around to subscribing 'to Bohr and Heisenberg's probability interpretation, which I have expounded ever since, though experiments have convinced me that it is full of pitfalls.'[198] In 1952, David Bohm published his article in which he re-examined the pilot-wave theory and proposed the introduction of hidden variables. These would not be excluded by John von Neumann's famous 'proof.' De Broglie concluded: 'Today the explanatory value of wave mechanics seems largely to have vanished. This sad fact is, I think, generally agreed upon, and even the partisans of probability are striving, with little apparent success, to introduce new and ever more abstract concepts, farther and farther removed from the images of classical physics... .'[199]

It might be worthwhile to add here a few remarks about Planck's point of view. In his opinions he had been close to those of Max von Laue, whom Einstein regarded, in his letter to Schrödinger on 22 December 1950, besides himself and Schrödinger, as the only contemporary physicist who saw that 'one cannot get around the assumption of reality — if only one is honest.''[200] In two late papers Planck looked for an 'Attempt to Form a Synthesis Between Wave Mechanics and Corpuscular Mechanics';[201] he tried to relate the wave mechanics and the classical mechanics of the particle to each other by taking the limit $h \to 0$. And he proposed a modification of the Schrödinger wave mechanics because: 'However small one may assume the quantum of action to be, a corpuscle never arises from a wave packet, at least not for a long time; on the contrary, each wave packet has to be dissolved in a short time, whereas the corpuscle retains its atomic structure for ever. Therefore, one concludes that the corpuscular mechanics contains certain features which are not contained in wave mechanics in its present form.'[201]

4

Epistemological Discussion with Einstein: Does Quantum Mechanics Describe Reality Correctly?

Niels Bohr recalled: 'In the very lively discussions on such points [at the 1927 Solvay Conference], which Lorentz, with his openness of mind and balanced attitude, managed to conduct in fruitful directions, ambiguities of terminology presented great difficulties for agreement regarding the epistemological problems. This situation was humorously expressed by Ehrenfest, who wrote on the blackboard the sentence from the Bible, describing the confusion of languages that disturbed the building of the tower of Babel.

'The exchange of views started at the sessions were eagerly continued within smaller groups during the evenings, and to me the opportunity of having longer talks with Einstein and Ehrenfest was a most welcome experience. Reluctance to renounce deterministic description in principle was especially expressed by Einstein, who challenged us with arguments suggesting the possibility of taking the interaction between the atomic objects and the measuring instruments more explicitly into account. Although our answers regarding the futility of this prospect did not convince Einstein, who returned to the problem at the next [Solvay Conference, 1930], the discussions were an inspiration to explore further the situation as regards analysis and synthesis in quantum physics and its analogue in other fields of human knowledge, where customary terminology implies attention to the conditions under which experience is gained.'[202]

The discussions between Einstein and Bohr, of which there exists almost no contemporary written record since they took place mainly outside the regular sessions, have become available through a later report which Niels Bohr gave in 1949 and contributed to the 70th birthday volume in honor of Einstein, edited by Paul Arthur Schilpp.[203] Since this account was given so much later by one of the major participants, it might be regarded as partly one-sided, on the one hand, and presenting a more elaborate view of Bohr's thoughts and arguments than may have actually been presented in 1927, on the other. Thus Einstein's arguments might look weaker than they actually were at that time, and we ought not to regard his return to some of his arguments at the 1930 Solvay Conference as expressing a stubborn mind. In order to reconstruct the original atmosphere of these highly important discussions, let us briefly give an account of the 1927 Solvay Conference and then proceed to the Bohr–Einstein dialogue.

4.1. The Fifth Solvay Conference (1927)

In his introduction to the general discussion at the fifth Solvay Conference, H. A. Lorentz remarked: 'We want to represent the phenomena by an image in our mind.'[204] The fifth Solvay Conference was held in Brussels from 24 to 29 October 1927, with H. A. Lorentz as President for the last time. Among the participants were M. Planck, A. Einstein, M. Knudsen, P. Langevin and M. Curie, who had already attended the first conference in 1911. Besides a few distinguished researchers like C. T. R. Wilson, A. H. Compton,

W. L. Bragg, I. Langmuir and O. W. Richardson, who had made important contributions to gathering empirical facts, the entire avant-garde who had created the new quantum mechanics was present: L. de Broglie, Erwin Schrödinger, Werner Heisenberg, Max Born, P. A. M. Dirac, Wolfgang Pauli, Hendrik A. Kramers and Niels Bohr. From the intermediate generation, P. Debye and P. Ehrenfest, who had also made major contributions to the development of quantum physics, took part, as did R. H. Fowler, the theoretical leader of the Cambridge school. In this illustrious list, only Pascual Jordan and Arnold Sommerfeld were missing.

The reports presented at the fifth Solvay Conference may be divided into two classes: certain specific topics, and an almost complete discussion of quantum mechanics by its major protagonists. Among the specific topics, talks were given by W. L. Bragg on 'Reflexions of X-Rays' and A. H. Compton on 'Electromagnetic Theory of Radiation.' But the major portion of this remarkable conference was spent on the presentation of the various aspects of quantum mechanics. This section was opened by L. de Broglie with a lecture on 'The New Dynamics of Quanta.'

Louis de Broglie had developed the wave picture of matter in several papers since 1922, culminating in his thesis presented in Paris in November 1924. Since that time he had worked on the interpretation of his fundamental equations and the one on which Schrödinger had founded wave mechanics. In particular, de Broglie discussed the true nature of the guiding wave of material particles. After Max Born had developed the statistical interpretation of Schrödinger's wave function, it became clear to him that it would not represent reality in the sense one knew from classical physics. But there did exist perhaps another solution to the dynamical equation — maybe properly generalized — which did not have the disadvantages of the Schrödinger wave, which perhaps represented the particle and its position at all times directly. This then could be a singular solution to the dynamical equations of motion. Between this singular solution to the wave equation and the Schrödinger wave function ψ, there should be a relation which allows one to take all the advantageous features of the wave function and transfer them to the singular solution which represents the reality of matter. Louis de Broglie then discussed the most recent diffraction experiments with electron beams and stated that they favored his new point of view.

The most serious objection to de Broglie's new approach to the nature of the quantum — it was only a tentative approach because he did not actually have his singular solution derived in a consistent manner from the wave equation — was raised by W. Pauli in the general discussion. He discussed a problem treated by Enrico Fermi: the impact of a particle with a rotator in the plane of motion of the particle in wave mechanics.[205] If one would treat the same problem in de Broglie's new theory 'it does not seem to me that the result would be compatible with the postulate of the quantum theory, namely that the rotator would also be in a quantum state after the impact.[206] Though de Broglie did not agree with Pauli's conclusion, he admitted later on that Pauli's objection had bothered him.[207] Also, Schrödinger, who did not wish to attribute too much 'reality' to the particle picture, turned against de Broglie's theory.

Erwin Schrödinger presented his own views in his report on 'The Wave Mechanics.' In particular, he talked about the interpretation of multidimensional wave functions. By that was meant the following. One can interpret the Schrödinger function of one particle related in some way to a spatial density of this particle. This kind of space–time interpretation of the Schrödinger function is still possible in a two-particle system, but then the 'interpretation' breaks down for systems containing more than three particles. Schrödinger, who wished to retain the wave picture as the essential physical concept, tried to keep the three-dimensional interpretation of his wave functions. But his constructions did not convince the quantum-mechanicians like Born and Heisenberg. A problem of N particles, Born noted, will have an eigenvalue problem of the order of ∞^{3n}

dimensions and it cannot be seen how it should reduce to ∞^3 dimensions. But Schrödinger expressed the hope that the degeneracy brought in by symmetry principles and the restriction will lead to the substantiation of his interpretation.

The principal 'technical' report on the new quantum mechanics was presented in a joint contribution of Max Born and Werner Heisenberg on 'The Quantum Mechanics.' There the mathematical method of matrix mechanics was given together with the transformation theory and its interpretation as well as the transition to wave mechanics, together with the immediate interpretation of the wave function which followed from the very manner of the transition. Also, a mathematically and conceptually consistent derivation of the uncertainty relation was presented, and finally the new applications, the problem of statistics and the the whole gamut of progress were treated. Max Born concluded his lecture by mentioning that one could not consider all problems as solved within the new framework, especially the relativistic problems, but the nondeterministic and probabilistic features would persist despite all the later developments.[208]

After this exhaustive report it remained for Niels Bohr to present the wider philosophical aspects and to ponder about the fundamental concepts in his famous lecture on 'The Quantum Postulate and the New Development of Atomistics.' He started his report by saying: 'In discussing the physical significance of the methods developed in quantum theory in the last [few] years, I would like to present the following general remarks concerning the description of principles which are the basis of the description of the atomic phenomena; these remarks may perhaps serve to reconcile the different viewpoints in this domain.'[209]

Bohr divided his report into seven sections. In the first he discussed the postulate of quanta and causality. The description of phenomena is based on observations. The quantum postulate signifies that each observation exerts an influence on the physical system that cannot be neglected. In this process the notions of time and space lose their immediate sense, as does the usual principle of causality. 'In reality,' he continued, 'the postulate of quanta places the description of quantum phenomena before elaborating a "theory of complementarity," in which the absence of contradictions cannot be judged other than in estimating the possibilities of observations. In describing the phenomena of electromagnetic radiation, the two dualistic aspects express the "complementary" features of nature.'[210]

In the second part, Bohr talked about the quantum of action and the new kinematics. This implies the existence of uncertainty relations, which can be extended to the relativistic theory. They express the limitations of classical concepts concerning space, time and causality. 'This state of affairs will be considered as a simple symbolic expression of the complementary nature of the description in space, time and the application of causality.'[211] Now the problems of the description of radiation phenomena could be resolved. It also solved the problem of particle identity.

In the third section, Bohr turned to the problem of measurement in the quantium theory and discussed Heisenberg's examples[212] and others like the tracks in Wilson cloud chambers, which were consistent with the description developed in quantum mechanics. In the section on the correspondence principle, Bohr established the connection between quantum mechanics and his former work in 1918. He discussed the wave-mechanical description of atomic phenomena according to de Broglie and Schrödinger. The relations between the Schrödinger equation and the corresponding classical equation were purely formal in nature. Bohr remarked: 'In the wave-mechanical equation, time and space as well as energy and momentum are applied in a purely formal manner.'[213] And further: 'In the interaction problem the desire to represent the facts intuitively conforms with the images in time and space is not justified at all. In fact, all our knowledge concerning the properties of atoms, as far as it does not refer to the motion of the entire system, is based on their reaction with radiation and impact.'[213] In classical mechanics, one can attribute a certain immediate 'reality' to free particles, but not so in the quantum theory.

The reality of the stationary states follows directly from the quantum postulate. This is not in contradiction with the $\Delta E \Delta t$ relation of the uncertainty principle, because Δt here applies to the 'length' of the wave packet.

Finally, in the seventh section, Bohr discussed the problem of elementary particles. The application of the principles of quantum theory to those phenomena was immediate. He concluded: 'I hope, however, that the notion of complementarity will describe the actual state of affairs conveniently, where a profound analogy with the general difficulties concerning the foundation of human concepts shows up, based upon the separation of subject and object.'[214]

In the general discussion, the philosophical problems or the problems concerning physical concepts or images were discussed thoroughly. H. A. Lorentz opened the discussions with a remark about his notion of physical images. For him an electron meant a particle which will be found at a certain instant at a well-defined point of space. He admitted the practical usefulness of the new theory, but he did not like to give up the principle of determinism completely. Born treated an example of the mechanics of microscopic particles in quantum mechanics in order to clarify the concepts of the theory. Einstein considered a similar problem, namely the propagation of a beam of electrons through a slit. Then, in his opinion, two views concerning the passage of one electron were possible. First, the Schrödinger wave describes not a single electron, not the elementary process, but an arbitrarily large ensemble of elementary processes. Second, the theory describes individual processes, i.e. one electron is represented by a de Broglie–Schrödinger wave packet. He expressed his opinion that the first conception does not necessarily include the energy and momentum conservation in individual elementary processes as concluded from the experiment of Geiger and Bothe. The second notion did not satisfy Einstein because in an interference pattern it implied an action-at-a-distance mechanism of a special kind which produces action at two different points of the screen. As Einstein noted, 'In my opinion one cannot raise that objection if one does not only describe the process by the wave of Schrödinger but at the same time localizes the particles during the propagation. I believe that de Broglie is right to do research in this direction. If one only operates with the Schrödinger wave, then the second interpretation of $|\psi|^2$ implies in my opinion a contradiction with the postulate of relativity.'[215] Then Pauli discussed the equivalence of the multidimensional Schrödinger function with a properly quantized classical wave system. He referred to what is nowadays called the method of second quantization, employed by Jordan and Wigner. There followed a longer discussion on the meaning of the reduction of the wave packet between Dirac, Born, Kramers and Heisenberg. The subject of the discussion changed to the considerations of photons and electrons, and remarks were made about de Broglie's new theory. Also, the questions concerning statistics were raised and answered. Heisenberg remarked in that connection that the question of statistics was more or less independent of the question of interaction. Various speakers, including Dirac, Pauli, Heisenberg and Ehrenfest, touched upon the problems connected with the extension of the quantum-mechanical scheme to field theory.

Max Born concluded the session on the general discussion with the statement: 'Also in classical theory the precision with which the future of a particle can be predicted depends on the precision of the measurement of the initial situation. But the manner of description of wave packets is different in quantum mechanics with the one in classical mechanics. It is [also] different in the two cases because the laws of propagation of wave packets are a bit different from each other in the two cases.'[216]

We have dealt with the reports and discussions at the fifth Solvay Conference a bit more extensively because, on the one hand, they formed the background against which the episte-mological private discussions between Bohr and Einstein (which were not recorded in the official proceedings of the conference) developed. Also, the point of view has arisen that the conference was mainly dominated by controversy, and that there was no relevant discussion besides that. In fact, there was a lively and relevant discussion on many physical aspects, including conceptual interpretation, and many participants took an active part in the exchange of ideas, in particular Schrödinger, Born, Pauli and de Broglie, but also the young explorers like Heisenberg and Dirac. It was not that the youngsters were entirely represented by their mentor Niels Bohr, but they contributed actively by their comments on the physical situation and expressed their individual points of view.

4.2. The Discussions on Epistemological Problems

In a brief exchange of notes during A. H. Compton's lecture at the fifth Solvay Conference, Ehrenfest passed on the following note to Einstein: 'Please don't laugh! There is a special section in the purgatory for professors of "quantum theory," where they would be obliged to listen to ten hours of lectures on classical physics every day!' Einstein responded: 'I simply laugh at the naivety. Who knows who would laugh in a few years!'[217]

Niels Bohr had met Albert Einstein for the first time in April 1920 on the occasion of his visit to Berlin to address a meeting of the German Physical Society, and he recalled that 'these fundamental questions formed the theme of our conversations. The discussions, to which I have often reverted in my thoughts, added to all my admiration for Einstein a deep impression of his detached attitude.'[218] By 'these fundamental questions' Bohr referred to the statistical nature of the laws describing the elementary atomic processes, as, for instance, expressed in Einstein's 1916/17 papers on the absorption and emission of radiation by atoms. But already at that time Bohr noted that 'a certain difference in attitude and outlook remained, since, with his mastery for coordinating apparently contrasting experiences without abandoning continuity and causality, Einstein was perhaps more reluctant to renounce such ideals than someone for whom renunciation in this respect appeared to be the only way open to proceed with the immediate task of coordinating the multifarious evidence regarding atomic phenomena, which accumulated from day to day in the exploration of this new field of knowledge.'[219]

Finally, in 1927, the new theory was complete, and 'several of us came to the conference with great anticipation to learn his reaction to the latest stage of the development, which, in our view, went far toward clarifying the problems which he had himself from the outset elicited so ingeniously,' Bohr remarked over two decades later.[220]

As the first example of his recollections Bohr referred to the diffraction of an electron wave going through a slit, which Einstein pointed out in one of the sessions at the fifth Solvay Conference and about which we have already mentioned. 'The apparent difficulty, in this description, which Einstein felt so acutely, is the fact that, if in the experiment the electron is recorded at one point (A) of the plate, then it is out of question of ever observing an effect of this electron at another point (B), although the laws of ordinary wave propagation offer no room for a correlation between two such events.

'Einstein's attitude gave rise to ardent discussions within a small circle, in which Ehrenfest, who through the years had been a close friend of us both, took part in a most active and helpful way. Surely, we all recognized that, in the above example, the situation presents no analogue to the application of statistics in dealing with complicated mechanical systems, but rather recalled the background for Einstein's own early conclusions about the unidirection of individual radiation effects, which contrasts so strongly with a simple wave picture. The discussions, however, centered on the question of whether the quantum-mechanical description exhausted the possibilities of accounting for observable phenomena or, as Einstein maintained, the analysis could be carried further and, especially, of whether a fuller description of the phenomena could be obtained by bringing into consideration the detailed balance of energy and momentum in individual processes.'[221]

This application of the laws of conservation means the following: if one supplies the slit (through which the electron wave passes) with an opening and closing shutter, one might be able to determine the position and momentum of the electron more accurately than given by Heisenberg's uncertainty relation, if the conservation of energy and momentum is taken into account properly. In particular, Einstein thought that it could be possible to determine in a two-slit experiment, given the accurate interference pattern, the slit through which the electron had passed. But an accurate knowledge of the slit in question would carry along a difference in momentum transfer to which (due to the uncertainty relation) an uncertainty in position follows, and this uncertainty in the position of the slit, bearing a diaphragm, would wipe out the interference pattern on the screen from which the momentum of the electron can be determined. 'We have here to do with a typical example of how the complementary phenomena appear under mutually exclusive experimental arrangements and are just faced with the impossibility, in the analysis of quantum effects, of drawing any sharp separation between an independent behavior of atomic objects and their interaction with the measuring instruments which serves to define the conditions under which the phenomena occur.'[222]

'Einstein's concern and criticism provided a most valuable incentive for us all to re-examine the various aspects of the situation as regards the description of atomic phenomena.'[223] Bohr invented all kinds of slit experiments and drew consequences from the uncertainty principle. The inclusion of clocks as measuring devices also does not help to bring the uncertainty product $\Delta E \Delta t$ below Planck's constant. Either the reading of the time from the clock or the knowledge of the energy transfer is accurate.

As essential features of the problems involved in the discussion, Bohr pointed out: 'The main point here is the distinction between the *objects* under investigation and the *measuring instruments* which serve to define, in classical terms, the conditions under which the phenomena appear.'[224]

From the discussion of the example which Einstein used in the general session, Bohr drew the following conclusion: 'To my mind, there is no other alternative than to admit that, in this field of experience, we are dealing with individual phenomena and that our possibilities of handling the measuring instruments allow us only to make a choice between the different complementary types of phenomena we want to study.'[225]

Thus far the 1927 Solvay Conference. At the sixth Solvay Conference, in 1930, Einstein carried on the discussion by raising further objections. This time he based his arguments on the fact that by the relation between energy and mass one should be able to weigh the energy and maybe by that obtain a more accurate control. He devised an experiment which consisted of a box filled with radiation. At a given instant the shutter is opened and radiation leaves the box. The energy content is obtained by weighing the box before and after the emission. This proposal offered a very hard question which led to the discussion of the relationship between the rate of the clock and its position in the gravitational field, which is connected with the principle of equivalence of inertial and gravitational masses. Einstein himself figured out the measuring device. One found that the balancing time T for the scale is the larger the more accurately one wants to measure the mass. Now a clock, when displaced in the direction of the gravitational force by an amount Δq, will change its rate in the course of the time interval T by an amount ΔT. If one takes the product $\Delta T \Delta E$ one finds it to be larger than or equal to Planck's constant. This answer of Bohr's, when he beat Einstein with his own weapon (general relativity), was considered one of the great triumphs of complementarity thinking.

As Bohr noted: 'Notwithstanding the most suggestive confirmation of the soundness and wide scope of the quantum-mechanical way of description, Einstein nevertheless, in a following conversation with me, expressed a feeling of disquietude as regards the apparent lack of firmly laid down principles for the explanation of nature, in which all could agree. From my viewpoint, however, I could only answer that, in dealing with the task of bringing order into an entirely new field of experience, we could hardly trust in any accustomed principles, however broad, apart from the demand of avoiding logical inconsistencies and, in this respect, the mathematical formalism of quantum mechanics should surely meet all requirements.'[226]

But Einstein did not consider the discussion as being closed. Bohr reported that Ehrenfest, shortly before his death in 1933, had informed him about a further investigation of Einstein's, indicating that in many complementarity experiments one can decide very late what quantity one wants to measure accurately. However, Bohr remarked that by considering the whole measuring apparatus one obtained the uncertainty results. There took place later discussions between Einstein and Bohr, but more in published papers than through personal exchange. Thus Bohr took a stand concerning the article of Einstein, Podolsky and Rosen[227] and Einstein's article in *Dialectica.*[228] However, both of these articles did not change the picture we have obtained from the lively interchange at the Solvay Conferences. We shall deal with these papers in their proper context, because they do not refer anymore to the question 'Is the quantum-mechanical description consistent?' but rather to the question 'Is the quantum-mechanical description of nature complete?'.

4.3. Bohr's Principle of Complementarity and the Copenhagen School

George Gamow wrote: 'Bohr's Institute quickly became the world center of quantum physics, and to paraphrase the old Romans, "all roads lead to Blegdamesvej 17." The Institute buzzed with young theoretical physicists and new ideas about atoms, atomic nuclei, and the quantum theory in general. The popularity of the Institute was due both to the genius of its director and to his kind, one might say fatherly, heart. Whereas another genius of that era, Albert Einstein, though a very kind man too, never formed what is known as a "school" around him but worked usually with just a single assistant to talk to, Bohr fathered many scientific "children." Almost every country in the world has physicists who proudly say: "I used to work with Bohr." '[229]

It is hard to say how Bohr, not only through his papers but in particular through his discussions, influenced the physics of this century. After all, he did not have his name on any of the decisive papers in which quantum or wave mechanics was created, if we do not count his paper on the correspondence principle among them. And, even in this paper, the central idea came from Einstein. Nevertheless, if one asked the creators of quantum mechanics like Heisenberg, Pauli and Dirac, by whom they had been influenced the most in their careers, they most often cited the name of Niels Bohr. As the only exception we might consider Max Born, and his interpretation of Schrödinger's wave function might be regarded as the greatest contribution to quantum mechanics (besides partly his role in the creation of matrix mechanics), whose work did not originate under the influence of Bohr.

In fact, in discussing the number of physical ideas which had been discovered in the golden years from 1924 to 1927, almost none came from Bohr's Institute: Heisenberg worked at Göttingen or Helgoland, and so did Born and Jordan. Pauli was in Hamburg, and Dirac produced his independent inventions in Cambridge. The wave mechanics, on the other hand, came out of Paris and Zurich; even the quantum statistics were developed in India, Berlin and Rome or Cambridge. Also, the continuation of quantum-mechanical ideas into quantum field theory did not occur in Copenhagen, nor did the fundamental applications of quantum mechanics to molecular problems. Thus, in fact, the influence of Bohr extended essentially to two important papers which he contributed himself: his presentation of the question of the uncertainty principle in his paper on complementarity[230] and his paper with Léon Rosenfeld on the foundation of field quantization.[231] But, once again, both papers were more of a reflective nature than active contributions, and from the physical point of view these questions had already been answered by the preceding pioneering papers of others. Bohr's 'only' later paper which dealt with physics was about a model of nuclear fission[232] and later publications in this field. Nevertheless, he was regarded by most of the leading physicists in the field of quantum theory as their mentor, and the new interpretation of microscopic phenomena became generally recognized as the 'Copenhagen interpretation.'

The framework of quantum theory had been completed to a certain extent in 1927 by Heisenberg's uncertainty relations. These provided at the same time the necessary foundation of the old heuristic principle which has since been used in 'deriving' quantum mechanics: the correspondence principle. At the same time, the quantum theory existed in at least two forms: matrix and wave mechanics. Already in spring 1926, Schrödinger had proved for the first time that the two methods yielded the same results and there was no reason to consider them as describing different physics. In Dirac's general operator theory, this equivalence became even more evident, together with the physical interpretation of the wave function. Though Born had derived the statistical interpretation of the wave function without using Dirac's theory, because it had not yet been elaborated at that time, the conceptually most satisfactory interpretation of Schrödinger's wave mechanics may be found by means of Dirac's transformation theory, including his original use of the delta function.

For Bohr, whose imprimatur the orthodox interpretation of quantum mechanics bears, and this in particular through the agreement of Werner Heisenberg, who had undertaken not only the first but also the most important steps leading to the new theory, the situation was rather different. His ideas concerning the new theory reached farther back.

Meyer-Abich, in his dissertation on Bohr's philosophical considerations regarding correspondence, individuality and complementarity, wrote: 'That Bohr's interpretation of quantum mechanics was given from a situation reaching farther backwards than from the immediate contemplation about the formal methods developed between 1925 and 1927, is proved by the fact that he did not take these methods in 1927 as the actual starting point but rather identified them as a confirmation of what had happened before, and the other fact that one cannot understand Bohr's thoughts concerning quantum mechanics without referring to philosophical questions. This also applies to the controversy between Bohr and Einstein. As to a hint about the day Bohr took hold of his orthodox interpretation of quantum theory, an old Copenhagen anecdote may be recounted: Bohr already said all that 20 years before quantum mechanics came into being.'[233]

In fact, the essential features which Bohr attributed to the new quantum theory bear a rather philosophical character: he talked about the interrelation of subject and object, whereas Heisenberg had talked about the impossibility of simultaneous measurements of canonically conjugate quantities. But Bohr's philosophy was rather subtle, as might be noticed from his own remarks: 'On that occasion [the 1927 Solvay Conference] an interesting discussion arose also about how to speak of the appearance of phenomena for which only predictions of statistical character can be made. The question was whether, as to the occurrence of individual effects, we should adopt a terminology proposed by Dirac, that we were concerned with a choice on the part of "nature" or, as suggested by Heisenberg, we should say that we have to do with a choice on the part of the "observer" constructing the measuring instruments and reading their recording. Any such terminology would, however, appear dubious since, on the one hand, it is hardly reasonable to endow nature with volition in the ordinary sense, and, on the other hand, it is certainly not possible for the observer to influence the events which may appear under the conditions he has arranged. To my mind, there is no other alternative than to admit that, in this field of experience, we are dealing with individual phenomena and that our possibilities of handling the measuring instruments allow us only to make a choice between the different complementary types of phenomena we want to study.'[234]

The question as to what the Copenhagen interpretation may be, can be answered in short by a passage from Werner Heisenberg's article for Bohr's 70th birthday *Festschrift*, under the title 'The Development of the Interpretation of Quantum Theory.' He wrote: 'Let us return to quantum mechanics. According to the situation, an individual atomic system can be represented by a wave function or by a statistical mixture of such functions, i.e. by an ensemble (mathematically, by a density matrix). If the system interacts with the external world, only the second approximation is possible, since we do not know the details of the "external world" system. If the system is closed, we may in some circumstances have, at least approximately, a "pure case," and the system is then represented by a vector in Hilbert space. The representation is, in this particular case, completely "objective," i.e. it no longer contains features connected with the observer's knowledge; but it is also completely abstract and incomprehensible, since the various mathematical expressions, $\psi(q)$, $\psi(p)$, etc., do not refer to a real property; it then, so to speak, contains no physics at all. The representation becomes a part of the description of Nature only by being linked to the question of how real or possible experiments will result. From this point we must take into consideration the interaction of the system with the measuring apparatus and use a statistical mixture in the mathematical representation of the larger system composed of the system and the measuring apparatus. It might appear that this could in principle be avoided if it were possible to separate the system and the measuring apparatus, as a compound system, entirely from the external world. However, Bohr has rightly pointed out on many occasions that the connection with the external world is one of the necessary conditions for the measuring apparatus to perform its function, since the behavior of the measuring apparatus must be capable of being represented as something actual, and therefore of being described in terms of simple concepts, if the apparatus is to be used as a measuring instrument at all, and the connection with the external world is therefore necessary. The compound system and measuring apparatus are therefore now described mathematically by a mixture, and thus the description contains, besides its objective features, the previously discussed statements about the observer's knowledge. If the observer later registers a certain behavior of the measuring apparatus as actual, he thereby alters the mathematical representation discontinuously, because a certain one among the various possibilities has proved to be the real one... .

'We see from this that a system cut off from the external world is potential but not actual in character, or, as Bohr has often expressed it, that the system cannot be described in terms of classical concepts. We may say that the state of the closed system represented by a Hilbert vector is indeed objective, but not real, and that the classical idea of "objective real things" must be here, to this extent, abandoned ... The description of a fact can be effected in terms of classical concepts in just the approximation in which classical physics can be used. The mathematics of quantum theory can be used for this description as well, i.e. the boundary between the object in quantum theory and the observer who describes or measures in time and space can be pushed further and further in the direction of the observer ... *Knowledge of the "actual" is thus*, from the point of view of quantum theory, *by its nature always incomplete knowledge.* For the same reason, the statistical nature of the laws of microscopic physics cannot be avoided.'[235]

5

Is the Quantum-Theoretical Description of Nature Complete?

In his article 'Maxwell's Influence on the Development of the Conception of Physical Reality,' Einstein introduced his remarks by saying: 'The belief in an external world independent of the percipient subject is the foundation of all science. But since our sense-perceptions inform us only indirectly of this external world, or Physical Reality, it is only by speculation that it can become comprehensible to us. From this it follows that our conceptions of Physical Reality can never be definitive; we must always be ready to alter them, to alter, that is, the axiomatic basis of physics, in order to take account of the facts of perception with the greatest possible logical completeness.' Einstein concluded his essay by saying: 'Yet I incline to the belief that physicists will not be permanently satisfied with such an indirect description of Reality, even if the theory can be fitted successfully to the General Relativity postulate. They would then be brought back to the attempt to realize that program which may suitably be called Maxwell's: the description of Physical Reality by fields which satisfy without singularity a set of partial differential equations.'[236]

Until 1930 Einstein tried hard to test quantum mechanics for its consistency and whether it described the empirical situation correctly. But the discussions he had had mainly with Bohr made it rather evident to him that the new theory was correct. Then he changed his attitude. Since, however, he was still uneasy about the new theory — better to say 'far from satisfied'[237] — Einstein tried to formulate his objection on a more conceptual or more philosophical basis. One of the most important papers in which he stated his points of view was entitled 'Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?' More specifically, he dealt in another paper with the aspects of 'Physics and Reality.' In the ensuing discussion, it was mainly Niels Bohr who took part, for he had tried to keep the discussion going since 1927. Later on, Max Born became an increasingly important discussion partner, but we shall postpone Einstein's arguments with Born a little bit longer.

5.1. 'Knowledge of Past and Future in Quantum Mechanics'

Under this heading, Albert Einstein, Richard Chace Tolman and Boris Podolsky wrote a letter on 26 February 1931 to the editor of the *Physical Review*.[238] The purpose of this note was to demonstrate that quantum mechanics limits not only the knowledge of the future but also the knowledge of the past.

As an example, they considered a box filled with identical particles, having two windows which could be opened at a given instant by a shutter. If one could now measure the momentum of the particle arriving first with large enough accuracy, then one might be able to determine the time when the shutter was open. And with the energy loss of the box (determined by weighing) one could further calculate the energy and velocity of the second particle, and from that the time of its arrival. This, they concluded, was 'a paradoxical result since energy and time are quantities which do not commute in quantum mechanics.'

In order to explain the apparent paradox they concluded that the measurement of the momentum of the first particle did disturb its momentum, and thus we cannot predict its path. In fact the situation was very clear: one had to determine the velocity or momentum of a particle at a given space point, and this could not be done beyond the accuracy of the uncertainty principle, and hence one cannot calculate the time at which the shutter opens. The 'remarkable' conclusion arises 'that the principles of quantum mechanics would actually impose limitations on the localization in time of a macroscopic phenomenon such as the opening and closing of a shutter.'[239]

Of course, this effect analyzed by Einstein, Tolman and Podolsky did not have any consequences for the interpretation of quantum mechanics. It was entirely covered by the interpretation, and just tried to clarify certain concepts and misunderstandings in the new theory.

5.2. The Completeness Problem

As Niels Bohr noted: 'While, so far, relatively few persons had taken part in the discussions reported in this article, Einstein's critical attitude towards the views on quantum theory adhered to by many physicists was soon after brought to public attention through a paper with the title "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?", published in 1935 by Einstein, Podolsky and Rosen.'[240]

This joint paper started with the following remark: 'Any serious consideration of a physical theory must take into account the distinction between objective reality, which is independent of any theory, and the physical concepts with which the theory operates. These concepts are intended to correspond with objective reality, and by means of these concepts we picture reality to ourselves.'[241]

To this article there exists fortunately a complete answer by Bohr himself, with exactly the same title.[242] We shall first present the main features of the Einstein–Podolsky–Rosen paper, and then discuss Bohr's response. We must emphasize the fact that the three authors called only such a theory satisfactory in which the description given by the theory was complete. And they specified the word 'complete.' 'Whatever the meaning assigned to the term *complete*, the following requirement for a complete theory seems to be a reasonable one: *every element of the physical reality must have a counterpart in the physical theory.* We shall call this the condition of completeness. The second question is thus easily answered, as soon as we are able to decide what the elements of physical reality are.'[243]

But now Einstein, Podolsky and Rosen had to give the meaning of physical reality, and they regarded the following criterion as reasonable: 'If, without in any way disturbing a system, we can predict with certainty (i.e. with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity.'[244]

Let us immediately insert here Bohr's answer to this criterion: 'Such an argumentation, however, would hardly seem suited to affect the soundness of quantum-mechanical description, which is based on a coherent mathematical formalism covering automatically any procedure of measurement like that indicated. The apparent contradiction in fact discloses only an essential inadequacy of the customary viewpoint of natural philosophy for a rational account of physical phenomena of the type with which we are concerned in quantum mechanics. Indeed the *finite interaction between object and measuring agencies* conditioned by the very existence of the quantum of action entails — because of the impossibility of controlling the reaction of the object on the measuring instruments, if these are to serve their purpose — a final renunciation of the classical ideal of causality and a radical revision of our attitude towards the problem of physical reality. In fact, as we shall see, a criterion of reality like that proposed [by Einstein, Podolsky and Rosen] contains — however cautious its formulation may appear — an essential ambiguity when it is applied to the actual problems with which we are here concerned.'[245]

Now Einstein, Podolsky and Rosen illustrated their point of view by considering a quantum-theoretical particle having one degree of freedom. If a state of this system ψ is an eigenfunction of an observable A with an eigenvalue a, then 'there is an element of physical reality corresponding to the physical quantity A.'[246] But if ψ is not an eigenfunction 'we can no longer speak of the physical quantity A having a particular value.'[246] Or: 'The usual conclusion from this in quantum mechanics is that *when the momentum of a particle is known, its coordinate has no physical reality.*'[246] 'From this it follows that either (1) *the quantum-mechanical description of reality given by the wave function is not complete,* or (2) *when the operators corresponding to two physical quantities do not commute the two quantities cannot have simultaneous reality.*'[246]

After this preliminary discussion they tried to point out a contradiction in quantum mechanics. They considered two systems I and II, which interact during a finite time from $t=0$ to $t=T$. The states of the two systems before $t=0$ should be known; thus one can calculate from Schrödinger's equation the state of the combined system for all times — call it Ψ. But one cannot calculate the states of the systems after the interaction separately. The reason is that $\psi(\mathrm{I}, \mathrm{II})$ will, in general, be a mixture with respect to the eigenstates of a physical quantity pertaining to the system I. If one now measures the quantity A after the interaction in the system I, one projects a certain eigenstate $\psi_k(\mathrm{I})$ with the eigenvalue a_k, and thus also reduces $\psi(\mathrm{I}, \mathrm{II})$ to a single term, the coefficient of which is the wave function of the system II after the interaction $\psi_k(\mathrm{II})$. If, on the other hand, one performs a measurement on the quantity B, one might project out an eigenstate $\psi_l(\mathrm{II})$ with an eigenvalue b_l. From this one would conclude that the first system is in the state $\psi_l(\mathrm{I})$.

The decisive point, however, is that one can perform two measurements of the same quantity A at the system I and obtain two different wave functions for the system II. But since there is no interaction between the two systems anymore, no real change could take place in the second system, and one has to conclude that one can attribute two different wave functions to the same reality.

Moreover, two measurements of the quantities A and B at the systems I and II, respectively, which leave the second system with a wave function $\psi_k(\mathrm{II})$ or $\psi_l(\mathrm{II})$, might be noncommutable, in principle. As an example, Einstein, Podolsky and Rosen considered two particles with the total wave function

$$\Psi(x_1, x_2) = \int_{-\infty}^{+\infty} e^{(2\pi i/h)(x_i - x_2 + x_0)}dp \quad (x_0 \text{ a constant}). \tag{11}$$

Then, if p is the momentum of the first particle, its eigenfunction is $\psi_p(\mathrm{I})$, and Ψ becomes

$$\Psi(x_1, x_2) = \int \psi_p(\mathrm{II})\, \psi_p(\mathrm{I})\, dp, \tag{12}$$

with $\psi_p(\mathrm{II}) = e^{-2\pi i/h(x_2 - x_0)p}$. $\psi_p(\mathrm{II})$ is also an eigenvector of the momentum operator P with eigenvalue p. Now they took the position operator Q and determined the eigenfunction $\psi_r(x_1) = \delta(x_1 - x_2)$ of the first particle via the total function $\Psi(x_1, x_2)$, the position eigenfunction $\psi_r(\mathrm{II})$ of the second particle. The eigenfunction $\psi_r(\mathrm{II})$ now describes the second system as well as the eigenfunction $\psi_p(\mathrm{II})$. Both belong to noncommuting operators.

Addressing this point, Bohr remarked: 'In fact, although any pair q and p of conjugate space and momentum variables obeys the rule of noncommutative multiplication expressed by [Eq. (4)], and can thus only be fixed with reciprocal latitudes given by [Eq. (8), the uncertainty relation], the difference $q_1 - q_2$ between two space coordinates referring to the constituents of the system will commute with the sum $p_1 + p_2$ of the corresponding momentum components, as follows directly from the commutability of q_1 with p_2 and q_2 with p_1. Both $q_1 - q_2$ and $p_1 + p_2$ can, therefore, be accurately fixed in a state of the complex system and, consequently, we can predict the values of either q_1 or p_1 if either q_2 or p_2, respectively, is determined by direct measurements.'[247]

Einstein, Podolsky and Rosen, however, drew the following conclusion: 'By measuring either A or B we are in a position to predict with certainty, and without in any way disturbing the second system, either the value of the quantity P (i.e. p_k) or the value of the quantity Q (i.e. q_l). In accordance with our criterion of reality, in the first case we must consider the quantity P as being an element of reality; in the second case the quantity Q is an element of reality. But, as we have seen, the two wave functions ψ_k and ψ_r belong to the same reality.'[248] Thus they claimed to have proved a contradiction: starting from the assumption that the wave function does give a complete description of the physical reality, one could derive, on the one hand, that two noncommuting quantities could not have simultaneous reality; however, on the other hand, one found a specific example that the two quantities had physical reality. This was a contradiction and, therefore, they concluded 'that the quantum-mechanical description of the physical reality given by the wave function is not complete.' But they were also optimistic enough to continue: 'While we have thus shown that the wave function does not provide a complete description of physical reality, we left open the question whether or not such a description exists. We believe, however, that such a theory is possible.'[248]

Bohr commented on their conclusion by saying: 'From our point of view we now see that the wording of the above-mentioned criterion of physical reality proposed by Einstein, Podolsky and Rosen contains an ambiguity as regards the meaning of the expression "without in any way disturbing a system." Of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure. But even at this state there is essentially the question of *an influence on the very conditions which define the possible types of predictions regarding the future behavior of the system.* Since these conditions constitute an inherent element of the description of any phenomenon to which the term "physical reality" can be properly attached, we see that the argumentation of the authors mentioned does not justify their conclusion that quantum-mechanical description is essentially incomplete. On the contrary, this description, as appears from the preceding discussion, may be characterized as a rational utilization of all possibilities of unambiguous interpretation of measurements, compatible with the finite and uncontrollable interaction between the objects and the measuring instruments in the field of quantum theory. In fact, it is only the mutual exclusion of any two experimental procedures, permitting the unambiguous definition of complementary physical quantities, which provides room for new physical laws, the coexistence of which might at first sight appear irreconcilable with the basic principles of science. It is just this entirely new situation as regards the description of physical phenomena that the notion of *complementarity* aims at characterizing.'[249]

5.3. Physics and Reality

As Niels Bohr noted: 'Einstein's own views at that time are presented in an article, "Physics and Reality," published in 1936 in the *Journal of the Franklin Institute*. Starting from a most illuminating exposition of the gradual development of the fundamental principles in the theories of classical physics and their relation to the problem of physical reality, Einstein here argues that the quantum-mechanical description is to be considered merely as a means of accounting for the average behavior of a large number of atomic systems, and his attitude to the belief that it should offer an exhaustive description of the individual phenomena is expressed in the following words: "To believe this is logically possible without contradiction; but it is so very contrary to my scientific instinct that I cannot forego the search for a more complete conception."'[250]

The motivation which Einstein gave when he turned to the rather philosophical questions in his article 'Physics and Reality' should be quoted: 'It has often been said, and certainly not without justification, that the man of science is a poor philosopher. Why then should it not be right for the physicist to let the philosopher do the philosophizing? Such might indeed be the right thing at a time when the physicist believes he has at his disposal a right system of fundamental concepts and fundamental laws which are so well-established that waves of doubt cannot reach them; but, it cannot be right at a time when the very foundations of physics itself have become problematic as they are now. At a time like the present, when experience forces us to seek a newer and more solid foundation, the physicist cannot simply surrender to the philosopher the critical contemplation of the theoretical foundations, for, he himself knows best, and feels more surely where the shoe pinches. In looking for a new foundation, he must try to make clear in his own mind just how far the concepts which he uses are justified, and are necessities.'[251]

In the very general introduction Einstein explained the steps which lead one to talk about a 'real external world.' One attributes to bodily objects a 'real existence' beyond our sense impressions, and one finds relations between the objects. But still these things have to be comprehensible: 'It was one of the great realizations of Immanuel Kant that the setting up of a real external world would be senseless without its comprehensibility.'[252] Then he turned to the 'Stratification of the Scientific System.' He stated: 'The aim of science is, on the one hand, a comprehension, as *complete* as possible, of the connection between the sense experiences in their totality, and, on the other hand, the accomplishment of this aim *by the use of a* *minimum of primary concepts and relations.* (Seeking, as far as possible, logical unity, in the world picture, i.e. paucity in logical consequences.)'[253] The development in science tends toward this final goal. The freedom of choosing the fundamental relations, Einstein described with the following picture: 'Rather, it is similar to that of a man engaged in solving a well-defined word puzzle. He may, it is true, propose any word as the solution; but, there is only *one* word which really solves the puzzle in all its forms. It is an outcome of faith that nature — as she is perceptible to our five senses — takes the character of such a well-formulated puzzle. The successes reaped up to now by science do, it is true, give a certain encouragement for this faith.'[254]

Then he ran through the development of physics, starting with mechanics. By the application of point mechanics to describe the phenomena of heat, the molecular theory of matter was put on a secure basis. The extension of Newton's mechanics to include the phenomena of optics and electricity was not likewise successful. 'The escape from this unsatisfactory situation by the electric field theory of Faraday and Maxwell represents probably the most profound transformation which has been experienced by the foundations of physics since Newton's time.'[255]

But shortly afterward Lorentz had to introduce the point-charged particle and a Newton-like equation of motion. In this theory the question arose whether one could explain the *total* inertia of an electron by the electromagnetic field: 'It is clear that this problem could be worked out satisfactorily only if the particles could be interpreted as regular solutions of the electromagnetic partial differential equations. The Maxwell equations in their original form do not, however, allow such a description of particles, because their corresponding solutions contain a singularity.'[256] All efforts to change Maxwell's equations toward this goal failed. 'The thing which deterred one in any further attempt in this direction was the lack of any systematic method leading to the solution. What appears certain to me, however, is that, in the foundations of any consistent field theory, there shall not be, in addition to the concept of field, any concept concerning particles. The whole theory must be based solely on partial differential equations and their singularity-free solution.'[256]

In a third section, Einstein turned to the theory of relativity including the latest results in the unified theory, which we have already discussed earlier. More interesting were his views on the quantum problem, to which he addressed himself in the fifth section: 'Quantum Theory and the Fundaments of Physics.'

In quantum mechanics, he said, one has to apply new fundamental concepts which deviate from those considered for field theory. It was significant for him that he introduced his remarks by saying: 'I shall try to outline here the path of ideas of de Broglie–Schrödinger *which is closer to the thinking of physicists* [my emphasis], and then tie to it general considerations.'[257] Einstein sketched the derivation of Schrödinger without worrying too much about the concepts. The problem was solely contained in the meaning of the wave function: the square of this function is to be connected with a probability.

After citing the successes of quantum mechanics, Einstein proceeded: 'Hardly ever a theory has been formulated which has provided the key to the explanation and calculation of such diverse experimental facts as quantum mechanics. Nevertheless, I believe that it tends to mislead us in searching for a unified basis of physics; it is, in my view, an *incomplete* representation of the real things, though it is the only relevant theory which can be founded on the concepts of material points and force (quantum correction to classical mechanics). To the incompleteness of the representation, however, necessarily the statistical character (incompleteness) of the laws is related.'[258]

In order to prove his assertion, Einstein considered a system which has eigenstates ψ_r and is at the time zero in the state ψ_1 with energy E_1. Then the system is disturbed in a short time, and a wave function ψ which is a mixture of the states of the previous free system can be calculated. But now he claimed that ψ does not describe the system because then the energy should be somewhere in between the states, which is disproved by Franck and Hertz's experiment. Thus ψ describes a statistical ensemble or mixture, and he concluded: 'If the ψ-function, disregarding special cases, only gives *statistical* statements about observables, then the reason for this is not only that the *procedure of measurement* introduces unknown, only statistically describable elements, but even more that the ψ-function does not describe the state of *one* single system at all. The Schrödinger equation determines the time change which is experienced by the system ensemble, be it with or without external effects on the single system.'[259] And he repeated the statements from the Einstein–Podolsky–Rosen paper, without any noticeable change. According to Einstein, quantum mechanics actually deals with ensembles and not with single and elementary processes. Then one can explain the big changes in a system made by small perturbations. 'The process occurring with the single system remains totally unexplained in such a consideration; the latter is totally eliminated from the representation by the statistical interpretation.'[260]

With respect to this personal view regarding the meaning of quantum theory, Einstein made the demand for a description of single systems which he called causal. He also criticized the concept of time in the Schrödinger equation, and stressed the difficulties which showed up in the relativistic quantum field theories of those days. As he remarked: 'They will pile up if one tries to keep up with the requirements of general relativity, whose principal justification nobody will deny.'[261] He also criticized the point of view that one had to abandon totally the concepts of space and time, and use only algebraic methods. 'For the time being this project seems similar to breathing in empty space.'[261]

Einstein reiterated: 'There is no doubt that quantum mechanics contains a good piece of truth and that it will be a test case of a future theoretical fundament, that it has to be deduced from this basis as a limiting case ... . But I believe that in searching for that basis quantum mechanics cannot serve as the *starting point,* as little as one can by starting from the wrong end of thermodynamics or statistical mechanics derive the basis of mechanics.'[261] With respect to this situation, he stressed the possibility of obtaining results from the unified field theory. As to some hopes in that direction, he cited results from a joint paper with Rosen.[262]

We note from his paper 'Physics and Reality' that Einstein *had not changed* his opinion concerning quantum theory following Bohr's paper in reply to his own. Bohr remarked: 'Even if such an attitude might seem to be well-balanced in itself, it nevertheless implies a rejection of the whole argumentation exposed in the preceding, aiming to show that in quantum mechanics we are not just dealing with an arbitrary renunciation of a more detailed analysis of atomic phenomena, but with a recognition that such an analysis is *in principle* excluded.'[263]

5.4. Quantum Mechanics and Reality

In an article in *Dialectica* in 1948 on the interpretation of quantum mechanics, Niels Bohr stated: 'As regards the question of the completeness of the quantum-mechanical mode of description, it must be recognized that we are dealing with a mathematically consistent scheme which is adapted within its scope to every process of measurement and the adequacy of which can only be judged from a comparison of the predicted results with actual observations. In this connection, it is essential to note that, in any well-defined application of quantum mechanics, it is necessary to specify the whole experimental arrangement and that, in particular, the possibility of disposing of the parameters defining the quantum-mechanical problem just corresponds to our freedom of constructing and handling the measuring apparatus, which in turn means the freedom to choose between the different complementary types of phenomena we wish to study.'[264]

In the same issue of *Dialectica,* in an article on 'Quantum Mechanics and Reality,' Einstein tried to express his principal dissatisfaction with quantum mechanics by saying that 'though this theory means an important and in some sense even final progress of physical knowledge,'[265] he turned to the example of the description of a free particle by a wave packet of finite extension. Since the energy and momentum of this particle cannot be stated accurately, two possibilities arise: (a) the free particle has actually a definite momentum and position, and thus the ψ-function gives an incomplete description; (b) the particle has in reality neither a definite momentum nor a definite position. Then sharp position in the measurement is created by the measurement itself. But then Einstein claimed that there are two wave functions which describe the reality — either sharp momentum or sharp position.

Then he turned to the description of reality in physics. 'Essential for the ordering of the objects introduced into physics seems to be further that at a definite instant of time these objects claim an existence independent of each other so far as these objects "are situated in different parts of the space."'[266]

Einstein claimed that quantum mechanics was not consistent with the principle of 'direct action.' Recalling his consideration that had been raised in the 1935 paper with Podolsky and Rosen, he came to the conclusion that though the result does not contradict quantum mechanics except for the principle of 'direct action,' because it meant that a measurement of the system I, spatially distant from II, would influence that system. 'But then all wave functions ascribed to the system by measuring various quantities of I have to be either the same, which is not the case, or the description of nature by quantum mechanics is incomplete.'[266]

We have already referred to the article of Niels Bohr in the same issue of *Dialectica,* in which he gave a brief account of his view.

Finally, Werner Heisenberg discussed 'The Concept of a "Closed Theory" in Modern Science.'[267] By a 'closed theory' — e.g. Newton's mechanics, Maxwell's electrodynamics, nonrelativistic quantum mechanics — Heisenberg meant a theory which (a) does not suffer from internal inconsistencies, (b) represents empirical facts, and (c) may and will have limitations. Despite the limitations, it allows us to use its concepts in a broader domain of experience.

6

Does God Play Dice?

In a letter to Max Born on 29 April 1924, Albert Einstein wrote: 'Bohr's opinion about radiation is of great interest. But I should not want to 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*, not only its moment to jump off, but also its direction. In that case, I would rather be a cobbler, or even an employee of a gaming-house, than a physicist. Certainly my attempts to give tangible form to the quanta have foundered again and again, but I am far from giving up hope. And even if it never works there is always that consolation that this lack of success is entirely mine.'[268]

Max Born and Albert Einstein, two physicists of nearly the same age, got to know each other quite early. Their first meeting took place at the *Naturforscherversammlung* in Salzburg in October 1909, where Einstein became well known because of his famous lecture on the nature of light. The first well-known work of Born was connected with an extension of Einstein's theory of specific heats. In 1913, Born became an extraordinary (i.e. associate) professor in the University of Berlin, and had often the opportunity to have discussions with Einstein. In 1919 he moved to Frankfurt, in an exchange of positions with Max von Laue (who wanted to be close to Max Planck). Max and Hedwig Born (his wife) and Einstein corresponded with each other frequently throughout their lives. In the earliest letters there was not much exchange about the nature of light-quanta. It seems that Born always accepted Einstein as the more ingenious friend and colleague, and did not dare to compare his own kind of computations with the sharp and original ideas of his great friend. The situation did not change greatly when Born moved to Göttingen in 1921 together with the experimentalist James Franck, and Göttingen became one of the foremost centers of research in quantum theory. Among Born's earliest assistants were Wolfgang Pauli and Werner Heisenberg (both of whom came from Sommerfeld in Munich), Pascual Jordan and Friedrich Hund. Born started quantum-theoretical calculations on atoms with Pauli. He reported to Einstein: 'W. Pauli is now my assistant; he is amazingly intelligent, and very able. At the same time he is very human for a 21-year-old — normal, gay and childlike. Unfortunately he wants to go away again in the summer to [Wilhelm] Lenz in Hamburg, as he had already promised.'[269] In summer 1922, Niels Bohr visited Göttingen to give a series of lectures, later called the 'Bohr Festival.' Heisenberg recalled in the *Bohr Memorial Volume*: 'For the first time I met Niels Bohr in Göttingen in summer 1922, when Bohr gave a series of lectures at the invitation of the faculty of sciences, which we liked to call the "Bohr Festival." Sommerfeld, my teacher in Munich, had taken me along to Göttingen, although I was at that time only a 20-year-old student in my fourth semester.'[270]

In 1923, Heisenberg went to work with Born in Göttingen, and Born and his collaborators worked on 'guessing' the correct quantum rules. It was in the course of this work that Heisenberg's 'mysterious' — later famous — paper 'Quantum-Theoretical Reinterpretation of Kinematic and Mechanical Relations' developed,[271] which initiated the new and correct quantum theory, and Born was the first to discover the matrix formulation of the theory. Already on 15 July 1925, Born informed Einstein that 'Heisenberg's latest paper, soon to be published, appears rather mystifying but is certainly true and profound; it enabled Hund to bring into order the whole of the periodic system with its complicated multiplets.'[272] 'As regards physics, ... your kind remarks about my activities spring from your kind heart. I am fully aware, however, that what I am doing is very ordinary stuff compared with your ideas and Bohr's. My thinking box (*Gedankenkasten*) is very shaky — there is not much in it, and what there is rattles to and fro, has no definite form, and gets more and more complicated. Your brain, heaven knows, looks much neater; its products are clear, simple, and to the point. With luck, we may come to understand them in a few years' time. This is what happened in the case of your and Bose's degeneracy statistics. Fortunately, Ehrenfest turned up here and cast some light on it.'[272] Born also referred to Louis de Broglie's wave theory of matter: 'Then I read Louis de Broglie's paper, and gradually saw what they are up to. I now believe that the wave theory of matter could be of very great importance. Our Mr. Elsasser's reflections are not yet in proper order. To begin with, it transpired that he made a considerable error in his calculations, but I still believe that the essence of his remarks, particularly on the reflection of electrons, can be salvaged. I am also speculating a little about de Broglie's waves. It seems to me that a connection of a completely formal kind exists between these and that other mystical explanation of reflection, diffraction and interference using "spatial" quantization which Compton and Duane proposed and which has been more closely studied by Epstein and Ehrenfest.'[273] Elsasser's ideas could be considered the first proof of de Broglie's wave theory of matter.

Soon, however, Born became more excited. Matrix mechanics took a definite shape in the collaboration with Heisenberg and Jordan in fall 1925.[274] And even Einstein noted in a letter to Mrs. Born, dated 7 March 1926: 'The Heisenberg–Born concepts leave us all breathless, and have made a deep impression on all theoretically-oriented people. Instead of dull resignation, there is now a singular tension in us sluggish (*Dickblüter*) people.'[275] But Einstein's excitement did not last very long. He wrote again to his friend Born: '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 it does not bring us any closer to the secret of the "Old One." I, at any rate, am convinced that *He* is not playing at dice.' [276]

Since that time differences of opinion arose between Einstein and Born, which did not destroy their friendship but produced great disagreements about their views on the quantum theory. Born worked hard again like a young man in the development of quantum mechanics with his younger collaborators, and took a very active part in the whole enterprise. Einstein remained an outsider. Later on, in 1933, both of them had to leave Germany; Einstein went to Princeton, and Born went first to Cambridge in England and then settled in Edinburgh, Scotland. They never met again personally, but their discussion and interchange grew through long distance exchange of correspondence. After Bohr, Born became the main discussion partner of Einstein about matters relating to the quantum problem in one of the most exciting dialogues in the history of science.

6.1. The 'Statistical' Einstein

In the 70th birthday volume for Einstein,edited by Paul Arthur Schilpp, Max Born was invited to contribute an essay, in which he wrote: 'Here I propose to discuss Einstein's contributions to statistical methods in physics. His publications on this subject can be divided into two groups: an early set of papers deals with classical statistical mechanics, whereas the rest is connected with Einstein's philosophy of science. He has seen more clearly than anyone before him the statistical background of the laws of physics, and he was a pioneer in the struggle for conquering the wilderness of quantum phenomena. Yet later, when out of his own work a synthesis of statistical and quantum principles emerged which seemed to be acceptable to almost all physicists, he kept himself aloof and sceptical. Many of us regard this as a tragedy — for him, as he gropes his way in loneliness, and for us who miss our leader and standard-bearer.'[277]

When Born wrote this essay in the late 1940s, he had started the deepest philosophical discussions of his life. He had contributed himself to many parts of theoretical physics and had developed many sound theories since the time he first developed the theory of specific heat together with his colleague Theodore von Kármán. But with respect to Einstein, he saw himself most of the time as a mere craftsman, thinking about technical details, in which Einstein took no great interest. However, from Born's laborious work important aspects of the theory of microscopic phenomena emerged, which made the 'technical' and 'down-to-earth' work immensely valuable. And, to Born, the ideas of Einstein served as the guiding light, which he took more seriously than Einstein himself in his later years. Let's see what Born cited as the characteristic contributions of Einstein to the statistical description of nature.

We know that from Einstein's work on the foundations of thermodynamics there emerged the theory of Brownian motion and a proof of the reality of the molecular structure of matter. From this work Born drew the following conclusion: 'But beyond this physical result, Einstein's theory of Brownian motion had a most important consequence for scientific methodology in general. The accuracy of measurement depends on the sensitivity of the instruments, and this again on the size and weight of the mobile parts and restoring forces acting on them. Before Einstein's work, it was tacitly assumed that progress in this direction was limited only by experimental technique. Now it becomes obvious that this was not so. If an indicator, like the needle of the galvanometer, became too small or the suspending fiber too thin, it would never be at rest but perform a kind of Brownian movement. This has in fact been observed. Similar phenomena play a large part in modern electronic technique, where the limit of observation can be heard as "noise" in a loudspeaker. There is a limit of observability given by the laws of nature themselves.'[278]

The next great contribution of Einstein, the concept of the light-quantum, was even more connected with the 'statistical' description of nature. From an interpretation of the empirical facts as expressed by Planck's law, Einstein derived in 1909 the dual nature of radiation, containing both the wave and the particle aspect. 'It became clear that the laws of microphysics differed fundamentally from the matter in bulk. Nobody has done more to elucidate this than Einstein,' said Born.[279]

After a long period in which he tried to unify the description of radiation, Einstein produced in 1916 a theory which did the job. In calculating the processes which lead to equilibrium between matter and radiation, he succeeded on the basis of very few assumptions about the elementary processes, to derive Planck's radiation formula. But this derivation was based on statistical laws, and the processes bear some similarity to radioactivity. And Born commented: 'Of course, I am sure that Einstein himself was — and still is — convinced that there are structural properties in the excited atom which determine the exact moment of emission, and that probability is called in only because of our incomplete knowledge of the prehistory of the atom. Yet the fact remains that he has initiated the spreading of indeterministic statistical reasoning from its original sources, radioactivity, into other domains of physics.'[280] From here the path opened for Niels Bohr, who, immediately after Einstein's 1916/17 publications, based his correspondence principle entirely on this work and pursued the statistical aspects further, till the correct formalism of microscopic theory emerged.

But, also, the second contribution of Einstein's concerning the unification of the two aspects of matter, i.e. the quantum theory of monatomic ideal gases, finally led in the same direction. 'Einstein is therefore clearly involved in the foundation of wave mechanics, and no alibi can disprove it.'[281] Thus Einstein 'from the very beginning used probability as a tool for dealing with nature just like any scientific device.'[281] What he thought about these tools, he formulated in his obituary of Ernst Mach: 'Concepts which have been proved to be useful in ordering things easily acquire such an authority over us that we forget their human origin and accept them as invariable. Then they become "necessities of thought," "given *a priori*," etc. The path of scientific progress is then, by such errors, barred for a long time. It is therefore no useless game if we are practicing to analyze current notions and to point out on what conditions their justification and usefulness depend, how they have grown especially from the data of experience. In this way their exaggerated authority is broken. They are removed, if they cannot properly legitimize themselves; corrected, if their correspondence to the given things is too negligently established; replaced by others, if a new system can be developed that we prefer for good reasons.'[282]

Now the question was whether the description of nature finally required the concept of probability, and this was the topic of Einstein's last discussion with his friend Max Born.

6.2. Einstein's Last Discussion About Statistical Causality and Determinism

The last phase of Einstein's discussions on the principles of the description of nature started rather early in letters to his friend Max Born. However, their tenor did not become very serious before 1947, when Born contributed his article to the Schilpp volume in honor of Einstein. Before that time, Einstein often mentioned that 'we have become antipodean in our scientific expectations. You believe in the God who plays dice, and I in complete law and order in a world which objectively exists, and which I, in a wildly speculative way, am trying to capture. I firmly *believe*, but I hope that someone will discover a more realistic way, or rather a more tangible basis than it has been my lot to find. Even the great initial success of quantum theory does not make me believe in the fundamental dice game, although I am well aware that our younger colleagues interpret this as a consequence of senility. No doubt the day will come when we shall see whose instinctive attitude was the correct one.'[283] On 3 March 1947, Einstein wrote to Born: 'I cannot make a case for my attitude in physics which you would consider at all reasonable. I admit, of course, that there is a considerable amount of validity in the statistical approach which you were the first to recognize clearly as necessary, given the framework of the existing formalism. I cannot seriously believe in it because the theory cannot be reconciled with the idea that physics should represent a reality in time and space, free from spooky actions at a distance. I am, however, not yet firmly convinced that it can really be achieved with a continuous field theory, although I have discovered a possible way of doing this which so far seems quite reasonable. The calculation difficulties are so great that I will be biting the dust long before I myself can be fully convinced of it. But I am quite convinced that someone will eventually come up with a theory whose objects, connected by laws, are not probabilities but considered facts, as used to be taken for granted until quite recently. I cannot, however, base this conviction on logical reasons, but can only produce my little finger as witness, i.e. I offer no authority which would be able to command any kind of respect outside my own hand.' [284]

In 1948 Einstein sent Born a reprint of his article in the *Dialectica* which we have discussed above, to inform him about his judgment. Beyond Bohr's criticism, which we have already quoted, Born pointed out in a letter to Einstein on 9 May 1948: 'It seems to me that your axiom of the "independence of spatially separated objects A and B" is not as convincing as you make it out. It does not take into account the fact of coherence; objects far apart in space which have a common origin need not be independent. I believe that this cannot be denied and simply has to be accepted. Dirac has based his whole book on this. You say: The methods of quantum mechanics enable one to determine ψ_2 of S_2 from ψ_{12}, provided a complete measurement, in the quantum-mechanical sense, of the spatial system S_1 exists as well. You evidently assume that ψ_{12} is already known. Therefore a measurement in S_1 does not really give any information about events occurring in far distant S_2, but only in association with the information about ψ_{12}, i.e. with the help of additional earlier measurements. In the optical example, we have information that both partial beams are produced from one single beam by one crystal.'[285] In a commentary on Einstein's article and his own reply, Born noted: 'In his [Einstein's] eyes, the theory of light must be considered incomplete as well. He looked forward to the creation of a more profound theory which would do away with this state of imperfection. So far his hopes have not been realized, and the physicists have good reasons for believing this to be impossible.'[286]

In a letter dated 15 September 1950, Einstein made further attempts to explain his philosophy. 'There is nothing analogous in relativity to what I call incompleteness of description in the quantum theory. Briefly it is because the ψ-function is incapable of describing certain qualities of an individual system, whose "reality" we none of us doubt (such as a macroscopic parameter).

'Take a (macroscopic) body which can rotate freely about an axis. Its state is fully determined by an angle. Let the initial conditions (angle and angular momentum) be defined as precisely as the quantum theory allows. The Schrödinger equation then gives the ψ-function for any subsequent time interval. If this is sufficiently large, all angles become (in practice) equally probable. But if an observation is made (e.g. by flashing a torch), a definite angle is found (with sufficient accuracy). This does not prove that the angle had a definite value before it was observed — but we believe this to be the case, because we are committed to the requirements of reality on the macroscopic scale. Thus, the ψ-function does not express the real state of affairs perfectly in this case. This is what I call "incomplete description."

'So far, you may not object. But you will probably take the position that a complete description would be useless because there is no mathematical relationship for such a case. I do not say that I am able to disprove this view. But my instinct tells me that a complete formulation of the relationships is tied up with a complete description of its factual state. I am convinced of this although, up to now, *success* has been against it. I also believe that the current formulation is true in the same sense as, e.g. thermodynamics, i.e. as far as the concepts used are adequate. I do not expect to convince you, or anybody else — I just want you to understand the way I think.

'I see from the last paragraph of your letter that you, too, take the quantum-theoretical description as incomplete (referring to an ensemble). But you are after all convinced that no (complete) laws exist for a complete description, according to the positivistic maxim *esse est percipi*. Well, this is a programmatic attitude, not knowledge. This is where our attitudes really differ. For the time being, I am alone in my views — as Leibniz was with respect to the absolute space of Newton's theory.'[287]

After Max Born retired from the Tait Chair of Natural Philosophy, University of Edinburgh, Einstein dedicated to him an article in the volume (*Festschrift*) *Scientific Papers Presented to Max Born.*[288] In this article, Einstein once more expressed his opinions concerning the quantum-mechanical description and proposed as an example the reflexion of a particle from two parallel walls. He concluded that the quantum theory does not pass into the classical description. The reason was that Einstein chose the wrong Schrödinger equation, as was later pointed out to him by Born.[289] Einstein expected that the quantum system passes into the classical system if the de Broglie wavelength is small, whereas Born passed over into classical mechanics by considering arbitrarily small wave packets in coordinate and momentum, the sharpness of which was due to the large mass of the particle. Einstein saw some difficulties with this prescription: 'I thought that an approximate agreement with classical mechanics was to be expected whenever the relevant de Broglie wavelengths are small in relation to the other relevant spatial measurements. I see, however, that you want to relate classical mechanics only to those ψ-functions which are narrow with respect to coordinates and momenta. But when one looks at it in this way, one could come to the conclusion that macro-mechanics cannot claim to describe, even approximately, most of the events in macro-systems that are conceivable on the quantum theory. For example, one would then be very surprised if a star, or a fly, seen for the first time, appeared even to be quasi-localized.

'But should one now adopt your point of view in spite of this, one should at least demand that a system which is "quasi-localized" at a certain time should *remain* so according to the Schrödinger equation. This is purely a mathematical problem, and you expect that the calculations would bear out this expectation. But this seems quite impossible to me. The easiest way to realize this is to consider the three-dimensional case (of a macro-body), which is represented by a "narrow" Schrödinger function in relation to position, velocity *and direction*. There it seems obvious, even without a mathematical "microscope," that the position must become more and more diffuse in the course of time ... . Oppenheimer has extricated himself by claiming that the time required by the process of getting more and more out of focus would be on a "cosmic" scale, and that one could ignore it for that reason. But one could easily quote some quite pedestrian examples where the divergence time is not all that long. I consider it too cheap a way of calming down one's scientific conscience. All the same it is not difficult to regard the step into probabilistic quantum theory as final ... . One can safely accept the fact that, according to this concept, the description of the single system is incomplete, if one assumes that there is no corresponding complete law for the complete description of a single system which determines its development in time.

'Then one needs to become involved with Bohr's interpretation that there is no reality independent of the probable subject.

'I do not believe, however, that this concept, though consistent in itself, is here to stay. But I maintain that it is the only one which does justice to the mechanism of the probabilistic quantum theory.'[290]

In a letter to Max Born, dated 12 January 1954, Einstein explained his opinion in more detail: 'My assertion is this: the ψ-function cannot be regarded as a complete description of the system, only as an incomplete one. In other words: there are attributes of the individual system whose reality no one doubts but which the description by means of the ψ-function does not include.

'I have tried to demonstrate this with a system which contains one "macro-coordinate" (coordinate of the center of a sphere of 1 mm diameter). The ψ-function selected was that of fixed energy. This choice is permissible, because our question by its very nature must be answered so that the answer can claim validity for every ψ-function. From the consideration of this simple special case, it follows that — apart from the existing macro-structure according to quantum theory — at any arbitrarily chosen time, the center of the sphere is just as likely to be in one position (possible in accordance with the problem) as in any other. This means that the description by a ψ-function does not contain anything which corresponds with a (quasi-)localization of the sphere at a selected time. The same applies to all systems where macro-coordinates can be distinguished ... . I now make the following assertion: if the description by a ψ-function could be regarded as the complete description of the physical condition of an individual system, one should be able to deduce from any ψ-function belonging to a system which has macro-coordinates. It is obvious that this is not so for the specific example which has been under consideration.

'Therefore the concept that the ψ-function *completely* describes the physical behavior of the individual single system is untenable. But one can well make the following claim: if one regards the ψ-function as the description of an *ensemble*, it furnishes statements which — as far as we can judge — correspond satisfactorily to those of classical mechanics and, at the same time, account for the quantum structure of reality. In my opinion the "localization theorem" forces us to regard the ψ-function generally as the description of an "ensemble," but not as the complete description of an individual single system. In this interpretation the paradox of the apparent coupling of spatially separated parts of the systems also disappears. Furthermore, it has the advantage that the description thus interpreted is an *objective* description whose concepts clearly make sense independently of the observation and of the observer.'[291]

Wolfgang Pauli, who visited Princeton in March and April 1954, clarified the discussion between Born and Einstein, the two old friends, greatly. In a letter to Born on 3 March 1954, he stated: 'Now from my conversations with Einstein I have seen that he takes exception to the assumption, essential to quantum mechanics, that *the state of a system is defined only by specification of an experimental arrangement. Einstein wants to know nothing of this.* If one were able to measure with sufficient accuracy, this would of course be as true for small macroscopic spheres as for electrons. It is, of course, demonstrable by specifying thought experiments, and I presume that you have mentioned and discussed some of these in your correspondence with Einstein. But Einstein has the philosophical prejudice that (for macroscopic bodies) a state (termed "real") can be defined "objectively" under *any* circumstances, i.e. *without* specification of the experimental arrangement used to examine the system (of the macro-bodies), or to which the system is being "subjected." It seems to me that the discussion with Einstein can be reduced to this hypothesis of his, which I have called the idea (or the "ideal") of the "detached observer." But to me and other representatives of quantum mechanics, it seems that there is sufficient experimental and theoretical evidence against the practicability of this ideal.'[292]

And, a little later, after he had studied the controversy in detail, Pauli wrote a second letter to Born, dated 31 March 1954: 'In particular, Einstein does not consider the concept of "determinism" to be as fundamental as it is frequently held to be (as he told me emphatically many times), and he denied energetically that he had ever put up a postulate such as (your letter [says]): "the sequence of such conditions must also be objective and real, i.e. automatic, machine-like, deterministic." In the same way, he *disputes* that he uses as criterion for the admissibility of a theory the question "Is it rigorously deterministic?"

'Einstein's point of departure is "realistic" rather than "deterministic," which means that his philosophical prejudice is a different one. His train of thought can reproduced briefly *thus*:

'1. A preliminary question: Do all mathematically possible solutions of the Schrödinger equation, even in the case of a macro-object, occur in nature under certain conditions (*in my opinion this question has to be answered in the affirmative whatever happens*) or only in those special cases where the position of the object is "exactly," "sharply" defined? … .

'2. Now to Einstein's *essential question*: *How are those solutions of the Schrödinger equation which do not belong to class K° (for example, macro-objects) to be interpreted in physical terms?*

'Here Einstein's reasoning is as follows:

'A. When one "looks at" a macro-body, it has a quasi-sharply-defined position, and it is not reasonable to invent a causal mechanism according to which the "looking" fixes the position. 'B. Therefore a macro-body must *always* have a quasi-sharply-defined position in the "objective description of reality." As those ψ-functions which do *not* belong to class K° cannot in principle be "thrown away," and must *also* be in accordance with nature, the *general* ψ-function can only be interpreted as an *ensemble* description. If one wants to assert that the description of a physical system by a ψ-function is *complete*, one has to rely on the fact that *in principle* the natural laws refer only to the ensemble description, which Einstein does not believe (not only in those at present known to us).

'What *I* do not agree with is Einstein's reasoning B (please note that the concept of "determinism" does not occur in it at all!). I believe it to be *untrue* that a "macro-body" always has a quasi-sharply-defined position, as I cannot see any fundamental difference between micro- and macro-bodies, and as one always has to assume a portion which is inderminate to a considerable extent wherever the *wave aspect* of the physical object concerned manifests itself. The appearance of a definite position x_0 during a subsequent observation (for example, "illumination of the place with a shaded lantern") ... is then regarded as being a "creation" existing outside the laws of nature, even though it cannot be influenced by the observer. The natural laws only say something about the *statistics* of these acts of observation.

'As O. Stern said recently, one should no more rack one's brain about the problem of whether something one cannot know anything about exists all the same, than about the ancient question of how many angels are able to sit on the point of a needle. But it seems to me that Einstein's questions are ultimately always of this kind.'[293] Pauli further pointed out that in quantum mechanics, knowledge due to an earlier observation is lost by a new measurement.

Max Born later worked out Einstein's problem — the reflection of a particle between two parallel walls.[294] Later on he remarked: 'Although this problem treats a physically trivial and practically unimportant case, it gives a clear insight into the connection between classical and quantum mechanics, and seems to me to be more useful than all philosophizing about those questions.'[295]

7

Mach contra Kant: Aspects of the Development of Einstein's Natural Philosophy

In an article in the *Naturwissenschaften* on 'The Physical Theory in the Light of New Researches,' in 1926, Max Planck wrote: 'Times have existed in which philosophy and science were hostile towards each other. These times have passed long ago. The philosophers have understood that one cannot prescribe to the scientists the methods according to which they work and the goals which they wish to pursue; and the scientists have agreed that the starting point of their researches is not contained entirely in the perceptions of the senses, and that also science cannot develop without a certain amount of metaphysics. In particular, the new physics emphasizes the old truth with the utmost acuteness: there exist realities which do not depend on our perceptions of senses, and there are problems and conflicts in which these realities attain a higher value for us than the richest treasures of our whole world of senses.'[296]

It is often told, and this fact can be proved by many statements he made in his papers, that the young Albert Einstein was one of the most pragmatic scientists who ever performed research. He was, as he confirmed frequently, deeply influenced by the philosophy of Ernst Mach, though he turned out — in part — to be his opponent, for instance in the question of the atomic and molecular structure of matter. One of the results of that influence was that Einstein preferred to call his early theories, including the special theory of relativity, 'heuristic points of view.' Later on, he put more emphasis on the fact that his theories ought to embrace several phenomena which could not be related by earlier theories. Although, in Mach's mind, this point of view led Einstein to consider speculations, he was never willing to accept himself the new attitude which Einstein took roughly since 1910 that could be regarded as agreeing with Mach's principle of the 'economy of thought.' But even later, Einstein was to reject the guiding philosophy of his youth rather clearly, when he emphasized the role which was played in theory by free inventions of the mind. In addition to this point, he developed a special view concerning the reality with which physics had to deal. In the latter attitude he came closer to Max Planck and, in particular, to the philosophical considerations of Immanuel Kant, whom he had not appreciated at all in his earlier years. We may, therefore, describe Einstein's philosophical path as leading from Mach to Kant.

7.1. The Heuristic Points of View

In his *Science of Mechanics*, Ernst Mach wrote: 'Nature is composed of sensations as its elements. ... Sensations are not signs of things; but, on the contrary, a thing is a thought-symbol for a compound sensation of relative fixedness. Properly speaking, the world is not composed of "things" as its elements, but colors, tones, pressures, spaces, times, in short what we call ordinary individual sensations.'[297] In 1905, Einstein submitted two papers of an apparently very different nature with respect to their goals and tools. In the shorter one, he considered certain phenomena of radiation, with a new 'heuristic point of view.'[298] Three months later, he submitted another paper, rather different in nature, dealing with 'The Electrodynamics of

Moving Bodies.'[299] This article was not only longer, but it talked about the introduction of new concepts like measurements of lengths and time, which were of a different nature than those used in classical mechanics, as introduced by Newton. The relativistic mechanics, which related 'events' in a new and more general form than classical physics, had to replace the Galilean–Newtonian theory. Though the research which led to the new kinematics was carried out with the critical attitude which Mach had introduced, and the phrases which Einstein used avoided any kind of speculation, the result was a complete theory, forming a new system. But still, in 1907, Einstein mentioned in his remarks about a note of Ehrenfest's: 'The principle of relativity or — more accurately expressed — the principle of relativity together with the principle of the constancy of the velocity of light should not be regarded as a "closed system"; in fact, not even as a system at all, but merely as a heuristic principle, which, considered *per se*, contains only assertions concerning rigid bodies, clocks, and light signals. Beyond this, the only result which the theory of relativity yields is that it imposes relations between physical laws which otherwise seem to be independent of each other.'[300]

One might interpret this passage in two ways. First, it uses one of Einstein's favorite phrases, the 'heuristic viewpoint,' which was much in the spirit of Mach's philosophy concerning the description of nature. On the other hand, it also considered as a statement of Einstein's knowledge that the theory of special relativity was not a completed system, and that one had, in fact, to go beyond its application and take into account also accelerated motions. Once again, Mach proved to be a good guide for the young Einstein. In reviewing Newton's work and concepts, Mach had rejected the interpretation of the famous 'Newton's pail-experiment.' 'Newton's experiment with the rotating pail of water simply informs us that the relative rotation of the water with respect to the sides of the vessel produces *no* noticeable centrifugal forces, but that such forces *are* produced by its relative rotation with respect to the mass of the earth and other celestial bodies. No one is competent to say how the experiment would turn out if the sides of the vessel were increased in thickness and mass until they were several leagues thick.'[301] And in the same section Mach studied in detail the relation between the masses and found that in dealing with them the influence of the rest of the world could not be disregarded. This leads to the famous Mach's principle, which states that the masses of the objects are created by the interaction with far distant masses. This concept went into the construction of Einstein's theory of gravitation as one of the main building blocks.

Thus far Mach's physics and philosophy had helped Einstein in developing his theories. And Einstein, in his *Autobiographical Notes,* judged correctly: 'We must not be surprised, therefore, that, so to speak, all physicists of the last century saw in classical mechanics a firm and final foundation for all of physics, indeed, for all natural science, and that they never grew tired in their attempts to base Maxwell's theory of electromagnetism, which, in the meantime, was slowly beginning to win out, upon mechanics as well. Even Maxwell and H. Hertz, who in retrospect appear as those who demolished the faith in mechanics as the final basis of all physical thinking, in their conscious thinking, adhered throughout to mechanics as the secure basis of physics. It was Ernst Mach who, in his *History of Mechanics,* shook this dogmatic faith; this book exercised a profound influence upon me in this regard while I was a student. I see Mach's greatness in his incorruptible skepticism and independence; in my younger years, however, Mach's epistemological position also influenced me very greatly, a position which today appears to me to be essentially untenable.'[302]

When did Einstein become free from Mach's ideas? It would be very tempting to assume that Einstein lost faith in Mach's philosophy when he found out that Mach's good judgment had finally failed him. This was in the question of the molecular constitution of matter. Einstein's theory of Brownian motion had resulted in a formula which could be checked completely by Jean Perrin and his school, and this experimental proof was completed by 1909, when Perrin published his long paper on the subject. Wilhelm Ostwald, one of the greatest opponents of the concept of molecules, had agreed that these experiments gave full evidence of their existence. Only Mach retained his hostile attitude. Einstein himself, when he attended the Congress of German Scientists and Physicians in Vienna in the fall of 1913, visited Mach on that occasion. Philipp Frank reported in his book on Einstein: 'Hence, after conversing awhile with Mach, Einstein raised the following question: "Let us suppose that by assuming the existence of atoms in a gas we were able to predict an observable property of this gas that could not be predicted on the basis of a nonatomistic theory. Would you then accept such a hypothesis even if the calculations required very complicated computations, comprehensible only with great difficulties? I mean, of course, that *from* this hypothesis *one* could infer the interrelation of several observable properties that without it remained unrelated. Is it 'economical' to assume the existence of atoms?" Mach answered: "If with the help of the atomistic hypothesis one could actually establish a connection between several observable properties which without it would remain isolated, then I should say that this hypothesis was an 'economical' one; because with its aid relations between various observations could be derived from a single assumption. Nor should I have any objection even if the requisite computations were complicated and difficult."

'Einstein was exceedingly satisfied with this statement and replied: "By 'simple' and 'economical' you mean, then, not a 'psychological economy' but rather a 'logical economy.' The observable properties should be derived from as few assumptions as possible, even though these assumptions appear 'arbitrary' and the computation of the results might be difficult."'[303]

Thus Einstein arrived at an agreement with Mach in this question though it probably meant less for Mach. Frank does not mention whether they also talked about relativity. Here Mach's last word was rather a rejection of the scientific process he had started, at least in Einstein's mind. In the preface to *The Physical Principles of Optics,* Mach wrote in July 1913, just a little before meeting with Einstein in Vienna, that he felt compelled 'to dismiss my contemplation of the relativity theory. ... The reason why, and the extent to which I discredit the present day relativity theory, which I find growing to be more and more dogmatical, together with the particular reasons which have led me to such a view — the considerations based on the physiology of the senses, the theoretical ideas, and above all the conceptions resulting from my experiments — must remain to be treated in the sequel.'[304] The second part never appeared, and also the printing of the first was delayed until after Mach's death in 1916.

7.2. The Economy of Thought

In his book *The Science of Mechanics,* Ernst Mach wrote: 'Experience alone, without the ideas that are associated with it, would forever remain strange to us. Those ideas that hold good throughout the widest domains of research and that supplement the greatest amount of experience, are the *most scientific.* The principle of continuity, the use of which everywhere pervades modern inquiry, simply prescribes a mode of conception which conduces in the highest degree to the economy of thought.'[305]

This 'economy' replaced in Mach's language the conventional notion of a theory, and even gave the formulation of a theory a certain direction. Mach compared his attitude with that which exists in the life of people. Though this view cannot be considered typical for the young Einstein, he nevertheless found it useful, as can, for example, be recognized from the conversation with Mach cited above. In Einstein's heart, besides the soul which created the 'heuristic points of view,' there was another soul. Already in his first published paper, he mentioned the search for a unification of theories.[306] The successful young Einstein seemed to have forgotten about this search for unity, but this was not altogether true. In his review article 'On the Principle of Relativity and the Consequences Drawn from It' for Johannes Stark's *Jahrbuch der Radioaktivität under der Elektronik,* he remarked concerning the deviation of his predicted results in Kaufmann's experiments: 'It has further to be mentioned that the theories of the motion of the electron of Abraham and Bucherer provide curves which fit better with the observed ones than the slope determined from the theory of relativity. However, in my opinion, those theories are rather improbable because their basic assumptions concerning the size of the moving electrons are not suggested by theoretical systems embracing larger complexes of phenomena.'[307]

Evidently, Einstein never abandoned the principle of unification of theories, and the search for it became already apparent in his endeavors to understand the light-quantum, which he saw in its most general aspect before even the most brilliant speculative scientists were able to follow him.

In his *Autobiographical Notes,* Einstein gave a later version of the principle of the 'economy of thought.' 'The second point of view [according to which a theory has to be criticized] is not concerned with the relation to the material of observation but with the premise of the theory itself, with what may briefly but vaguely be characterized as the "naturalness" or "logical simplicity" of the premises (of the basic concepts and the relations between these which are taken as a basis). This point of view, an exact formulation of which meets with great difficulties, has played an important role in the selection and evaluation of theories, since time immemorial. The problem here is not simply one of a kind of enumeration of the logically independent premises (if anything like this were at all unequivocally possible), but that of a kind of reciprocal weighing of incommensurable qualities. Furthermore, among theories of an equally "simple" foundation, that one is to be taken as superior which most sharply delimits the qualities of systems in the abstract (i.e. contains the most definite claims). Of the "realm" of theories I need not speak here, inasmuch as we are confining ourselves to such theories whose object is the *totality* of all physical appearances. The second point of view may be characterized as concerning itself with the "inner perfection" of the theory. We prize a theory more highly if, from the logical standpoint, it is not the result of an arbitrary choice among theories, which, among themselves, are of equal value and analogously constructed.'[308]

In an article on 'Mach, Einstein, and the Search for Reality,' Gerald Holton considered this unifying trend in Einstein's research as already opposed to Mach's philosophy.[309] However, based on available sources, we prefer to consider the 'search for unity' *per se* not to be at variance with Mach's philosophical principles. In particular, we refer to the exchange of ideas, which, from Philipp Frank's report, occurred during Einstein's visit in 1913.

7.3. 'Theories Are Free Inventions of the Mind'

In an address on 'The Principles of Research' in honor of Max Planck's 60th birthday, Albert Einstein said: 'The supreme task of the physicist is to arrive at those universal elementary laws from which the cosmos can be built up by pure deduction. There is no logical path to these laws; only intuition, resting on sympathetic understanding of experience, can reach them. In this methodological uncertainty, one might suppose that there were any number of possible systems of theoretical physics all equally well justified; and this opinion is no doubt correct, theoretically. But the development of physics has shown that at any given moment, out of all conceivable constructions, a single one has always proved itself decidedly superior to all the rest. Nobody who has really gone deeply into the matter will deny that in practice the world of phenomena uniquely determines the theoretical system in spite of the fact that there is no logical bridge between phenomena and their theoretical principles; that is what Leibniz described so happily as a "pre-established harmony." Physicists often accuse epistemologists of not paying sufficient attention to this fact. Here, it seems to me, lie the roots of the controversy carried on some years ago between Mach and Planck.'[310]

Concerning Einstein's philosophy, his biographer (and himself a philosopher of science) Philipp Frank remarked: 'Since the positivistic conception of physics had been stimulated strongly by Einstein's pioneering work in the theory of relativity and in atomic physics, many people regarded Einstein as a kind of patron saint of positivism. To the positivists he seemed to bring the blessing of science, and to their opponents he was an evil spirit. Actually his attitude to positivism was by no means simple. The contradictions in his personality that we have observed in his conduct as a teacher and his attitude to political questions also manifested themselves in his philosophy.'[311]

To clarify this statement a little more, we should point out that Frank regarded himself as a positivist and disciple of Ernst Mach, whose lectures he had attended in Vienna, and whose successor he had been (after Einstein) in Prague. Frank described as the positivistic criterion of science that 'only those propositions should be employed from which statements regarding observable phenomena can be deduced.'[312] He was very surprised to find that in 1929, at the Congress of German Scientists and Physicians in Prague, Einstein was not in agreement with the positivistic ideas of Mach.[313] In fact, Einstein's views had since quite a while ago gone beyond Mach's philosophy, as can be seen from his address in honor of Planck's 60th birthday (cited earlier). From that, one might judge that Einstein was influenced in his views by Planck himself, with whom he exchanged ideas frequently during his stay in Berlin, and about whom he had changed some of his opinions and prejudices.[314] But this influence alone was not the whole truth. Einstein arrived at his 'later' philosophy quite independently.

One reason was that the theory of gravitation forced him to invent his theories rather far from primary empirical evidence. That is, given only a few facts, he had to erect a high structure, and it was not possible to find the results piecemeal, while checking against experimental data. And, as for the extension of the theory, which should unify gravitation and electromagnetism as well as include quantum effects, he had even less information from direct observation. Worse still, the empirical facts had helped to establish the theory (quantum mechanics), which he did not consider to be the final answer.

The direct evidence was very often apparently against Einstein's theories. For example, in the case of the light-quantum, Einstein's old (1905) hypothesis was not definitely established empirically until 1924. During that time, Niels Bohr had drawn many consequences directly from experiments; and Einstein's opposition to Bohr arose at the time when Bohr denied the conservation of energy and momentum in the elementary process of the scattering of light by matter. Fortunately, the later experiments turned out in Einstein's favor, but Einstein remained suspicious of Bohr's positivistic attitude.

Einstein expressed his point of view extremely clearly in his Herbert Spencer Lecture, delivered at Oxford on 10 June 1933: 'The natural philosophers of those days [Newton's days] were, on the contrary, most of them possessed with the idea that the fundamental concepts and postulates of physics were not in the logical sense free inventions of the human mind but could be deduced from experience by "observation" — that is to say, by logical means. A clear recognition of the erroneousness of this notion really only came with the general theory of relativity, which showed that one could take account of a wider range of empirical facts, and that, too, in a more satisfactory and complete manner, on a foundation quite different from the Newtonian. But quite apart from the question of superiority of one or the other, the fictitious character of fundamental principles is perfectly evident from the fact that we can point to two essentially different principles, both of which correspond with experience to a large extent; this proves at the same time that every attempt at a logical deduction of the basic concepts and postulates of mechanics from elementary experiences is doomed to failure.

'If, then, it is true that the axiomatic basis of theoretical physics cannot be extracted from experience but must be freely invented, can we ever hope to find the right way? ... . I answer without hesitation that there is, in my opinion, a right way, and that we are capable of finding it. Our experience hitherto justifies us in believing that nature is the realization of the simplest conceivable mathematical ideas. I am convinced that we can discover by means of purely mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena. Experience may suggest the appropriate mathematical concepts, but they most certainly cannot be deduced from it. Experience remains, of course, the sole criterion of the physical utility of a mathematical construction. But the creative principle resides in mathematics. In a certain sense, therefore, I hold it true that pure thought can grasp reality, as the ancients dreamed.'[315]

Let us compare these statements with the corresponding ones to be found in Mach's *Science of Mechanics*. 'The function of science, as we take it, is to replace experience. Thus, on the one hand, science must remain in the province of experience, but, on the other, must hasten beyond it, constantly expecting confirmation, constantly expecting the reverse. Where neither confirmation nor refutation is possible, science is not concerned.'[316] And a little later: 'We fill out the gaps in experience by the ideas that experience suggests.'[317]

7.4. Between Scylla and Charybdis

In a lecture on 'Experiment and Theory in Physics,' Max Born said: 'It is natural that a man should consider the work of his hands or his brain to be useful and important. Therefore nobody will object to an ardent experimentalist boasting of his measurements and rather looking down on the "paper and ink" physics of his theoretical friend, who on his part is proud of his lofty ideas and despises the dirty fingers of the other. But in recent years this kind of friendly rivalry has changed into something more serious. In Germany a school of extreme experimentalists, led by Lenard and Stark, has gone so far as to reject theory altogether as an invention of the Jews and to declare experiment to be the only genuine "*Aryan*" method of science. There is also a movement in the opposite direction which — though not racial — is much less radical, claiming that to the mind well trained in mathematics and epistemology the laws of Nature are manifest without appeal to experiment. Two distinguished astronomers, Milne and Eddington, follow the philosophy, though it seems to lead them in rather different directions.'[318]

Arthur Stanley Eddington, whose views Born attacked in this lecture, had developed 'on pure reason' a theory 'explaining' two important numbers which occur in nature: the fine-structure constant $e^2/\hbar c$ and the ratio of masses of the proton and electron.[319] He had introduced a 'phase space' of E-numbers, in fact a free invention of his mind, and Born commented upon this by saying: 'I am far from attacking Eddington's theories or from doubting his results. If they should turn out to be right I shall rejoice. But I shall not attribute this (possible) success to Eddington's philosophy, as a doctrine which could be followed by others, but to his personal genius and intuition.'[320]

In a letter to Max Born on 7 September 1944, Einstein mentioned his published lecture, and said: 'I have read your lecture against Hegelianism [*Hegelei*] with great interest. It represents to us theoreticians the quixotic element — or should I say the seducer?

Where this evil is altogether missing the inveterate philistine rules. I am therefore confident that "Jewish physics" is not to be killed. Moreover I have to confess that your deliberations remind me of the beautiful proverb "*Junge Huren — alte Betschwestern*" ["young whores — old bigots"], particularly when I think of Max Born. But I cannot really believe that you have completely and honestly struggled your way through the latter category.'[283]

It is clear from this letter that Einstein was defending the free intuition which Born, on the other hand, attacked only insofar as it did not relate to experience or claimed to be able to deduce the laws of nature without an empirical premise. Another important trap which, in Born's opinion, could hinder the progress of science was the unjustified application of 'operationalism.' Operationalism went back to Mach, who claimed that the description of nature should deal directly with measurable objects. P. W. Bridgman developed operationalism into a philosophical system.[321] In his Vanuxen lectures Bridgman, after examining Einstein's considerations of simultaneity in the theory of special relativity, stated: 'What Einstein was in effect doing in this instance was to inquire into the meaning of simultaneity, and he was finding the meaning by analyzing the physical operations employed in applying the concept in a concrete instance. It cannot be claimed, I suppose, that Einstein was the first to use this technique; but it is simply that the use of it by him occurred under conditions which dramatically focused attention on its importance, so that physicists are now apparently permanently "reconditioned" in this respect.'[322] And, a little later, Bridgman mentioned: 'The procedure of Einstein was in sharp contrast with the former method of defining concepts, as for example the celebrated definition of Newton of absolute time as that which flows uniformly, independent of material happenings. In the first place this definition was in terms of properties, instead of operations, and in the second place the properties themselves had no operational definition in terms of actual physical operations, but were defined in terms of metaphysical and idealized operations, which could therefore contain no assurance that they correspond to what will be found in experience. As a matter of fact they were found not to have such correspondence to a sufficient degree.'[323]

While Bridgman found the young Einstein to be in accord with the sound principle of operationalism, where only concepts are used that can be defined by a physical operation which may actually be performed, he pointed out that 'Einstein did not carry over into his general relativity theory the lessons and insights which he himself has taught us in his special relativity.'[324] The reason which he cited was that Einstein almost entirely concentrated on the coordinate system used in specifying physical events and neglected the events themselves, while only the events can be dealt with by the operational method.

In his reply to Bridgman's criticism, Einstein answered briefly: 'In order to be able to consider a logical system as physical theory it is not necessary to demand that all of its assertions can be independently interpreted and "tested" "operationally"; *de facto* this has never yet been achieved by any theory and cannot at all be achieved. In order to be able to consider a *physical* theory it is only necessary that it implies empirically testable assertions in general.'[325]

7.5. Presuppositions and Anticipations

In his preface to *The Metaphysical Foundation of Science*, the philosopher Immanuel Kant wrote: 'A rational description of nature deserves the name of science only if the natural laws on which it is based are recognized *a priori* and are not merely empirical laws. One calls a description of nature of the first type a *pure* one; the description according to the second type, however, is called "applied recognition of reason." '[326] (Kant's original text in German runs as follows: '*Eine rationale Naturlehre verdient also den Namen einer Naturwissenschaft nur als dann, wenn die Naturgesetze, die ihr zum Grunde liegen, a priori erkannt werden und nicht blosse*

Erfahrungsgestze sind. Mann nennt eine Naturerkenntnis von der ersten Art rein, die von der zweiten Art aber wird angewandte Vernumfterkenntnis genannt.')[326]

In his Herbert Spencer Lecture at Oxford, Einstein declared: 'We have thus assigned to pure reason and experience their places in a theoretical system of physics. The structure of the system is the work of reason; the empirical contents and their mutual relation must find their representation in the conclusion of the theory. In the possibility of such a representation lie the sole value and justification of the whole system, and especially of the concepts and fundamental principles which underlie it. Apart from that, these latter are free inventions of the human intellect, which cannot be justified either by the nature of that intellect or any such fashion *a priori.*

'These fundamental concepts and postulates, which cannot be further reduced logically, form the essential part of a theory which reason cannot touch. It is the grand object of all theory to make these irreducible elements as simple and as few in number as possible, without having to renounce the adequate representation of any empirical content whatever.'[327]

Einstein had never admired the philosophy of Kant, who meant a great deal to many other German physicists. Instead, he appreciated Hume, and regarded Kant's ideas often as a misinterpretation of Hume's original intention. Thus, for instance, he remarked in his *Autobiographical Notes*: 'Hume saw clearly that certain concepts, as for example that of causality, cannot be deduced from the material of experience by logical methods. Kant, thoroughly convinced of the indispensability of certain concepts, took them — just as they are selected — to be necessary premises of every kind of thinking and differentiated them from concepts of empirical origin. I am convinced, however, that this differentiation is erroneous, i.e. that it does not do justice to the problem in a natural way. All concepts, even those which are closest to experience, are from the point of view of logic freely chosen conventions, just as in the case with the concept of causality, with which this problematic concerned itself in the first instance.'[328]

In fact, the older and wiser Einstein could still not forgive that Kant, before 1800, had raised Newton's concepts about space and time to *a priori* concepts. And he was still young enough to say that even very fundamental concepts are not good enough to serve as long-lived guiding principles. They are, he said, free inventions of the human mind anyway. Both opinions were very characteristic of Einstein and the situation in which he found himself during most of his life: impatient with people who did not follow the latest results of science and knowledge; and in a steady search for the true solution which unifies the description of the world.

It is not of great use to study the relations of Einstein, the physicist and natural philosopher, to a pure philosopher of long ago — in fact from a past century — too much in detail. Questions of the kind asked by Kant were rather different from those which Einstein was willing to answer. The intellectual scheme and organization of philosophers is very often strange to physicists working closely on the track of nature. Philosophers are more concerned with the nature of human beings and their understanding of the world, while physicists try to forget about the concerns of human beings altogether. They first make the human being an objective observer and try to exclude his influence on the observed nature as much as possible. In the sense of philosophy, Niels Bohr could be regarded as much more connected with the methods and goals of philosophy; his ideas about subject and object became very fruitful. But even his influence was rather small.

With these reservations, we may be able to consider Einstein's relations to philosophers with proper care. In his younger years, if Einstein adhered to any philosophy, it was Ernst Mach's; and Mach was too close to sensory observation to appreciate Kant's abstractions. The same abstractions seemed, however, to petrify concepts — like absolute space and time — which could not be upheld. As Einstein noted in his obituary of Mach, which we have already cited: 'Concepts which have been proved to be useful in ordering things easily acquire such an authority over us that we forget their human origin and accept them as invariable. ... They are removed, if they cannot properly legitimize themselves; corrected, if their correspondence to the given things was too negligently established; replaced by others, if a new system can be developed that we prefer for good reasons.'[282, 329] Thus the absolute space and absolute time, to the fixation of which Kant devoted two sections in his 'transcendental aesthetic,' not to say most of it,[330] were abolished in favor of a new system which described not only the empirical facts (which emerged at the beginning of the 20th century) more appropriately, but also 'unified' for the first time the concepts of mechanics and electrodynamics.

No doubt, the *a priori* thinking was not of any help when one looked for a new theory, which simply needed imagination. It might be useful to compare Einstein's attitude with that of Max Planck, who, like Einstein himself, had introduced a new concept but seemed to carry on his research less restlessly than Einstein. In his lecture on 'The Unity of the Physical View,' delivered at Leyden on 9 December 1908, Planck said: 'What is here absolutely the only important thing is the recognition of such a fixed goal, even if it can never be reached fully, and this goal is — not the total adaptation of our thoughts to our sensations but — *the total separation of the physical world view from the individuality of the creative mind.*'[331] He saw the things that lie behind an absolute nature given as independent of us all. And he did not see in sense experiences any basis for the foundation of physics. The physical world view need not be constant but be changed by people and times. Of course, theories have to be checked experimentally — after all Planck's own blackbody radiation law arose in close connection with experiments but it revealed an important property of nature, the constant quantum of action.

Einstein admired unity and logical simplicity, while Planck searched for an absolute and unchangeable world view. For Planck, reality was the absolute truth, while for Einstein, reality had conceivable features, at least conceivable to the human mind: causal connections exist in a theory of the greatest mathematical simplicity. And, above all, Einstein's attitude was to be able to 'play' with theories because, after all, they 'are free inventions of the mind.' This attitude also separated Einstein to a great extent from his contemporary Eddington. Eddington was a master of mathematics, and he tried to derive the entire physics in a deductive manner from a freely invented theory or system, but he soon became a slave of his inventions and believed that *they were the truth,* an error to which Einstein never succumbed.

7.6. Intuition and Experience

In one of his "Last Essays," Henri Poincaré remarked: 'I shall conclude that there is in all of us an intuitive notion of the continuum of any number of dimensions whatever because we possess the capacity to construct a physical and mathematical continuum; and that this capacity exists in us before any experience because, without it, experience properly speaking would be impossible and would be reduced to brute sensations, unsuitable for any organization; and because this intuition is merely the awareness that we possess this faculty. And yet this faculty could be used in different ways; it could enable us to construct a space of three dimensions. It is the exterior world, it is experience which induces us to make use of it in one sense rather than in the other.'[332] And Henry Margenau wrote: 'Einstein's position cannot be labeled by any one of the current names of philosophic attitudes; it contains features of rationalism and extreme empricism, but not in logical isolation.'[333]

In contrast to Mach, the mathematician and physicist Henri Poincaré did not accept the point of view that the general laws of physics are only simple economical summaries of observed facts. But in a way he assumed the other extreme. As Philipp Frank wrote: 'According to him, the general propositions of science, such as the theorem about the sum of the angles of a triangle, the law of inertia in mechanics, the law of conservation of energy, are not statements about reality, but arbitrary stipulations about how words, such as "straight lines," "force," "energy," are to be employed in the propositions of geometry, mechanics, and physics.'[334] Since Poincaré stressed the importance of definitions, his conclusion was: 'The general laws of physics are free creations of the human mind.' This is a phrase which we also meet in Einstein's writings. And, in fact, among the books which Einstein read in his young years in Zurich and in Bern with fellow members of the 'Olympia Academy,' Poincaré's writings were very important.

However, in Einstein's mind this 'positivism' of Poincaré took a rather different form than in the philosophy of other physicists. He was neither so attached to mathematics that he became sterile in creating physics, as happened to Poincaré, nor so attached to experimental work that he developed it into operationalism like Bridgman. For Einstein the great problem had arisen quite early in his research: 'How to understand the "quantum"?' And he knew perfectly that neither the most elegant mathematics nor the most brilliant experiments could solve the riddle; but one had to sit down and force one's greatest fantasy to obtain the desired solution. In his article 'Physics and Reality,' he summarized: 'Physics constitutes a logical system of thought which is in a state of evolution, whose basis cannot be distilled, as it were, from experience by an inductive method, but can only be arrived at by free intuition. The justification (truth content) of the system rests in the verification of the derived propositions by sense experiences, whereby the relations of the latter to the former can only be comprehended intuitively. Evolution is proceeding in the direction of increasing simplicity of the logical basis. In order further to approach this goal, we must resign ourselves to the fact that the logical basis departs more and more from the facts of experience, and that the path of our thought from the fundamental basis of those derived propositions, which correlate with sense experiences, becomes continually harder and harder.'[335]

7.7. What Is Reality?

In his article 'Maxwell's Influence on the Evolution of the Ideal of Physical Reality,' Einstein wrote: 'The belief in an external world independent of the perceiving subject is the basis of all natural science. Since, however, sense perception only gives information of this external world or "physical reality" indirectly, we can only grasp the latter by speculative means. It follows from this that our notions of physical reality can never be final.'[336] On the other hand, Heisenberg remarked: 'In fact, our ordinary description of nature, and the idea of exact laws, rests on the assumption that it is possible to observe the phenomena without appreciably influencing them. To coordinate a definite cause to a definite effect has sense only when both can be observed without introducing a foreign element disturbing their interrelation. The law of causality, because of its very nature, can only be defined for isolated systems, and in atomic physics even approximately isolated systems cannot be observed. This might have been foreseen, for in atomic physics we are dealing with entities that are (so far as we know) ultimate and indivisible. There exist no infinitesimals by the aid of which an obervation might be made without appreciable perturbation.'[337]

Einstein had a different tendency in viewing reality. It has often been stated that his view was connected with his preference for continuity and causality. For instance, Henry Margenau noted: '[Einstein's] preference is here clearly stated. Reality is to be regarded as a continuous manifold. This view has inspired Einstein's recent researches, his quest for a unified field theory on the model of general relativity, which would include the laws of the electromagnetic as well as those of the gravitational fields.'[338]

Let us examine the source from where Einstein's concept of causality and hence reality developed. He had read and admired Hume, as he had done Mach, in his early student days and he never ceased to estimate him very highly. In fact, Einstein had read Hume thoroughly, and the choice of his example to demonstrate the incompleteness of the description by quantum mechanics shows this explicitly. Namely, this example goes back to his study of Hume. In his *Treatise of Human Nature,* Hume said: 'The idea, then, of causation must be derived from some *relation* among objects; and that relation we must now endeavor to discover. I find in the first place, that whatever objects are considered as causes or effects, are *contiguous;* and that nothing can operate in a time or place which is ever so little removed from those of its existence. Though distant objects may sometimes seem productive of each other, they are commonly found upon examination to be linked by a chain of causes, which are contiguous by themselves, and to the distant objects; and when in any particular instance we cannot discover this connection, we still presume it to exist. We may therefore consider the relation of *contiguity* as essential to that causation; at least we may suppose it as such, according to the general opinion, till we can find a more proper occasion to clear up this matter, by examining what objects are or are not susceptible of juxtaposition and conjunction.

'The second relation I shall observe as essential to causes and effects, is not so universally acknowledged, but is liable to some controversy. It is that of *priority* of time in the cause before effect. ... It is an established maxim both of natural and moral philosophy, that an object which exists for any time in its full perfection without producing another, is not its sole cause, but is assisted by some other principle, which pushes it from its state of inactivity, and makes it exert that energy, of which it was secretly possessed. Now if any cause may be perfectly contemporary with its effect, it is certain, according to this maxim, that they must all of them be so.

... The consequence of this would be no less than the destruction of that succession of causes, which we observe in the world; and indeed, the utter annihilation of time. For if one cause were co-temporary with its effect, and this effect with *its* effect, and so on, it is plain, there would be no such thing as succession, and all objects must be co-existent.'[339]

In physical language, this passage means: 'When we observe that a stone A strikes a stone B and sets it in motion, we usually express this occurrence as follows: stone A has caused stone B to move. By experience we can only confirm the fact that whenever A strikes B, B is set in motion. Before Hume it was usually said that this connection is a necessary one. In physics, however, the word "necessary" can have no meaning other than "regularly connected."'[340]

Quantum theory has caused a serious revision of these considerations which are so natural to the human mind because they are directly related to the daily experience in a world of human measure. In a world of microparticles the situation is different. Here observation gives rise to a considerable effect because the testing probes are of the same size as the observed object and disturb it. The objects cannot be described anymore with the concepts to which Hume appealed.

7.8. Description and Reality

In his *Critique of Pure Reason,* Kant wrote: 'There can be no doubt that all our knowledge begins with experience ... . But though all our knowledge begins with experience, it does not follow that it all arises out of experience. ... We are in possession of certain modes of *a priori* knowledge, and even the common understanding is not without them.'[341] Kant, in the first edition of *Critique of Pure Reason* (1781), had developed a system which was to put the sciences and the reasoning on a transcendental basis. Whereas empiricists assume that all knowledge starts and ends with experienced facts, here the emphasis is laid on the ordering of experiences, which rests on the existence of ordering principles and functions which do not stem from experience. In fact, Ernst Cassirer, the most scientific Kantian scholar of the 20th century, called his most important book *Substance and Function.*[342] In it he stated: 'This reproduction of the manifold and ceaselessly changing material of perception to ultimate *constant relations* must be granted without limitations by even the most radical "empiricism." For the assumption of this fundamental relation is all that remains for empricism of the concept of the "object" and thus the concept of nature.'[343] And, a little later, he remarked: 'To describe a group of phenomena, then, means not merely to record receptively the sensory impressions received from them, but to transform them intellectually. From among the theoretically known and developed forms of mathematical connection (for instance, from among the forms of pure geometry), a selection and combination must be made such that the elements given here and now appear as constructively deduced elements in the system which arises. The logical moment given here cannot be denied even in the theories of empiricism, or under whatever names it may be concealed. "The adjustment of ideas to reality" presupposes the very concept of this reality, and thus a system of intellectual postulates.'[344]

However, one has to emphasize an important fact about the critical philosophy of Kant and his successors, which Alfred C. Elsbach pointed out in *Kant und Einstein*: 'Here, in the critical philosophy one assumes the validity of science and deduces from it the critical theory of knowledge.'[345] Elsbach examined the results of the critical philosophy and came to the conclusion that the contradiction between experience and certain statements of the Kantian scheme is only an apparent one. He referred, of course, to Kant's *a priori* concepts of space and time, as expressed in his *Critique of Pure Reason*: 'Space is a necessary *a priori* representation, which underlies all outer intuitions.'[346] In his thesis, Kant had already stated: 'The concept of space is not abstracted from outer sensations.'[347] And 'Time is the formal *a priori* condition of all appearances whatsoever. (The idea of time does not originate in the senses, but is presupposed by them.)'[348,349]

In the theory of relativity, these statements which come from Newton's theory do not hold, because both the absolute space and the absolute time are concepts which lose their validity for motions with velocities close to that of light. Thus Einstein believed that Kant's scheme was too narrow to account for the new development. But Elsbach remarked after a careful study of Kant's critical philosophy: 'We can exclude the situation that physics is at variance with Kant's philosophy because the coincidence of both is a necessary consequence of the structure of the critical philosophy.'[350] Einstein summarized his position in his lecture on 'Geometry and Experience' at the Prussian Academy of Sciences on 27 January 1921: 'Geometry thus completed is evidently a natural science; we may in fact regard it as the most ancient branch of physics. Its affirmation rests essentially on induction from experience. ... I attach special importance to the view of geometry which I have just set forth, because without it I should be unable to formulate the theory of relativity.' And, a little later, he said: 'For if contradictions between theory and experience manifest themselves, we should rather decide to change the physical laws than to change axiomatic Euclidean geometry. If we reject the relation between the practical-rigid body and geometry, we shall indeed not free ourselves from the convention that Euclidean geometry is to be retained as the simplest.'[351]

Thus there is no contradiction between the Einstein of relativity theory and Kant's scheme, even expressed in the statements made by Kant in his *Metaphysical Foundations of Science*: 'Rational science deserves, therefore, the name science only then, if the natural laws on which it is founded, are recognized *a priori* and are not mere laws of empirics.'[352]

7.9. Science and Hypothesis

In his book *Science and Hypothesis,* Henri Poincaré wrote: 'For a superficial observer, scientific truth is beyond the possibility of doubt; the logic of science is infallible, and if scientists are sometimes mistaken, this is only from their mistaking their rules. ... On a little more reflection it was perceived how great a place hypothesis occupies; that the mathematician cannot do without it, still less than the experimenter. And then it was doubted if all these constructions were really solid, and believed that a breath would overthrow them. To be skeptical in this fashion is still to be superficial. To doubt everything and to believe everything are two equally convenient solutions; each saves us from thinking.'[353]

In Poincaré's popular and 'philosophical' book *Science and Hypothesis,* which Einstein had carefully studied in his Bern days before 1905, the author had carefully examined the role of hypothesis in mathematics and in physics. He started by considering numbers, their operations and relation to experience, and the notion of the continuum. In Part Two, he examined geometry, including the non-Euclidean geometries. In Chapter III, a very interesting discussion was mentioned that might have influenced Einstein in his search for relativity. Poincaré referred to the theorem of Sophus Lie, which stated that in an n-dimensional space in which the motion of a *rigid figure* is possible, and in which p conditions are required to determine the position of the figure in space, the number of geometries compatible with these premises will be limited. Now Riemann admitted in general an infinity of possible geometries, which depend on how the length of the curve is defined. 'That is perfectly true,' Poincaré said, 'but most of these definitions are incompatible with the motion of a rigid figure, which in Lie's theorem is supposed to be possible. These geometries of Riemann, in many ways so interesting, could never therefore be other than purely analytic and would not lend themselves to demonstrations analogous to those of

Euclid.' And he added: 'In other words, *the axioms of geometry are merely disguised definitions.* Then what are we to think of the question: Is the Euclidean geometry true? It has no meaning. ... One geometry cannot be more true than another; it can only be *more convenient.* Now, Euclidean geometry is, and will always remain, the most convenient.' Since solid bodies allow us to determine a position, 'if there were no solid bodies in nature, there would be no geometry.'[354] 'We see that experience plays an indispensable role in the genesis of geometry; but it would be an error thence to conclude that geometry is, even in part, an experimental science. If it were experimental, it would be only approximative and provisional. And what rough approximation! Geometry would be only the study of the movements of solids; but in reality it is not occupied with a natural solid, it has for its object ideal solids, absolutely rigid, which are only a simplified and very remote image of natural solids. The notion of these ideal solids is drawn from all parts of our mind, and experience is only an occasion which induces us to bring it forth from them.'[354]

In Part Three, Poincaré turned to the concept of forces. The principles of mechanics take an ambiguous role between definitions and statements which cannot be disproved (hypothesis). For example, the principle of the conservation of energy can always be satisfied by adding new forms of energy. 'The principles of mechanics, then, present themselves to us under two different aspects. On the one hand, they are truths founded on experiment and approximately verified so far as concerns almost isolated systems. On the other hand, they are postulates applicable to the totality of the universe and regarded as rigorously true.

'If these postulates possess a generality and a certainty which are lacking to the experimental verities whence they are drawn, this is because they reduce in the last analysis to the mere convention which we have the right to make, because we are certain beforehand that no experiment can ever contradict it.

'This convention, however, is not absolutely arbitrary; it does not spring from our caprice; we adopt it because certain experiments have shown us that it would be convenient.'[355]

In Part Four, Poincaré dealt with the modern theories. He remarked: 'A collection of facts is no more a science than a heap of stones is a house.'[356] The real role in science is played by hypothesis. 'All generalization is hypothesis.'[357] There are three kinds of hypothesis: the first ones are perfectly natural, the second ones he called neutral (they might serve for computational help, etc.); the third kind finally can be confirmed or condemned. 'They will be always fruitful,' said Poincaré.[358]

Concerning the new theories, Poincaré discussed the theory of Hendrik Lorentz, which he called 'the most satisfactory theory we have.'[359] But he criticized Lorentz's concept of aether, and more so that of Joseph Larmor: 'With Lorentz, we do not know what the motions of the aether are; thanks to this ignorance, we may suppose them such that, composing those of matter, they establish the equality of action and reaction. With Larmor, we know the motions of the aether [it has a velocity in the direction of the magnetic force], and we can ascertain that the compensation does not take place.'[360]

Great problems remained in other fields. 'This conception, which attaches itself to the kinetic theory of gases, has cost great efforts and has not, on the whole, been fruitful; but it may become so. This is not that the new radiations seem connected with the phenomena of luminescence; not only do they excite fluorescence, but they sometimes take birth in the same conditions as it [does].'[361]

Now, as an interesting intermission, Poincaré added a chapter on probability and stated his results; 'To undertake any calculation of probability, and even for that calculation to have any meaning, it is necessary to admit, as point of departure, a hypothesis or convention which has always something arbitrary about it. In the choice of this convention, we can be guided only by the principle of sufficient reason. Unfortunately this principle is very vague and very elastic, and in the cursory examination we have just made, we have seen it take many different forms. The form under which we have met it most often is the belief in continuity, a belief which it would be difficult to justify by apodictic place to examine whether it does not lead to contradictions and whether it is in continuity with the true nature of things.

'We signalize, however, M. Gouy's original ideas on the Brownian movement. According to this scientist, this singular motion should escape Carnot's principle. The particles which it puts in swing would be smaller than the links of that so compacted skein; they would therefore be fitted to disentangle them and hence to make the world go backward. We should almost see Maxwell's demon at work.'[362]

New phenomena occurred, like cathode rays, X-rays, etc. 'Herein is a whole world which no one had suspected. How many unexpected guests must be stowed away! No one can foresee the place they will occupy. But I do not believe they will destroy the general unity; I think they will complete it. On the one hand, in fact, reasoning, but without which science would be impossible.'[363]

Poincaré's *Science and Hypothesis*, which Albert Einstein studied as a young man, left an indelible impression on his mind about the work he should do in theoretical physics as well as helping him to formulate his own views about the nature of physical reality. We have pursued only certain pertinent threads in the fabric of Einstein's conception of physics and reality in this treatise.

Notes and References

1. From 'An Interview with Einstein,' by I. Bernard Cohen, made two weeks before Einstein died on 18 April 1955; in *Scientific American* **193**, 69–73 (1955), in particular p. 73.
2. A. Einstein, *Quantenmechanik und Wirklichkeit, Dielectica* **2**, 320–324, see p. 321.
3. A. Einstein, *Folgerungen aus den Capillaritätserscheinungen, Annalen der Physik* **4**, ser. 4, 513–523 (1901).
4. N. Bohr, manuscript 'On the Constitution of Atoms and Molecules' (June–July 1912, edited by L. Rosenfeld, Copenhagen and New York, 1963), p. 28.
5. For an account of Bohr's early work on the theory of atomic spectra, see Jagdish Mehra and Helmut Rechenberg, *The Historical Development of Quantum Theory* (Springer-Verlag New York), Volume 1, Part 1 (1982).
6. N. Bohr, On the Constitution of Molecules, *Phil. Mag.* **26**, 1–25, 476–502, 857–873 (1913).
7. A. Sommerfeld, *Annalen der Physik (Leipzig)* **51**, 1 (1916).
8. J. Stark, *Annalen der Physik (Leipzig)* **43**, 965 (1914).
9. P. S. Epstein, *Annalen der Physik (Leipzig)* **50**, 489 (1916).
10. The first selection rule was obtained by A. Rubinowicz, *Physikalische Zeitschrift* **19**, 441, 465 (1918).

11. The normal Zeeman effect could be explained with the help of the classical considerations of H. A. Lorentz, P. Debye, *Göttinger Nachrichten* (1916), and A. Sommerfeld, *Physikalische Zeitschrift* **17**, 491(1961), reported the calculation within the framework of the Bohr–Sommerfeld atomic theory. F. Paschen and E. Back, *Annalen der Physik (Leipzig)* **39**, 897 (1912), **40**, 960 (1913), discovered the fact that for lines even with a complicated fine structure in strong fields, the normal splitting into three lines is obtained.
12. See, e.g., the specific heat at low temperatures measured by A. Eucken, *Verh. Deutsch. Phys. Ges.* **15**, 57 (1913), and the attempts of A. Einstein and O. Stern and others.
13. P. S. Epstein, *Annalen der Physik (Leipzig)* **50**, 489 (1916); **51**, 168 (1916).
14. K. Schwarzschild, *Sitz. Ber. Preuss. Akad. Wiss. (Berlin)*, 1916.
15. A. Einstein, *Verh. Deutsch. Phys. Ges.* **19**, 82 (1917).
16. P. Ehrenfest, 'Remark Concerning the Specific Heat of Diatomic Gases,' *Verh. Deutsch. Phys. Ges.* **15**, 45 (1913); 'A Theorem of Boltzmann and Its Connection with the Theory of Quanta,' *Proc. Kon. Akad. Wet. Amsterdam* **16**, 591 (1913).
17. P. Ehrenfest, *loc. cit.*, see Van der Waerden, *Sources of Quantum Mechanics* (Amsterdam/New York, 1967), pp. 80–83.
18. In one case the adiabatic invariant $2T/v$ is identical with Sommerfeld's phase integral (1).
19. See Van der Waerden, *loc. cit*, pp. 92–93.
20. A. Einstein, *Verh. Deutsch. Phys. Ges.* **18**, 318 (1916).
21. P. Ehrenfest, *Proc. Amst. Acad.* **16**, 59 (1914); *Phys. Zeit.* **15**, 657 (1914); *Annalen der Physik (Leipzig)* **51**, 327 (1916); *Phil. Mag.* **33**, 500 (1917).
22. N. Bohr, 'On the Quantum Theory of Line Spectra,' *Kgl. Dansk. Vid. Selsk. Skr. Heft* **8** (1918); quoted after Van der Waerden, *loc. cit.*, pp. 96–97.
23. The third part was not published until 1922.

24. Van der Waerden, *loc. cit.*, p. 101.
25. N. Bohr, in Van der Waerden, pp. 110–111, and in Mehra and Rechenberg, *loc. cit.*
26. R. Ladenburg, *Z. Phys.* **4**, 451–458 (1921).
27. H. A. Kramers, *Nature* **113**, 673–676 (1924); **114**, 310–311 (1924).
28. J. H. Van Vleck, *Phys. Rev.* **24**, 330–365 (1924).
29. H. A. Kramers and W. Heisenberg, *Z. Phys.* **31**, 681–708 (1925).
30. W. Kuhn, *Z. Phys.* **33**, 408–412 (1925).
31. W. Thomas, *Naturwissenschaften* **13**, 627 (1925).
32. W. Heisenberg, *Z. Phys.* **33**, 879–893 (1925).
33. N. Bohr, *loc. cit.*, in Van der Waerden, pp. 100–101; also Mehra and Rechenberg, Vol. 1, Part 1 (1982).
34. W. Pauli, *Handbuch der Physik*, Vol. 23 (Eds. H. Geiger and K. Scheel, Berlin, 1926); Pauli's review article on the modern quantum mechanics in *Handbuch der Physik* (1933).
35. Heisenberg had mentioned his procedure first in a letter to R. Kronig, dated 5 June 1925. See Van der Waerdern, p. 24, and Mehra and Rechenberg, Vol. 2 (1982).
36. M. Born, in Van der Waerden, p. 37; Mehra and Rechenberg, Vol. 3 (1982).
37. M. Born and P. Jordan, *Z. Phys.* **34**, 858–888 (1925).
38. M. Born, W. Heisenberg and P. Jordan, *Z. Phys.* **35**, 557–615 (1926).
39. P. A. M. Dirac, *Proc. R. Soc. London* **A109**, 642–653 (1926).
40. P. A. M. Dirac, *Proc. R. Soc. London* **A110**, 561–569 (1926).
41. On 19 July 1925, at the railway station in Hanover, Pauli remarked to Born, 'You are only going to spoil Heisenberg's physical ideas by your futile mathematics!' when Born invited Pauli to collaborate with him in formulating matrix mechanics. The occasion was a meeting of the German Physical Society held in Hanover, where Born had gone from Göttingen and Pauli had come from Hamburg. See Van der Waerden, p. 37, and Mehra and Rechenberg, Vol. 3 (1982).

42. W. Pauli, *Z. Phys.* **36**, 336–363 (1926).
43. See Van der Waerden, p. 58, and Mehra and Rechenberg, Vol. 3 (1982).
44. See Van der Waerden, 1967, p. 59, and Mehra and Rechenberg, Vol. 3 (1982).
45. P. A. M. Dirac, *Proc. R. Soc. London* **A109**, 642 (1926).
46. L. de Broglie, *Thèses* (Paris, 1924); *Ann. Phys. (Paris)* **3**(10), 22 (1925).
47. The theory of the ideal Bose–Einstein gas was developed by A. Einstein in three communications to the Prussian Academy (Berlin) in 1924 and 1925.
48. Our quotations are from E. Schrödinger's *Collected Papers on Wave Mechanics* (London, 1928), p. 9. Schrödinger's paper on gas theory appeared in *Phys. Zeit.* **27**, 95 (1926).
49. In essence the square of its integral over space has to exist.
50. Of course, in the Hamilton or Schrödinger operator H, there entered the electric potential e^2/r.
51. Schrödinger's first communication appeared in *Annalen der Physik (Leipzig)* **79**, 361 (1926); the second part was contained in the same volume, and was received on 23 February 1926.
52. E. Schrödinger, *Annalen der Physik (Leipzig)* **80** (1926); **81** (1926).
53. E. Schrödinger's fourth communication was published in the next volume.
54. There exists a small parameter λ, according to which one can expand and solve the more difficult problem.
55. This paper was received by the *Annalen der Physik* on 23 June 1926, while the preceding one was received on 10 May 1926.
56. E. Schrödinger, *Annalen der Physik (Leipzig)* **79** (1926).
57. E. Schrödinger, *Annalen der Physik* **79** (1926).
58. E. Schrödinger, *loc. cit.*
59. E. Schrödinger, *loc. cit.*
60. M. Born, *Z. Phys.* **38**, 803–804 (1926).

61. M. Born and N. Wiener, *Z. Phys.* **36**, 74–87 (1926).
62. M. Born, *Z. Phys.* **37**, 863–867 (1926).
63. M. Born, *loc. cit.*, p. 863.
64. M. Born, *loc. cit.*, p. 863.
65. M. Born, *loc. cit.*, p. 865–866.
66. The scattering processes seemed to Born to be one of the experimentally most convincing evidences of the quantum effects. After all, in Göttingen, he had been collaborating with James Franck.
67. M. Born, *Z. Phys.*, **38**, 803–827 (1926).
68. M. Born, *loc. cit.*, p. 808.
69. M. Born, *loc. cit.*, pp. 826–827.
70. M. Born, *Z. Phys.*, **40**, 167–192 (1926).
71. W. Heisenberg, *Z. Phys.* **43**, 172 (1927).
72. Pauli's remark was in regard to Heisenberg's paper on the uncertainty principle, Ref. 71, which was received by the journal on 23 March 1927.
73. *Theoretical Physics in the Twentieth Century*, Eds. M. Fierz and V. F. Weisskopf (London/New York, 1960).
74. Heisenberg in Ref. 73, pp. 44–45.
75. W. Heisenberg, *loc. cit.*, p. 45.
76. W. Heisenberg, *loc. cit.*, p. 46.
77. N. Bohr, reprinted in *Atomic Theory and the Description of Nature*, originally published in *Nature* (1927).
78. N. Bohr, *loc. cit.*, p. 63.
79. N. Bohr, *loc. cit.*, p. 66.
80. N. Bohr, *loc. cit.*, p. 67–68.
81. N. Bohr, *loc. cit.*, p. 75.
82. A. Einstein, lecture delivered in Japan in 1922 (Kaizo, 1923).
83. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1923), p. 359.
84. A. Einstein, *Autobiographical Notes*, in P. A. Schilpp: *Albert Einstein: Philosopher–Scientist*, p. 15.
85. C. Seelig, *A. Einstein und die Schweiz*, p. 35.

86. E. Mach, *Die Mechanik in ihrer Entwicklung historischkritisch dargesetellt* (Leipzig, 1883), and numerous later editions; English translation by T. J. McCormack (The Open Court, LaSalle, Illinois, 1893, 9th edition, 1960).
87. A. Einstein, *Autobiographical Notes, loc. cit.*, p. 21.
88. P. W. Bridgman, in his *Operationalism: The Nature of Physical Theories* (Princeton, 1936).
89. E. Mach, *loc. cit.*, p. 586.
90. This idea is similar to the one expressed by R. Avenarius in his book *Philosophie als Denken der Welt gemäss dem Prinzip des kleinsten Kraftmasses* (Leipzig, 1886).
91. E. Mach, in a letter; quoted following the preface of K. Menger to *Science of Mechanics*, p. xiv.
92. E. Mach, *loc. cit.*, p. 598.
93. A. Einstein, *Autobiographical Notes, loc. cit.*, p. 21.
94. A. Einstein, *Autobiographical Notes, loc. cit.*, pp. 35, 37.
95. A. Einstein, *Phys. Zeit.* **10**, p. 820.
96. A. Einstein, *loc. cit.*, p. 821.
97. A. Einstein, *loc. cit.*, pp. 824–825.
98. J. C. Slater, *Nature* **47**, 307 (1924).
99. N. Bohr, *Z. Phys.* **13**, 117 (1923).
100. N. Bohr, H. A. Kramers and J. C. Slater, *Phil. Mag.* **47**, 785–802 (1924); *Z. Phys.* **24**, 69–87 (1924).
101. N. Bohr, H. A. Kramers and J. C. Slater, *loc. cit.*, p. 791.
102. A. H. Compton, *Phys. Rev.* **21**, 483 (1923); P. Debye, *Phys. Zeit.* **24**, 161 (1923).
103. W. Pauli, *Z. Phys.* **18**, 272 (1923); A. Einstein and P. Ehrenfest, *Z. Phys.* **14**, 301 (1923).
104. 'On an Experiment Concerning the Elementary Process of Light-Quantum,' *Sitz. Ber. Preuss. Akad. Wiss. (Berlin)* (1921), p. 882.
105. Letter from A. Einstein to P. Ehrenfest, mentioned in M. J. Klein, 'The First Phase of the Bohr–Einstein Dialogue,' *Hist. Stud. Phys. Sci.*, Vol. II, p. 10.

106. M. J. Klein, *loc. cit.*, p. 12.
107. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1922), p. 18; Einstein himself corrected the error.
108. See the citation of a letter from A. Sommerfeld to A. H. Compton, in *J. Franklin Institute* **198**, 70 (1924).
109. N. Bohr's 1921 Solvay lecture was presented by P. Ehrenfest under the title *'L'application de la théorie des quanta aux problèmes atomiques'* (Paris, 1923).
110. For further remarks, see M. J. Klein, *loc. cit.*, pp. 19–22.
111. N. Bohr, *Z. Phys.* **13**, 117 (1923).
112. N. Bohr, Ref. 111, translated in *Suppl. Proc. Camb. Phil. Soc.* (1924), in particular p. 40.
113. See B. L. Van der Waerden, *Sources of Quantum Mechanics* (1967), p. 13.
114. E. Schrödinger, for instance, considered the paper a return to the continuum theory of radiation, in his paper *'Bohr's neue Strahlungshypothese und der Energiesatz,' Naturwissenschaften* **12**, 720 (1924). In a similar manner, R. Becker reacted in his paper *'Ueber Absorption und Dispersion in Bohr's Quantentheorie,' Z. Phys.* **27**, 173 (1924).
115. P. Jordan, *'Zur Theorie der Quantenstrahlung,' Z. Phys.* **30**, 297–319 (1924).
116. W. Bothe and H. Geiger, *Z. Phys.* **26**, 44 (1924) contained the proposal for the experiment, and in *Z. Phys.* **26**, 639 (1925) the results were confirmed.
117. A. H. Compton and A. W. Simon, *Phys. Rev.* **26**, 289 (1925).
118. M. J. Klein quoted the letter, *loc. cit.*, pp. 32–33.
119. A. H. Compton and A. W. Simon, *Phys. Rev.* **26**, 299 (1925).
120. Quoted in M. J. Klein, *loc. cit.*, p. 35.
121. Postscript from July 1925 to his paper *'Ueber die Wirkung von Atomen bei Stössen,' Z. Phys.* **34**, 142 (1925).
122. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss. (Berlin)* (1923), pp. 359–364.

123. A. Hermann (Ed.), *Einstein–Sommerfeld Briefwechsel* (Schwabe & Co., Basel/Stuttgart, 1968), p. 81.
124. 'Maxwell's Influence on the Development of the Conception of Physical Reality,' in J. C. Maxwell, *A Commemoration Volume* (Cambridge, 1931), p. 72.
125. A. Einstein, *Annalen der Physik (Leipzig)* **17**, 132–148 (1905).
126. A. Einstein, *Annalen der Physik (Leipzig)* **17**, 549–560 (1905).
127. A. Einstein, *Annalen der Physik (Leipzig)* **17**, 891–921 (1905).
128. A. Einstein, in the paper entitled (in English) 'Does the Inertia of a Body Depend on Its Energy Content?', *Annalen der Physik (Leipzig)* **18**, 639–641 (1905).
129. A. Einstein, *Annalen der Physik (Leipzig)* **20**, 627 (1906).
130. W. Kaufmann, *Annalen der Physik (Leipzig)* **19**, 87 (1906), whose experiments seemed to confirm another law; but the more precise experiments of A. Bucherer, *Annalen der Physik (Leipzig)* **28**, 513 (1908); **30**, 974 (1909), favored the Lorentz–Einstein solution.
131. A. Einstein, 'Remarks on a Note of P. Ehrenfest,' *Annalen der Physik (Leipzig)* **23**, 206–208 (1907).
132. A. Einstein, *loc. cit.*, p. 207.
133. A. Einstein and J. Laub, *Annalen der Physik (Leipzig)* **23**, 371–384 (1907); **26**, 532–540, 541–550 (1908); **28**, 445–447 (1909).
134. A. Einstein, in J. Stark (Ed.), *Jahrbuch der Radioaktivität und der Elektronik* **4**, 411–462 (1907); errata, **5**, 98–99 (1907).
135. A. Einstein, *loc. cit.*, p. 459.
136. A. Einstein, *loc. cit.*, p. 462.
137. Ref. 123.
138. A. Einstein, *Annalen der Physik (Leipzig)* **35**, 898–908 (1911).
139. A. Einstein, *Annalen der Physik (Leipzig)* **38**, 443–458 (1912).
140. A. Einstein and M. Grossmann, *Z. Math. Phys.* **62**, 225–261 (1913), in particular p. 225; *Z. Math. Phys.* **63**, 215–225 (1914).

141. A. Einstein and A. D. Fokker, *Annalen der Physik (Leipzig)* **44**, 325–328 (1914); *Scientia (Bologna)* **15**, 337–348 (1914); *Phys. Zeit.* **15**, 176–180 (1914).
142. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1914), pp. 1030–1085.
143. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss. (Berlin)* (1915), pp. 778–789.
144. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss. (Berlin)* (1915), pp. 831–839.
145. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss. (Berlin)* (1915), pp. 844–847, esp. p. 847.
146. Jagdish Mehra, *Einstein, Hilbert, and the Theory of Gravitation* (D. Reidel, Boston/Dordrecht, 1974); 'One Month in the History of the Discovery of General Relativity Theory,' *Foundations of Physics Letters* **11**(1), 41–60 (1998).
147. A. Einstein, Ref. 143, p. 779.
148. A. Einstein, Ref. 145, p. 847; *Sitz. Ber. Preuss. Akad. Wiss.* (1916), pp. 184–187.
149. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (22 June 1916), p. 696.
150. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1917), pp. 142–152.
151. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1916), pp. 1111–1116.
152. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1918), pp. 448–459.
153. A. Einstein, *Naturwissenschaften* **7**, 776 (1919).
154. 'Do Gravitational Fields Play an Essential Role in the Construction of Material Elementary Particles?', *Sitz. Ber. Preuss. Akad. Wiss.* (1919), pp. 349–356, esp. p. 349.
155. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1921), pp. 261–264.
156. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1923), pp. 32–38.
157. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1923); *Nature* **112**, 448–449 (1922).
158. A. Einstein, Ref. 157, esp. p. 140.

159. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1925), pp. 414–419.
160. A. Einstein, *Physica* **5**, 330–334 (1925); esp. p. 334.
161. A. Einstein, 'On the Formal Relations of Riemannian Curvature Tensor to the Field Equations of Gravitation,' *Ann. Phys. Math.* **97**, 99–103 (1927); 'On Kaluza's Theory of the Relation Between Gravity and Electricity,' *Sitz. Ber. Preuss. Akad. Wiss.* (1927), pp. 23–30.
162. A. Einstein and J. Grommer, *Sitz. Ber. Preuss. Akad. Wiss.* (1927), pp. 2–13.
163. A. Einstein and J. Grommer, Ref. 162, p. 4.
164. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1927), pp. 235–245.
165. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1928), pp. 224–227.
166. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1929), pp. 2–7; 156–159; (With Th. de Donder) *Revue Générale de l'Eléctricité; Ann. Inst. H. Poincaré* **1**, 1–24 (1929); *Math. Annalen* **102**, 685–697 (1930).
167. Ref. 166, *Math. Annalen* **102**, p. 697.
168. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1930), pp. 110–120.
169. A. Einstein and W. Mayer, *Sitz. Ber. Preuss. Akad. Wiss.* (1930), pp. 110–120.
170. A. Einstein, *Sitz. Ber. Preuss. Akad. Wiss.* (1931), pp. 257–265 (with W. Mayer).
171. A. Einstein and W. Mayer, *Sitz. Ber. Preuss. Akad. Wiss.* (1931), pp. 541–557; (1932), pp. 130–137.
172. A. Einstein and W. Mayer, Ref. 171, p. 557.
173. A. Einstein and W. Mayer, *Sitz. Ber. Preuss. Akad. Wiss.* (1932), pp. 522–550; *Proc. Akad. Wet.* **36**, 615-619 (1934); *Ann. Math.* **35**, 104–110 (1934).
174. A. Einstein and W. Mayer, Ref. 173, *Proc. Akad. Wet.* (1934), p. 619.
175. A. Einstein and N. Rosen, *Phys. Rev.* **48**, 73–77 (1935); (With L. Infeld and B. Hoffmann, *Ann. Math.* **39**, 65–100 (1938); (With L. Infeld) **41**, 455–464 (1939).
176. A. Einstein and P. Bergmann, *Ann. Math.* **39**, 683–670 (1938).

177. A. Einstein and V. Bargmann, *Ann. Math.* **45**, 1–14, 15–23 (1944).
178. A. Einstein and W. Pauli, *Ann. Math.* **44**, 131–137 (1943).
179. A. Einstein, *Ann. Math.* **46**, 578–584 (1945); (With E. G. Strauss) *Rev. Mod. Phys.* **20**, 35–39 (1948); *Can. J. Math.* **3**, 209–241 (1949).
180. A. Einstein, *The Meaning of Relativity*, fifth edition (Princeton University Press, Princeton, New Jersey, 1955).
181. A. Einstein, Ref. 180, p. 164.
182. A. Einstein, Ref. 180, p. 165.
182a. A. Einstein, Ref. 180, p. 166.
182b. A. Einstein, Ref. 180, p. 182.
183. K. Przibram (translated by M. J. Klein), *Letters on Wave Mechanics* (Philosophical Library, New York), pp. 23, 26.
184. Ref. 183, p. 3.
185. Ref. 183, p. 43.
186. A. Einstein, first letter to Schrödinger in Ref. 183, 16 April 1926, Ref. 183, p. 23.
186. A. Einstein, Ref. 183, p. 25.
187. A. Einstein, Ref. 183, p. 25.
188. E. Schrödinger to A. Einstein, 23 April 1926, Ref. 183, p. 26.
189. E. Schrödinger to A. Einstein, Ref. 183, pp. 26, 27.
190. A. Einstein to E. Schrödinger, 26 April 1926, Ref. 183, p. 28; the entire exchange of letters concerning Schrödinger's creation of wave mechanics is discussed in Mehra and Rechenberg, *The Historical Development of Quantum Theory* (1987), Vol. 5, Part 2.
191. A. Einstein to E. Schrödinger, 31 May 1928, Ref. 183.
192. For a full account of the fifth and sixth Solvay Conferences on Physics, see Jagdish Mehra, *The Solvay Conferences on Physics* (D. Reidel, Boston/Dordrecht, 1975), and Mehra and Rechenberg, *The Historical Development of Quantum Theory*, Vol. 6, Part 1, Chap. II (Springer-Verlag, New York, 2000).

193. E. Schrödinger to A. Einstein, Ref. 183, pp. 30–31.
194. E. Schrödinger, 'Are There Quantum Jumps?', *British Journal for the Philosophy of Science* **3**, 113 (1952).
195. E. Schrödinger, Ref. 194, p. 123.
196. L. de Broglie, 'Will Quantum Physics Remain Indeterminist?', in *New Perspectives in Physics*, second printing (New York, 1962).
197. L. de Broglie, Ref. 196, p. 94.
198. L. de Broglie, Ref. 196, p. 96.
199. L. de Broglie, Ref. 196, p. 107.
200. A. Einstein to E. Schrödinger, *Letters on Wave Mechanics*, Ref. 183, p. 39.
201. M. Planck, *Annalen der Physik (Leipzig)* **37**, 261–277 (1940); **38**, 272–273 (1940); **40**, 481–492 (1941).
202. N. Bohr, 'The Solvay Meetings and the Development of Physics,' in *'La Théorie Quantique des Champs'* (Proceedings of the 12^{ième} *Conseil de Physique*, October 1961), pp. 27–28; see also Mehra and Rechenberg, Vol. 6, Part 1, Ref. 192.
203. N. Bohr, in P. A. Schilpp, *loc. cit.*, see also Mehra, *The Solvay Conferences on Physics*, and Mehra and Rechenberg, Ref. 192, Part 1, Chap. II.
204. H. A. Lorentz, in his introduction to the general discussion of the fifth Solvay Conference, *'Éléctrons et Photons'* (Paris, 1928), p. 248.
205. W. Pauli, *Z. Phys.* **40**, 399 (1926).
206. W. Pauli, at the fifth Solvay Conference, Ref. 204, p. 282.
207. L. de Broglie, Ref. 196, p. 96.
208. M. Born, in Ref. 204, p. 178.
209. N. Bohr, in Ref. 204, p. 215.
210. N. Bohr, Ref. 204, p. 217.
211. N. Bohr, in Ref. 204, p. 222.
212. W. Heisenberg, *Z. Phys.* **43**, 172 (1927).
213. N. Bohr, Ref. 204, p. 235.
214. N. Bohr, Ref. 204, p. 247.

215. A. Einstein, in Ref. 204, pp. 255–256.
216. M. Born, in Ref. 204, p. 289.
217. A copy of the sheet on which this exchange of notes between Ehrenfest and Einstein was made was provided to me by Helen Dukas in 1971; I quoted it in my book *The Solvay Conferences on Physics* (1975), and these remarks are reproduced in Mehra and Rechenberg, Vol. 6, Part 1, Chap. II (2000).
218. N. Bohr, in P. A. Schilpp, *loc. cit.*, pp. 205–206.
219. N. Bohr, Ref. 218, p. 206.
220. N. Bohr, Ref. 218, p. 212.
221. N. Bohr, Ref. 218, p. 213.
222. N. Bohr, Ref. 218, p. 218.
223. N. Bohr, *loc. cit.*, p. 218.
224. N. Bohr, *loc. cit.*, p. 222.
225. N. Bohr, *loc. cit.*, p. 223.
226. N. Bohr, *loc. cit.*, p. 228.
227. A. Einstein, B. Podolsky and N. Rosen, *Phys. Rev.* **47**, 777 (1935).
228. A. Einstein, *Dialectica* **1** (1948).
229. G. Gamow, *Thirty Years That Shook Physics* (Garden City, New York, 1966), p. 51.
230. N. Bohr, *Naturwissenschaften* **16**, 245 (1928).
231. N. Bohr and L. Rosenfeld, *Kgl. Dansk. Vid., Mat. Fys. Medd.* **12**(8) (1933).
232. N. Bohr, *Nature* **143**, 330 (1939).
233. K. Meyer-Abich, *Korrespondenz, Individualität und Komplementarität* (Wiesbaden, 1965), p. 133.
234. N. Bohr, in P. A. Schilpp, *loc. cit.*, p. 223.
235. W. Heisenberg, 'The Development of the Interpretation of Quantum Theory,' in *N. Bohr and the Development of Physics*, Ed. W. Pauli (New York, 1955), pp. 26–27.
236. A. Einstein, introduction and final remark in 'Maxwell's Influence on the Development of the Conception of Physical

Reality,' in *J. C. Maxwell: A Commemoration Volume*, pp. 66, 73.
237. A. Einstein, in P. A. Schilpp, *loc. cit.*, p. 228.
238. A. Einstein, R. C. Tolman and B. Podolsky, *Phys. Rev.* **37**, 780–781 (1931).
239. A. Einstein, R. C. Tolman and B. Podolsky, Ref. 237, p. 781.
240. N. Bohr, in P. A. Schilpp, *loc. cit.*, p. 231.
241. A. Einstein, B. Podolsky and N. Rosen, *Phys. Rev.* **47**, 777–780 (1935).
242. Ref. 241, p. 777.
243. N. Bohr, *Phys. Rev.* **48**, 696–702 (1935).
244. A. Einstein, B. Podolsky and N. Rosen, Ref. 241, p. 777.
245. N. Bohr, Ref. 243, p. 696.
246. A. Einstein *et al.*, Ref. 241, p. 778.
247. N. Bohr, in P. A. Schipp, *loc. cit.*, p. 233.
248. A. Einstein *et al.*, Ref. 244, p. 780.
249. N. Bohr, Ref. 243, p. 702.
250. N. Bohr, in P. A. Schilpp, *loc. cit.*, 235.
251. A. Einstein, *J. Franklin Institute* **221**, 290–323 (1936); English translation: *A. Einstein: The Man and His Theories* (New York, 1962).
252. A. Einstein, Ref. 251, p. 131.
253. A. Einstein, Ref. 251, p. 132.
254. A. Einstein, Ref. 251, pp. 133–134.
255. A. Einstein, Ref. 251, p. 143.
256. A. Einstein, Ref. 251, p. 145.
257. A. Einstein, Ref. 251: There does not exist an English translation of the last two sections in the reference given to the English translation of Einstein's article *'Physik und Realität.'*
258. A. Einstein, Ref. 251, p. 339.
259. A. Einstein, Ref. 251, pp. 340–341.
260. A. Einstein, Ref. 251, p. 340.
261. A. Einstein, Ref. 251, p. 343.

262. A. Einstein, B. Podolsky and N. Rosen, Ref. 241 (1935).
263. N. Bohr, in P. A. Schilpp, *loc. cit.*, p. 235.
264. N. Bohr, *Dialectica* **2**, 312–319 (1948), in particular pp. 316–324.
265. A. Einstein, Ref. 228, p. 320.
266. A. Einstein, Ref. 228, p. 321.
267. W. Heisenberg, *Dialectica* **2**, 331–336 (1948).
268. A. Einstein to Max and Hedwig Born, *The Born–Einstein Letters* (The Macmillan Press, London and Basingdale, 1971), p. 82, letter dated 29 April 1924.
269. M. Born to A. Einstein, 21 October 1921, Ref. 268, p. 57.
270. W. Heisenberg, in the *Bohr Memorial Volume* (Amsterdam, 1967).
271. W. Heisenberg, Ref. 32.
272. M. Born to A. Einstein, 15 July 1925, Ref. 268, p. 84.
273. M. Born to A. Einstein, 15 July 1925, Ref. 268, p. 83.
274. M. Born and P. Jordan, *Z. Phys.* **34**, 858–888 (1925); M. Born, W. Heisenberg and P. Jordan, *Z. Phys.* **35**, 557 (1926).
275. A. Einstein to Hedwig Born, 7 March 1926, Ref. 268, p. 88.
276. A. Einstein to M. Born, 4 December 1926, Ref. 268, p. 91.
277. M. Born, 'Einstein's Statistical Theories,' in P. A. Schilpp, *loc. cit.*, pp. 163–198, esp. pp. 163–164.
278. M. Born, *Natural Philosophy of Cause and Chance* (Oxford, 1949); (Dover, New York, 1964), pp. 63–64.
279. M. Born, in P. A. Schilpp, *loc. cit.*, p. 170.
280. M. Born, in P. A. Schilpp, p. 173.
281. M. Born, in P. A. Schilpp, p. 174.
282. A. Einstein, *Phys. Zeit.* **17**, 101 (1916); English translation in M. Born, in P. A. Schilpp, *loc. cit.*, pp. 175–176.
283. A. Einstein to M. Born, 7 September 1944, Ref. 268, p. 149.
284. A. Einstein to M. Born, 3 March 1947, Ref. 268, pp. 157–158.
285. M. Born to A. Einstein, 9 May 1948, Ref. 268, pp. 173–174.
286. M. Born's commentary on the letter to A. Einstein, Ref. 285, is contained in Ref. 268, p. 176.

287. A. Einstein to M. Born, 15 September 1950, Ref. 268, pp. 187–189.
288. *Scientific Papers Presented to Max Born on His Retirement from the Tait Chair of Natural Philosophy in the University of Edinburgh* (Hafner, New York, 1953).
289. M. Born to A. Einstein, 26 November 1953, Ref. 268, pp. 205–207.
290. A. Einstein to M. Born, 3 December 1953, Ref. 268, pp. 208–209.
291. A. Einstein to M. Born, 12 January 1954, Ref. 268, pp. 214–215.
292. W. Pauli to M. Born, 3 March 1954, Ref. 268, pp. 217–219.
293. W. Pauli to M. Born, 31 March 1954, Ref. 268, pp. 221–225.
294. M. Born, 'Continuity, Determinism and Reality,' *Kgl. Dansk Vid. Selskab. Medd.* **30** (2) (1955).
295. Commentary upon the letter from W. Pauli to M. Born, dated 15 April 1954 (letter No. 116, in Ref. 268), p. 117. In 1954, Max Born was awarded the Nobel Prize for Physics (which he shared with Walther Bothe) for his formulation of the statistical interpretation of the wave function.
296. M. Planck, 'Physical Theory in the Light of New Researches,' *Naturwissenschaften* **14**, 249–261 (1926); esp. p. 261.
297. E. Mach, *The Science of Mechanics*, Ref. 86; see p. 579.
298. A. Einstein, 'On a Heuristic Point of View Concerning the Creation and Transformation of Light,' *Annalen der Physik (Leipzig)* **17**, 132–148 (1905).
298. A. Einstein, 'The Electrodynamics of Moving Bodies,' *Annalen der Physik (Leipzig)* **17**, 891–921 (1905).
300. A. Einstein, 'Remarks About a Note of P. Ehrenfest: "The Translation of Deformed Electrons and the Area Theorem,"' in *Annalen der Physik (Leipzig)* **23**, 206–208 (1907); see esp. p. 207.
301. E. Mach, *The Science of Mechanics*, Ref. 86, p. 284.

302. A. Einstein, *Autobiographical Notes,* in P. A. Schilpp, *loc. cit.,* p. 21.
303. P. Frank, *Einstein: His Life and Times,* second edition (New York, 1963), p. 105.
304. E. Mach, *The Physical Principles of Optics* (Dover, New York, 1953), pp. vii–viii.
305. E. Mach, *The Science of Mechanics,* Ref. 86, p. 587.
306. A. Einstein, 'Inferences Drawn from the Phenomena of Capillarity,' Ref. 3.
307. A. Einstein, 'On the Principle of Realivity and the Consequences Drawn from It,' in J. Stark's *Jahrbuch der Radioaktivität und der Elektronik* **4**, 411–462 (1907).
308. In his *Autobiographical Notes,* Einstein gave a later version of the principle of 'economy of thought,' p. 231.
309. G. Holton, 'Mach, Einstein, and the Search for Reality,' in R. S. Cohen and R. J. Seeger (Eds.), *Ernst Mach, Physicist and Philosopher* (D. Reidel, Dordrecht/Holland, 1970), pp. 165–199; see esp. p. 179.
310. A. Einstein, 'Principles of Research,' address in honor of M. Planck's 60th birthday, in *Ideas and Opinions* (New York, 1963), pp. 226–227.
311. P. Frank, *Einstein: His Life and Times* (New York, 1963), p. 214.
312. P. Frank, *loc. cit.,* p. 39.
313. P. Frank, *loc. cit.,* p. 215.
314. For instance, concerning the definition of entropy, see Einstein's papers on the Bose statistics (1924, 1925).
315. A. Einstein, The Herbert Spence Lecture, delivered at Oxford on 10 June 1933, in *Ideas and Opinions,* pp. 273–274.
316. E. Mach, *Science of Mechanics,* pp. 586–587; E. Mach, *loc. cit.,* p. 588.
317. E. Mach, see Ref. 316.
318. M. Born, lecture on 'Experiment and Theory in Physics,' held at the Durham Philosophical Society and the Pure Science

Society in Newcastle-upon-Tyne on 21 May 1943 (Cambridge, 1944), p. 1.
319. A. S. Eddington, *Relativity Theory of Protons and Electrons* (Cambridge, 1936).
320. M. Born, *loc. cit.*, p. 38.
321. P. W. Bridgman, *The Nature of Physical Theory* (Princeton, 1936), which evolved from his three Vanuxen lectures, given in December 1935; or *The Logic of Modern Physics.*
322. P. W. Bridgman, Vanuxen lectures, p. 8.
323. P. W. Bridgman, *loc. cit.*, pp. 9–11.
324. P. W. Bridgman, 'Einstein's Theories and the Operational Point of View,' in P. A. Schilpp, *loc. cit.*, p. 12.
325. A. Einstein, 'Reply to Criticisms,' in P. A. Schilpp, *loc. cit.*, p. 679.
326. I. Kant, preface to *The Metaphysical Foundation of Science*, quoted according to O. Buek's edition, *Philosophische Bibliothek*, Vol. 48 (Dürr'sche Buchhandlung, Leipzig).
327. A. Einstein, 'On the Method in Theoretical Physics,' The Herbert Spencer Lecture, 1933; translated into English in *Ideas and Opinions* (New York, 1954), p. 272.
328. A. Einstein, *Autobiographical Notes, loc. cit.*, p. 13.
329. A. Einstein, obituary of E. Mach, Ref. 282.
330. I. Kant, *Critique of Pure Reason*, Transcendental Doctrine of Elements, First Part.
331. M. Planck, 'The Unity of Physical View,' lecture delivered in Leyden on 9 December 1908; *Collected Works*, Vol. III, p. 27.
332. H. Poincaré, 'Why Space Has Three Dimensions,' in *Mathematics and Science, Last Essays* (Dover, New York, 1963), p. 44.
333. H. Margenau, 'Einstein's Conception of Reality,' in P. A. Schilpp, *loc. cit.*, p. 247.
334. P. Frank, Ref. 311, p. 41.
335. A. Einstein, 'Physics and Reality,' in *Ideas and Opinions, loc. cit.*, p. 322.

336. A. Einstein, 'Maxwell's Influence on the Evolution of the Idea of Physical Reality,' in *J. C. Maxwell: A Commemoration Volume* (Cambridge, 1931), introduction.
337. W. Heisenberg, *The Physical Principles of the Quantum Theory* (Dover, New York, 1930), p. 63.
338. H. Margenau, Ref. 333, p. 258.
339. D. Hume, *Treatise on Human Nature*, Book I, Part III, 'Of Probability and of the Idea of Cause and Effect' (Oxford, 1967), Ed. L. A. Selby-Bigge, pp. 75–76.
340. P. Frank, Ref. 311, p. 51.
341. I. Kant, *Critique of Pure Reason*, translated by N. Kemp-Smith (London, 1964), introduction, pp. 41, 43.
342. E. Cassirer, *Substance and Function* (Berlin, 1910); translated by W. C. and M. C. Swabey (Chicago, 1923); Dover reprint (1953).
343. E. Cassirer, Ref. 342, p. 260.
344. E. Cassirer, *loc. cit.*, p. 264.
345. A. C. Elsbach, *Kant und Einstein* (Walter de Guyter & Co., Berlin and Leipzig, 1924), p. 55.
346. A. L. Elsbach, Ref. 345, p. 68.
347. I. Kant's inaugural dissertation, translated by J. Handyside (Chicago, 1929), p. 59.
348. I. Kant, Ref. 347.
349. A. L. Elsbach, *loc. cit.*, p. 77.
350. A. L. Elsbach, *loc. cit.*, p. 196.
351. A. Einstein, 'Geometry and Experience,' lecture at the Prussian Academy of Sciences, Berlin, 27 January 1921; in *Ideas and Opinions*, pp. 232–246, esp. pp. 235–236.
352. I. Kant, *Metaphysical Foundations of Science*; see the preface in the German edition by O. Buek, entitled *Kleinere Schriften zur Naturphilosophie* (Leipzig, 1909), p. 191.
353. H. Poincaré, *Science and Hypothesis*; English translation by G. B. Halsted, in the *Foundation of Science* (New York, 1913), p. 27.

354. H. Poincaré, Ref. 353, pp. 65, 73.
355. H. Poincaré, *loc. cit.*, pp. 123–124.
356. H. Poincaré, *loc. cit.*, p. 127.
357. H. Poincaré, *loc. cit.*, p. 133.
358. H. Poincaré, *loc. cit.*, p. 136.
359. H. Poincaré, *loc. cit.*, p. 149.
360. H. Poincaré, *loc. cit.*, p. 159.
361. H. Poincaré, *loc. cit.*, p. 152.
362. H. Poincaré, Ref. 361.
363. H. Poincaré, *loc. cit.*, pp. 172–173.