The Philosophy of Quantum Mechanics

THE INTERPRETATIONS OF QUANTUM MECHANICS IN HISTORICAL PERSPECTIVE

Max Jammer

About the Author

MAX JAMMER, a native of Berlin (Germany), studied physics, philosophy and history of science at Hebrew University and at Harvard University (Ph.D. in experimental molecular spectroscopy). After having taught at Harvard, the University of Oklahoma and Boston University he was appointed chairman of the physics department, and later Rector and President, of Bar-Ilan University in Israel. He is also co-founder of the Institute for the History and Philosophy of Science, Tel-Aviv University. He was Visiting Professor at the Swiss Federal Institute of Technology in Zurich, the University of Göttingen, Institut Henri Poincaré in Paris, Columbia University and other universities in the United States and Canada. His books *Concepts of Space* (with a Foreword by Albert Einstein, *Foundations of Dynamics, Concepts of Mass in Classical and Modern Physics* and *The Conceptual Development of Quantum Mechanics,* all of which have been published in several languages besides English, have gained international recognition. Professor Jammer is a member of several learned societies and scientific academies, a contributor to numerous scholarly publications, including encyclopedias of physics and philosophy, and a member of the editorial boards of several scientific journals. He is recipient of the American Academy of Arts and Sciences Monograph Prize. At present he serves as Distinguished Visiting Professor at the City University of New York, Queens College.

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PREFACE

Never in the history of science has there been a theory which has had such a profound impact on human thinking as quantum mechanics; nor has there been a theory which scored such spectacular successes in the prediction of such an enormous variety of phenomena (atomic physics, solid state physics, chemistry, etc.). Furthermore, for all that is known today, quantum mechanics is the only consistent theory of elementary processes.

Thus although quantum mechanics calls for a drastic revision of the very foundations of traditional physics and epistemology, its mathematical apparatus or, more generally, its abstract *formalism* seems to be firmly established. In fact, no other formalism of a radically different structure has ever been generally accepted as an alternative. The *interpretation* of this formalism, however, is today, almost half a century after the advent of the theory, still an issue of unprecedented dissension. In fact, it is by far the most controversial problem of current research in the foundations of physics and divides the community of physicists and philosophers of science into numerous opposing "schools of thought."

In spite of its importance for physics and philosophy alike, the interpretative problem of quantum mechanics has rarely, if ever, been studied *sine ira et studio* from a general historical point of view. The numerous essays and monographs published on this subject are usually confined to specific aspects in defense of a particular view. No comprehensive scholarly analysis of the problem in its generality and historical perspective has heretofore appeared. The present historico-critical study is designed to fill this lacuna.

The book is intended to serve two additional purposes.

Since the book is not merely a chronological catalogue of the various interpretations of quantum mechanics but is concerned primarily with the analysis of their conceptual backgrounds, philosophical implications, and interrelations, it may also serve as a general introduction to the study of the logical foundations and philosophy of quantum mechanics. Although indispensable for a deeper understanding of modern theoretical physics, this subject is seldom given sufficient consideration in the usual textbooks and lecture courses on the theory. The historical approach, moreover, has the didactical advantage of facilitating such a study for the uninitiated reader.

Finally, because of its detailed documentation the book may also be used as a guide to the literature of the subject. Great care has been taken to provide accurate and up-to-date references to the international literature on the topics discussed. The reader should find it easy to pursue any specific detail in which he happens to be interested. To make the book self-contained and understandable not only to the specialist but also to the general reader familiar with the rudiments of quantum physics, proofs of all the theorems which are of decisive importance for the interpretative problem are given either in detail or in outline. In addition, much of the material which is required to understand the text but is usually not included in courses on quantum mechanics is explained either in full or at least to such an extent that no difficulties should arise in following the arguments. In particular, for the convenience of the reader the essentials of lattice theory, a subject seldom studied by physicists but indispensable for a comprehension of quantum logic and related topics, are summarized in an appendix at the end of the book. Concise as it is, this summary contains all prerequisites to prove the theorems referred to in the text.

If the reader is interested only in the philosophical aspects of the subject, he may well omit some of the more technical and mathematical sections of the text and yet be able to follow the main argument without serious loss of continuity.

The notation, made uniform as much as possible, is always explained in the text. To keep the book to manageable length, repetitions of footnotes are avoided. To this end, an abbreviation like footnote 2 in Chapter 3, "Ref. 2-1 (1969, p. 105; 1971, p. 73)," is meant to refer to page 105 of the 1969 publication and to page 73 of the 1971 publication mentioned in footnote 1 of Chapter 2. In references to the same chapter the chapter number is omitted. A similar notation is adopted for references to mathematical equations.

The book has its origin in lecture notes for a graduate course on the history and philosophy of modern physics which I gave in 1968 at Columbia University (New York). The first four chapters of the book were written during my visits to the Minnesota Center for Philosophy of Science (Minneapolis), the Max-Planck-Institute (Munich and Starnberg), and the Niels Bohr Institute (Copenhagen). Chapter 5 is based on a paper which I read in 1971 at Lomonosov State University (Moscow) on the occasion of the XIIIth International Congress of the History of Science. The subsequent two chapters are expanded versions of talks which I gave in 1972 and 1973 at the International School of Physics "Enrico Fermi" at

Varenna (Italy), at the University of Florence, and at the Universities of Amsterdam. Chapter 8 was written during my visit to the Universities of Berlin, Göttingen, Hamburg, and Marburg. The last three chapters were completed at Simon Fraser University (British Columbia, Canada), the University of Alberta (Edmonton, Canada), and Reed College (Portland, Oregon) where I served in 1973 as Andrew Mellon Distinguished Visiting Professor.

These lecture engagements enabled me to consult many libraries and archives and, more important, to establish personal contact with the leading quantum theorists of our time. Disregarding what the pragmatic humanist F. C. S. Schiller once called "the curious etiquette which apparently taboos the asking of questions about a philosopher's meaning while he is alive," I unscrupulously interrogated many prominent authorities about numerous details of their work in the foundations of quantum mechanics. Their readiness and frankness in answering my questions enabled me to obtain much of the information first hand, an opportunity invaluable for one who works in the current history of physics.

I thus owe a great debt of gratitude. Most influential on my view of the role of philosophy in physics were the discussions with, and writings of, Professors Herbert Feigl, Paul K. Feyerabend, Henry Margenau, Ernest Nagel, and Wolfgang Stegmüller. As to physics proper, I wish to express my gratitude to Professors Louis de Broglie, Paul A. M. Dirac, Tsung-Dao Lee, and Eugene P. Wigner for having read parts of the manuscript or having given me the privilege of discussing with them numerous subjects dealt with in the book. I would also like to acknowledge my indebtedness to Professors Leslie E. Ballentine and David Bohm for having read substantial parts of the typescript, and to Professors Friedrich Bopp, Jeffrey Bub, Richard Friedberg, Kurt Hübner, Friedrich Hund, Josef M. Jauch, Pascual Jordan, Gerhart Lüders, Peter Mittelstaedt, Wilhelm Ochs, Rudolf Peierls, Constantin Piron, Mauritius Renninger, Nathan Rosen, Léon Rosenfeld, Mendel Sachs, Erhard Scheibe, Abner Shimony, Georg Süssmann, and Carl Friedrich von Weizsäcker for their patience in discussing with me in stimulating conversations many aspects of their work. I am also grateful to Professors Assène B. Datzeff, John Stewart Bell, Dimitrii I. Blokhintsev, Wolfgang Büchel, Hilbrand J. Groenewold, Grete Henry-Hermann, Banesh Hoffmann, Edwin C. Kemble, Alfred Landé, Sir Karl R. Popper, and Martin Strauss as well as to Dr. Hugh Everett III, Mrs. Edith London, and Mrs. (Polly) Boris Podolsky for their cooperation in providing epistolary information. Finally, I wish to thank my colleagues Professors Marshall Luban and Paul Gluck for their critical reading of the typescript of the book.

Needless to say, the responsibility for any errors or misinterpretations rests entirely upon me.

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CONTENTS

THE PHILOSOPHY OF QUANTUM MECHANICS

FORMALISM
and Interpretations

Chapter One

1.1. THE FORMALISM

The purpose of the first part of this introductory chapter is to present a brief outline of the mathematical formalism of nonrelativistic quantum mechanics of systems with a finite number of degrees of freedom. This formalism, as we have shown elsewhere,[1] was the outcome of a complicated conceptual process of trial and error and it is hardly an overstatement to say that it preceded its own interpretation, a development almost unique in the history of physical science. Although the reader is assumed to be acquainted with this formalism, its essential features will be reviewed, without regard to mathematical subtleties, to introduce the substance and terminology needed for discussion of the various interpretations.

Like other physical theories, quantum mechanics can be formalized in terms of several axiomatic formulations. The historically most influential and hence for the history of the interpretations most important formalism was proposed in the late 1920s by John von Neumann and expounded in his classic treatise on the mathematical foundations of quantum mechanics.[2]

In recent years a number of excellent texts[3] have been published which discuss and elaborate von Neumann's formalism and to which the reader is referred for further details.

Von Neumann's idea to formulate quantum mechanics as an operator calculus in Hilbert space was undoubtedly one of the great innovations in modern mathematical physics.[4]

[1]M. Jammer, *The Conceptual Development of Quantum Mechanics* (McGraw-Hill, New York, 1966, 1968, 1973): *Ryōshi Riki-gaku Shi* (Tokyo Tosho, Tokyo, 1974).

[2]J. von Neumann, *Mathematische Grundlagen der Quantenmechanik* (Springer, Berlin, 1932, 1969; Dover, New York, 1943); *Les Fondements Mathématiques de la Mécanique Quantique* (Alcan, Paris, 1946); *Fundamentos Matemáticos de la Mecánica Cuántica* (Instituto Jorge Juan, Madrid, 1949); *Mathematical Foundations of Quantum Mechanics* (Princeton University Press, Princeton, N.J., 1955); *Matematičeskije Osnovi Kvantovoj Mekhaniki* (Nauka, Moscow, 1964).

[3]G. Fano, *Metodi Matematici della Meccanica Quantistica* (Zanichelli, Bologna, 1967); *Mathematical Methods of Quantum Mechanics* (McGraw-Hill, New York, 1971). B. Sz.-Nagy, *Spektraldarstellung linearer Transformationen des Hilbertschen Raumes* (Springer, Berlin, Heidelberg, New York, 1967); J. M. Jauch, *Foundations of Quantum Mechanics* (Addison-Wesley, Reading, Mass., 1968); B. A. Lengyel, "Functional analysis for quantum theorists," *Advances in Quantum Chemistry* **1968**, 1–82; J. L. Soulé, *Linear Operators in Hilbert Space* (Gordon and Breach, New York, 1968); T. F. Jordan, *Linear Operators for Quantum Mechanics* (Wiley, New York, 1969); E. Prugovečki, *Quantum Mechanics in Hilbert Space* (Academic Press, New York, London, 1971).

[4]For the history of the mathematical background of this discovery see Ref. 1 and M. Bernkopf, "The development of function spaces with particular reference to their origins in integral equation theory," *Archive for History of Exact Sciences* **3**, 1–96 (1966); "A history of infinite matrices," *ibid.*, **4**, 308–358 (1968); E. E. Kramer, *The Nature and Growth of Modern*

A *Hilbert space* $\mathcal{H}$, as abstractly defined by von Neumann, is a linear strictly positive inner product space (generally over the field $\mathcal{F}$ of complex numbers) which is complete with respect to the metric generated by the inner product and which is separable. Its elements are called *vectors*, usually denoted by $\psi, \varphi, \ldots$, and their inner or scalar product is denoted by (φ, ψ), whereas the elements of $\mathcal{F}$ are called *scalars* and usually denoted by $a, b, \ldots$. In his work on linear integral equations (1904–1910) David Hilbert had studied two realizations of such a space, the Lebesgue space $\mathfrak{L}^2$ of (classes of) all complex-valued Lebesgue measurable square-integrable functions on an interval of the real line R (or R itself), and the space $\mathfrak{l}^2$ of sequences of complex numbers, the sum of whose absolute squares converges. Impressed by the fact that by virtue of the Riesz-Fischer theorem these two spaces can be shown to be isomorphic (and isometric) and hence, in spite of their apparent dissimilarity, to be essentially the same space, von Neumann named all spaces of this structure after Hilbert. The fact that this isomorphism entails the equivalence between Heisenberg's matrix mechanics and Schrödinger's wave mechanics made von Neumann aware of the importance of Hilbert spaces for the mathematical formulation of quantum mechanics.

To review this formulation let us recall some of its fundamental notions. A (closed) *subspace* S of a Hilbert space $\mathcal{H}$ is a *linear manifold* of vectors (i.e., closed under vector addition and multiplication by scalars) which is closed in the metric and hence a Hilbert space in its own right. The *orthogonal complement* $S^{\perp}$ of S is the set of all vectors which are orthogonal to all vectors of S. A mapping $\psi \to \varphi = A\psi$ of a linear manifold $\mathfrak{D}_A$ into $\mathcal{H}$ is a *linear operator* A, with *domain* $\mathfrak{D}_A$, if $A(a\psi_1 + b\psi_2) = aA\psi_1 + bA\psi_2$ for all ψ_1, ψ_2 of $\mathfrak{D}_A$ and all a, b of $\mathcal{F}$. The image of $\mathfrak{D}_A$ under A is the *range* $\mathfrak{R}_A$ of A. The linear operator A is *continuous* if and only if it is *bounded* [i.e., if and only if $\|A\psi\|/\|\psi\|$ is bounded, where $\|\psi\|$ denotes the *norm* $(\psi,\psi)^{1/2}$ of ψ]. A' is an *extension* of A, or $A' \supseteq A$, if it coincides with A on $\mathfrak{D}_A$ and $\mathfrak{D}_{A'} \supseteq \mathfrak{D}_A$. Since every bounded linear operator has a unique continuous extension to $\mathcal{H}$, its domain can always be taken as $\mathcal{H}$.

The *adjoint* A^+ of a bounded linear operator A is the unique operator A^+ which satisfies $(\varphi, A\psi) = (A^+\varphi, \psi)$ for all φ, ψ of $\mathcal{H}$. A is *self-adjoint* if $A = A^+$. A is unitary if $AA^+ = A^+A = I$, where I is the identity operator. If S is a subspace of $\mathcal{H}$, then every vector ψ can uniquely be written $\psi = \psi_S + \psi_{S^\perp}$, where ψ_S is in S and $\psi_{S^\perp}$ is in $S^{\perp}$, so that the mapping $\psi \to \psi_S = P_S\psi$ defines the *projection* P_S as a bounded self-adjoint idempotent (i.e., $P_S^2 = P_S$) linear operator. Conversely, if a linear operator P is

Mathematics (Hawthorn, New York, 1970), pp. 550–576; M. Kline, *Mathematical Thought from Ancient to Modern Times* (Oxford University Press, New York, 1972), pp. 1091–1095.

bounded, self-adjoint, and idempotent, it is a projection. Projections and subspaces correspond one to one. The subspaces S and T are *orthogonal* [i.e., $(\varphi,\psi)=0$ for all φ of S and all ψ of T], in which case we also say that P_S and P_T are orthogonal if and only if $P_SP_T=P_TP_S=0$ (null operator); and $\Sigma_{j=1}^{N}P_{S_j}$ is a projection if and only if $P_{S_j}P_{S_k}=0$ for $j\neq k$.

$S\subset T$ (i.e., the subspace S is a subspace of T, in which case we also write $P_S\leqslant P_T$) if and only if $P_SP_T=P_TP_S=P_S$. In this case P_T-P_S is a projection into the orthogonal complement of S in T, that is, the set of all vectors of T which are orthogonal to every vector of S.

For an unbounded linear operator A—which if it is symmetric [i.e., if $(\varphi,A\psi)=(A\varphi,\psi)$ for all φ,ψ of $\mathfrak{D}_A$] cannot, according to the Hellinger-Toeplitz theorem, have a domain which is $\mathcal{H}$ but may have a domain which is dense in $\mathcal{H}$—the self-adjoint is defined as follows. The set of all vectors φ for which there exists a vector φ^* such that $(\varphi,A\psi)=(\varphi^*,\psi)$ for all ψ of $\mathfrak{D}_A$ is the domain $\mathfrak{D}_{A^+}$ of the adjoint of A and the adjoint A^+ of A is defined by the mapping $\varphi\rightarrow\varphi^*=A^+\varphi$. A is self-adjoint if $A=A^+$.

According to the *spectral theorem*,[5] to every self-adjoint linear operator A corresponds a unique *resolution of identity*, that is, a set of projections $E^{(A)}(\lambda)$ or briefly E_λ, parametrized by real λ, such that (1) $E_\lambda\leqslant E_{\lambda'}$ for $\lambda\leqslant\lambda'$, (2) $E_{-\infty}=0$, (3) $E_\infty=I$, (4) $E_{\lambda+0}=E_\lambda$, (5) $I=\int_{-\infty}^{\infty}dE_\lambda$, (6) $A=\int_{-\infty}^{\infty}\lambda\, dE_\lambda$ [which is an abbreviation of $(\varphi,A\psi)=\int_{-\infty}^{\infty}\lambda\, d(\varphi,E_\lambda\psi)$, where the integral is to be interpreted as the Lebesgue-Stieltjes integral[6]], and finally (7) for all λ, E_λ commutes with any operator that commutes with A. The *spectrum* of A is the set of all λ which are not in an interval in which E_λ is constant. Those λ at which E_λ is discontinuous ("jumps") form the *point spectrum* which together with the *continuous spectrum* constitutes the *spectrum*.

Now, λ is an *eigenvalue* of A if there exists a nonzero vector φ, called *eigenvector* belonging to λ, in $\mathfrak{D}_A$ such that $A\varphi=\lambda\varphi$. An eigenvalue is

[5]This theorem was proved by von Neumann in "Allgemeine Eigenwerttheorie Hermitischer Funktionaloperatoren," *Mathematische Annalen*, **102**, 49–131 (1929), reprinted in J. von Neumann, *Collected Works*, A. H. Taub, ed. (Pergamon Press, New York, 1961), Vol. 2, pp. 3–85. It was proved independently by M. H. Stone using a method earlier applied by T. Carleman to the theory of integral equations with singular kernel, cf. M. H. Stone, *Linear Transformations in Hilbert Space* (American Mathematical Society Colloquium Publications, Vol. 15, New York, 1932), Ch. 5. Other proofs were given by F. Riesz in 1930, B. O. Koopman and J. L. Doob in 1934, B. Lengyel in 1939, J. L. B. Cooper in 1945, and E. R. Lorch in 1950.

[6] $\int_a^b f(\lambda)dg(\lambda)$ is defined as $\lim\Sigma_{j=1}^{n}f(\lambda_j')[g(\lambda_{j+1})-g(\lambda_j)]$, where $\lambda_1,\lambda_2,\ldots,\lambda_n$ is a partition of the interval $[a,b]$, λ_j' is in the jth interval, and the limes denotes the passage to $\lambda_{j+1}-\lambda_j=0$ for all j.

nondegenerate if the subspace formed by the eigenvectors belonging to this eigenvalue is one-dimensional.[7] Every λ in the point spectrum of A is an eigenvalue of A. If the spectrum of A is a nondegenerate point spectrum $\lambda_j (j=1,2,\ldots)$, then the *spectral decomposition* (6) of A reduces to $A=\Sigma\lambda_j P_j$, where P_j is the projection on the eigenvector ("ray") φ_j belonging to λ_j. In fact, in this case $dE_\lambda = E_{\lambda+d\lambda} - E_\lambda \neq 0$ only if λ_j lies in $[\lambda,\lambda+d\lambda)$ where dE_λ becomes P_j. To vindicate this conclusion by an elementary consideration, let $\psi=\Sigma\varphi_j(\varphi_j,\psi)$ be an expansion of any vector ψ in terms of the eigenvectors φ_j of A; then $A\psi=\Sigma\lambda_j\varphi_j(\varphi_j,\psi)=\Sigma\lambda_j P_j\psi$ for all ψ.

With these mathematical preliminaries in mind and following von Neumann, we now give an axiomatized presentation of the formalism of quantum mechanics. The primitive (undefined) notions are *system, observable* (or "physical quantity" in the terminology of von Neumann), and *state*.

AXIOM I. To every system corresponds a Hilbert space $\mathcal{H}$ whose vectors (state vectors, wave functions) completely describe the states of the system.

AXIOM II. To every observable $\mathcal{A}$ corresponds uniquely a self-adjoint operator A acting in $\mathcal{H}$.

AXIOM III. For a system in state φ, the probability $\text{prob}_A(\lambda_1,\lambda_2|\varphi)$ that the result of a measurement of the observable $\mathcal{A}$, represented by A, lies between λ_1 and λ_2 is given by $\|(E_{\lambda_2}-E_{\lambda_1})\varphi\|^2$, where E_λ is the resolution of the identity belonging to A.

AXIOM IV. The time development of the state vector φ is determined by the equation $H\varphi=i\hbar\partial\varphi/\partial t$ (Schrödinger equation), where the Hamiltonian H is the evolution operator and $\hbar$ is Planck's constant divided by 2π.

AXIOM V. If a measurement of the observable $\mathcal{A}$, represented by A, yields a result between λ_1 and λ_2, then the state of the system immediately after the measurement is an eigenfunction of $E_{\lambda_2}-E_{\lambda_1}$.

The *correspondence Axioms* I and II associate the primitive notions with mathematical entities. Von Neumann's original assumption that observables and self-adjoint operators stand in a one-to-one correspondence and that all nonzero vectors of the Hilbert space are state vectors had to be

[7]The dimension of a Hilbert space is the cardinality of a complete orthonormal system of vectors in it.

abandoned in view of the existence of superselection rules, discovered in 1952 by G. C. Wick, E. P. Wigner, and A. S. Wightman.

The often postulated statement that the result of measuring an observable $\mathcal{A}$, represented by A, is an element of the spectrum of A follows as a logical consequence from Axiom III. Moreover, the theorem that the expectation value $\mathrm{Exp}_\varphi A$ of $\mathcal{A}$ for a system in state φ, defined by the self-explanatory expression $\lim_{\Delta=0}\Sigma_j\lambda_j\ \mathrm{prob}_A(\lambda_j,\lambda_j+\Delta|\varphi)$, is $(\varphi,A\varphi)$ can easily be proved on the basis of Axioms I to III. Conversely, by the technique of characteristic functions as used in the theory of probability, it can be shown that this theorem entails Axiom III. Let us add that in the simple nondegenerate discrete case the just-mentioned definition of $\mathrm{Exp}_\varphi A$ becomes $\Sigma\lambda_j\mathrm{prob}_A(\lambda_j|\varphi)$, where, according to Axiom III, this probability $\mathrm{prob}_A(\lambda_j|\varphi)$ is given by $|(\varphi_j,\varphi)|^2$.

"Quantum statics," the part of quantum mechanics which disregards changes in time, is based, as we see, essentially only on one axiom, Axiom III. This axiom, moreover, is the only one which establishes some connection between the mathematics and physical data and therefore plays a major role for all questions of interpretations. In its ordinary interpretation it contains as a particular case Born's well-known probabilistic interpretation of the wave function according to which for a measurement of the position observable $\mathcal{Q}$ the probability density of finding the system at the position q is given by $|\psi(q)|^2$. In fact, if the operator Q, representing the observable $\mathcal{Q}$, is defined by $Q\psi(q)=q\psi(q)$, its spectral decomposition is given by $E_\lambda\psi(q)=\psi(q)$ for $q\leqslant\lambda$ and $E_\lambda\psi(q)=0$ for $q>\lambda$ and hence, according to Axiom III, the probability that $\lambda_1\leqslant q<\lambda_2$ is $\|(E_{\lambda_2}-E_{\lambda_1})\psi\|^2 = \int_1^2|\psi(q)|^2dq$, which proves the contention.

Axiom IV, the axiom of "quantum dynamics," can be replaced by postulating a one-parameter group of unitary operators $U(t)$ acting on the Hilbert space of the system such that $\varphi(t)=U(t)\varphi(0)$, and applying Stone's theorem according to which there exists a unique self-adjoint operator H such that $U(t)=\exp(-itH)$; it may also be equivalently formulated in terms of the statistical operator. Finally, Axiom V states that in the discrete case, immediately after having obtained the eigenvalue λ_j of A when measuring $\mathcal{A}$, the state of the system is an eigenvector of P_j, the projection on the eigenvector belonging to λ_j; for this reason Axiom V is called the "projection postulate." It is more controversial than the rest and has indeed been rejected by some theorists on grounds to be discussed in due course.

Although a complete derivation of all quantum mechanical theorems, with the inclusion of those pertaining to simultaneous measurements and identical particles, would require some additional postulates, these five axioms suffice for our purpose to characterize von Neumann's formalism of quantum mechanics, which is the one generally accepted.

In addition to the notions of system, observable, and state, the notions of *probability* and *measurement* have been used without interpretations. Although von Neumann used the concept of probability, in this context, in the sense of the frequency interpretation, other interpretations of quantum mechanical probability have been proposed from time to time. In fact, all major schools in the philosophy of probability, the subjectivists, the a priori objectivists, the empiricists or frequency theorists, the proponents of the inductive logic interpretation and those of the propensity interpretation, laid their claim on this notion. The different interpretations of probability in quantum mechanics may even be taken as a kind of criterion for the classification of the various interpretations of quantum mechanics. Since the adoption of such a systematic criterion would make it most difficult to present the development of the interpretations in their historical setting it will not be used as a guideline for our text.[8]

Similar considerations apply a fortiori to the notion of measurement in quantum mechanics. This notion, however it is interpreted, must somehow combine the primitive concepts of system, observable, and state and also, through Axiom III, the concept of probability. Thus measurement, the scientist's ultimate appeal to nature, becomes in quantum mechanics the most problematic and controversial notion because of its key position.

The major part of the operator calculus in Hilbert space and, in particular, its spectral theory had been worked out by von Neumann before Paul Adrien Maurice Dirac published in 1930 his famous treatise[9] in which he presented a conceptually most compact and notationally most elegant formalism for quantum mechanics. Even though von Neumann admitted that Dirac's formalism could "scarcely be surpassed in brevity and elegance," he criticized it as deficient in mathematical rigor, especially in view of its extensive use of the (at that time) mathematically unacceptable delta-function. Later, when Laurent Schwartz' theory of distributions made it possible to incorporate Dirac's improper functions into the realm of rigorous mathematics—a classic example of how physics may stimulate

[8]The reader interested in working out such a classification will find for his convenience bibliographical references in Selected Bibliography I in the Appendix at the end of this chapter. M. Strauss' essay "Logics for quantum mechanics," *Foundations of Physics* **3**, 265–276 (1973), contains useful suggestions of how to carry out such a classification.

[9]P. A. M. Dirac, *The Principles of Quantum Mechanics* (Clarendon Press, Oxford, 1930, 1935, 1947, 1958); *Die Prinzipien der Quantenmechanik* (Hirzel, Leipzig, 1930); *Les Principes de la Mécanique Quantique* (Presses Universitaires de France, Paris, 1931); *Osnovi Kvantovoj Mekhaniki* (GITTL, Moscow, Leningrad, 1932, 1937).

the growth of new branches in mathematics—Dirac's formalism seemed not to be assimilable to von Neumann's.[10] Yet due to its immediate intuitability and notational convenience Dirac's formalism not only survived but became the favorite framework for many expositions of the theory. The possibility of assimilating Dirac's formalism with von Neumann's approach has recently become the subject of important investigations such as Marlow's[11] presentation of the spectral theory in terms of direct integral decompositions of Hilbert space, Roberts'[12] recourse to "rigged" Hilbert spaces as well as the investigations by Hermann[13] and Antoine.[14]

Other formalisms of quantum mechanics such as the algebraic approach, initiated in the early 1930s by von Neumann, E. P. Wigner, and P. Jordan and elaborated in the 1940s by I. E. Segal, or the quantum logical approach, started by G. Birkhoff and von Neumann in 1936 and perfected by G. Mackey in the late 1950s, the former leading to the C^*-algebra theory of quantum mechanics and the latter to the development of modern quantum logic, will be discussed in their appropriate contexts. On the other hand, we shall hardly feel the need to refer to the S-matrix approach, which, anticipated in 1937 by J. A. Wheeler,[15] was developed in 1942 by Werner Heisenberg[16] for elementary particle theory—although it has recently been claimed[17] to be the most appropriate mathematical framework for a "pragmatic version" of the Copenhagen interpretation of the theory. Nor shall we have many occasions to refer to the interesting path integral

[10]Von Neumann apparently rejected this possibilty: "It should be emphasized that the correct structure does not consist in a mathematical refinement and explication of the Dirac method but rather necessitates a procedure differing from the very beginning, namely, the reliance on the Hilbert theory of operators." Preface, Ref. 2.

[11]A. R. Marlow, "Unified Dirac-von Neumann formulation of quantum mechanics," *Journal of Mathematical Physics* **6**, 919–927 (1965).

[12]J. E. Roberts, "The Dirac bra and ket formalism," *Journal of Mathematical Physics* **7**, 1097–1104 (1966); "Rigged Hilbert spaces in quantum mechanics," *Communications in Mathematical Physics*, **3**, 98–119 (1966).

[13]R. Hermann, "Analytic continuation of group representations," *Communications in Mathematical Physics* **5**, 157–190 (1967).

[14]J. P. Antoine, "Dirac formalism and symmetry problems in quantum mechanics," *Journal of Mathematical Physics* **10**, 53–69, 2276–2290 (1969).

[15]J. A. Wheeler, "On the mathematical description of light nuclei by the method of resonating group structure," *Physical Review* **52**, 1107–1122 (1937).

[16]W. Heisenberg, "'Beobachtbare Grössen' in der Theorie der Elementarteilchen," *Zeitschrift für Physik* **120**, 513–538 (1942).

[17]H. P. Stapp, "S-matrix interpretation of quantum mechanics," *Physical Review* **D3**, 1303–1320 (1971); "The Copenhagen interpretation," *American Journal of Physics* **40**, 1098–1116 (1972).

approach which Richard P. Feynman[18] developed when, in the course of his graduate studies at Princeton, he extended the concept of probability amplitude superpositions to define probability amplitudes for any motion or path in space-time, and when he showed how ordinary quantum mechanics results from the postulate that these amplitudes have a phase proportional to the classically computed action for the path. Suffice it to point out that Feynman's approach has recently been used to emphasize that "the wave theory [is] for particles...as inevitable and necessary as Huygen's wave theory for light."[19]

Since our presentation follows the historical development which was predominantly influenced by von Neumann's ideas, these alternative formalisms will play a subordinate role in our discussion, especially in the later chapters and in particular in our account of the quantum theory of measurement. Our disregard of these other formalisms should therefore not be interpreted as a depreciation of their scientific importance.

1.2. INTERPRETATIONS

Having reviewed the formalism of the quantum theory let us now turn to the question of what it means to *interpret* this formalism. This is by no means a simple question. In fact, just as physicists disagree on what *is* the correct interpretation of quantum mechanics, philosophers of science disagree on what it *means* to interpret such a theory. If for mathematical theories the problem of interpretation, usually solved by applying the language of model theory (in the technical sense) requires a conceptually quite elaborate apparatus, then for empirical theories—which differ from the former not so much in syntax as in semantics—the problem is considerably more difficult. A comprehensive account of the various views on this issue, such as those expressed by Peter Achinstein, Paul K. Feyerabend, Israel Scheffler, or Marshall Spector, to mention only a few leading specialists in this subject, would therefore require a separate monograph as voluminous as the present book. Since, however, the issue has an important relevance for our subject we cannot afford to ignore it completely but shall confine ourselves to some brief and nontechnical comments. Our discussion will be based on the so-called partial interpretation thesis for two reasons: it provides the most convenient framework in terms of which

[18]R. P. Feynman, "Space-time approach to non-relativistic quantum mechanics," *Reviews of Modern Physics* **20**, 367–385 (1948).
[19]D. B. Beard and C. B. Beard, *Quantum Mechanics with Applications* (Allyn and Bacon, Boston, 1970), p. XIII.

the problem can be presented and it seems to be the most widely accepted view among philosophers of science.

This view, which became the standard conception of logical empiricism and has been elaborated in great detail by Richard B. Braithwaite, Rudolf Carnap, Carl G. Hempel, Ernest Nagel, and Wolfgang Stegmüller among others, holds that a physical theory is a partially interpreted formal system. To explain what this means it is useful to distinguish between at least two components of a physical theory T: (1) an abstract formalism F and (2) a set R of rules of correspondence. The formalism F, the logical skeleton of the theory, is a deductive, usually axiomatized calculus devoid of any empirical meaning;[20] it contains, apart from logical constants and mathematical expressions, nonlogical (descriptive) terms, like "particle" and "state function," which, as their name indicates, do not belong to the vocabulary of formal logic but characterize the specific content of the subject under discussion. Although the names of these nonlogical terms are generally highly suggestive of physical significance, the terms have no meaning other than that resulting from the place they occupy in the texture of F; like the terms "point" or "congruent" in Hilbert's axiomatization of geometry they are only implicitly defined. Thus F consists of a set of primitive formulae, which serve as its postulates, and of other formulae which are derived from the former in accordance with logical rules. The difference between primitive terms in F, which are undefined, and non-primitive terms, which are defined by the former, should not be confused with the difference between theoretical terms and observational terms, which will now be explained.

To transform F into a hypothetic deductive system of empirical statements and to make it thus physically meaningful, some of the nonlogical terms, or some formulae in which they occur, have to be correlated with observable phenomena or empirical operations. These correlations are expressed by the rules of correspondence R or, as they are sometimes called, coordinating definitions, operative definitions, semantical rules, or epistemic correlations. F without R is a meaningless game with symbols, R without F is at best an incoherent and sterile description of facts. The rules of correspondence which assign meaning to some of the nonlogical terms are expressed not in the language of the theory, the object language, but in a so-called metalanguage which contains terms supposed to be antecedently understood. The observational terms, that is, the nonlogical terms to

[20]It should be noted that, because of Axiom III, the "von Neumann formalism," as presented above, is not a pure formalism in the sense of the present context. This fact, however, does not affect our present considerations. A suggestion to "derive" the interpretative element (of Axiom III) or its equivalent from a purely mathematical formalism will be discussed in connection with the so-called multi-universes theory in Chapter 11.

which R assigns empirical meaning, need not occur just in the postulates of F; usually F is interpreted "from the bottom" and not "from the top." Let us denote the formalism F, when thus partially interpreted by means of the correspondence rules R, by the symbol F_R. Clearly, a different set R' of such rules yields a different $F_{R'}$.

It has been claimed by some positivistically inclined philosophers of science that a physical theory is precisely such an F_R. In their view, a physical theory is not an explanation but rather, as Pierre Duhem once expressed it, "a system of mathematical propositions whose aim is to represent as simply, as completely, and as exactly as possible a whole group of experimental laws," requirements which can be met on the basis of F and R alone.

Other schools of thought contend that a system of description, however comprehensive and accurate it may be, does not constitute a physical theory. Like Aristotle, who once said that "men do not think they know a thing until they have grasped the 'why' of it," they maintain that a full-fledged theory must have, in addition, an explanatory function. Some also claim that the value of a scientific theory is not gauged by the faithfulness of its representation of a given class of known empirical laws but rather by its predictive power of discovering as yet unknown facts. In their view F_R has to be supplemented by some unifying principle which establishes an internal coherence among the descriptive features of the theory and endows it thereby with explanatory and predictive power. The proposal of such a principle is usually also called an "interpretation" but should, of course, be sharply distinguished from its homonym in the sense of introducing R. The former is an interpretation of F_R, the latter an interpretation of F. It is the interpretation of F_R which gives rise to the much debated philosophical problems in physics, such as the ontological question of "physical reality" or the metaphysical issue of "determinism versus indeterminism."

The quest for explanatory principles is considerably facilitated by the construction of a "picture" or a model M for the theory T, a process which is also often referred to as an "interpretation" of the theory. In fact, M is often defined as a fully interpreted system, say of propositions, whose *logical* structure is similar or isomorphic to that of F_R but whose *epistemological* structure differs significantly from that of F_R insofar as in F_R the logically posterior propositions ("at the bottom") determine the meaning of terms (or propositions) occurring at its higher levels whereas in the model M the logically prior propositions ("at the top") determine the meaning of the terms (or propositions) occurring at the lower levels. It is this feature which gives the model its unifying character and explanatory nature. Apart from being a thought-economical device aiding one to memorize in "one look" all major aspects of the theory, M may also be heuristically most useful by pointing to new avenues of research which without M would perhaps not have suggested themselves. The model M thus becomes instrumental in strengthening the predictive power of T. But it should also be noted that there exists always the danger that adventitious features of M may erroneously be taken as constitutive and hence indispensable ingredients of T itself, or M may be identified with T itself, an error not infrequently committed in the history of the interpretations of quantum mechanics. It is worthwhile to point out in this context that the Copenhagen interpretation, by rejecting the very possibility of constructing an M for T, became virtually immune against this fallacy.

Having thus far encountered three different meanings of "interpretations," the interpretation of F by R, the interpretation of F_R by additional principles, and the construction of M, we are now led to a fourth meaning of this term which is intimately connected with the construction of M. It may well happen that for one reason or another a suggested model M exhibits most strikingly *many* major relations of the structure of F or of F_R *but not all* of them. It may then prove advisable to modify not M but F to obtain isomorphism between the two structures. Strictly speaking, such a proposal replaces the original theory T by an alternative theory T'. But since the modifications incurred are, as a rule, only of minor extent, the new theory T' with its model M will—in conformance with the common parlance in physical literature—also be called an "interpretation" of the original theory T, especially if the modifications proposed do not imply observable, or for the time being observable, experimental effects. An example is the replacement of the Schrödinger equation by a nonlinear equation as suggested on various occasions by Bohm, Vigier, Terletzkii, or others. If a distinction is of importance we shall use different terms. In our treatment of hidden variables, for example, we distinguish between "hidden-variable *interpretations*" which refer to the unmodified formalism and "hidden-variable *theories*" which refer to a modified formalism.

A particular case of an interpretation of T in terms of a model suggests itself if T can be subsumed as part of a more general theory T^* which is fully or partially interpreted. This is always possible if there exists a theory T^* such that the formalism F of T is identical with, or part of, the formalism F^* of T^*. Most of the semiclassical interpretations of quantum mechanics, which will be discussed in Chapter 2, and in particular the hydrodynamical interpretations of the quantum theory, are illustrations in point.

That M can also be used to examine the logical consistency of a physical theory was noted by Dirac when he wrote that although "the main object of physical science is not the provision of pictures" and "whether a picture exists or not is a matter of only secondary importance," one may "extend the meaning of the word 'picture' to include any way of looking at the fundamental laws which make their self-consistency obvious."

In all physically important theories not all the nonlogical terms in F are given empirical meaning through the rules of correspondence R. In contrast to the observational terms the nonlogical terms which are not directly interpreted through R are called "theoretical terms." As mentioned earlier, they are only implicitly or contextually defined through the role they play within the logical structure of F. It is because of this fact that we say that T is only "partially" interpreted.

This state of affairs thus leads naturally to the question whether it would be possible to eliminate systematically all theoretical terms and to change thereby the status of a partially interpreted theory to that of a fully interpreted theory without, however, changing its empirical content. An affirmative answer was given by the school of logical constructionists who like Karl Pearson or Bertrand Russell insisted that "wherever possible, logical constructions are to be substituted for inferred entities." In their views all theoretical terms are logical constructions which can be reduced to their constitutive elements, that is, to observed objects or events or properties; consequently, every proposition in T which contains a theoretical term may be replaced, without loss or gain in empirical meaning, by a set of propositions which contain only observational terms.

To illustrate how the introduction of theoretical terms is likely to lead to empirical discoveries and how by a purely logical procedure theoretical terms may be replaced by observational terms let us consider the following simple example.

We assume that a theory T contains *three observational terms* a, b, and c, denoting, for example, certain set-theoretical predicates, and *three theoretical terms* x, y, and z which will soon be specified more closely. We also assume that the formalism F of T contains as (primitive) "logical constants" *equality* $=$, assumed to be reflexive, symmetrical, and transitive, and (set-theoretical) *intersection* $\cap$, assumed to be associative, symmetrical, and idempotent. The latter is used to define, within F, the notion of *inclusion* $\subseteq$ by stipulating that $m \subseteq n$ if and only if $m \cap n = m$ (m, n, and p are used to denote any terms in T, whether observational or theoretical). It is further assumed that equal terms can be substituted for each other so that, for example, if $m = n$ and $n \subseteq p$, then $m \subseteq p$. It is then easy to prove within the formalism F without any further assumptions the following theorems:

THEOREM 1. If $m \subseteq n$ and $n \subseteq m$, then $m = n$.

THEOREM 2. $m \cap n \subseteq m$.

Let us finally assume that concerning the observational terms only the two following empirical laws are known (for the time being):

$$(E_1)\quad a \cap b \subseteq c; \qquad (E_2)\quad a \cap c \subseteq b. \tag{E}$$

The three theoretical terms by virtue of which, as we shall presently see, the theory will not only account for the two empirical laws (E) but will also obtain predictive power in the sense mentioned above will be contextually related to the observational terms by three theoretical laws:

$$(U_1)\quad a = y \cap z; \qquad (U_2)\quad b = x \cap z; \qquad (U_3)\quad c = x \cap y. \tag{U}$$

It should be clear, first of all, that the x, y, and z are uninterpreted theoretical terms, for, although contextually meaningful, none of them can be expressed solely by observational terms since the equations (U) are not solvable for x, y, or z. Second, our theory now accounts for the two known empirical laws (E_1) and (E_2). These laws can now be derived as logical consequences from the theoretical laws. Thus to derive (E_1) we note that in view of the fundamental assumptions and Theorem 2,

$$a \cap b = (y \cap z) \cap (x \cap z) = x \cap y \cap z \subseteq x \cap y = c$$

Third, our theory suggests the new empirical law

$$b \cap c \subseteq a, \tag{E_3}$$

which, like (E_1) and (E_2), can be derived from the theoretical laws (U). It is thus due to the theoretical terms, as we see, that the theory becomes an instrument for new discoveries.

Let us now see how by a refinement of the formalism F, that is, by a purely logico-mathematical extension of F without adding any empirical or theoretical laws, the theoretical terms can be transformed into observational terms. To this end we introduce the associative, symmetrical, and idempotent (set-theoretic) *union* $\cup$ which we assume to satisfy the distributive law $(m \cup n) \cap p = (m \cap p) \cup (n \cap p)$ and the inclusion law $m \subseteq m \cup n$. Two more theorems can now be established within the extended formalism:

THEOREM 3. $m \cup (m \cap n) = m$.

THEOREM 4. If $n \subseteq m$, then $m \cup n = m$.

By means of the newly introduced $\cup$ the theoretical terms can now be defined as follows:

$$(D_1)\quad x = b \cup c; \qquad (D_2)\quad y = a \cup c; \qquad (D_3)\quad z = a \cup b. \qquad (D)$$

By virtue of these definitions (D) the laws (U) can now be derived from the empirical laws (E_1), (E_2), and (E_3) and thus, having originally been theoretical laws, now become "theorems." To illustrate this for (U_1):

$$y \cap z = (a \cup c) \cap (a \cup b) = (a \cap (a \cup b)) \cup (c \cap (a \cup b))$$
$$= (a \cap a) \cup (a \cap b) \cup (c \cap a) \cup (c \cap b) = a \cup (b \cap c) = a$$

where use has been made of the postulated properties of the operations involved, of Theorem 3 and 4, and of (E_3). In the theory based on the thus extended formalism all theoretical terms and all theoretical laws have been reduced, as we see, to observational terms and observational laws, respectively. Our example, of course, in no way indicates whether such a procedure is always feasible. It seems to suggest, however, that the implementation of such a procedure stifles the creative power of the theory and renders it incapable to adapt itself to the discovery of new facts.

That our example typifies, though in an extremely simplified manner, the general situation, that is, that under very liberal conditions, satisfiable in virtually all known scientific theories, theoretical terms can indeed be systematically eliminated without loss of empirical content, was shown almost 50 years ago by Frank P. Ramsey[21] and, three decades later, in a different way, by William Craig.[22] Craig's eliminability theorem states, roughly speaking, that for every theory T which contains observational and theoretical terms, there exists a theory T' which yields every observational (empirical) theorem of T but contains in its extralogical vocabulary only observational terms. Craig's result, important as it is for theoretical logic, does not provide a practical solution of the interpretation problem since T' turns out to be of an unwieldy and unmanageable structure. Ramsey's elimination procedure, technically less complicated, also leads to a substitute theory T^*, which is free of all theoretical terms and preserves all observational consequences of T. It is, however, as Richard B. Braithwaite, the editor of Ramsey's posthumously published papers, pointed out, open to the objection that it sacrifices the heuristic fertility, creativity, and what is often called the "open texture" of the theory.

[21] F. P. Ramsey, *The Foundations of Mathematics and Other Logical Essays* (Routledge and Kegan Paul, London; Harcourt Brace, New York, 1931; Littlefield, Patterson, N.J., 1960), Ch. IX.

[22] W. Craig, "On axiomatizability within a system," *Journal of Symbolic Logic* **18**, 30–32 (1953); "Replacement of auxiliary expressions," *Philosophical Review* **65**, 38–55 (1956).

Braithwaite's contention, alluded to previously at the end of our example, "that theoretical terms can only be defined by means of observable properties on condition that the theory cannot be adapted properly to apply to new situations," can be illustrated, following Carl G. Hempel,[23] by the following simple example: "Suppose that the term 'temperature' is interpreted, at a certain stage of scientific research, only by reference to the readings of a mercury thermometer. If this observational criterion is taken as just a partial interpretation (namely as a sufficient but not necessary condition), then the possibility is left open of adding further partial interpretations, by reference to other thermometrical substances which are usable above the boiling point or below the freezing point of mercury." Clearly, this procedure makes it possible to extend considerably the range of applicability of physical laws involving the term "temperature." "If, however, the original criterion is given the status of a complete definiens, then the theory is not capable of such expansion; rather, the original definition has to be abandoned in favor of another one, which is incompatible with the first."

In our study of the interpretations of quantum mechanics we shall encounter numerous similar examples. In fact, the very notion of the state function ψ, undoubtedly the most important theoretical term in quantum mechanics, provides such an example. For Born's interpretation, which, as we have pointed out, was incorporated into von Neumann's axiomatization of quantum mechanics, is just such a partial interpretation. As it is most generally expressed, it describes the state function as a generator of probability distributions over the eigenvalues of self-adjoint operators, the probabilities being given by the absolute squared values of the expansion coefficients in the expansion of the function in terms of the basis consisting of the normalized eigenfunctions of the operator under discussion. It excludes neither additional partial interpretations nor even the possibility of associating with the "generator" itself an observational meaning, provided the observational consequences of Born's interpretation are preserved. We shall see later how in certain interpretations of quantum mechanics which intend to obtain precisely such an objective, the resulting inflexibility leads to incompatibilities with established facts.

It should, however, be kept in mind that even if all theoretical terms had been reduced to observational terms, the result would merely be a fully observationally interpreted formalism in the sense of F_R. Although this might well impose conceptional limitations on the interpretation of F_R in the more general sense, that is, in the choice of providing explanatory

[23]C. G. Hempel, "The theoretician's dilemma," *Minnesota Studies in the Philosophy of Science* **2**, 37–98 (1958); reprinted in C. G. Hempel, *Aspects of Scientific Explanation* (Free Press, New York; Collier-Macmillan, London, 1965), pp. 173–226.

principles based on acceptable ontological or metaphysical assumptions, it would not unambiguously determine the latter. It is due to this residual degree of freedom that philosophical considerations become relevant to the interpretations of quantum mechanics.[24]

APPENDIX

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[24] For an application of the considerations of Section 1.2, especially with respect to the distinction between F and F_R, to a physical theory other than quantum mechanics we refer the reader to the controversy between Henry Margenau and Richard A. Mould, on the one hand, and Herbert Dingle, on the other, concerning the interpretation of the special theory of relativity. Cf. H. Margenau and R. A. Mould, "Relativity: An epistemoloical appraisal," *Philosophy of Science* **24**, 297–307 (1957). H. Dingle, "Relativity and electromagnetism: An epistemological appraisal," *ibid.*, **27**, 233–253 (1960). For bibliographical references to Section 1.2 see Selected Bibliography II in the Appendix at the end of this chapter.

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Early SEMICLASSICAL Interpretations

Chapter Two

2.1. THE CONCEPTUAL SITUATION IN 1926/1927

The development of modern quantum mechanics had its beginning in the early summer of 1925 when Werner Heisenberg, recuperating on the island of Heligoland from a heavy attack of hay fever, conceived the idea of representing physical quantities by sets of time-dependent complex numbers.[1] As Max Born soon recognized, the "sets" in terms of which Heisenberg had solved the problem of the anharmonic oscillator were precisely those mathematical entities whose algebraic properties had been studied by mathematicians ever since Cayley published his memoir on the theory of matrices (1858). Within a few months Heisenberg's new approach[2] was elaborated by Born, Jordan, and Heisenberg himself into what has become known as matrix mechanics, the earliest consistent theory of quantum phenomena.

At the end of January 1926 Erwin Schrödinger, at that time professor at the University of Zürich, completed the first part of his historic paper "Quantization as an Eigenvalue Problem."[3] He showed that the usual, although enigmatic, rule for quantization can be replaced by the natural requirement for the finiteness and single-valuedness of a certain space function. Six months later Schrödinger published the fourth communication[4] of this paper, which contained the time-dependent wave equation and

[1]For historical details cf. Ref. 1-1 (pp. 199–209) and W. Heisenberg, "Erinnerungen an die Zeit der Entwicklung der Quantenmechanik," in *Theoretical Physics in the Twentieth Century: A Memorial Volume to Wolfgang Pauli* (Interscience, New York, 1960), pp. 40–47; *Der Teil und das Ganze* (Piper, Munich, 1969), pp. 87–90; *Physics and Beyond* (Harper and Row, New York, 1971), pp. 60–62.

[2]W. Heisenberg, "Über quantentheoretische Umdeutung kinematischer und mechanischer Beziehungen," *Zeitschrift für Physik* **33**, 879–893 (1925); reprinted in *Dokumente der Naturwissenschaft* (Battenberg, Stuttgart, 1962), Vol. 2, pp. 31–45, and in G. Ludwig, *Wellenmechanik* (Akademie Verlag, Berlin; Pergamon Press, Oxford; Vieweg & Sohn, Braunschweig, 1969), pp. 193–210; English translation "Quantumtheoretical reinterpretation of kinematic and mechanical relations," in B. L. van der Waerden, *Sources of Quantum Mechanics* (North-Holland, Amsterdam, 1967; Dover, New York, 1967), pp. 261–276) or "The interpretation of kinematic and mechanical relationships according to the quantum theory" in G. Ludwig, *Wave Mechanics* (Pergamon Press, Oxford, 1968), pp. 168–182.

[3]E. Schrödinger, "Quantisierung als Eigenwertproblem," *Annalen der Physik* **79**, 361–376 (1926); reprinted in E. Schrödinger, *Abhandlungen zur Wellenmechanik* (Barth, Leipzig, 1926, 1928), pp. 1–16 and in *Dokumente der Naturwissenschaft*, Vol 3 (1963), pp. 9–24, as well as in G. Ludwig, *Wellenmechanik*, pp. 108–122. English translation "Quantization as a problem of proper values" in E. Schrödinger, *Collected Papers on Wave Mechanics* (Blackie & Son, London, 1928), pp. 1–12; "Quantization as an Eigenvalueproblem," in G. Ludwig, *Wave Mechanics*, pp. 94–105; French translation in E. Schrödinger, *Mémoires sur la Mécanique Ondulatoire* (Alcan, Paris, 1933), pp. 1–19.

[4]Vierte Mitteilung, *Annalen der Physik* **81**, 109–139 (1926). For additional reference see preceding footnote.

time-dependent perturbation theory and various other applications of the new concepts and methods. By the end of February of that year, after having completed his second communication, Schrödinger[5] discovered, to his surprise and delight, that his own formalism and Heisenberg's matrix calculus are mathematically equivalent in spite of the obvious disparities in their basic assumptions, mathematical apparatus, and general tenor.

Schrödinger's contention of the equivalence between the matrix and wave mechanical formalisms gained further clarification when John von Neumann,[6] a few years later, showed that quantum mechanics can be formalized as a calculus of Hermitian operators in Hilbert space and that the theories of Heisenberg and Schrödinger are merely particular representations of this calculus. Heisenberg made use of the sequence space $\mathfrak{l}^2$, the set of all infinite sequences of complex numbers whose squared absolute values yield a finite sum, whereas Schrödinger made use of the space $\mathfrak{L}^2(-\infty, +\infty)$ of all complex-valued square-summable (Lebesgue) measurable functions; but since both spaces, $\mathfrak{l}^2$ and $\mathfrak{L}^2$, are infinite-dimensional realizations of the same abstract Hilbert space $\mathcal{H}$, and hence isomorphic (and isometric) to each other, there exists a one-to-one correspondence, or mapping, between the "wave functions" of $\mathfrak{L}^2$ and the "sequences" of complex numbers of $\mathfrak{l}^2$, between Hermitian differential operators and Hermitian matrices. Thus solving the eigenvalue problem of an operator in $\mathfrak{L}^2$ is equivalent to diagonalizing the corresponding matrix in $\mathfrak{l}^2$.

That a full comprehension of the situation as outlined was reached only after 1930 does not change the fact that in the summer of 1926 the mathematical formalism of quantum mechanics reached its essential completion. Its correctness, in all probability, seemed to have been assured by its spectacular successes in accounting for practically all known spectroscopic phenomena,[7] with the inclusion of the Stark and Zeeman effects, by its explanation, on the basis of Born's probability interpretation, of a multitude of scattering phenomena as well as the photoelectric effect. If we recall that by generalizing the work of Heisenberg and Schrödinger, Dirac soon afterward, in his theory of the electron,[8] accounted for the spin whose existence had been discovered in 1925, and that the combination of these

[5]E. Schrödinger, "Über das Verhältnis der Heisenberg-Born-Jordanschen Quantenmechanik zu der meinen," *Annalen der Physik* **79**, 734–756 (1926).

[6]See Ref. 1-2.

[7]For details see Ref. 1-1 (pp. 118–156).

[8]P. A. M. Dirac, "The quantum theory of the electron," *Proceedings of the Royal Society of London A* **117**, 610–624 (1928); **118**, 351–361 (1928). For historical details see also J. Mehra, "The golden age of theoretical physics: P. A. M. Dirac's scientific work from 1924 to 1933." in *Aspects of Quantum Theory*, A. Salam and E. P. Wigner, eds. (Cambridge University Press, London, New York, 1972), pp. 17–59.

ideas with Pauli's exclusion principle gave a convincing account of the periodic system of the elements, we will understand that the formalism established in 1926 was truly a major breakthrough in the development of modern physics.

But as we know from the preceding chapter a formalism is not yet a full-fledged theory. A theory should also contain a set R of rules of correspondence and an explanatory principle or model M. The importance of these various components of a physical theory was only gradually understood in the course of the development of theoretical physics. Thus in Aristotelian physics, which conceived physical reality from the viewpoint of somewhat naive realism, the application of such a scheme would have made little sense. With the mathematization of physical concepts in the times of Galileo and Newton the role of physical models began to gain an increasing importance. However, in Newtonian physics the supposedly immediate intuitability of its fundamental notions foreclosed a full recognition of the rules of correspondence. It was only with the advent of Maxwell's theory of the electromagnetic field which defied immediate picturability that physicists became fully aware of the epistemological issues involved in theory construction, a process which reached its culmination with the establishment of the highly sophisticated theories in microphysics.

The statement that in quantum mechanics the formalism preceded its interpretation of course does not mean that the formalism had been developed in a complete vacuum. What had happened prior to 1926 was rather a process comparable to the mathematical deciphering of a numerical cryptogram in which some of the symbols had been interpreted in accordance with the rules of correspondence of classical physics. A typical example was the Balmer series, which, with the help of the Rydberg constant, expressed a puzzling mathematical relation between the wave numbers of the hydrogen spectral lines. True, when Bohr "explained" the Balmer series in 1913 he proposed a model, but this model soon turned out to be inadequate. When 13 years later Schrödinger "solved" this cryptogram again by postulating what became known as the "Schrödinger equation" and certain boundary conditions to be imposed on its solutions, he established a formalism in terms of newly formed concepts such as the wave function. The code was broken, but only in terms of a new, though more compact, different code. That the importance of the rules of correspondence and their implications for the meaning of a physical theory were fully recognized even then is well illustrated by an episode reported by Schrödinger himself.[9] When strolling along Berlin's *Unter den Linden*

[9]E. Schrödinger, "Might perhaps energy be a merely statistical concept?," *Nuovo Cimento* **9**, 162–170 (1958); quotation on p. 170.

discussing his new ideas with Einstein, Schrödinger was told by Einstein: "Of course, every theory is true, provided you suitably associate its symbols with observed quantities."

The situation in 1927 was therefore essentially this: The new formalism of wave mechanics which Schrödinger had established contained in its higher-level propositions a number of uninterpreted terms, such as the wave function, but made it possible to deduce certain lower-level propositions that involved parameters which could be associated with empirically meaningful conceptions such as energy or wave lengths. What was called for, apart from possibly additional rules of correspondence for higher-level terms, was primarily some unifying explanatory principle or some model in the sense described above.

Both aims would have been reached at once by showing that the formalism F of Schrödinger's wave mechanics could be regarded as being part of, or at least isomorphic with, the formalism F^* of another theory T^* which was fully interpreted. This was precisely the method by which Schrödinger, soon after having completed the remarkable discovery of the formalism of wave mechanics, tried to provide it with a satisfactory interpretation.

2.2. SCHRÖDINGER'S ELECTROMAGNETIC INTERPRETATION

Up to the third communication of his historic paper the function ψ, referred to as the "mechanical field scalar" [*mechanischer Feldskalar*], had merely been defined in a purely formal way as satisfying the mysterious wave equation

$$\Delta\psi - \frac{8\pi^2 mV}{h^2}\psi - \frac{4\pi m}{ih}\frac{\partial\psi}{\partial t} = 0 \tag{1}$$

where $\psi = \psi(r,t) = \psi(x,y,z,t)$ for a one-particle system or $\psi = \psi(x_1,\ldots,z_n,t)$ for a system of n particles. To account for the fact that a system under discussion, for instance, the hydrogen atom, emits electromagnetic waves, whose frequency is equal to the difference of two proper values divided by h (Bohr's frequency condition), and to be able to derive consistently the intensities and polarizations of these waves, Schrödinger thought it necessary to ascribe to the function ψ an electromagnetic meaning.

At the end of his paper on the equivalence between matrix and wave mechanics Schrödinger had made such an assumption by postulating that the space density of the electrical charge is given by the real part of

$$\psi\frac{\partial\psi^*}{\partial t} \tag{2}$$

where ψ^* denotes the conjugate complex of ψ. By expanding ψ in discrete eigenfunctions, $\psi=\Sigma c_k u_k(r)e^{2\pi i E_k t/h}$, ($c_k$ are taken as real) he obtained for the space density (2) the expression

$$2\pi\sum_{(k,m)} c_k c_m \frac{E_k-E_m}{h}u_k(r)u_m(r)\sin\left[\frac{2\pi t}{h}(E_m-E_k)\right] \tag{3}$$

in which each combination (k,m) is taken only once. Using (3) for the calculation of the x-component of the dipole moment Schrödinger obtained a Fourier expansion in which only the term differences (differences of eigenvalues) appear as frequencies—which shows that the components of the dipole moment oscillate at just these frequencies known to be radiated—and in which the coefficient of each term is of the form $\int u_k(r)xu_m(r)dr$, the square of which is proportional to the intensity of the radiation of this component. Pointing out that "the intensity and polarization of the corresponding part of the emitted radiation have now been made completely understandable on the basis of classical electrodynamics," Schrödinger proposed in the beginning of March 1926 the first epistemic correlation between the newly established formalism F_q of quantum mechanics in terms of the ψ function and the fully operationally interpreted classical theory of electromagnetic radiation. Since ψ appears in the assumed expression for the charge density as given by the real part of $\psi(\partial\psi^*/\partial t)$ in a rather indirect and strange way, Schrödinger could not yet conceive it as an element of a descriptive physical picture, although he was firmly convinced that it represents something physically real. In fact, his suspicion of not yet having found the correct interpretation was greatly increased when he realized that the space density (3), when integrated over the whole of space, yields zero, due to the orthogonality of the proper functions $u_k(x)$, and not, as required, a time-independent finite value.

In the last section of the fourth communication ("§7 The physical significance of the field scalar") of his paper "Quantization as an Eigenvalue Problem" Schrödinger resolved this inconsistency by replacing the previous expression (2) for the charge density by the "weight function" [*Gewichtsfunktion*]

$$\psi\psi^* \tag{4}$$

multiplied by the total charge e. Using the wave equation (1) it was an easy matter to show that the time derivative of $\int \psi\psi^* dr$ (integrated over the whole of the configuration space) vanishes.

Moreover, since the resulting integrand $\psi^*\Delta\psi - \psi\Delta\psi^*$, apart from the coefficient $ieh/4\pi m$, is the divergence of the vector $\psi^*\nabla\psi - \psi\nabla\psi^*$, the "flow behavior" [*Strömungsverhältnis*] of the electricity is subject to an equation of continuity

$$\frac{\partial}{\partial t}(e\psi\psi^*) = -\nabla S \tag{5}$$

where the current density S is given by

$$S = \frac{ieh}{4\pi m}(\psi\nabla\psi^* - \psi^*\nabla\psi). \tag{6}$$

Since in the case of a one-electron system[10] where

$$\psi = \sum_k c_k u_k(r) e^{2\pi i(\nu_k t + \theta_k)} \tag{7}$$

the current density S is

$$S = \frac{eh}{2\pi m} \sum_{(k,m)} c_k c_m (u_k \nabla u_m - u_m \nabla u_k) \sin[2\pi(\nu_k - \nu_m)t + \theta_k - \theta_m] \tag{8}$$

Schrödinger concluded that, if only a single proper vibration or only proper vibrations belonging to the same proper value are excited, the current distribution is stationary, since the time-dependent factor in (8) vanishes. He could thus declare: "Since in the unperturbed normal state one of these two alternatives must occur in any case, one may speak in a certain sense of a *return to the electrostatic and magnetostatic model of the atom*. The absence of any radiative emission of a system in its normal state is thus given a surprisingly simple solution."

Clearly, the revised interpretation of ψ in accordance with (4) rather than (2) left the former explanation of the selection and polarization rules intact. Substitution of (7) in (4) yields for the charge density ρ

$$\rho = +e \sum_{(k,m)} c_k c_m u_k u_m e^{2\pi i[(\nu_k - \nu_m)t + \theta_k - \theta_m]} \tag{9}$$

[10] c_k, θ_k are real constants and $u_k(r)$ is assumed to be a real function, an assumption not affecting the generality of the conclusion.

and for the x-component of the dipole moment

$$M_x = -2 \sum_{(k,m)} c_k c_m a_{km}^{(x)} \cos[2\pi(\nu_k - \nu_m)t + \theta_k - \theta_m] + \text{const.} \tag{10}$$

where

$$a_{km}^{(x)} = e \int u_k(r) x u_m(r) dr. \tag{11}$$

Schrödinger was now in a position to check the correctness of his assumption (4) by calculating the a_{km} in those cases where the u_k are sufficiently well defined such as in the cases of the Zeeman and Stark effects. If $a_{km}^{(x)} = a_{km}^{(y)} = a_{km}^{(z)} = 0$, the spectral line was absent; if $a_{km}^{(x)} \neq 0$ but $a_{km}^{(y)} = a_{km}^{(z)} = 0$, the line was linearly polarized in the x-direction; and so on. Thus the relation between the squares of the a_{km} yielded correctly the intensity relations between the nonvanishing components in the Zeeman and Stark patterns of hydrogen.

Since the preceding conclusions remain valid also in the general case of n-particle systems and the electric charge densities, represented as products of waves, give the correct radiation amplitudes, Schrödinger interpreted quantum theory as a simple classical theory of waves. In his view, physical reality consists of waves and waves only. He denied categorically the existence of discrete energy levels and quantum jumps, on the grounds that in wave mechanics the discrete eigenvalues are eigenfrequencies of waves rather than energies, an idea to which he had alluded at the end of his first communication. In the paper "On Energy Exchange According to Wave Mechanics,"[11] which he published in 1927, he explained his view on this subject in great detail. Applying the time-dependent perturbation theory, the foundations of which he had laid in his fourth communication, to two weakly interacting systems with pairs of energy levels of the same energy difference, one system having the levels E_1 and E_2, the other E_1' and E_2', where $E_2 - E_1 = E_2' - E_1' > 0$, he argued as follows.

Let the wave equation for the unprimed system be

$$\Delta\psi - \left(\frac{8\pi^2 m}{h^2}\right)U\psi - \left(\frac{4\pi i}{h}\right)\frac{\partial\psi}{\partial t} = 0$$

with the eigenvalues E_1 and E_2 corresponding to the eigenfunctions ψ_1 and

[11] E. Schrödinger, "Energieaustausch nach der Wellenmechanik," *Annalen der Physik* **83**, 956–968 (1927); "The exchange of energy according to wave mechanics," *Collected Papers*, pp. 137–146; "Échanges d'énergie d'après la mécanique ondulatoire," *Mémoires*, pp. 216–270.

ψ_2, respectively, and let the wave equation for the primed system

$$\Delta'\psi' - \left(\frac{8\pi^2 m}{h^2}\right)U'\psi' - \left(\frac{4\pi i}{h}\right)\frac{\partial\psi'}{\partial t} = 0$$

have the eigenvalues E_1' and E_2', corresponding to the eigenfunctions ψ_1' and ψ_2', respectively; the wave equation for the combined system (with vanishing coupling)

$$(\Delta + \Delta')\Psi - \left(\frac{8\pi^2 m}{h^2}\right)(U + U')\Psi - \left(\frac{4\pi i}{h}\right)\frac{\partial\Psi}{\partial t} = 0$$

has consequently the degenerate eigenvalue $E = E_1 + E_2' = E_1' + E_2$, corresponding to the two eigenfunctions $\Psi_a = \psi_1\psi_2'$ and $\Psi_b = \psi_1'\psi_2$.

Introducing a weak perturbation and applying perturbation theory Schrödinger showed in the usual way that within the course of time the state of the combined system oscillates between Ψ_a and Ψ_b at a rate proportional to the coupling energy, and that in this resonance phenomenon the amplitude of ψ_1' increases at the expense of that of ψ_1 while at the same time the amplitude of ψ_2' increases at the expense of that of ψ_2. Thus without postulating discrete energy levels and quantum energy exchanges and without conceiving the eigenvalues as something other than *frequencies*, we have found, Schrödinger contended, a simple explanation of the fact that physical interaction occurs preeminently between *those* systems which, in terms of the older theory, provide for the "emplacement of identical energy elements."

The quantum postulate, in Schrödinger's view, is thus fully accounted for in terms of a resonance phenomenon, analogous to acoustical beats or to the behavior of "sympathetic pendulums" (two pendulums of equal, or almost equal, proper frequencies, connected by a weak spring). The interaction between two systems, in other words, is satisfactorily explained on the basis of purely wave-mechanical conceptions *as if* [*als ob*] the quantum postulate were valid—just as the frequencies of spontaneous emission are deduced from the time-dependent perturbation theory of wave mechanics *as if* there existed discrete energy levels and *as if* Bohr's frequency postulate were valid. The assumption of quantum jumps or energy levels, Schrödinger concluded, is therefore redundant: "to admit the quantum postulate in conjunction *with* the resonance phenomenon means to accept *two* explanations of the same process. This, however, is like offering two excuses: one is certainly false, usually both." In fact, Schrödinger claimed, in the correct description of this phenomenon one should not apply the concept of energy at all, but only that of frequency: Let one state be characterized by the combined frequency $\nu_1 + \nu_2'$ and the other by $\nu_1' + \nu_2$; the frequency condition $h\nu_2 - h\nu_1 = h\nu_1' - h\nu_2'$, which Bohr interpreted as meaning that the unprimed system performs a quantum jump from the lower level $E_1 = h\nu_1$ to the higher level $E_2 = h\nu_2$ while the primed system undergoes the transition from the higher level $E_2' = h\nu_2'$ to the lower $E_1' = h\nu_1'$, is merely the conservation theorem of frequencies of exchange:

$$\nu_1 + \nu_1' = \nu_2 + \nu_2'. \tag{12}$$

In a similar vein, Schrödinger maintained, the wave picture can be extended to account, merely in terms of frequencies and amplitudes, for all known quantum phenomena, including the Franck-Hertz experiment and even the Compton effect, the paradigm of particle physics. As he had shown in a preceding paper,[12] the Compton effect can be described as a Bragg type of reflection of one progressive wave by another; the interference pattern is formed by one wave and its reflected wave which constitutes some kind of moving Bragg crystal mirror for the other wave and vice versa.

How Schrödinger justified his rejection of the energy concept in microphysics can be seen from an interesting passage in a letter he wrote to Max Planck on May 31, 1926: "The concept 'energy' is something that we have derived from macroscopic experience and really *only* from macroscopic experience. I do not believe that it can be taken over into micro-mechanics just like that, so that one may speak of the energy of a single partial oscillation. The energetic property of the individual partial oscillation is *its frequency*."[13] Schrödinger never changed his view on this point. Three years before his death (January 4, 1961) he wrote a paper entitled "Might Perhaps Energy be a Merely Statistical Concept?"[14] in which he argued that energy, just like entropy, has merely a statistical meaning and that the product $h\nu$ has for microscopic systems *not* the (macroscopic) meaning of energy.

How a purely undulatory conception of physical reality can nevertheless account for the phenomenology of a particle physics was already intimated by Schrödinger in terms of wave packets in his second communication,[15] but it was fully worked out only in the early summer of 1926. In a paper written before the publication of the fourth communication, "On the

[12]E. Schrödinger, "Der Comptoneffekt," *Annalen der Physik* **82**, 257–265 (1927); *Abhandlungen*, pp. 170–177; *Collected Papers* pp. 124–129; *Mémoires*, pp. 197–205.

[13]Schrödinger, Planck, Einstein, Lorentz: *Letters on Wave Mechanics*, K. Przibram, ed. (Philosophical Library, New York, 1967), p. 10, *Briefe zur Wellenmechanik* (Springer, Wien, 1963), p. 10.

[14]Ref. 9.

[15]*Annalen der Physik* **79**, 489–527 (1926); Ref. 3.

Continuous Transition from Micro- to Macromechanics,"[16] Schrödinger illustrated his ideas on this issue by showing that the phenomenological behavior of the linear harmonic oscillator can be fully explained in terms of the undulatory eigenfunctions of the corresponding differential equation. Having found at the end (section 3: Applications) of his second communication that these normalized eigenfunctions are given by the expressions $(2^n n!)^{-1/2}\psi_n$, where

$$\psi_n = \exp(-\tfrac{1}{2}x^2)\, H_n(x) \exp(2\pi i\nu_n t) \tag{13}$$

and where $\nu_n = (n+\frac{1}{2})\nu_0$ and $H_n(x)$ is the Hermite polynomial of order n, Schrödinger now used them for the construction of the wave packet

$$\psi = \sum^{\infty} \left(\frac{A}{2}\right)^n \frac{\psi_n}{n!} \tag{14}$$

where A is a constant large compared with unity.[17] As shown by a simple calculation the real part of ψ turns out to be

$$\exp\left[\frac{A^2}{4} - \tfrac{1}{2}(x - A\cos 2\pi\nu_0 t)^2\right] \cos\left[\pi\nu_0 t + (A \sin 2\pi\nu_0 t)\left(x - \frac{A}{2}\cos 2\pi\nu_0 t\right)\right]. \tag{15}$$

The first factor in (15) represents a narrow hump having the shape of a Gaussian error curve and located at a given moment t in the neighborhood of

$$x = A\cos 2\pi\nu_0 t \tag{16}$$

in accordance with the classical motion of a particulate harmonic oscillator, while the second factor simply modulates this hump. Furthermore, Schrödinger pointed out, this wave group as a whole does not spread out in space in the course of time and since the width of the hump is of the order of unity and hence small compared with A, the wave packet stimulates the appearance of a pointlike particle. "There seems to be no doubt," Schrödinger concluded his paper, "that we can assume that similar wave packets

[16] E. Schrödinger, "Der stetige Übergang von der Mikro- zur Makromechanik," *Die Naturwissenschaften* **14**, 664–666 (1936); *Abhandlungen*, pp. 56–61; *Collected Papers*, pp. 41–44; *Mémories*, pp. 65–70.

[17] Since $x^n/n!$ as a function of n has for large x a single sharp maximum at $n = x$, the dominant terms are those for which $n \approx A$.

can be constructed which orbit along higher-quantum number Kepler ellipses and are the wave-mechanical picture [*undulationsmechanische Bild*] of the hydrogen atom."

This (undulatory) physical picture, based on the wave mechanical formalism, was the theme on which Schrödinger lectured before the German Physical Society in Berlin on July 16, 1926. The lecture was entitled "Foundations of an Atomism Based on the Theory of Waves" and was chaired by Walther Nernst, although it was on Max Planck's initiative that Eduard Grüneisen as president of the Berlin branch of the Society had extended this invitation to Schrödinger. Planck, it will be recalled, showed great interest and even enthusiasm in Schrödinger's work from its very inception. One week later Schrödinger addressed the Bavarian branch of the Society, with Robert Emden in the chair, on the same topic. It was on the basis of this physical picture that in 1947 Schrödinger could refer to Leucippus and Democritus, the originators of the classical conception of atoms, as the first quantum physicists, in an article[18] entitled "2400 Years of Quantum Mechanics" and that in 1950 he began his essay[19] "What is an Elementary Particle?" with the statement "Atomism in its latest form is called quantum mechanics."

The "natural" and "intuitable" interpretation of quantum mechanics as proposed by Schrödinger had, however, to face serious difficulties. In a letter of May 27, 1926, to Schrödinger, Hendrik Antoon Lorentz expressed with respect to one-particle systems his preference for the wave mechanical over the matrix mechanical approach because of the "greater intuitive clarity" of the former; notwithstanding he pointed out that a wave packet which when moving with the group velocity should represent a "particle" "can never stay together and remain confined to a small volume in the long run. The slightest dispersion in the medium will pull it apart in the direction of propagation, and even without that dispersion it will always spread more and more in the transverse direction. Because of this unavoidable blurring a wave packet does not seem to me to be very suitable for representing things to which we want to ascribe a rather permanent individual existence."

Schrödinger received this letter from Haarlem on May 31; as we know from his letter to Planck which he dispatched in Zürich on the same day, he had just finished his calculation concerning the particle-like behavior of the oscillating wave packet referred to above. He thus felt entitled to write in this letter to Planck "I believe that it is only a question of computational skill to accomplish the same thing for the electron in the hydrogen atom.

[18] E. Schrödinger, "2400 Jahre Quantenmechanik," *Annalen der Physik* **3**, 43–48 (1948).
[19] *Endeavour* **9**, 109–116 (1950).

The transition from microscopic characteristic oscillations to the macroscopic 'orbits' of classical mechanics will then be clearly visible, and valuable conclusions can be drawn about the phase relations of adjacent oscillations."

That Schrödinger's optimism was exaggerated became clear when, 10 months later, Heisenberg—in the paper[20] in which he published what became known as the "Heisenberg relations"—pointed out that if Schrödinger's assumption were correct, "the radiation emitted by an atom could be expanded into a Fourier series in which the frequencies of the overtones are integral multiples of a fundamental frequency. The frequencies of the atomic spectral lines, however, according to quantum mechanics, are never such integral multiples of a fundamental frequency—with the exception of the special case of the harmonic oscillator."[21]

A second, no less serious difficulty of the wave picture of physical reality concerns the dimensionality of the configurational space of ψ. It is this difficulty to which Lorentz referred when he expressed to Schrödinger in the above-mentioned letter his preference of wave mechanics, "so long as one only has to deal with the three coordinates x, y, z. If, however, there are more degrees of freedom" Lorentz wrote, "then I cannot interpret the waves and vibrations physically, and I must therefore decide in favor of matrix mechanics." Lorentz' proviso referred, of course, to the fact that for a system of n particles the wave ψ becomes a function of $3n$ position coordinates and requires for its representation a $3n$-dimensional space. In rebuttal of this objection one could, of course, point out that in the treatment of a macromechanical system the vibrations, which undoubtedly have real existence in the three-dimensional space, are most conveniently computed in terms of normal coordinates in the $3n$-dimensional space of Lagrangian mechanics.

Schrödinger was fully aware of this complication. "The difficulty," he wrote in his paper on the equivalence between his own and Heisenberg's approach, "encountered in the *poly*-electron problem, in which ψ is actually a function in *configuration* space and not in the real space, should not

[20]W. Heisenberg, "Über den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik," *Zeitschrift für Physik* **43**, 172–198 (1927); reprinted in *Dokumente der Naturwissenshaft*, Vol. 4 (1963), pp. 9–35.

[21]Another exception, not mentioned by Heisenberg, is the case of the potential $V = V_0(a/x - x/a)^2$ which leads to a spectrum identical with that of an oscillator with angular frequency $(8V_0/ma^2)^{1/2}$. Cf. I. I. Gol'dman, V. D. Krivchenkov, V. I. Kogan, and V. M. Galitskii, *Problems in Quantum Mechanics* (Infosearch, London, 1960), p. 8, (Addison-Wesley, Reading, Mass., 1961), p. 3. For recent work on the problem of coherence of wave packets cf. R. J. Glauber, "Classical behavior of systems of quantum oscillators," *Physics Letters* **21**, 650–652 (1966).

remain unmentioned." In a footnote to the same paper Schrödinger admitted, in particular, the conceptual inconsistency of using, for instance, in the wave mechanical treatment of the hydrogen atom, the formula for the electrostatic potential of classical particle physics,[22] adding that the possibility must be reckoned with that the carrying-over of the formula for the classical energy function loses its legitimacy "when *both* 'point charges' are actually extended states of vibrations which penetrate each other." Schrödinger's concern was fully vindicated by the later development of quantum electrodynamics.

The following three additional difficulties confronting Schrödinger's realistic interpretation of the wave function were not yet fully assessed at that time: (1) ψ is a *complex* function; (2) ψ undergoes a *discontinuous* change during a process of measurement; and (3) ψ *depends on the set of observables* chosen for its representation, for example, its representation in momentum space differs radically from its representation in position space.

The first of these difficulties was thought to be solvable since every complex function is equivalent to a pair of real functions. The necessity of complex phases for the explanation of the quantum mechanical interference phenomena became apparent only when Born proposed his probabilistic interpretation of the ψ function. The (later much debated) abrupt transition of ψ into a new configuration, the so-called reduction of the wave packet referred to in (2) above, came to the forefront of foundational studies only with the development of the quantum mechanical theory of measurement. Finally, the representation-dependency of ψ was a consequence of the Dirac-Jordan transformation theory which also was a development following Schrödinger's early results.

2.3. HYDRODYNAMIC INTERPRETATIONS

While Schrödinger's attempt to interpret quantum mechanics found its support primarily in the analogy to wave phenomena, the similarity of the wave equation and its implications with the equations of hydrodynamical flow formed the basis for another early attempt to account for quantum mechanical processes in terms of classical continuum physics. The earliest

[22]Cf. in this context Schrödinger's admission of the impossibility of solving the hydrogen-atom problem exclusively in terms of a field theory by using the potential obtained by the variation of the field Lagrangian, at the end of his paper "Der Energieimpulssatz der Materiewellen," *Annalen der Physik* **82**, 265–273 (1927); *Abhandlungen*, pp. 178–185; *Collected Papers*, pp. 130–136; *Mémoires*, pp. 206–215.

hydrodynamic interpretation was proposed by Erwin Madelung (Ph.D. Göttingen, 1905), professor of theoretical physics at the University of Frankfurt-am-Main from 1921, who is widely known for his theory of ionic crystals (Madelung constant), on which he had worked with Born while still in Göttingen, and for his textbook on mathematics for physicists.[23]

Starting[24] with Schrödinger's equation [the conjugate of (1)]

$$\Delta\psi - \frac{8\pi^2 m}{h^2} V\psi - \frac{4\pi i m}{h}\frac{\partial 4}{\partial t} = 0 \tag{17}$$

and writing

$$\psi = \alpha e^{i\beta} \tag{18}$$

where α and β are real, Madelung obtained for the purely imaginary part of (17)

$$\operatorname{div}(\alpha^2 \operatorname{grad}\varphi) + \frac{\partial \alpha^2}{\partial t} = 0 \tag{19}$$

where

$$\varphi = -\frac{h}{2\pi m}\beta. \tag{20}$$

Equation 19 has the structure of the hydrodynamical equation of continuity

$$\operatorname{div}(\sigma u) + \frac{\partial \sigma}{\partial t} = 0. \tag{21}$$

On the basis of this analogy Madelung interpreted α^2 as the density σ and φ as the velocity potential (velocity $u = \operatorname{grad}\varphi$) of a hydrodynamic flow process which is subject to the additional condition expressed by the real part of (17), that is, in terms of φ,

$$\frac{\partial\varphi}{\partial t} + \tfrac{1}{2}(\operatorname{grad}\varphi)^2 + \frac{V}{m} - \frac{\Delta\alpha}{\alpha}\frac{h^2}{8\pi^2 m^2} = 0 \tag{22}$$

[23]E. Madelung, *Die Mathematischen Hilfsmittel des Physikers* (Springer, Leipzig, 1922, 1925, 1935; Dover, New York, 1943).

[24]E. Madelung, "Quantentheorie in hydrodynamischer Form," *Zeitschrift für Physik* **40**, 322–326 (1926).

Now Euler's hydrodynamic equation

$$F-\frac{1}{\sigma}\operatorname{grad}p=\tfrac{1}{2}\operatorname{grad}u^2+\operatorname{curl}u\times u+\frac{\partial u}{\partial t} \tag{23}$$

where

$$F=-\operatorname{grad}U \tag{24}$$

is the force per unit mass, U the potential per unit mass, and p the pressure, can be written for irrotational motions (i.e., if a velocity potential exists) in the simpler form

$$\operatorname{grad}\left[\frac{\partial\varphi}{\partial t}+\tfrac{1}{2}(\operatorname{grad}\varphi)^2+U+\frac{p}{\sigma}\right]=0 \tag{25}$$

where

$$\nabla P=\frac{1}{\sigma}\nabla p.$$

If, therefore, the negative term in (22) is identified with the force-function of the inner forces of the continuum, $\int dp/\sigma$, the motion described by Schrödinger's equation appears as an irrotational hydrodynamical flow subjected to the action of conservative forces.

In the case of Schrödinger's time-independent equation

$$\Delta\psi_0+\frac{8\pi^2m}{h^2}(E-V)\psi_0=0 \tag{26}$$

and its solution

$$\psi=\psi_0\exp\frac{2\pi iEt}{h}, \tag{27}$$

clearly

$$\frac{\partial\alpha}{\partial t}=0 \qquad \text{and} \qquad \frac{\partial\varphi}{\partial t}=-E/m. \tag{28}$$

Hence (22) implies[25]

$$E=\frac{m}{2}(\operatorname{grad}\varphi)^2+V-\frac{\Delta\alpha}{\alpha}\frac{h^2}{8\pi^2m}. \tag{29}$$

[25]The last (negative) term in (29), which was to play an important role in Louis de Broglie's pilot wave theory and in David Bohm's hidden-variable theory, was later called the "quantum potential."

An eigenfunction of (26) thus represents, despite its time factor, a stationary flow pattern ($\partial u/\partial t = 0$) and, with $\sigma = \alpha^2 = \rho/m$ corresponding to the normalization $\int \sigma \, d\tau = 1$, the total energy

$$E = \int d\tau \left(\frac{\rho}{2} u^2 + \sigma V - \sqrt{\sigma} \, \Delta\sigma \, \frac{h^2}{8\pi^2 m} \right) \tag{30}$$

turns out to be the space integral of a kinetic and potential energy density just as in the classical mechanics of continuous media.

Since, conversely, Schrödinger's equation can be derived from the two hydrodynamic equations (19) and (22), these comprise, Madelung maintained, the whole of wave mechanics in an immediately intuitable form. "It thus appears," he declared, "that the current problem on quanta has found its solution in a hydrodynamics of continuously distributed electricity with a mass density proportional to the charge density." But, as he admitted himself, all difficulties have not yet been removed. Thus, for example, the last term in (30), representing the mutual interaction of the charge elements, should depend not only on the local density and its derivation but also on the total charge distribution. Moreover, he conceded, although the absence of emission in the ground state finds its natural explanation, no such explanation can be given for processes of radiative absorption.

Another complication, of a more conceptual nature, not mentioned by Madelung, concerns any attempt to reduce atomic physics to a hydrodynamic theory of a nonviscous irrotational fluid moving under conservative forces. Such a theory is based on the *idealized* notion of a continuous fluid and is never strictly applicable to a real fluid, which is a discrete assemblage of molecules. In other words, a theory which deliberately disregards atomicity is used to account for the behavior of atoms!

Shortly after the publication of Madelung's paper A. Isakson[26] of the Polytechnical Institute in Leningrad investigated on what additional assumptions the Hamilton-Jacobi equation of classical mechanics leads to the Schrödinger equation and, generalizing the treatment for relativistic motions, arrived at certain formulae which suggested a hydrodynamic interpretation. He refrained, however, from comparing his conclusions with those of Madelung and confined himself to the purely mathematical aspects involved.

[26] A. Isakson, "Zum Aufbau der Schrödinger Gleichung," *Zeitschrift für Physik* **44**, 893–899 (1927).

That Schrödinger's wave mechanics should be interpreted as a hydrodynamic theory of a viscous and compressible fluid was proposed in 1927 by Arthur Korn,[27] a graduate of the Technische Hochschule in Breslau and from 1914 professor at the Technische Hochschule in Berlin. Korn, who had made important contributions to the development of radio communication and picture transmission, had published in 1892 a hydrodynamic theory of gravitation and of electricity which he subsequently extended to optics and spectroscopy. Like Madelung, though nowhere referring to him, Korn postulated a velocity potential φ in terms of which the energy equation assumes the form

$$\frac{m}{2}(\operatorname{grad}\varphi)^2 = E - U. \tag{31}$$

Following Schrödinger, he defined ψ by the relation

$$\varphi = \frac{h}{2\pi m}\log\psi \tag{32}$$

so that

$$\Delta\varphi = \frac{h}{2\pi m}\frac{\Delta\psi}{\psi} - \frac{4\pi}{h}(E-U). \tag{33}$$

Now, continued Korn, if $h\Delta\varphi$ can be neglected when compared with $4\pi(E-U)$, it follows that

$$\Delta\psi = \frac{8\pi^2 m(E-U)}{h^2}\psi. \tag{34}$$

Alternatively, setting

$$\psi = \text{const.}\sin\left[2\pi\left(\frac{t}{\tau} - \frac{\varphi}{\lambda}\right)\right], \tag{35}$$

he obtained, under the same assumption,

$$\Delta\psi = \frac{2(E-U)}{m}\frac{\tau^2}{\lambda^2}\frac{\partial^2\psi}{\partial t^2}. \tag{36}$$

[27] A. Korn, "Schrödingers Wellenmechanik und meine mechanischen Theorien," *Zeitschrift für Physik* **44**, 745–753 (1927).

Although (34) and (36), according to Korn, represent Schrödinger's partial differential equations and hence suffice for the calculation of the quantum mechanical proper value problems, Korn pointed to the following difficulty: the differential equation (31) of classical mechanics is incompatible with the assumption

$$\Delta\varphi=0 \tag{37}$$

and hence should be regarded as only an approximation of the real equation which, more closely approximated, should read

$$\frac{m}{2}(\operatorname{grad}\varphi)^2=E-U+\epsilon\Delta\varphi, \tag{38}$$

where ϵ is a small quantity. Classical mechanics, continued Korn, corresponds to the case $\epsilon=0$ while the case $\epsilon\neq 0$ (but small) leads to the results of quantum mechanics.

Later Korn showed how his hydrodynamic theory of a compressible fluid, whose internal friction is characterized by a small constant, accounts classically for the presence of the last term in (38). Korn's theory suffers from the inconsistency that for the explanation of electromagnetic phenomena this fluid [*Zwischenmedium*] is supposed to be incompressible, whereas his proposed explanation of quantum phenomena requires its compressibility.

2.4. BORN'S ORIGINAL PROBABILISTIC INTERPRETATION

Meanwhile, almost simultaneously with the appearance of Schrödinger's fourth communication, a new interpretation of the ψ-function was published which had far-reaching consequences for modern physics not only from the purely technical point of view but also with respect to the philosophical significance of its contents. Only four days after Schrödinger's concluding contribution had been sent to the editor of the *Annalen der Physik* the publishers of the *Zeitschrift für Physik* received a paper, less than five pages long, titled "On the Quantum Mechanics of Collision Processes,"[28] in which Max Born proposed, for the first time, a probabilistic interpretation of the wave function implying thereby that microphysics must be considered a probabilistic theory. Although Born—due to an

[28]M. Born, "Zur Quantenmechanik der Stossvorgänge," *Zeitschrift für Physik* **37**, 863–867 (1926); reprinted in M. Born, *Ausgewählte Abhandlungen* (Vandenhoek & Ruprecht, Göttingen, 1963), Vol. 2, pp. 228–232, and in *Dokumente der Naturwissenschaft*, Vol. 1 (1962), pp. 48–52.

extensive collaboration with his assistants, Heisenberg and Jordan—was personally deeply involved in the rise of matrix mechanics, he was greatly impressed by Schrödinger's new approach, so much so in fact that for the study of collision phenomena he preferred the *formalism* of wave mechanics over that of matrix mechanics, stating that "among the various forms of the theory only Schrödinger's formalism proved itself appropriate for this purpose; for this reason I am inclined to regard it as the most profound formulation of the quantum laws."[29] But Schrödinger's undulatory *interpretation* seemed to him untenable.

When Born was awarded the Nobel Prize "for his fundamental work in quantum mechanics and especially for his statistical interpretation of the wave function," as the official declaration of the Royal Swedish Academy of November 3, 1954, stated, he explained the motives of his opposition to Schrödinger's interpretation as follows: "On this point I could not follow him. This was connected with the fact that my Institute and that of James Franck were housed in the same building of the Göttingen University. Every experiment by Franck and his assistants on electron collisions (of the first and second kind) appeared to me as a new proof of the corpuscular nature of the electron."[30]

Born discussed the quantum mechanical treatment of collision processes, of which his short article gave only a preliminary report, in greater detail in two subsequent papers[31] and developed systematically what has since become known as the "Born approximation." His treatment of the scattering of electrons by a center of force with a spherically symmetric potential V was essentially an application of the perturbation theory to the scattering of plane waves, the initial and final wave functions being both approximately plane waves when far from the scattering center.

To the system of an electron of energy $E = h^2/2m\lambda^2$ coming from the $+Z$-direction and approaching an atom whose unperturbed eigenfunctions are $\psi_n^0(q)$, Born ascribed the combined eigenfunction $\psi_{nE}^0(q,z) = \psi_n^0(q)\sin(2\pi z/\lambda)$. Taking $V(x,y,z,q)$ as the potential energy of interaction between the electron and the atom, Born obtained from the theory of

[29] Ref. 28 (p. 864).

[30] M. Born, "Bemerkungen zur statistischen Deutung der Quantenmechanik," in *Werner Heisenberg und die Physik unserer Zeit* (Vieweg, Braunschweig, 1961), p. 103. Cf. also M. Born, *Experiment and Theory in Physics* (Cambridge University Press, London, 1943), p. 23.

[31] M. Born, "Quantemechanik der Stossvorgänge," *Zeitschrift für Physik* **38**, 803–827 (1926), "Zur Wellenmechanik der Stossvorgänge," *Göttinger Nachrichten* **1926**, 146–160; reprinted in *Ausgewählte Abhandlungen*, Vol. 2, pp. 233–257, 284–298, *Dokumente der Naturwissenschaft*, Vol. 1, pp. 53–77, 78–92; G. Ludwig, *Wellenmechanik*, pp. 237–259, "Quantum mechanics of collision processes," in G. Ludwig, *Wave Mechanics*, pp. 206–225.

perturbations for the scattered wave at great distance from the scattering center the expression

$$\psi_{nE}^{(1)}(x,y,z,q)=\sum_m \int\int d\omega \psi_{nm}^{(E)}(\alpha,\beta,\gamma)\sin K_{nm}^{(E)}(\alpha x+\beta y+\gamma z+\delta)\psi_n^0(q) \tag{39}$$

where $d\omega$ is an element of the solid angle in the direction of the unit vector whose components are α,β, and γ, and where $\psi_{nm}^{(E)}(\alpha,\beta,\gamma)$ is a wave function which determines what was subsequently called the differential cross section for the direction (α,β,γ).

If the preceding formula, said Born, admits a corpuscular interpretation, there is only one possibility: $\psi_{nm}^{(E)}$, or rather $|\psi_{nm}^{(E)}|^2$, as Born added in a footnote to the preliminary report, measures the probability that the electron which approached the scattering center in the direction of the Z-axis is found scattered in the direction defined by α,β,γ. In view of the crucial importance of Born's probabilistic conception of the ψ-function for all subsequent interpretations of the theory let us rephrase in an elementary way the preceding analysis in modern notation.

Assuming that the wave function of the scattered electron is periodic in time, Born could confine himself to the time-independent Schrödinger equation $(-\hbar^2/2m)\Delta\psi+V(r)\psi=E\psi$ for which he found a solution that contained the incoming plane wave $\psi_0=\exp\,(ikz-i\omega t)$ and the outgoing scattered wave $\psi_s=f(k,\theta)[\exp(ikr-i\omega t)/r]$. Interpreting $|f(k,\theta)|^2 d\Omega$ as the probability that the electron is scattered into the element of solid angle $d\Omega$, he realized that this conclusion is but a special case of the more general assumption that $\psi^*\psi\,d\tau$ measures the probability of the particle to be found in the spatial element $d\tau$, for this assumption proves valid not only for $\psi=\psi_s$ but also for $\psi=\psi_0$, provided the incoming wave function has been appropriately normalized. It follows, said Born, that wave mechanics does not give an answer to the question: What, precisely, is the state after the collision? It answers only the question: What is the probability of a definite state after the collision? In the first of his more detailed papers on collisions he described the situation as follows: "The motion of particles conforms to the laws of probability, but the probability itself is propagated in accordance with the law of causality."[32]

Born's probabilistic interpretation, apart from being prompted by the corpuscular aspects in Franck's collision experiments, was also influenced,

[32]"Die Bewegung der Partikel folgt Wahrscheinlichkeitsgesetzen, die Wahrscheinlichkeit selbst aber breitet sich im Einklang mit dem Kausalgesetz aus." Ref. 31 (p. 804).

as Born himself admitted,[33] by Einstein's conception of the relation between the field of electromagnetic waves and the light quanta. As Born repeatedly pointed out, Einstein regarded the wave field as a kind of "phantom field" [*Gespensterfeld*] whose waves guide the particle-like photons on their path in the sense that the squared wave amplitudes (intensities) determine the probability of the presence of photons or, in a statistically equivalent sense, their density. In fact, if we recall that in accordance with de Broglie's principal thesis the wave function for an ordinary plane light wave of frequency $\nu = E/h$ and wave length $\lambda = h/p$, that is,

$$u(x,t) = \exp\left[2\pi i\nu\left(t - \frac{x}{c}\right)\right] = \exp\left[\frac{2\pi i}{h}(Et - px)\right], \tag{40}$$

represents also the de Broglie wave function for a particle of energy E and momentum p, being the eigenfunction of the Schrödinger wave equation

$$\frac{d^2\psi}{dx^2} + \frac{8\pi^2 m}{h^2}E\psi = 0 \tag{41}$$

where $E = p^2/2m$, we understand that Born's probabilistic interpretation was, in the last analysis, but a plausible carrying-over, or rather extension and generalization, of Einstein's conception of the phantom field to particles other than photons.

In the just mentioned lecture delivered in 1955, three days before Einstein's death, Born declared explicitly that it was fundamentally Einstein's idea which he (Born) applied in 1926 to the interpretation of Schrödinger's wave function " and which today, appropriately generalized, is made use of everywhere." Born's probability interpretation of quantum mechanics thus owes its existence to Einstein, who later became one of its most eloquent opponents.

Early in October 1926 Born completed a paper[34] on the adiabatic principle in quantum mechanics in which he generalized his probabilistic interpretation for arbitrary quantum transitions. Accepting Schrödinger's formalism, but not his interpretation of it as a "causal continuum theory in the classical sense," Born pointed out that the wave mechanical formulation, rather than necessarily implying a continuum interpretation, may well

[33]Interview with M. Born, October 18, 1962 (Archive for the History of Quantum Physics). Cf. also M. Born, "Albert Einstein und das Lichtquantum," *Die Naturwissenschaften* **11**, 425–431 (1955).

[34]M. Born, "Das Adiabatenprinzip in der Quantenmechanik," *Zeitschrift für Physik* **40**, 167–192 (1926); *Ausgewählte Abhandlungen*, Vol. 2, pp. 258–283; *Dokumente der Naturwissenschaft*, Vol. 1, pp. 93–118.

be "amalgamated" [*Verschmelzt*] with the description of atomic processes in terms of discrete quantum transitions (quantum jumps). Starting, this time, with Schrödinger's time-dependent equation (1) Born considered the solution

$$\psi(x,t)=\sum_n c_n\psi_n(x)\exp(-i\omega_n t) \tag{42}$$

where the $\psi_n(x)$ are the eigenfunctions of the corresponding time-independent Schrödinger equation for the energies $E_n=\hbar\omega_n$, and raised the question of the physical meaning of such a $\psi(x,t)$. He rejected Schrödinger's answer that ψ in (42) denotes the state of a single atom undergoing simultaneously many proper vibrations on the grounds that in an ionization process, that is, a transition from a state of the discrete spectrum to one of the continuous spectrum, the singleness of the latter state or "orbit" is conspicuously revealed by its visible trace in the Wilson chamber.

Born was led therefore to the conclusion that, in accordance with Bohr's model of the atom, the atom at a given time occupies only *one* stationary state. He thus interpreted $|c_n|^2$ in (42) as the probability that the atom is found in the state characterized by E_n or briefly n. Moreover, if the system, originally in state $\psi_n\exp(-i\omega_n t)$, is acted on by an external perturbation [*äussere Einwirkung*] which lasts from $t=0$ to $t=T$, then for $t>T$ the system is described by the wave function

$$\psi_n(x,t)=\sum_m b_{nm}\psi_m\exp(-i\omega_m t) \tag{43}$$

and $|b_{nm}|^2$ is the probability for the transition from state n to state m. "The individual process, the 'quantum jump,'" continued Born, "is therefore not causally determined in contrast to the a-priori probability of its occurrence; this probability is ascertainable by the integration of Schrödinger's differential equation which is completely analogous to the corresponding equation in classical mechanics, putting into relation two stationary time-intervals separated by a finite temporal interval. The jump thus passes over a considerable abyss [*der Sprung geht also über einen beträchtlichen Abgrund*]; whatever occurs during the transition can hardly be described within the conceptual framework of Bohr's theory, nay, probably in no language which lends itself to visualizability." Finally—and in our present context this is of only secondary importance—Born showed that for infinitely slow perturbations the transition probabilities vanish and he thus proved the validity of the adiabatic theorem for quantum mechanics.

Summarizing Born's *original* probabilistic interpretation of the ψ-function we may say that $|\psi|^2 d\tau$ measures the probability density of finding the particle within the elementary volume $d\tau$, *the particle being conceived in* *the classical sense as a point mass possessing at each instant both a definite position and a definite momentum.* Contrary to Schrödinger's view, ψ does not represent the physical system nor any of its physical attributes but only our *knowledge* concerning the latter.

Born's interpretation could easily meet the five difficulties encountered by the Schrödinger interpretation. The spreading out of the ψ-function and its multidimensionality formed no serious obstacle since ψ itself was not regarded as something physically real; the complex amplitude is dealt with by associating meaning only to its squared absolute value which is always a nonnegative real number; the discontinuous change of ψ (or "reduction of the wave packet") in the case of a measurement signifies, not as in Schrödinger's theory a sudden collapse of a widely spread-out wave, but merely a change in our knowledge of the physical situation which occurs the moment we become aware of the result of the measurement; and finally, the dependence of the ψ-function upon the choice of the variables used for its formation or, in short, its representation-dependency has to be expected since the knowledge about position gained from the "position representation" is naturally different from the knowledge about momentum gained from the "momentum representation" (the ψ-function in momentum space).

The earliest successes scored by Born's interpretation occurred in the field where it originated and where its application was most natural: in the problems of atomic scattering. Still in the fall of 1926 Wentzel,[35] applying Born's approximation method to the scattering of electrically charged particles by a charged scattering center, derived Rutherford's experimentally well-confirmed scattering formula within the framework of wave mechanics. Born's interpretation served Faxén and Holtsmark,[36] Mott,[37] and Bethe[38] in their investigations of the passage of slow and fast particles through matter, in the course of which the mysterious Ramsauer-Townsend effect was fully explained on the basis of wave mechanics.

In spite of all these successes Born's original probabilistic interpretation proved a dismal failure if applied to the explanation of diffraction phenomena such as the diffraction of electrons. In the double-slit experi-

[35]G. Wentzel, "Zwei Bemerkungen über die Streuung korpuskularer Strahlen als Beugungserscheinung," *Zeitschrift für Physik* **40**, 590–593 (1926).

[36]H. Faxén and J. Holtsmark, "Beitrag zur Theorie des Durchgangs langsamer Elektronen durch Gase," *Zeitschrift für Physik* **45**, 307–324 (1927).

[37]N. F. Mott, "The solution of the wave equation for the scattering of particles by a Coulombian centre of field," *Proceedings of the Royal Society of London A* **118**, 542–549 (1928).

[38]H. Bethe, "Zur Theorie des Durchgangs schneller Korpuskularstrahlen durch Materie," *Annalen der Physik* **5**, 325–400 (1930).

ment, for example, Born's original interpretation implied that the blackening on the recording screen behind the double-slit, with both slits open, should be the superposition of the two individual blackenings obtained with only one slip opened in turn. The very experimental fact that there are regions in the diffraction pattern not blackened at all with both slits open, whereas the same regions exhibit strong blackening if only one slit is open, disproves Born's original version of his probabilistic interpretation. Since this double-slit experiment can be carried out at such reduced radiation intensities that only one particle (electron, photon, etc.) passes the apparatus at a time, it becomes clear, on mathematical analysis, that the ψ-wave associated with each particle interferes with itself and the *mathematical* interference is manifested by the *physical* distribution of the particles on the screen. The ψ-function must therefore be something *physically real* and not merely a representation of our knowledge, if it refers to particles in the classical sense. But then the five above-mentioned difficulties defy all attempts of solution.

In fact, Heisenberg, who soon accepted Born's ideas, thought it necessary, in view of the fact that these ψ-waves evolve in time and propagate in space in accordance with Schrödinger's equation, not to regard them as merely a mathematical fiction but to ascribe to them some kind of physical reality. As Heisenberg wrote later, these probability waves were conceived by him as "a quantitative formulation of the concept of δύναμις [possibility] or, in the later Latin version, *potentia*, in Aristotle's philosophy. The concept that events are not determined in a peremptory manner, but that the possibility or 'tendency' for an event to take place has a kind of reality —a certain intermediate layer of reality, halfway between the massive reality of matter and the intellectual reality of the idea or the image—this concept plays a decisive role in Aristotle's philosophy. In modern quantum theory this concept takes on a new form; it is formulated quantitatively as probability and subject to mathematically expressible laws of nature.[39]

2.5. DE BROGLIE'S DOUBLE-SOLUTION INTERPRETATION

At about the time Born proposed his probabilistic interpretation, Louis de Broglie developed what he later called "the theory of the double solution." His first paper[40] on this subject, written in the summer of 1926, tried to reconcile Einstein's light quanta with the optical phenomena of in-

[39]W. Heisenberg, "Planck's discovery and the philosophical problems of atomic physics," in *On Modern Physics* (C. N. Polter, New York; Orion Press, London, 1961), pp. 9–10.

[40]L. de Broglie, "Sur la possibilité de relier les phénomènes d'interference et de diffraction à

terference and diffraction by regarding these quanta (or photons as they were subsequently called) as singularities of a field of waves. The solution of the wave equation

$$\Delta u = \frac{1}{c^2}\frac{\partial^2 u}{\partial t^2} \tag{44}$$

in classical optics, de Broglie argued, is given by a function of the form

$$u = a(x,y,z)\exp\{i\omega[t - \varphi(x,y,z)]\}, \tag{45}$$

which satisfies the boundary conditions imposed by the presence of screens, apertures, or other obstacles encountered by the waves; in the "new optics of light quanta," on the other hand, its solution should be given by a function

$$u = f(x,y,z,t)\exp\{i\omega[t - \varphi(x,y,z)]\} \tag{46}$$

where the phase φ is the same as before but $f(x,y,z,t)$ has "mobile singularities" along the curves n normal to the phase fronts $\varphi = \text{constant}$.

"These singularities," de Broglie declared, "constitute the quanta of radiative energy." Substituting (45) and (46) in (44) and separating the imaginary part of the resulting relation he obtained the equations

$$\frac{2}{a}\frac{da}{dn} = -\Delta\varphi \Big/ \frac{\partial\varphi}{\partial n} \tag{47}$$

$$\frac{\partial\varphi}{\partial n}\frac{\partial f}{\partial n} + \tfrac{1}{2}f\Delta\varphi = -\frac{1}{c^2}\frac{\partial f}{\partial t}. \tag{48}$$

Reasoning that the quotient $f/(\partial f/\partial n)$ vanishes at the position M of the particle and knowing that the velocity of the quantum of light, when passing through M at time t, is given by

$$v = -\left(\frac{\partial f/\partial t}{\partial f/\partial n}\right)_{M,t}, \tag{49}$$

for

$$df = \frac{\partial f}{\partial n}dn + \frac{\partial f}{\partial t}dt = 0,$$

la théorie des quanta de lumière," *Comptes Rendus* **183**, 447–448 (1926); reprinted in L. de Broglie, *La Physique Quantique Restera-t-elle Indeterministique*? (Gauthier-Villars, Paris, 1953), pp. 25–27.

de Broglie concluded from (48) that

$$v = c^2\left(\frac{\partial f}{\partial n}\right)_M \tag{50}$$

so that φ—just as Madelung's φ—plays the role of a velocity potential.

The curves n or "lines of flow" form tubes in which the particles move forward. If ρ denotes the density of the latter and σ the variable cross section of these tubes, then along a given tube

$$\rho v \sigma = \text{const.} \tag{51}$$

or, taking logarithmic derivatives,

$$\frac{1}{\rho}\frac{d\rho}{dn} + \frac{1}{v}\frac{dv}{dn} + \frac{1}{\sigma}\frac{d\sigma}{dn} = 0 \tag{52}$$

where the last term $(1/\sigma)d\sigma/dn$ is twice the mean curvature of the surface[41] $\varphi = \text{constant}$, namely $(\Delta\varphi - \partial^2\varphi/\partial n^2)/(\partial\varphi/\partial n)$ so that, in view of (50),

$$\frac{1}{\rho}\frac{d\rho}{dn} = -\frac{\Delta\varphi}{\partial\varphi/\partial n} \tag{53}$$

or by virtue of (47)

$$\rho = \text{const.}\, a^2. \tag{54}$$

Finally, since the squared amplitude of the solution of the classical wave equation measures the intensity of the radiation, the last equation, according to which the density of the light quanta is proportional to the intensity, offers a satisfactory explanation of the phenomena of interference and diffraction on the basis of the corpuscular conception of light.

In a second paper[42] de Broglie carried over these considerations for the interpretation of the Schrödinger wave function and the motion of particles. "In micromechanics as in optics continuous solutions of the wave equations provide merely statistical information; an exact microscopic description undoubtedly requires the use of singularity solutions representing the discrete structure of matter and radiation," he declared.

In the spring of 1927 de Broglie brought these ideas to maturity and presented them in the form of what he called the "theory of double

[41]See, e.g., H. Poincaré, *Cours de Physique Mathématique—Capillarité* (G. Carré, Paris, 1895), p. 51.

[42]L. de Broglie, "La structure de la matière et du rayonnement et la mécanique ondulatoire," *Comptes Rendus* **184**, 273–274 (1927); reprinted in Ref. 40 (1953, pp. 27–29).

solution."[43] According to this theory the wave equation admits *two* different kinds of solution: a continuous ψ-function with statistical significance and a singularity solution whose singularity constitutes the physical particle under discussion.

To follow de Broglie's line of reasoning, let us consider the Klein-Gordon equation

$$\Box\psi = \frac{m^2c^2}{\hbar^2}\psi, \tag{55}$$

which at the time of its appearance in 1926 was supposed to describe electrons. Although, as we now know, it applies only to zero spin particles, let us, for simplicity, use (55) as the wave equation. Its plane monochromatic wave solution, as can be easily checked, is

$$\psi = a\exp\left(\frac{i\varphi}{\hbar}\right) \tag{56}$$

where a is a constant and $\varphi = Et - pr$. If we now assume that (55) has in addition the singularity wave solution

$$u = f(x,y,z,t)\exp\left(\frac{i\varphi}{\hbar}\right) \tag{57}$$

with the same phase φ as in (56), $\Box f = 0$ must be satisfied.

In view of the Lorentz invariance of the wave equation we may transform to a reference system where f does not depend on t so that f has to satisfy the condition $\Delta f = 0$. In this proper reference system, at the origin of which the particle is found, the spherically symmetric solution is obviously

$$f(x_0,y_0,z_0) = \frac{C}{r_0} \tag{58}$$

where $r_0 = (x_0^2 + y_0^2 + z_0^2)^{1/2}$ is the distance of the field point from the origin and

$$u(x_0,y_0,z_0,t_0) = \frac{C}{r_0}\exp\left(\frac{imc^2t_0}{\hbar}\right). \tag{59}$$

Transforming again to a reference system in which the particle moves

[43]L. de Broglie, "La mécanique ondulatoire et la structure atomique de la matiére et du rayonnement," *Journal de Physique et du Radium* **8**, 225–241 (1927); reprinted in Ref. 40 (1953, pp. 29–54).

along the z-axis with velocity v, we obtain

$$u(x,y,z,t)=C\left[x^2+y^2+\frac{(z-vt)^2}{1-\beta^2}\right]^{-1/2}\exp\left[\frac{i}{\hbar}(Et-pz)\right]. \tag{60}$$

The particle is thus described by the mobile singularity of u.

One may now imagine a current of many such particles, all moving with the same velocity v parallel to the z-axis and described by the Schrödinger solution $\psi=a\exp(i\varphi/\hbar)$; for such a current the space density ρ may be set equal to Ka^2 where K is a constant. If, however, we consider only a single particle and do not know on which trajectory parallel to the z-axis it moves or at what time it passes through a given z, we may express the probability density of finding the particle at a given elementary volume by

$$\rho=a^2=|\psi|^2. \tag{61}$$

Thus whereas the continuous solution ψ, in accordance with Born's interpretation, measures the probability, the singular solution u describes the particle itself.

De Broglie now showed that even if it is assumed that the wave equation, satisfied by u, is nonlinear inside a small region but obeys the linear equation on a small sphere S surrounding this region, the velocity v of that singularity region is given by the negative gradient of the phase $\varphi=\varphi(x,y,z,t)$ of the singularity solution $u=f\exp(i\varphi/\hbar)$, divided by the mass m. Indeed, if we substitute u into (55) we obtain for the imaginary part

$$\frac{1}{c^2}\frac{\partial\varphi}{\partial t}\frac{\partial f}{\partial t}-\nabla\varphi\,\nabla f=\tfrac{1}{2}f\Box\varphi. \tag{62}$$

Since the energy is constant, $\varphi(x,y,z,t)=Et-\varphi_1(x,y,t)$. Near a point M on S the direction normal to $f=$const. is grad$f$, and gradφ_1 is the direction n of the motion of the singularity region and $f/(\partial f/\partial s)=0$. From (62) or

$$\frac{1}{c^2}E\frac{\partial f}{\partial t}+\frac{\partial\varphi_1}{\partial n}\frac{\partial f}{\partial s}\cos(n,s)=\tfrac{1}{2}f\Box\varphi$$

we obtain after division by $\partial f/\partial s$ for the velocity v_s of $f=$const. (in the s

direction)

$$v_s = \frac{c^2}{E} |\mathrm{grad}\, \varphi_1| \cos(n,s) \tag{63}$$

where

$$v_s = -\left(\frac{\partial f/\partial t}{\partial f/\partial s}\right)_{M,t}$$

Finally we see that [since $v_s = v \cos(n,s)$] the velocity v of the singularity region is given by the formula

$$v = -\mathrm{grad}\, \frac{\varphi}{m}. \tag{64}$$

Formula 64, which may be regarded as an extrapolation of the well-known formula $p = -\mathrm{grad}\ S$ of the classical Hamilton-Jacobi theory beyond the limits of classical mechanics, was called by de Broglie the "guidance formula." It permits one to deduce the trajectory of the particle from the sole knowledge of the ψ-function.

The preceding treatment, as de Broglie showed, can easily be generalized for particles which move in the field of a static force derivable from a potential U. In this case the guidance formula turns out to be

$$v = -\frac{c^2}{E-U}\, \mathrm{grad}\, \varphi. \tag{65}$$

De Broglie thus proposed a version of quantum mechanics in which the corpuscle, identified as an energy concentration in the singularity region of u, preserves essentially its classical nature. But, unlike the classicle particle, it is guided by an extended wave ψ and thus is subject to diffraction effects which may be produced by obstacles at a great distance from it. The wave-particle duality, in brief, has been reduced by de Broglie to a wave-particle synthesis: not wave *or* particle but wave *and* particle constitute physical reality!

2.6. LATER SEMICLASSICAL INTERPRETATIONS

It should not be thought that attempts to interpret quantum mechanics in terms of hydrodynamical models were confined to the early stages of that theory. To give an example of more recent work along these lines, let us refer to Oscar Buneman (Bunemann) who in a series of unpublished papers and in his lectures at Cambridge University in the early 1950s proposed hydrodynamic models for the electron clouds of atoms and for electrons themselves. His more recent theory[44] of continuum electrodynamics, which he reported at the Forty-First National Congress of Physics in Pisa, June 1955, and his conception of "plasma models" were a straightforward development of his hydrodynamic interpretation. He admitted,[45] however, that a pure electromagnetic electron model is as unacceptable as the pure electrostatic model which had been rejected at the turn of the century, since charged (or current-carrying) matter will not hold together of its own accord without gravity or other external forces.

Madelung's hydrodynamical model, based originally on the notion of a fluid assumed to undergo only potential flow, was extended in the early 1950s by Takehiko Takabayasi[46] in Japan and by Mario Schönberg[47] in Brazil, who showed that the quantum potential $-(h^2/8\pi^2 m)\Delta\alpha/\alpha$ may be conceived as originating from an internal stress in the fluid, even though—in contrast to classical hydrodynamics—this stress depends on derivatives of the fluid density. Takabayasi's reference to complicated fluctuations about the motion of constant velocity by which he explained how due to the quantum potential the trajectories deviate from the purely classical ones, and Schönberg's recourse to a turbulent medium for essentially the same purpose, introduced notions which linked their hydrodynamic models with certain stochastic interpretations that will be discussed in another context.

The same applies to the hydrodynamic interpretation proposed by David Bohm and Jean-Pierre Vigier.[48] In this model particle-like inhomogeneities of a conserved fluid of density $\alpha^2 = |\psi|^2$ and local current velocity grad S/m (S corresponds to Madelung's β multiplied by $h/2\pi$) are constantly subjected to random perturbations arising from the interaction of the particles with a subquantum medium; by postulating this background medium, assumed to be entirely chaotic and to escape ex-

[44]O. Buneman, "Continuum electrodynamics and its quantization," *Nuovo Cimento, Supplement* **4**, 832–834 (1956).

[45]Letter from Buneman to the author, dated June 10, 1970.

[46]T. Takabayasi, "On the formulation of quantum mechanics associated with classical pictures," *Progress of Theoretical Physics* **8**, 143–182 (1952); "Remarks on the formulation of quantum mechanics with classical pictures and on relations between linear scalar fields and hydrodynamical fields," *ibid.*, **9**, 187–222 (1953).

[47]M. Schönberg, "A non-linear generalization of the Schrödinger and Dirac equations," *Nuovo Cimento* **11**, 674–682 (1954); "On the hydrodynamical model of the quantum mechanics," *ibid.* **12**, 103–133 (1954).

[48]D. Bohm and J. P. Vigier, "Model of the causal interpretation of quantum theory in terms of a fluid with irregular fluctuations," *Physical Review* **96**, 208–216 (1954).

perimental observation though everywhere present in space, Bohm and Vigier revived to some extent the discredited ether conception.[49] Also in 1954 Herbert W. Franke[50] published a paper on the hydrodynamic interpretation which emphasized on the one hand the heuristic advantages and on the other the conceptual limitations of such models.

More recently, elaborate investigations of the hydrodynamic model were carried out by Lajos Jánossy and his collaborator, Maria Ziegler-Náray, at the Central Research Institute of Physics in Budapest, Hungary, to obtain physically significant new results. In a series of publications[51] they showed that the hydrodynamic interpretation can be extended to the case of a charged particle moving under the influence of an electromagnetic field.

Thus the quantum mechanical equation

$$\frac{1}{2m}\left(-i\hbar\nabla-\frac{e}{c}A\right)^2\psi+(e\varphi+V)\psi=i\hbar\frac{\partial\psi}{\partial t}, \tag{66}$$

which describes the motion of a charged particle in a field, determined by the vector potential A and the scalar potential φ, could be replaced by the hydrodynamic equations

$$\operatorname{div}(\rho v)+\frac{\partial\rho}{\partial t}=0, \tag{67}$$

the equation of continuity, and

$$\rho_m\frac{dv}{dt}=-\rho\operatorname{grad}(V+Q)+\frac{\rho e}{c}(v\times H)+\rho_e E, \tag{68}$$

where

$$\rho_m=m\rho,\quad \rho_e=e\rho,\qquad \text{and}\quad Q=-\frac{\hbar^2}{2m}\frac{\nabla^2\rho^{1/2}}{\rho^{1/2}}. \tag{69}$$

[49]In their subsequent paper "Relativistic hydrodynamics of rotating fluid masses," *Physical Review* **109**, 1881–1889 (1958), Bohm and Vigier generalized their approach to a hydrodynamical interpretation of the Dirac and Kemmer wave equation, hoping to provide thereby a physical basis for a causal interpretation also of relativistic wave equations.

[50]H. W. Franke, "Ein Strömungsmodell der Wellenmechanik," *Acta Physica Academiae Scientiarum Hungaricae* **4**, 163–172 (1954).

[51]L. Jánossy, "Zum hydrodynamischen Modell der Quantenmechanik," *Zeitschrift für Physik* **169**, 79–89 (1962). L. Jánossy and M. Ziegler, "The hydrodynamical model of wave mechanics," *Acta Physica Academiae Scientiarum Hungaricae* **16**, 37–48 (1963); *ibid.*, 345–354 (1964); **20**, 233–251 (1966); **25**, 99–109 (1968); **26**, 223–237 (1969); **27**, 35–46 (1969); **30**, 131–137 (1971); *ibid.*, 139–143 (1971).

(Q, often referred to as the "quantum mechanical potential," has to be interpreted in the present context as an "elastic potential" whose gradient yields the interior force which, together with an exterior force, produces the acceleration of the fluid.) The last equation manifests ostensibly the action of the Lorentz force on the elements of the fluid.

Jánossy and his collaborators also showed how the hydrodynamic interpretation can be extended to account for particles described by the Pauli equation. By expressing the Pauli equation in terms of hydrodynamic variables as a system of equations which describe motions in an elastic medium they succeeded in proving that there exists a one-to-one correspondence between the normalized solutions of the wave equation and the solutions of the hydrodynamic equations which satisfy the appropriate initial conditions. Even the spin-orbit coupling can be accounted for on this interpretation. The difficulties which arise in extending this interpretation to a many-body system, they suggested, are not of a mathematical nature but are connected with a still unsolved physical problem. Throughout their work Planck's $\hbar$ is regarded as a constant which characterizes the elastic properties of the system. A different attempt at interpreting quantum mechanics as a hydrodynamic theory was made in 1965 by H. P. Harjes;[52] more recently an interpretation of this type was proposed as the foundation for a theory of elementary particles by Ludwig G. Wallner.[53]

That the semiclassical interpretations, and in particular Schrödinger's may have significant relevance not only for quantum mechanics but also for quantum electrodynamics has been claimed most recently by Edwin T. Jaynes of Washington University, St. Louis, Missouri, whose pioneering work on the connection between information theory and statistical mechanics is well known. Drawing attention to the important role of semiclassical ideas in current experimental work in quantum optics, such as in the study of laser dynamics or coherent pulse propagation, Jaynes[54] proposed a tentative interpretation of radiative processes which according to the usual (Copenhagen) interpretation should defy any detailed description. Jaynes' "neoclassical radiation theory," starting with a theory of the dipole moment of the individual nonrelativistic spinless hydrogen atom,

[52] H. P. Harjes, "Versuch einer hydrodynamischen Interpretation der Schrödingertheorie" (Thesis, Technische Hochschule Hannover, unpublished, 1965).

[53] L. G. Wallner, "Hydrodynamical analogies to quantum mechanics," *Symposium Report, International Atomic Energy Agency* (Vienna, 1970), pp. 479–480. (Abstract)

[54] E. T. Jaynes, "Survey of the present status of neoclassical radiation theory," Lecture given at the Third Rochester Conference on Coherence and Quantum Optics, June 21, 1972. (Preprint)

rescuscitated and elaborated Schrödinger's early interpretation of the wave function. Moreover, by constructing a completely classical Hamiltonian, leading to an equation of motion identical with the conventional Schrödinger equation, and an interaction Hamiltonian which is quadratic rather than linear in its variables and thus couples the atom to the field parametrically, Jaynes proved that action is conserved.

This conservation law of action whose absence, in the early development of the quantum theory, had greatly impeded[55] the general acceptance of Planck's introduction of the quantum of action h, has far-reaching implications for the laws of energy exchange between field and matter and accounts, as Jaynes was able to show, for the $E = h\nu$ quantum effects. Should Jaynes' neoclassical approach, which so far seems to be only in an initial stage of its development, prove to be viable on future evidence, Schrödinger's semiclassical interpretation of quantum mechanics may well be destined to command much higher respect than it does today.

The preceding interpretations of the quantum mechanical formalism tried to reduce quantum theory to classical physics by showing, or rather by trying to show, that the formalism of quantum mechanics is identical with, or only a slightly modified version of, the formalism of a particular branch of classical physics. For Schrödinger it was the classical theory of electricity in conjunction with the assumption that wave phenomena are the basic processes in nature; for Madelung it was classical hydrodynamics; for Korn it was a generalized version of classical physics which comprised both quantum theory and conventional classical physics and was modified only to the extent that it could not get into perceptible conflict with well-established empirical verifications of classical physics. All these attempts and their subsequent revivals were prompted by the belief that once newly discovered regularities can be subsumed under existing general laws, a full clarification of the new situation has been obtained. That the covering laws occasionally have to be generalized to an extent empirically compatible with the established theory is a practice repeatedly used in the history of theoretical physics.

A reductivistic interpretation, as we may briefly call approaches like those of Schrödinger, Madelung, and Korn, supplies automatically a physical picture or model M for the new theory T; it simply carries over the model of the *explicans* into the theory to be interpreted. This reasoning is based on the idea that physical entities which formally satisfy the same mathematical relations are ultimately alike. Although only a heuristic device and by no means a cogent law, this idea not only led to the

[55]Cf. Ref. 1-1 (p. 24).

conceptual unification of apparently disparate branches of theoretical physics, it also opened up new vistas of physical knowledge. In fact, quantum theory itself owes very much to this idea. Thus, to mention only one example, Einstein's conception of the photon was prompted by the mathematical identity between the formulae for the entropies of radiation and of an ideal gas.[56]

The view that a formal identity between mathematical relations betrays the identity of the physical entities involved—a kind of assumption often used in the present-day theory of elementary particles—harmonizes with the spirit of modern physics according to which a physical entity does not do what it does because it is what it is, but is what it is because it does what it does. Since what it "does" is expressed by the mathematical equations it satisfies, physical entities which satisfy identical formalisms have to be regarded as identical themselves, a result in which the mathematization of physics, started by the Greeks (Plato), has reached its logical conclusion.

One may be tempted to argue that according to this point of view, interpretations such as those proposed by Schrödinger and Madelung should ultimately be identical or at least equivalent since they refer to the same mathematical relations (Schrödinger's equation). In spite of certain similarities (due to their common point of departure), such as the equation of continuity or conservation, they must be regarded as fundamentally disparate: according to Schrödinger ψ itself and alone, according to Madelung both α and β possess physical reality.

[56]Cf. *Ibid.*, (pp. 28–30).

The INDETERMINACY Relations

Chapter Three

3.1. THE EARLY HISTORY OF THE INDETERMINACY RELATIONS

Late in the summer of 1926 Schrödinger, invited by Sommerfeld, gave a talk on his new wave mechanics in Munich. Schrödinger's elegant treatment of the hydrogen problem, compared with the matrix mechanical solution by Pauli,[1] showed the superiority of wave mechanics over matrix mechanics, so that Schrödinger's *interpretation* of the wave mechanical formalism also was favorably accepted by most participants of the Munich seminar. Heisenberg's objections—for example, that Planck's basic radiation law could not be understood at all within the framework of Schrödinger's interpretation—were regarded as pedantic; thus, to mention only one typical reaction, Wilhelm Wien, the director of the Munich Institute of Experimental Physics, rejected Heisenberg's criticisms, with the remark that now that Schrödinger had proved once and for all the absurdity of "quantum jumps" and had thus put an end to a theory based on such notions, it would be only a question of time to solve all the remaining problems by wave mechanics.

Shortly after the meeting Heisenberg wrote to Niels Bohr about Schrödinger's lecture. It was probably the contents of this letter[2] which prompted Bohr to invite Schrödinger to spend a week or two in Copenhagen for a discussion on the interpretation of quantum mechanics; and it was Schrödinger's September 1926 visit to Bohr's Institute which precipitated, at least indirectly, a development that ultimately culminated in Bohr's enunciation of the complementarity interpretation.

Born's paper[3] on the adiabatic principle in which through his statistical interpretation he succeeded to, as he phrased it, "amalgamate" to some extent the opposing views of Schrödinger on the one hand and of Bohr and Heisenberg on the other, had not yet been published. Although Schrödinger's proof of the *formal* equivalence between wave and matrix mechanics had been known for six months, the gulf between the conceptual interpretations underlying these rival formulations was far from being bridged. In fact, it was during Schrödinger's visit to Copenhagen that the conflict of opinion came to the open and seemed irreconcilable. The clash of their views is best characterized by the fact, reported by Heisenberg,[4] that at the

[1]W. Pauli, "Über das Wasserstoffspecktrum vom Standpunkt der neuen Quantenmechanik," *Zeitschrift für Physik* **36**, 336–363 (1926).

[2]Cf. W. Heisenberg, Ref. 2-1 (1969, p. 105; 1971, p. 73).

[3]See Ref. 2-34.

[4]W. Heisenberg, "The development of the interpretation of the quantum theory," in *Niels Bohr and the Development of Physics*, W. Pauli, ed. (Pergamon Press, Oxford, 1955), pp. 12–29; "Die Entwicklung der Deutung der Quantentheorie," *Physikalische Blätter* **12**, 289–304 (1956); reprinted in *Erkenntnisprobleme der Naturwissenschaften*, L. Krüger, ed. (Kiepenheuer

end of the debate Schrödinger exclaimed: "If all this damned quantum jumping [*verdammte Quantenspringerei*] were really to stay, I should be sorry I ever got involved with quantum theory," whereupon Bohr replied: "But we others are very grateful to you that you did, since your work did so much to promote the theory."

Although Schrödinger failed to convince Bohr and Heisenberg, who shortly before had moved to Copenhagen, the Bohr-Schrödinger debate stimulated animated discussions which continued long after Schrödinger left Copenhagen. In fact, as a result of this debate, Bohr and Heisenberg, although convinced of the untenability of Schrödinger's conceptions, felt the need of further clarifying the relation between quantum mechanics, as conceived by them, and the data of experience.

Starting with what seemed to be a most simple observational phenomenon, they tried to analyze how the path of an electron, as observed in the Wilson cloud chamber, can be accounted for on the basis of their theory. In matrix mechanics the concept of "path" or "orbit" of an electron is not immediately defined, whereas in wave mechanics any wave packet would soon disperse in its motion to an extent incompatible with the lateral dimensions of such a "path." Pondering about this difficulty during Bohr's absence for a short vacation in February 1927, Heisenberg, not seeing any way to resolve this impasse, was forced to conclude that the very formulation of the problem had to be revised. On the one hand he found the mathematical formalism of quantum mechanics too successful to be revoked, and on the other hand he observed the "path" of the particle in the Wilson chamber. But how to connect these two? It was at this point that he recalled his talk to the Berlin Physics Colloquium in the spring of 1926 and the subsequent conversation[5] with Einstein on the meaning of "observation" in physics. Einstein had said: "It is the theory which decides what we can observe."[6] Heisenberg now felt that the solution of the problem lay in this statement. For if it can be shown that the theory denies the strict observability of the trajectory of the particle (position and momentum) and instead regards the "observed" phenomenon in the Wilson chamber as only a discrete sequence of imprecisely defined positions, as indicated by the condensed water droplets, a consistent connection between the mathematical formalism and observational experience may be established.

& Witsch, Cologne, Berlin, 1970), pp. 412–427. Ref. 2-1 (1969, p. 108; 1971, p. 75).

[5]For details cf. W. Heisenberg, "Die Quantenmechanik und ein Gespräch mit Einstein" in Ref. 2-1, (1969, pp. 90–100; 1971, pp. 62–69).

[6]"Erst die Theorie entscheidet darüber, was man beobachten kann." Ref. 2-1 (1969, p. 92; 1971, p. 63).

As his own statements of those days and later reminiscences fully confirm, in the search for an interpretation of the still mysterious formalism of quantum mechanics, Heisenberg recalled also how Einstein's analysis of the simultaneity of spatially separated events resolved the baffling contradictions of prerelativistic optics and electrodynamics. Deep in his heart Heisenberg cherished the hope that an operational analysis of the concepts of position and velocity, or rather their reinterpretation, would do for the mechanics of micro-objects just what Einstein's analysis of the notion of simultaneity had done for the mechanics of high-speed phenomena. Just as it was meaningless to speak of the simultaneity of two distant events before the introduction of an appropriate synchronization of clocks, so it is "meaningless to speak of the place of a particle with a definite velocity," said Heisenberg. And, indeed, his historic paper[7] on the indeterminary relations began with this statement: "If one wants to clarify what is meant by 'position of an object' [*Ort des Gegenstandes*], for example, of an electron, he has to describe an experiment by which the 'position of an electron' can be measured; otherwise this term has no meaning at all."

Although it would have been rash to classify Heisenberg as a pure operationalist,[8] for he fully agreed with Einstein that what is observed or not is ultimately decided by theory, his paper could easily be interpreted as an attempt to base quantum mechanics on the operational limitations of measurability. The abstract preceding the paper lent strong support to this view. Referring to the indeterminacy relations for canonically conjugate quantities such as position and momentum or energy and time, Heisenberg stated: "This indeterminacy is the essential reason for the occurrence of statistical relations in quantum mechanics." ["*Diese Ungenauigkeit ist der eigentliche Grund für das Auftreten statistischer Zusammenhänge in der Quantenmechanik.*"][9]

In a résumé[10] on the development of the quantum theory between 1918 and 1928, published in 1929, Heisenberg declared that the indeterminacy relations, insofar as they express a limitation of the applicability of the

[7]Ref. 2-20.

[8]Even for Bridgman, who was sympathetic to positivism, Heisenberg's apparently operationalistic and positivistic declarations were merely "a sort of philosophical justification for its success i.e. of matrix mechanics, rather... than an indispensable part in the formulation of the theory." Cf. P. W. Bridgman, *The Nature of Physical Theory* (Dover, New York, 1936), p. 65. Arnold Sommerfeld, on the other hand, saw in Heisenberg a devoted disciple of Mach; cf. A. Sommerfeld, "Einige grundsätzliche Bemerkungen zur Wellenmechanik," *Physikalische Zeitschrift* **30**, 866–871 (1929), esp. p. 866.

[9]Ref. 2-20 (p. 172).

[10]W. Heisenberg, "Die Entwicklung der Quantentheorie 1918–1928," *Die Naturwissenschaften* **17**, 490–496 (1929).

concepts of the particle theory alone, do not suffice for an interpretation of the formalism. "Rather, as Bohr has shown, it is the simultaneous recourse to the particle picture and the wave picture that is necessary and sufficient to determine in all instances the limits to which classical concepts are applicable." ["*Vielmehr zeigte Bohr, dass eben die gleichzeitige Benützung des Partikelbildes und des Wellenbildes notwendig und hinreichend ist, um in allen Fällen die Grenzen abzustecken, bis zu denen die klassischen Begriffe anwendbar sind.*"][11]

For the physicist, however, who was not particularly interested in epistemological subtleties, it was tempting and persuasive to regard Heisenberg's relations as a kind of an operational foundation of quantum mechanics, just as the impossibility of a *perpetuum mobile* (of the first kind) could be, and was, regarded as the foundation of energetics or as the impossibility of detecting an ether-drift was regarded as the foundation of special relativity. No wonder that as early as July 1927 Kennard in a review article[12] called Heisenberg's relations "the core of the new theory" [*der eigentliche Kern der neuen Theorie*].

Pauli began the exposition of quantum theory in his well-known encyclopedia article[13] with the statement of the Heisenberg relations, and it was due to him that Hermann Weyl's book[14] on group theory and quantum mechanics, which appeared in its first edition in 1928, also assigned to these relations an integral part in the logical structure of the whole theory. Since then many authors of textbooks on quantum mechanics, like March (1931), Kramers (1937), Dushman (1938), Landau and Lifshitz (1947), Schiff (1949), and Bohm (1951) have adopted the same approach.

In 1934, however, Karl Popper[15] challenged the claim of assigning logical priority to the Heisenberg relations over the other principles of the theory on the alleged grounds that its statistical character is due to these indeterminacies. Rejecting this analysis of the relation between the inde-

[11] *Ibid.*, p. 494.

[12] E. H. Kennard, "Zur Quantenmechanik einfacher Bewegungstypen," *Zeitschrift für Physik* **44**, 326–352 (1927), quotation on p. 337.

[13] W. Pauli, "Die allgemeinen Prinzipien der Wellenmechanik," *Handbuch der Physik* (H. Geiger and K. Scheel), 2nd edition, Vol. 24 (Springer, Berlin, 1933), pp. 83–272; the article (except the last few sections) is reprinted in *Handbuch der Physik* (*Encyclopedia of Physics*) (S. Flügge), Vol. 5 (Springer, Berlin, Göttingen, Heidelberg, 1958), pp. 1–168. Valuable information on the older quantum theory is contained in W. Pauli, "Quantentheorie," *Handbuch der Physik* (H. Geiger and K. Scheel), 1st edition, Vol. 23 (Springer, Berlin, 1926), pp. 1–278.

[14] H. Weyl, *Gruppentheorie und Quantenmechanik* (Hirzel, Leipzig, 1928); *The Theory of Groups and Quantenmechanics* (Methuen, London, 1931; Dover, New York, 1950).

[15] K. Popper, *Logik der Forschung* (Springer, Wien, 1935); *The Logic of Scientific Discovery* (Basic Books, New York, 1959), p. 223.

terminacy formulae and the statistical or probabilistic interpretation of the theory, Popper pointed out that we can derive the Heisenberg formulae from Schrödinger's wave equation (which is to be interpreted statistically), but not this latter from the Heisenberg formulae; if we are to take due account of these relations of derivability, then the interpretation of the Heisenberg formulae will have to be revised.

We shall postpone the discussion of alternative interpretations—and in particular the statistical reinterpretation suggested by Popper according to which Heisenberg's formulae express merely statistical scatter relations between the parameters involved—to a later section. But we wish to point out that Popper's criticism could not have been directed against Bohr, who never regarded the Heisenberg relations as the logical foundation of the theory nor as identical with the complementarity principle, which will be discussed in the next chapter. It is, in our view, historically wrong to claim that complementarity and indeterminacy were regarded as synonymous. Thus Vladimir Alexandrovitch Fock, for whom complementarity was "an integral part of quantum mechanics" and "a firmly established objectively existing law of nature," erred when he made the statement "At first the term complementarity signified that situation which arose directly from the uncertainty relations. Complementarity concerned the uncertainty in coordinate measurement and in the amount of motion...and the term 'principle of complementarity' was understood as a synonym for the Heisenberg relations."[16]

True, the terms complementarity and Heisenberg-indeterminacy were often considered synonyms. Thus, for example, Serber and Townes, in a paper[17] read at the 1960 New York symposium on quantum electronics, spoke about the "limits on electro-magnetic amplification due to complementarity" when they referred to the indeterminacy relationship between the phase φ and the number of phonons n in an electromagnetic wave, that is, the relation $\Delta\varphi\,\Delta n \geqslant \frac{1}{2}$ which determines the limit of performance of a maser amplifier. That complementarity and Heisenberg-indeterminacy are certainly not synonymous follows from the simple fact that the latter, as we shall presently see, is an immediate mathematical consequence of the *formalism* of quantum mechanics or, more precisely, of the Dirac-Jordan transformation theory, whereas complementarity is an extraneous *in-*

[16]V. A. Fock, "Kritika vzgliadov Bora na kvantovuiu mekhaniku," (A criticism of Bohr's views on quantum mechanics), *Uspekhi Fisiĉeskich Nauk* **45**, 3–14 (1951); "Kritik der Anschauungen Bohrs über die Quantenmechanik," *Sowjetwissenschaft* **5**, 123–132 (1952); (revised) *Czechoslovak Journal of Physics* **5**, 436–448 (1955).

[17]A. Serber and C. H. Townes, "Limits on electromagnetic amplification due to complementarity," *Quantum Electronics—A Symposium* (Columbia University Press, New York, 1960), pp. 233–255.

terpretative addition to it. In fact, the quantum mechanical formalism with the inclusion of the Heisenberg relations can be, and has been, interpreted in a logically consistent way without any recourse to complementarity.

3.2. HEISENBERG'S REASONING

After these digressions let us return to the conceptual origin of the Heisenberg relations. The problem which Heisenberg faced in 1927 was twofold: (1) Does the formalism allow for the fact that the position of a particle and its velocity are determinable at a given moment only with a limited degree of precision?[18] (2) Would such imprecision, if admitted by the theory, be compatible with the optimum of accuracy obtainable in experimental measurements?

Before we discuss Heisenberg's answers to these questions let us make the following terminological remarks. The term used by Heisenberg in these considerations was *Ungenauigkeit* (inexactness, imprecision) or *Genauigkeit* (precision, degree of precision). In fact, in his classic paper these terms appear more than 30 times (apart from the adjective *genau*), whereas the term *Unbestimmtheit* (indeterminacy) appears only twice and *Unsicherheit* (uncertainty) only three times. Significantly, the last term, with one exception (p. 186), is used only in the Postscript, which was written under the influence of Bohr. In general we shall adhere to the following terminology:

1. If the emphasis lies on the absence of (subjective) knowledge of the values of the observables we shall use the term *uncertainty* in conformance with Heisenberg's usage,[19]
2. If the emphasis lies on the supposedly objective (i.e., observer-independent) absence of (precise) values of observables we shall use the term *indeterminateness*.[20]

[18]For the sake of historical accuracy it should be mentioned that the same question had been raised in the fall of 1926 by P. M. Dirac. In his paper "The physical interpretation of the quantum dynamics," *Proceedings of the Royal Society A* **113**, 621–641 (1926), received December 2, 1926, Dirac anticipated Heisenberg when he wrote (p. 623): "One cannot answer any question on the quantum theory which refers to numerical values for both the q and the p."

[19]Cf. W. Heisenberg, *Die physikalischen Prinzipien der Quantentheorie* (Hirzel, Leipzig, 1930; Bibliographisches Institut, Mannheim, 1958), p. 15; *The Physical Principles of the Quantum Theory* (University of Chicago Press, Chicago, 1930; Dover, New York, n.d.), p. 20; *I Principi Fisici della Teoria dei Quanta* (G. Einaudi, Torino, 1948); *Les Principes Physiques de la Théorie des Quanta* (Gauthier-Villars, Paris, 1957, 1972) *Fizičeskije Principi Kvantovoj Teorii* (Moscow, 1932).

[20]Cf. D. Bohm, *Causality and Chance in Modern Physics* (Routledge and Kegan Paul, London,

3. If neither aspect is emphasized we shall use *indeterminacy* as a neutral term.

To answer question 1 above, Heisenberg[21] resorted to the Dirac-Jordan transformation theory as follows. For a Gaussian distribution of the position coordinate q the state function or "probability amplitude," as Heisenberg called it, is given by the expression

$$\psi(q)=\text{const.}\exp\left[\frac{-q^2}{2(\delta q)^2}\right],$$

where δq, the half-width of the Gaussian hump, denotes (according to Born's probabilistic interpretation) the distance in which the particle is almost certainly situated and hence the indeterminacy in position ($\delta q = \sqrt{2}\,\Delta q$ where Δq is the standard deviation). In accordance with the transformation theory the momentum distribution is $|\varphi(p)|^2$ where $\varphi(p)$ is obtained by the Fourier transformation:

$$\varphi(p)=\int_{-\infty}^{\infty}\exp\left(\frac{-2\pi ipq}{h}\right)\psi(q)\,dq$$

or

$$\varphi(p)=\int_{-\infty}^{\infty}\exp\left[-\frac{1}{2}\left(\frac{q}{\delta q}+\frac{2\pi ip\,\delta q}{h}\right)^2\right]\exp\left(\frac{-2\pi^2p^2(\delta q)^2}{h^2}\right)dq.$$

Putting

$$\frac{q}{\delta q}+\frac{2\pi ip\,\delta q}{h}=y$$

and integrating, Heisenberg obtained

$$\varphi(p)=\text{const.}\exp\left(\frac{-2\pi^2p^2(\delta q)^2}{h^2}\right),$$

1957), footnote on p. 85; *Pritsinost i Slutsainost v Sovremennoi Fisike* (Foreign Literature Publications, Moscow, 1959).

[21]Ref. 2-20.

which shows that the indeterminacy in momentum is given by

$$\delta p = \frac{h/2\pi}{\delta q}.$$

Hence

$$\delta q\, \delta p = \frac{h}{2\pi}$$

or

$$\Delta q\, \Delta p = \frac{h}{4\pi}. \tag{1}$$

To find the answer to question 2 above Heisenberg asked whether a close examination of measuring processes themselves does not lead to a result which violates the restriction imposed by the relation (1); Heisenberg analyzed what has since become known as the "gamma-ray microscope experiment." His point of departure here was the operational view that a scientific concept is a condensed code of operations and its meaning, in the last analysis, a definite relation between sense impressions of the observer. For, he said, to understand the meaning of the concept "place" or "position" of a particle, such as an electron, one has to refer to a definite experiment by which "the position" is to be determined; otherwise the concept has no meaning. One may, for example, illuminate the electron and observe it under the microscope. Since in accordance with the optical laws of resolution the precision increases the smaller the wave length of the radiation (illumination), a gamma-ray microscope promises maximum accuracy in position determination. Such a procedure, however, involves the Compton effect. "At the moment of the position determination [*im Augenblick der Ortsbestimmung*], that is, when the quantum of light is being diffracted by the electron, the latter changes its momentum discontinuously [*unstetig*]. This change is greater the smaller the wave length of light, that is, the more precise the position determination. Hence, at the moment when the position of the electron is being ascertained [*in dem Augenblick, in dem der Ort des Elektrons bekannt ist*] its momentum can be known only up to a magnitude that corresponds to the discontinuous change; thus, the more accurate the position determination, the less accurate the momentum determination and vice versa" [*also je genauer der Ort bestimmt ist, desto ungenauer ist der Impuls bekannt und umgekehrt*].[22]

Heisenberg also showed by an analysis of a Stern-Gerlach experiment

[22]Ref. 2-20 (p. 175).

for the determination of the magnetic moment of an atom that the uncertainty in measuring the energy ΔE is smaller the longer the time Δt spent by the atom in crossing the deviating field. Since the potential energy E of the deviating force cannot be allowed to change within the width d of the atomic beam by more than the energy difference ΔE of the stationary states, if the energy of these states is to be measured, $\Delta E/d$ is the maximum value of the deviating force; the angular deviation φ of the beam of atoms with momentum p is then given by $\Delta E \Delta t/dp$; since, however, φ must be at least as large as the natural diffraction at the slit defining the width d of the beam, that is, λ/d, where according to the de Broglie relation $\lambda = h/p$, Heisenberg concluded that $\lambda/d = h/pd \lesssim \Delta E \Delta t/pd$ or

$$\Delta E \Delta t \gtrsim h. \tag{2}$$

This equation, Heisenberg declared, "shows how an accurate determination of energy can be obtained only by a corresponding indeterminacy in time."

As we see from this almost verbatim presentation of Heisenberg's argument, it interpreted these indeterminacies as pertaining to an *individual* particle (sample) and not as a statistical spread of the results obtained when measuring the positions or momenta of the members of an ensemble of particles. Furthermore, Heisenberg's reference to the discontinuous change of momentum due to the Compton effect did not provide a full justification of his conclusion, for, as Bohr pointed out when he read the draft of Heisenberg's paper, the finite aperture of the microscope has to be taken into account. Indeed, in the Postscript to the paper Heisenberg acknowledged Bohr's criticism when he wrote that Bohr drew his attention to the fact that "essential points" had been omitted, for example, "the necessary divergence of the radiation beam" under the microscope; "for it is only due to this divergence that the direction of the Compton recoil, when observing the position of the electron, is known with an uncertainty that leads to the result (1)."

In fact, a complete analysis of the gamma-ray microscope experiment should start with the theorem, known from Abbe's theory of optical diffraction, that the resolving power of the microscope is given by the expression $\lambda/2\sin\epsilon$ (in air) where λ is the wave length of the light used and 2ϵ is the angle subtended by the diameter of the lens at the object point. Any position measurement involves therefore an uncertainty in the x-direction of the object-plane

$$\Delta x = \frac{\lambda}{2\epsilon} \tag{3}$$

If a light-quantum of wave length λ, and hence of momentum h/λ, approaches along the x-axis an electron of parallel momentum p_x, the total momentum (before the collision) is $\pi=(h/\lambda)+p_x$. For the electron to be observed by the microscope, the light-quantum must be scattered into the angle 2ϵ, somewhere between PA and PB (extreme backward scattering and extreme forward scattering, Figure 1), and has correspondingly a wave length between λ' and λ'', due to the Compton effect. The x-component of the momentum of the scattered light-quantum lies consequently between $-h\sin\epsilon/\lambda'$ and $+h\sin\epsilon/\lambda''$. If, correspondingly, p_x' and p_x'' denote the x-component of the electron in these two extreme scattering situations, the conservation of linear momentum requires that

$$p_x' - \frac{h\sin\epsilon}{\lambda'} = \pi = p_x'' + \frac{h\sin\epsilon}{\lambda''}$$

or

$$p_x' - p_x'' = \Delta p_x = \frac{2h\sin\epsilon}{\lambda}, \tag{4}$$

where λ' and λ'' have been replaced by λ since only the order of magnitude is of interest. Since there is no way—and this is the important point of the whole story—to tell *precisely* in what direction within the angle 2ϵ the light-quantum has been scattered, the indeterminacy of the x-component of the electron's momentum after the collision cannot be decreased and this, together with Δx, precludes any precise determination or prediction of the particle's trajectory after the collision (or in other words, after the measurement). Clearly, $\Delta x\,\Delta p \sim h$.

Although Bohr accepted the conclusions of Heisenberg's paper he disagreed with the general trend of its reasoning. In fact, he even tried to persuade Heisenberg not to publish the paper, at least not in the form it was written. The controversy was quite bitter and "very disagreeable." Said Heisenberg: "I remember that it ended with my breaking out in tears because I just couldn't stand this pressure from Bohr."[23] The issue was not about the conclusions, that is, about the validity of the indeterminacy relations, but rather about the conceptual foundations on which they were established.

Heisenberg's conception of indeterminacy as a limitation of the applicability of classical notions, like position or momentum, to microphysical phenomena, did not tally with Bohr's view according to which they were an indication, not of the inapplicability of *either* the language of

[23]Interview with Heisenberg on February 25, 1963. *Archive for the History of Quantum Physics.*

particulate physics *or* the language of undulatory physics, but rather of the impossibility of using *both* modes of expression simultaneously in spite of the fact that only their combined use provides a full description of physical phenomena. Whereas for Heisenberg the reason of indeterminacy was discontinuity, whether expressed in terms of particle physics or of wave physics, for Bohr the reason was the wave-particle duality. "That is the center of the whole story, and we have to start from that side of the story in order to understand it," he insisted, whereupon Heisenberg retorted: "Well, we have a consistent mathematical scheme and this consistent mathematical scheme tells us everything which can be observed. Nothing is in nature which cannot be described by this mathematical scheme."

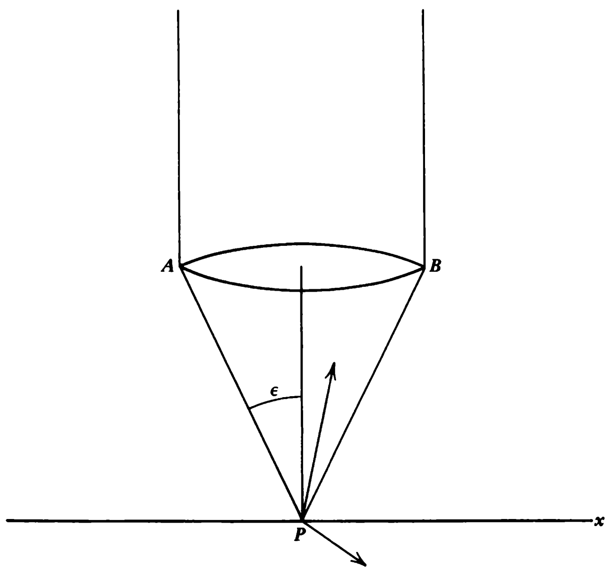

Figure 1.

Such an argument, however, did not appeal to Bohr for whom "mathematical clarity had in itself no virtue" and "a complete physical explanation should absolutely precede the mathematical formulation."[24] Mathematics, Bohr said, could not prove any physical truth, it could only

[24]W. Heisenberg, "Quantum theory and its interpretation," in *Nields Bohr—His Life and Work as seen by his Friends and Colleagues*, S. Rozental, ed. (North-Holland, Amsterdam; Wiley, New York, 1967), p. 98.

show the consistency of a formalism and its suitability to express relations among physical data.

And yet the role of the mathematical formalism in the quantum theory was not the main issue of their disagreement. The issue of the controversy may be clarified by the following remarks. It will have been noticed that in the derivation of the indeterminacy relations from the analysis of Heisenberg's thought-experiments, use has been made of the Einstein-de Broglie relations $\lambda = h/p$ or $\nu = E/h$. These relations obviously connect wave attributes with particle attributes and thus express the wave-particle dualism. In fact, every derivation of the Heisenberg relations from the analysis of thought-experiments *must* somewhere have recourse to the Einstein-de Broglie equations, for otherwise the whole reasoning would remain classical and no indeterminacy relation could be derived.

To illustrate this point once more, let us recall another well-known thought-experiment. Consider a "particle," originally moving in the y-direction, passing through a slit of width Δx, so that its position in the x-direction is defined with indeterminacy Δx (Figure 2). Thus far the terminology of classical particle mechanics has been used; however, this is abandoned as soon as reference is made to the "interference" occurring behind the slit. From undulatory optics it is known that the angle α, defining the first interference minimum, is given by $\sin\alpha = \lambda/2\Delta x$, where λ is the wave length involved. Since $\sin\alpha = \Delta p/p$ and $\lambda = h/p$, where again explicit reference is made to the Einstein-de Broglie equation, the Heisenberg formula $\Delta x\, \Delta p \approx h$ follows.

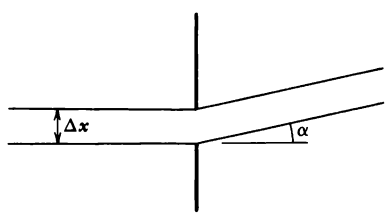

Figure 2.

The indispensability of using the Einstein-de Broglie equations for the derivation of the Heisenberg relations was in Bohr's view an indication that the wave-particle duality or, more generally, the necessity of two mutually exclusive descriptions of physical phenomena is the ultimate foundation of the whole theory. Heisenberg, on the other hand, cognizant of the fact that the indeterminacy relations are logical deductions from the mathematical formalism, did not consider the wave-particle duality as a necessary presupposition of the theory. Being convinced that the mathematical scheme does allow one to predict every experiment, Heisenberg thought it irrelevant whether to use words like "waves" or words like "particles" for the description of what was going on. In fact, as he later admitted, the papers[25] soon published by Jordan and Klein and by Jordan and Wigner, who proved the equivalence between the ordinary Schrödinger theory description of a system of *n* particles and the operator (second-quantized) description for particles obeying Fermi statistics (1928), showing thereby the possibility of using at will either language, that of particles or that of waves, made him "very happy"; for they proved exactly what he had in mind, that "the particle picture and the wave picture are merely two different aspects of one and the same physical reality."

From the historical point of view it is interesting to note that Oskar Klein, who was at that time on Copenhagen, and "took part quite frequently in these discussions" between Bohr and Heisenberg, might have been prompted by this controversy between Bohr and Heisenberg to devote himself to the problem which led to the Klein-Jordan-Wigner results. According to Jordan,[26] it was Pauli's skillful diplomacy which prevented a serious conflict between Bohr and Heisenberg: Pauli tried to convince both sides that the dispute was only about the *Rangordnung der Begriffe* [precedence-order of the concepts]. Heisenberg's point of departure was the statement that "all concepts which are used in classical physics to describe mechanical systems can in analogy be exactly defined also for atomic processes" [*Alle Begriffe, die in der klassischen Theorie zur Beschreibung eines mechanischen Systems verwendet werden, lassen sich auch für atomare Vorgänge analog den klassischen Begriffen exakt definieren.*][27] for to define a concept means to prescribe a procedure to measure the quantity referred to by the concept.

The limitations imposed by the indeterminacy relations do not restrict definability, since each of the canonically conjugate quantities, if considered alone, can be measured in principle with arbitrary accuracy. If, however, simultaneous measurement of such conjugate quantities have to be considered, useless speculations can be avoided if it is agreed to define place, time, momentum, and energy only in accordance with the indeterminacy relations, that is, to base their definability on their measurability.

[25] P. Jordan and O. Klein, "Zum Mehrkörperproblem der Quantentheorie," *Zeitschrift für Physik* **45**, 751–765 (1927). P. Jordan and E. Wigner, "Über das Paulische Äquivalenzverbot," *ibid.*, **47**, 631–651 (1928).
[26] Interview with Jordan in Hamburg, June 28, 1971.
[27] Ref. 2-20 (p. 179).

This reduction of definability to measurability was unacceptable to Bohr. For, as he subsequently declared, when speaking about conjugate quantities, "the reciprocal uncertainty which always affects the values of these quantities is essentially an outcome of the limited accuracy with which changes in energy and momentum can be *defined*" (not "measured"!).

Bohr's point of departure was the fundamental wave-particle duality which found its expression in the individuality of atomic processes and which consequently led to the question concerning the limits within which physical objects of such nature can be described in terms of classical concepts; the limitation of measurability confirms the limitation of definability but does not logically precede it. Bohr's position could be supported by the argument that, as mentioned previously, any derivation of the indeterminacy relations from thought-experiments, that is, of Heisenberg's formulation of the limitations of measurability, had to be based on the Einstein-de Broglie equations, which, in turn, connect features of the wave and particle descriptions—and thus implicitly presuppose wave-particle duality.

When Heisenberg realized Bohr's point a compromise was reached. Heisenberg sent the above-mentioned Postscript to the editor of the *Zeitschrift für Physik* and declared in it that still unpublished investigations by Bohr would lead to a deeper insight and to an important refinement of the results obtained in the present paper. Heisenberg added that, as Bohr would show in detail, indeterminacies in observation not only result from the occurrence of discontinuities but are intimately connected with the requirement of accounting simultaneously for the conflicting experiences encountered on the one hand in the theory of corpuscles and on the other in the theory of waves. He concluded with the words: "I am greatly indebted to Professor Bohr for having had the opportunity of seeing and discussing his new investigations which are soon to be published as an essay on the conceptual structure of the quantum theory."[28]

These concluding remarks alluded, of course, to Bohr's ideas about complementarity; and it is probably in view of these remarks that it is frequently claimed that Bohr derived the notion of complementarity from Heisenberg's relations. That this claim is erroneous, and that Heisenberg's work only prompted Bohr to give his thoughts on complementarity, which can be traced back at least to July 1925, a consistent and final formulation, has been shown elsewhere.[29] That Bohr had indeed been preoccupied with

[28]Ref. 2-20 (pp. 197–198).

[29]Ref. 1-1 (pp. 345–352). Cf. also K. M. Meyer-Abich, *Korrespondenz, Individualität und Komplementarität* (Steiner, Wiesbaden, 1965), and G. Holton, "The roots of complementarity," *Daedalus* **99**, 1015–1055 (1970), reprinted in G. Holton, *Thematic Origins of Scientific*

these ideas for some time prior to their enunciation in September 1927 may have been at least one of the reasons for the fact that during the period of the most dramatic development in quantum mechanics, from July 1925 until September 1927, he had not published one scientific paper. But now, in the early fall of 1927, his ideas on complementarity had reached maturity and seemed to have been fully corroborated by the results arrived at by Heisenberg. If, as we have mentioned, the complementarity principle was for Bohr the outcome of philosophical deliberations of a most general nature and hence a principle of general epistomological relevance to science, Heisenberg's relations were, in Bohr's view, a striking mathematical demonstration of the validity of this principle in microphysics and a proof of the necessity of applying the principle to the study of micro-objects.

In his Chicago lectures[30] Heisenberg, following Kennard's[31] elaboration of his own derivation (as given above), proved the indeterminacy relations as follows (we abbreviate $h/2\pi$ by $\hbar$, $d\psi/dx$ by ψ', etc., and assume ψ to be normalized). The relations

$$\langle x\rangle=\int\psi^* x\psi\,dx,\qquad \langle x^2\rangle=\int\psi^* x^2\psi\,dx,\qquad \langle p\rangle=-i\hbar\int\psi^*\psi'\,dx,$$

$$\langle p^2\rangle=-\hbar^2\int\psi^*\psi''\,dx,\qquad \int\psi^{*\prime}\psi'\,dx=-\int\psi^*\psi''\,dx,$$

$$\int x(\psi^*\psi'+\psi^{*\prime}\psi)\,dx=-1$$

and the inequality

$$|\psi'+\alpha x\psi|^2\geqslant 0,$$

where α is an arbitrary real constant, imply that

$$\alpha^2\langle x^2\rangle+\frac{\langle p^2\rangle}{\hbar^2}\geqslant\alpha$$

By choosing $\alpha=(2\langle x^2\rangle)^{-1}$ and assuming $\langle x\rangle=\langle p\rangle=0$ in $(\Delta x)^2=\langle x^2\rangle-\langle x\rangle^2$, etc, Heisenberg obtained (1).

D. ter Haar and W. M. Nicol[32] regarded this derivation as "not completely rigorous" on the ground that the choice of α was *ad hoc* and "by choosing a different value one would derive relations between Δx and Δp which would be completely different from the Heisenberg relations." To

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Thought (Harvard University Press, Cambridge, Mass., 1973), pp. 115–161.

[30]Ref. 19.

[31]Ref. 12.

[32]D. ter Haar and W. M. Nicol, "Proof of the Heisenberg relations," *Nature* **175**, 1046 (1955).

obtain (1) rigorously these authors pointed out that the smallest combination of $(\Delta x)^2$ and $(\Delta p)^2$, subject to $\alpha^2(\Delta x)^2 - \alpha + (\Delta p)^2/\hbar^2 \geqslant 0$, which occurs when the equality sign holds (i.e. when $\psi' + \alpha x\psi = 0$) should be taken; since in all other cases the situation is less favorable, $\Delta x \Delta p \geqslant \hbar/4\pi$ holds quite generally. This reasoning, in turn, was criticized as "not completely satisfactory" by F. M. Gomide and C. Braga Rego[33] who derived (1) from the nonpositivity of the discriminant of the preceding quadratic trinomial in α and justified Heisenberg's procedure as "always valid, independently of the nature of ψ."

3.3. SUBSEQUENT DERIVATIONS OF THE INDETERMINACY RELATIONS

Studying the problem of whether Heisenberg's relations apply to any pair of noncommuting operators, Edward Uhler Condon[34] contended (1) that noncommutativity does not necessarily imply such relations, (2) that, in fact, some simultaneous values of two noncommuting operators may be known precisely, and (3) that precision may be limited even if the operators do commute. To prove point 1 Condon considered the hydrogen wave function

$$\psi_{nlm} = R(r)\exp(im\varphi)P_l^m(\cos\theta)$$

for which the component L_z of the angular momentum has the value $m\hbar$; since for this state $\Delta L_z = 0$, clearly $\Delta L_x \Delta L_z = 0$ although L_x and L_z do not commute. To prove point 2 above Condon chose the state ψ_{n00} for which $L_x = L_y = L_z = 0$ so that also $\Delta L_x = \Delta L_y = \Delta L_z = 0$. Finally, to prove point 3 Condon pointed out that for ψ_{n10} (i.e., $L_z = 0$) $L_x L_y - L_y L_x = 0$ although $\Delta L_x \neq 0$ and $\Delta L_y \neq 0$.

Five weeks after Condon had finished his paper Howard Percy Robertson, a colleague of Condon at Princeton's Palmer Physical Laboratory, in a short paper[35] proved for the first time quite generally that the product of the standard deviations of two self-adjoint operators A and B is never less than half the absolute value of the mean of their commutator $C = i(AB - BA)$. His proof has been adopted by most modern tests. With A_1 defined

[33]F. M. Gomide and G. Braga Rego, "On Heisenberg's proof of the uncertainty relations," *Anais da Academia Brasileira de Ciencias* **28**, 179–181 (1956).

[34]E. U. Condon, "Remarks on uncertainty principles," *Science* **69**, 573–574 (1929).

[35]H. P. Robertson, "The uncertainty principle," *Physical Review* **34**, 163–164 (1929). While an assistant at that time to Hermann Weyl (who spent the academic year 1928–1929 at Princeton) Robertson translated into English Weyl's treatise mentioned in Ref. 14.

by $A_1 = A - \langle A \rangle$ so that the standard deviation ΔA is $\langle A_1{}^2 \rangle^{1/2}$ (and similarly for B_1) and D defined by $D = A_1 + i\lambda B_1$, where λ is a real number, it easily follows that

$$0 \leqslant \langle D^\dagger D \rangle = \lambda^2(\Delta B)^2 - \lambda \langle C \rangle + (\Delta A)^2.$$

Since therefore the discriminant of this quadratic polynomial in λ cannot be positive, $\Delta A \, \Delta B \geqslant \frac{1}{2} |\langle C \rangle|$ or

$$\Delta A \, \Delta B \geqslant \tfrac{1}{2} |\langle AB - BA \rangle| \tag{5}$$

For $A = q$ and $B = p$ Robertson obtained $C = ih/2\pi$ and thus $\Delta q \Delta p \geqslant h/4\pi$ in agreement with Heisenberg's result.[36] The apparent discrepancy in the denominator (in Heisenberg's formula the denominator is 2π) was soon clarified by Robert William Ditchburn,[37] who also showed (in collaboration with John Lighton Synge) that the equality holds if and only if the scatter is a Gaussian distribution (as assumed by Heisenberg).

A further improvement of the general formula was obtained by Schrödinger, who, as we know from his correspondence with Bohr and Einstein,[38] was greatly interested in various implications of the Heisenberg relations, such as their implications for the discernibility of the discrete energy levels of the atoms of an ideal gas in an enclosure. In the spring of 1930 he studied the problem of how to distribute, in an optimal simultaneous measurement of p and q, the unavoidable indeterminacy $h/4\pi$ between the two variables in such a way that at a given later time the position indeterminacy would be minimal. Sommerfeld, with whom he discussed this problem, drew his attention to the papers by Condon and Robertson. Schrödinger[39] soon realized that Robertson's result could be

[36] On the apparent contradiction arising from applying the operator p on one of its eigenfunctions, say, φ so that $p\varphi = b\varphi$ and $ih/2\pi = [\varphi, (qp - pq)\varphi] = b(\varphi, q\varphi) - b(\varphi, q\varphi) = 0$ see E. R. Davidson, "On derivations of the uncertainty principle," *Journal of Chemical Physics* **42**, 1461–1462 (1965), and R. Yaris, "Comment on 'On derivation of the uncertainty principle,'" *ibid.*, **44**, 425–426 (1966). For an extension of the Heisenberg relations to normal operators, that is, operators which commute with their adjoint, see M. Bersohn, "Uncertainty principle and normal operators," *American Journal of Physics* **34**, 62–63 (1966), and W. E. Brittin, "Uncertainty principle and normal operators—Some comments," *ibid.*, 957–959 (1966).

[37] R. W. Ditchburn, "The uncertainty principle in quantum mechanics," *Proceedings of the Royal Irish Academy* **39**, 73–80 (1930).

[38] Letter to Bohr, dated May 13, 1928 (Bohr Archive, Copenhagen); letter to Einstein, dated May 30, 1928 (Einstein Estate, Princeton).

[39] E. Schrödinger, "Zum Heisenbergschen Unschärfeprinzip," *Berliner Berichte* **1930**, 296–303. Schrödinger generalized Robertson's derivation by introducing $D = A + \alpha B + i\beta B$, where α and β are real numbers, and deducing from $0 < (D\psi, D\psi)$ the inequality expressing the nonpositivity of the discriminant; by an appropriate choice of α and β he then obtained (6).

strengthened, since for any two self-adjoint operators A and B and for any state ψ

$$(\Delta A)^2(\Delta B)^2 \geqslant |\langle \tfrac{1}{2}(AB - BA)\rangle|^2 + (\tfrac{1}{2}\langle AB + BA\rangle - \langle A\rangle\langle B\rangle)^2. \qquad (6)$$

Even though the last squared term in Schrödinger's formula often vanishes, as it does in the case of all optimal simultaneous measurements of canonically conjugate variables, Schrödinger's formula constitutes an important refinement of Robertson's result.

Quite a few of the early texts on quantum mechanics considered Heisenberg's thought-experiments as the "experimental" or "logical" foundations of quantum mechanics. Although these thought-experiments played, as we shall see, an extremely important role in the development of the theory and its interpretation, primarily because of their alleged operational interpretation of the indeterminacy relations, they were open to the charge that they tacitly assume to some extent the ontology of classical physics, which they explicitly purport to deny.

This criticism seems to have been raised for the first time explicitly by Ch. R. von Liechtenstern[40] who pointed out in 1954 that in Heisenberg's account of his gamma-ray microscope experiment the existence of the electron's momentum must be assumed to exist, for otherwise it could not be "disturbed," whereas the very same account intends to prove its nonexistence since it cannot be precisely measured.

Let us add a few more general remarks on the Heisenberg relations. It should be pointed out, first, that the general idea of a fundamental uncertainty or indeterminacy in the physical world is as old as the idea of strict determinism. Plato's doctrine of the unintelligible subatomic sub-

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The attempts, following Schrödinger, at further narrowing down, generalizing, or alternatively deriving the relation are too numerous to be listed in full. Around 1950 alone, e.g., the following papers were published on this issue: G. Bodiou, "Renforcement des relations d'incertitude en statistique quantique par l'introduction d'un coefficient complexe de corrélation," *Comptes Rendus* **228**, 540–542 (1949); A. Gamba, "Sulla relazione di indeterminazione," *Nuovo Cimento* **7**, 378–379 (1950); "The uncertainty relation," *Nature* **166**, 653–654 (1950); L. Castoldi, "Sulla relazione di indeterminazione," *Nuovo Cimento* **7**, 961–962 (1950); R. L. Reed and M. Dresden, "Thee uncertainty principle for an arbitrary number of variables," *Physical Review* **79**, 200–201 (1950); *Bulletin of the American Physical Society* **25**, 3 (1950); G. Rideau, "Sur la quatrième relation d'incertitude," *Comptes Rendus* **232**, 2007–2009 (1951).

For an interesting geometrical derivation and interpretation of the Heisenberg relations and their generalizations such as the Schrödinger inequality see J. L. Synge, "Geometrical approach to the Heisenberg uncertainty relations and its generalization," *Proceedings of the Royal Society A* **325**, 151–156 (1971).

[40]Ch. R. von Liechtenstern, "Die Beseitigung von Widersprüchen bei der Ableitung der Unschärferelation," *Proceedings of the Second International Congress of the International Union for the Philosophy of Science (Zurich, 1954)* (Editions du Griffon, Neuchatel, 1955), pp. 67–70.

stratum, as described in his *Timaeus* (28D-29B), can be regarded as "an ancient anticipation of a most recent development [Heisenberg's principle]."[41]

For further examples of such anticipations in ancient or classical physics and philosophy as well as for an analysis of the role of indeterminacy in modern classical physics, as propounded most eloquently by Max Born, the reader is referred to our essay "Indeterminacy in Physics."[42] That, in particular, Henri Bergson, especially in his *Essai sur les Données Immédiates de la Conscience,*[43] expressed ideas strikingly analogous to Heisenberg's principle has been pointed out by Louis de Broglie.[44] So much on the prehistory of the indeterminacy relations.

Once announced, Heisenberg's relations or "the *principle* of indetermination" as they were called for the first time by Arthur E. Ruark,[45] became a favorite subject of discussions in physics and subsequently also in philosophy. Thus one of the main objectives of Frederick Alexander Lindemann's *The Physical Significance of the Quantum Theory*[46] was to show how numerous quantum phenomena, previously incompletely understood, could easily be accounted for on the basis of these relations. The importance of the Heisenberg relations in physics proper is, of course, well known to every student of modern physics and need not be described in detail. Let us refer only to a study, not so well known, which deals with the consistency of the relations themselves, to John Hasbrouch van Vleck's proof[47] that the well-known increase of the "uncertainty product" $\Delta q\,\Delta p$ in

[41]P. Friedländer, *Plato—An Introduction* (Pantheon Books, New York, 1958), p. 251.

[42]M. Jammer, "Indeterminacy in Physics," in *Dictionary of the History of Ideas*, (Charles Scribner's Sons, New York, 1973), Vol. 2, pp. 586–594.

[43]H. Bergson, *Essai sur les Données Immédiates de la Conscience* (Alcan, Paris, 1889, 1909, 1938); *Time and Free Will, an Essay on the Immediate Data of Consciousness* (Sonnenschein, London, New York, 1910; George Allen & Unwin, London, 1913, 1950).

[44]L. de Broglie, "Les conceptions de la physique contemporaine et les idées de Bergson sur le temps et le mouvement," in *Physique et Microphysique* (A. Michel, Paris, 1947), pp. 191–211; "The concepts of contemporary physics and Bergson's ideas on time and motion," in *Physics and Microphysics* (Grosset and Dunlap, New York, 1966), pp. 186–194. Cf. also *Bergson and the Evolution of Physics*, P. A. Y. Gunter, ed. (University of Tennessee Press, Knoxville, 1969), pp. 45–62, and Chapter 12 in M. Căpek, *Bergson and Modern Physics* (Boston Studies in the Philosophy of Science, Vol. 7) (Reidel, Dordrecht-Holland, 1971), pp. 284–291.

[45]A. E. Ruark, "Heisenberg's indetermination principle and the motion of free particles," *Bulletin of the American Physical Society* **2**, 16 (1927); *Physical Review* **31**, 311–312 (1928).

[46]F. A. Lindemann, *The Physical Significance of the Quantum Theory* (Clarendon Press, Oxford, 1932). For a more recent example of a similar reasoning cf. R. C. Harney, "A method for obtaining force law information by applying the Heisenberg uncertainty principle," *American Journal of Physics* **41**, 67–70 (1973).

[47]J. H. van Vleck, "Note on Liouville's theorem and the Heisenberg uncertainty principle," *Philosophy of Science* **8**, 275–279 (1941).

time does not conflict with the Liouville theorem concerning the stationary character of distributions in phase space.

3.4. PHILOSOPHICAL IMPLICATIONS

The first to draw philosophical conclusions from these relations was Heisenberg himself. Identifying the law of causality, in its strong formulation, with the statement that "the exact knowledge of the present allows the future to be calculated," Heisenberg pointed out that "it is not the conclusion but the hypothesis that is false," for the unascertainability of exact initial values, as stated by the principle, rules out the strict predictability of future events. "Since all experiments obey the quantum laws and, consequently, the indeterminacy relations, the incorrectness of the law of causality is a definitely established consequence of quantum mechanics itself," Heisenberg declared.[48]

Heisenberg's solution of the causality problem took modern philosophy, as Moritz Schlick[49] later admitted, by surprise, since even the mere possibility of such a solution had never been anticipated in spite of the profusion of discussions on this problem for many generations.

However, in 1929 Hugo Bergmann[50] of the Hebrew University in Jerusalem claimed that Heisenberg's refutation of the causality law is logically unsound because a conditional statement "if...then...," that is, a logical implication, is not refuted by disproving the validity of its premise or hypothesis, that is, by proving that the "If"-clause cannot be realized (satisfied) or is "false," as Heisenberg, according to Bergmann, "inaccurately" stated; the falsity of the premise of an implication, Bergmann contended, by no means entails the falsity of the implication itself, which alone is the causality law; quantum mechanics has therefore in no way refuted the law of causality, though it may have shown, at most, its inapplicability.[51] Bergmann seems to have ignored that Heisenberg anticipated such an objection and that from his operational or even positivist

[48]Ref. 2-20 (p. 197).

[49]M. Schlick, "Die Kausalität in der gegenwärtigen Physik," *Die Naturwissenschaften* **19**, 145–162 (1931).

[50]H. Bergmann, *Der Kampf um das Kausalgesetz in der jüngsten Physik* (Vieweg, Braunschweig, 1929), p. 39.

[51]"Von einer definitiven Feststellung der Ungültigkeit des Kausalgesetzes durch die Quantentheorie kann also keine Rede sein, sondern höchstens von seiner Unanwendbarkeit." *Op. cit.* Bergmann's criticism was later challenged by H. Rohracher in his "Kritische Betrachtungen zur Leugnung der Kausalität durch W. Heisenberg," in *Erkenntnis und Erziehung* (Österreichischer Bundesverlag, Wien, 1961), pp. 105–123.

point of view, which he espoused at that time, "inapplicability" (Bergmann's admitted *Unanwendbarkeit*) and "invalidity" were for the physicist synonymous. Said Heisenberg: "...it is possible to ask whether there is still concealed behind the statistical universe of perception [*hinter der wahrgenommenen statistischen Welt*] a 'true' universe [*wirkliche Welt*] in which the law of causality would be valid. But such speculation seems to us to be without value and meaningless, for physics must confine itself to the description of the relationships between perceptions."[52]

This declaration of allegiance to the positivistic veiwpoint on physics sounds strange or even inconsistent if we recall that the point of departure of Heisenberg's reasoning which led him to the indeterminacy relations was Einstein's apothegm that "only theory determines what can be observed." In fact, Heisenberg soon abandoned this positivistic attitude. In a lecture on the role of the indeterminacy relations in modern physics which he delivered in Vienna on December 9, 1930, in which he again acknowledged his intellectual indebtedness to Einstein, he formulated the law of causality as follows: "If at a certain time all data [*Bestimmungstücke*] are known for a given system then it is possible to predict unambiguously the physical behavior of the system also for the future."[53] This time, however, Heisenberg declared that not the hypothesis ("If"-sentence) but the consequence ("Then"-sentence) is false, since the precise knowledge of the data, that is, the Schrödinger function, admits in general only statistical conclusions. However, the causality law, Heisenberg continued, can be expressed also as follows: "If at a certain time all data are known for a given system then there exist, at any later time, experiments the result of which can be exactly predicted, provided the system is subjected to no other disturbances than those necessary for the performance of the experiment."

Heisenberg now added: "Whether a regularity of this kind may be still regarded as causality or not is purely a question of taste." [*Ob man eine derartige Gesetzmässigkeit noch kausal nennen will oder nicht ist eine reine Geschmacksfrage.*]

In an address[54] delivered on September 6, 1930, in Königsberg, Heisenberg had acknowledged the meaningfulness and validity of a restricted [*eingeschränktes*] law of causality.

Another attempt to save strict determinism and causality against the background of Heisenberg's indeterminacy was made by the French

[52]Ref. 2-20 (p. 197).

[53]W. Heisenberg, "Die Rolle der Unbestimmtheitsrelationen in der modernen Physik," *Monatshefte für Mathematik und Physik* **38**, 365–372 (1931).

[54]W. Heisenberg, "Kausalgesetz und Quantenmechanik," *Erkenntnis* **2**, 172–182 (1931).

philosopher Léon Brunschwicg. According to Brunschwicg[55] microphysical reality per se is ruled by rigorous causal laws whereas the indeterminacy, expressed by the Heisenberg principle, is only apparent for it is due merely to the disturbance produced by the very act of observing the microphysical object; this disturbance, though itself a physical process, involving energy exchange, and wholly deterministic, causes the indeterminacy. "The uncertainty relations," Brunschwicg declared, "merely mean that the determinism of the observed phenomenon is in itself nothing but an abstraction because it is inseparable from the determinism by which the act of observation is ruled."

In short, Brunschwicg interpreted the Heisenberg relations rather as a confirmation of causality than its denial [*cela n'entraîne nullement la rupture du déterminisme*]. A different argument, to the same end, has been advanced by Johannes Erich Heyde,[56] according to whom the Heisenberg relations imply the impossibility of proving the existence of causality but not the possibility of proving the nonexistence of causality.

Since the impact of quantum mechanics and its interpretations on modern philosophy is not our major concern it would lead us too far afield to enlarge this digression. The preceding remarks were meant only as illustrations of the importance of the indeterminacy relations even for problems which transcend the immediate confines of physics. Nor shall we enlarge on the interesting use made of the indeterminacy relations with regard to the philosophical problems about the concept of motion and, in particular, Zeno's famour paradoxes.[57]

Let us add, instead, a few remarks on those philosophical implications of the indeterminacy relations to which their originator seems to have ascribed foremost importance. To the question whether it is not deplorable that the classical intuitable [*anschaulische*] and deterministic picture of physical reality had to yield ground to the modern abstract and indeterministic theory of quanta with its indeterminacies, Heisenberg replied[58] that quantum physics, on closer analysis, proves from the epistemological

[55]L. Brunschwicg, "Science et la prise de conscience," *Scientia* **55**, 329–340 (1934); *La Physique du XXe Siècle et la Philosophie* (Hermann, Paris, 1936).

[56]J. E. Heyde, *Entwertung der Kausalität?* (Kohlhammer, Stuttgart; Europa Verlag, Zurich, Wien, 1957), p. 65.

[57]Cf. L. de Broglie, *Matière et Lumière* (Michel, Paris, 1937, 1948), pp. 282–283; *Matter and Light* (Norton, New York, 1939; Dover, New York, 1946), pp. 245–255; A. Ushenko, "Zeno's paradoxes," *Mind* **55**, 151–165 (1946); P. T. Landsberg, "The uncertainty principle as a problem in philosophy," *Mind* **56**, 260–266 (1947); H. Hörz, "Die philosophische Bedeutung der Heisenbergschen Unbestimmtheitsrelationen," *Deutsche Zeitschrift für Philosophie* **8**, 702–709 (1960).

[58]Ref. 53.

point of view more satisfactory than classical physics. A theory, he argued, which conceives matter as built up of elementary particles of "small" but finite magnitude, such as electrons or protons, obtains ultimate consistency only if it somehow deprives of any meaning any questions as to what happens in regions still "smaller." Hence if elementary particles are denied picturability and if such questions are made meaningless because of the indeterminacy relations, the theory becomes what may be called "epistemologically closed in the small." The indeterminacy relations bestow on quantum mechanics epistemological closure in the small, which therefore makes quantum mechanics superior to the classical theory of atoms. Heisenberg's statement that modern atomic physics has shown for the *first time* how such a closure is conceivable [*die moderne Atomphysik hat zum ersten Male gezeigt, wie ein solcher Abschluss der Welt im kleinen prinzipiell denkbar ist*] may be challenged. For there are good reasons to claim that such a closure had been envisaged by Plato in his remarkable theory of atoms—which, by the way, being more closely related to modern conceptions than Democritean atomism, had, as we know, a profound influence on Heisenberg's intellectual development.

3.5. LATER DEVELOPMENTS

Since we shall have to resume our discussion of the Heisenberg principle in various contexts later on, for example, in connection with the complementarity interpretation, the Bohr-Einstein controversy, quantum logic, and the theory of measurement, we confine ourselves at present to a rather brief outline of some of its major subsequent developments.

The scientific—and, no less so, the semiscientific and philosophical—literature of the 1930s to the 1950s virtually abounded with papers on the Heisenberg relations and their philosophical implications.The less technical they were, the greater was the freedom of their interpretation. Often one author in the course of his exposition gave different interpretations. For example, Arthur Stanley Eddington's *New Pathways of Science*,[59] which enjoyed a great popularity before World War II, describes the principle as reflecting the "wave constitution of electrons and protons" (p. 107) but later explains it as a consequence of interactions and compares the observer to "the comedian with an armful of parcels, each time he picks up one he drops another" (p. 100), on another occasion (p. 98) the principle is said to assert that only "half of the symbols represent knowable quantities." No doubt for Eddington these various formulations were merely

[59] A. S. Eddington, *New Pathways of Science* (Cambridge University Press, Cambridge, 1935).

different expressions of one and the same fundamental idea.[60] But the critical reader of this book or of similar works must have felt bewildered by the "many faces" of the principle.

That this was the case is best illustrated by the fact that Ernan McMullin, presently chairman of the Philosophy Department of Notre Dame University, wrote a Ph.D. thesis[61] in 1954 on the different meanings of the "quantum principle of uncertainty." He distinguished between at least four major classes of interpretations: Heisenberg's principle is regarded (1) as a *principle of impossibility* according to which it is impossible to measure simultaneously conjugate variables, (2) as a *principle of limitations* in measurement precision according to which the accuracy of previously acquired knowledge about one variable decreases by measuring its conjugate, (3) as a *principle of statistics* relating the scatter of one sequence of measurements with that of another, and (4) as a *mathematical principle* expressing the duality or complementarity of quantum phenomena. McMullin's remarks on the meaning of "simultaneity" as used in this context and on the nature of quantum discontinuities remain worth reading today. A few years later Herbert Hörz, presently chairman of the Department of Marxist-Leninist Philosophy at Humboldt University in Berlin (DDR), also chose Heisenberg's principle and its philosophical significance as subject for his Ph.D. dissertation.[62] Although almost diametrically opposed in their ideologies, McMullin and Hörz arrived at essentially identical conclusions as far as purely physical issues are concerned.

[60]Heisenberg's principle was of basic importance for Eddington's attempt to reconcile relativity with quantum theory, a task to which he dedicated the last 10 years before his death in 1944. Cf. Section 1 (The Uncertainty of the Reference Frame) of his posthumous *Fundamental Theory* (Cambridge University Press, Cambridge, 1948) or his manuscript on the transfer problem, published in C. W. Kilmister, *Sir Arthur Eddington* (Pergamon Press, Oxford, 1966), pp. 244 *et seq.*

[61]E. McMullin, *The Principle of Uncertainty* (Dissertation, Catholic University of Louvain, 1954), unpublished.

[62]H. Hörz, *Die philosophische Bedeutung der Heisenbergschen Unbestimmtheitsrelationen* (Dissertation, Humboldt University, Berlin, 1960), unpublished. For a summary see H. Hörz, "Die philosophische Bedeutung der Heisenbergschen Unbestimmtheitsrelationen," *Deutsche Zeitschrift für Philosophie* **8**, 702–709 (1960). Cf. also H. Hörz, *Atome, Kausalität, Quantensprünge* (Deutscher Verlag der Wissenschaften, Berlin, 1964), pp. 30–85. Heisenberg's philosophy of physics (especially his interpretation of the indeterminacy relations) was also the subject of Patrick A. Heelan's Ph.D. thesis (1964) at the Catholic University of Louvain. Cf. P. A. Heelan, S. J., *Quantum Mechanics and Objectivity* (Nijhoff, The Hague, 1965), esp. pp. 36–43. In contrast to McMullin, Heelan was interested primarily in the philosophical implications of Heisenberg's work with respect to the problem of objectivity and the "crisis of realism." Unlike McMullin's, Heelan's conclusions differ considerably from those obtained by Hörz.

Anticipating certain considerations which will be discussed later, we wish to point out at this juncture that of all the interpretations listed by these authors the following two proved most important for the development of quantum mechanics:

1. The *nonstatistical interpretation* I_1 according to which it is impossible, in principle, to specify precisely the simultaneous values of canonically conjugate variables that describe the behavior of a single (individual) physical system,
2. The *statistical interpretation* I_2 according to which the product of the standard deviations of two canonically conjugate variables has a lower bound[63] given by $h/4\pi$.

I_1, which originated, as we have seen, with Heisenberg's thought-experiments, was for many years the dominant interpretation and was adopted by almost all textbooks on quantum mechanics. I_2, which was proposed by Popper and elaborated, as we shall see, by Margenau and later by the advocates of the ensemble interpretation of quantum mechanics, has been gaining acceptance since 1965. Whereas I_1 was generally regarded as being based on arguments involving specific thought-experiments, I_2 was held to be established as a straightforward logico-mathematical consequence of the very formalism of the theory. In view of the fact that the battle between the partisans of these two interpretations still continues to be waged unabatedly it will be worthwhile to conclude this chapter with a few remarks on the logical relations between these two interpretations and on the question of the extent to which they are supported by experience.

As intimated in our historical account, thought-experiments, such as the Heisenberg gamma-ray microscope experiment, were too spurious to serve as rigorous arguments in support of I_1. On the other hand, I_2 being essentially a mathematical consequence of the basic formulae of quantum mechanics, could hardly be rejected without introducing major modifications into the whole theory. Does it therefore follow that I_1 has no logical justification whatever?

[63]The existence of such a lower bound does not necessarily imply that the standard deviations involved are reciprocally related, i.e., inversely proportional to each other (reciprocity claim). For, without additional assumptions, an inequality, such as $\Delta q \Delta p > h/4\pi$, can never lead to an equality, such as $\Delta q = \text{const.}/\Delta p$ or $\Delta p = \text{const.}/\Delta q$. The generally unwarranted reciprocity claim seems to be retraceable to Heisenberg's original formulation of his principle and to Bohr's repeated use of the term "reciprocal uncertainty." For arguments against the general validity of the reciprocity claim and for examples of particular cases of its strict validity (such as in the case of adiabatic changes of bound states) see P. Kirschenmann, "Reciprocity in the

In contrast to those who flatly reject I_1 or claim that between I_1 and I_2 there is a logical gap that can never be bridged, we contend that I_1 is a logical consequence of I_2 if a certain measurement-theoretical assumption A is accepted. According to this assumption every measurement involved in this context is a measurement of the first kind[64] (in Pauli's terminology), that is, every measurement is repeatable and, if immediately repeated, yields the same result as its predecessor.

To show that on this assumption I_2 implies I_1 we proceed as follows. Suppose it were possible to simultaneously measure on an individual system two canonically conjugate variables with arbitrary precision. It should then also be possible, in principle at least, to filter out an ensemble of systems for which the values of these variables lie within arbitrarily small intervals. It now follows from assumption A that for either variable the standard deviation with respect to the ensemble can be made arbitrarily small. Hence the product of the two deviations cannot have a positive lower bound, in contradiction to I_2. As long as measurements of the first kind are available to measure the variables under discussion it is unjustified to maintain I_2 and to reject I_1. In other words, a necessary condition for the abandonment of I_1 under retention of I_2 is the unavailability of appropriate measurements of the first kind. As we see, the whole issue is intimately connected with the quantum theory of measurement, one of the most problematic and controversial parts of the theory.

Turning now to the question of the empirical support, we unhesitatingly declare that rarely in the history of physics has there been a principle of such universal importance with so few credentials of experimental tests. In fact, as far as I_1 is concerned, no methods seem presently to be available to measure, for example, the position and the momentum of an individual electron simultaneously with sufficient precision to evaluate the errors involved. Indeed, in spite of the fact that, as we shall see, only very few "extremists" denied altogether the simultaneous measurability of such observables, no such measurement on individual particles has ever been performed with sufficient precision to be of any significance for our problem.

Thus, to mention a typical proposal along these lines, more than 15 years ago P. R. Ryason[65] of California Research Corporation, Richmond, California, suggested testing directly the time-energy indeterminacy relation by pulsed-field desorption of adatoms on a metal tip at extremely low

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uncertainty relations," *Philosophy of Science* **40**, 52–58 (1973).

[64]For details see Chapter 11.

[65]P. R. Ryason, "Proposed direct test of the uncertainty principle," *Physical Review* **115**, 784–785 (1959).

temperatures (0.4°K), applying the technique of field-ion microscopy. Yet the proposed experiment, which Ryason declared is "technologically feasible" in 1959, seems never to have been carried out. Those who contend that the Heisenberg principle, if interpreted in the sense of I_1, has experimentally never been confirmed certainly have a strong point. But let it also be said that no experiment performed so far has ever disconfirmed the principle either.

I_2 obviously enjoys a far better empirical backing. In fact, many measurements often referred to as vindications of the Heisenberg relations are experimental corroborations precisely of I_2. Thus, to mention a recent study of this problem, J. Clifton Albergotti, chairman of the Physics Department of the University of San Francisco, examined how far a number of outstanding precision experiments measure up with respect to the Heisenberg relations. Although the problem as posed by Albergotti was to decide whether "modern experiments are limited by the uncertainty principle or by the instruments used to perform the experiments"—the former case, if borne out, representing in his view "a practical demonstration of the uncertainty principle" while the latter would leave the principle as a matter of "strictly academic concern"—his study,[66] if carefully scrutinized, is precisely an examination of how far I_2 can be supported by experience. Among the experiments investigated were linear air track experiments for which the indeterminacy products had a lower bound of about $3\times10^{26}\hbar$, measurements of the angular momentum of light with a lower bound of about $4\times10^{14}\hbar$, a high-resolution electron microscope experiment with a lower bound of $20\hbar$, and, with the smallest indeterminacy product found so far, a Mössbauer effect experiment on iron-57 with only $6\hbar$. Clearly, I_2, though perhaps not verified, is far from being falsified.

Space does not allow the description of other recent investigations of the nature of the indeterminacy relations. To show that such discussions are still of actual interest we refer the reader to an essay by L. V. Prokhorov,[67] to a reformulation of these relations by Louis de Broglie in terms of premeasurement and postmeasurement indeterminacies,[68] and to a proposal of a thought-experiment[69] to disprove the principle. Finally, we wish

[66] J. C. Albergotti, "Uncertainty principle—Limited experiments," *The Physics Teacher* **11**, 19–23 (1973).

[67] L. V. Prokhorov, "O sootnoshenjakh njeopredeljenostei v kvantovoj mekhanike," *Vestnik Leningradskovo Universiteta* **22**, 29–35 (1968).

[68] L. de Broglie, "Sur l'interpretation des relations d'incertitude," *Comptes Rendus* **268**, 277–280 (1969).

[69] M. C. Robinson, "A thought experiment violating Heisenberg's uncertainty principle,"

to point out that certain modern developments have cleared the way for the establishment of theories on the simultaneous measurement of incompatible observables. One of the earliest, if not the earliest explicit affirmation of such a possibility was made in 1950 by Henry Margenau.[70] Since Heisenberg's principle, according to Margenau, correlates merely the spread in an ensemble [*Kollektiv*] of, say, position measurements with the spread in an ensemble of momentum measurements it nowhere suggests "with precision what may be expected in a *single* measurement of any kind." Margenau thus saw no difficulty in using two microscopes, one employing gamma rays and the other waves of suitable, greater length, one for locating the electron and the other for determining its momentum. In Margenau's view there is no law of quantum mechanics which basically prohibits such a double measurement from succeeding. "If it were repeated, the values of x and p would spread in accordance with the uncertainty principle. Of course, no one would say after one such pair of measurements that he knows x and p with precision, any more than he would say after one X measurement that he knows the electron's position. The first statement is illicit not because it contradicts the uncertainty principle but because it, like the second statement, contradicts the quantum mechanical meaning of a physical state."[71]

It is only since the mid-1960s that these ideas have been further elaborated.[72] Thus far, however, little consensus has been reached on the very basic issues of such theories, primarily, it seems, because of diverging

Canadian Journal of Physics **47**, 963–967 (1969); J. J. Billette, C. Campillo, C. Lee, R. D. McConnell, G. Pariseau, and G. Fischer, "Concerning 'A thought experiment violating Heisenberg's uncertainty principle'," *ibid.*, 2415–2416; L. E. Ballentine, "The uncertainty principle and the statistical interpretation of quantum mechanics," *ibid.*, 2417–2418.

[70]H. Margenau, *The Nature of Physical Reality* (McGraw-Hill, New York, 1950), pp. 375–377.

[71]On the last point cf. Chapter 10.

[72]E. Arthurs and J. L. Kelly, "On the simultaneous measurements of a pair of conjugate observables," *Bell System Technical Journal* **44**, 725–729 (1965). C. Y. She and H. Heffner, "Simultaneous measurement of noncommuting observables," *Physical Review* **152**, 1103–1110 (1966). E. Prugovečki, "On a theory of measurement of incompatible observables in quantum mechanics," *Canadian Journal of Physics* **45**, 2173–2219 (1967). J. L. Park and H. Margenau, "Simultaneous measurability in quantum theory," *International Journal of Theoretical Physics* **1**, 211–283 (1968). H. D. Dombrowski, "On simultaneous measurements of incompatible observables," *Archive for Rational Mechanics and Analysis* **35**, 178–210 (1969). J. L. Park and H. Margenau, "The logic of noncommutability of quantum mechanical operators and its empirical consequences," in *Perspectives in Quantum Theory*, W. Yourgrau and A. van der Merwe, eds. (MIT Press, Cambridge, 1971), pp. 37–70. E. Prugovečki, "A postulational framework for theories of simultaneous measurement of several observables," *Foundations of Physics* **3**, 3–18 (1973), and a criticism thereof in H. Margenau and J. L. Park, "The physics and the semantics of quantum measurement," *ibid.*, 19–28.

definitions of "simultaneous measurements." It is certainly too early to form a balanced judgment on the legitimacy and prospects of such theories. For these reasons as well as for the fact that an intelligent discussion on this subject presupposes some familiarity with the quantum theory of measurement we abstain here from going into any details.

Early Versions of the COMPLEMENTARITY Interpretation

Chapter Four

4.1. BOHR'S COMO LECTURE

In the fall of 1927 the International Congress of Physics, commemorating the centenary of Alessandro Volta's death, convened in the Italian city of Como where Volta was born and died. Among the many physicists who attended this metting, chaired by Quirino Majorana, were Niels Bohr, Max Born, Satyandra Nath Bose, William Lawrence Bragg, Marcel Brillouin, Louis de Broglie, Arthur Holly Compton, Peter Debye, William Duane, Enrico Fermi, James Franck, Yakov Fraenkel, Walter Gerlach, Werner Heisenberg, Max von Luue, Hendrik Antoon Lorentz, Robert Andrews Millikan, Friedrich Paschen, Wolfgang Pauli, Max Planck, Owen Williams Richardson, Ernest Rutherford, Arnold Sommerfeld, Otto Stern, Richard Chase Tolman, Robert Williams Wood, and Pieter Zeeman—a veritable summit metting of the world of physics at that time. Only Einstein and Ehrenfest, though invited, were not present!

It was before this illustrious audience, assembled in the auditorium of the Istituto Carducci, that Bohr, on September 16, 1927, in a lecture[1] entitled "The Quantum Postulate and the Recent Development of Atomic Theory," presented for the first time in public his ideas on complementarity. Bohr began his address with the following words:

> I shall try, by making use only of simple considerations and without going into details of technical mathematical character, to describe to you a certain general point of view which I believe is suited to give an impression of the general trend of the development of the theory from its very beginning and which I hope will be helpful in order to harmonize the apparently conflicting views taken by different scientists.

After confronting the classical description according to which physical phenomena can be observed without being thereby essentially disturbed, with the description of quantum phenomena, subject to the quantum postulate, according to which every atomic process is characterized by an

[1]*Atti del Congresso Internazionale dei Fisici, Como, 11–20 Settembre 1927* (Zanichelli, Bologna, 1928), Vol. 2, pp. 565–588; the substance of the lecture is reprinted in *Nature* **121**, 580–590 (1928) and in N. Bohr, *Atomic Theory and the Description of Nature* (Cambridge University Press, Cambridge, London, 1934), pp. 52–91; "Das Quantenpostulat und die neuere Entwicklung der Atomistik," *Die Naturwissenschaften* **16**, 245–257 (1928). N. Bohr, *Atomtheorie und Naturbeschreibung* (Springer, Berlin, 1931), pp. 34–59. "Kvantepostulatet og atomteoriens seneste udvikling," in *Atomteori og Naturbeskrivelse* (Lunos Bogtrykkeri, Copenhagen, 1929), pp. 40–68, as well as in *Atomteorien og Grundprincipperne for Naturbeskrivelsen* (Schultz, Copenhagen, 1958), pp. 47–76. *Théorie Atomique et Description des Phenomènes* (Gauthier-Villars, Paris, 1932). N. Bohr, *Izbrannije Naučnije Trudy* (Collected Scientific Works), Vol. 2 (Essays, 1925–1961) (Nauka, Moscow, 1971).

essential discontinuity or, as he called it, individuality, Bohr continued: "On one hand, the definition of the state of a physical system, as ordinarily understood, claims the elimination of all external disturbances. But in that case, according to the quantum postulate, any observation will be impossible, and, above all, the concepts of space and time lose their immediate sense. On the other hand, if in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible, and there can be no question of causality in the ordinary sense of the word. The very nature of the quantum theory thus forces us to regard the space-time coordination and the claim of causality, the union of which characterizes the classical theories, as complementary but exclusive features of the description, symbolizing the idealization of observation and definition respectively." This statement, in which Bohr introduced the term "complementary" for the first time, calling "space-time coordination" and the "claim of causality" complementary to each other, contained the essence of the earliest version of what later became known as the "complementarity interpretation" or "Copenhagen interpretation" of quantum mechanics.

As we shall see, the Copenhagen interpretation is not a single, clear-cut, unambiguously defined set of ideas but rather a commom denominator for a variety of related viewpoints. Nor is it necessarily linked with a specific philosophical or ideological position. It can be, and has been, professed by adherents to most diverging philosophical views, ranging from strict subjectivism and pure idealism through neo-Kantianism, critical realism, to positivism and dialectical materialism. Furthermore, it is a viewpoint not necessarily confined to the interpretation of quantum physics. In 1930, in his Faraday Lecture,[2] Bohr had already extended the idea to statistical thermodynamics when he claimed that "the very concept of temperature stands in an exclusive relation to a detailed description of the behavior of the atoms in the bodies concerned."

Bohr was also the first to extend the principle of complementarity to biology. In an address delivered at the opening meeting of the International Congress on Light Therapy which was held in Copenhagen on August 15, 1932, Bohr explained that the revision of the foundation of physics,

> extending to the very question of what may be meant by a physical explanation, has not only been essential for the elucidation of the situation in atomic

[2]N. Bohr, "Chemistry and the quantum theory of atomic constitution," Lecture delivered before the Fellows of the Chemical Society at Salter's Hall, May 8, 1930; reprinted in *Journal of the Chemical Society* **1932**, 349–383.

> theory, but has also created a new background for the discussion of the relation of physics to the problems of biology.... We should doubtless kill an animal if we tried to carry the investigation of its organs so far that we could describe the rôle played by single atoms in vital functions.... On this view, the existence of life must be considered as an elementary fact that cannot be explained, but must be taken as a starting point in biology, in a similar way as the quantum of action, which appears as an irrational element from the point of view of classical mechanical physics, taken together with the existence of the elementary particles, forms the foundation of atomic physics.[3]

In 1932 Pascual Jordan[4] published similar ideas in an essay on quantum mechanics and biology, in which he presented vitalism and physicalism as complementary aspects in the study of living nature.

A few years later, at the Copenhagen International Congress of Anthropological and Ethnological Sciences (August 1938), Bohr[5] explained to his audience in the large hall of Kronborg Castle the role of complementarity relations in the study of primitive cultures in human society.

In the 1948 issue of *Dialectica*, which was devoted entirely to discussions about complementarity, the mathematician and philosopher Ferdinand Gonseth published an article[6] in which he claimed that since knowledge progresses through an unveiling of successive horizons of reality and since the concept of complementarity refers to the relationship between any two horizons in this dialectic process, complementarity has a potential applicability in *all* areas of systematic research.

Complementarity has been applied to psychology, linguistics, ethics, and theology.[7] Thus, to mention just one example for the last-named applica-

[3]N. Bohr, "Light and life," *Nature* **131**, 421–423, 457–459 (1933); reprinted in Ref. 1 (1934, pp. 3–12); "Lys og liv," *Naturens Verden* **17**, 49–54 (1933); "Licht und Leben," *Die Naturwissenschaften* **21**, 245–250 (1933).

[4]P. Jordan, "Die Quantenmechanik und die Grundprobleme der Biologie und Psychologie," *Die Naturwissenschaften* **20**, 815–821 (1932); "Quantenphysikalische Bemerkungen zur Biologie und Psychologie," *Erkenntnis* **4**, 215–252 (1934); "Ergänzende Bemerkungen über Biologie und Quantenmechanik," *ibid.*, **5**, 348–352 (1935).

[5]N. Bohr, "Natural philosophy and human culture," *Nature* **143**, 268–272 (1939), reprinted in N. Bohr, *Atomfysik og Menneskelig Erkendelse* (Schultz, Copenhagen, 1957), pp. 35–44; *Atomic Physics and Human Knowledge* (Chapman and Hall, London; Wiley, New York, 1958), pp. 23–31; *Atomphysik und menschliche Erkenntnis* (Vieweg, Braunschweig, 1958); *Physique Atomique et Connaissance Humaine* (Gauthier-Villars, Paris, 1961), pp. 27–34; *Atomnaja Fizika i Čelovečeskoje Posnanje* (Erevan, Aipetrat, 1963).

[6]F. Gonseth, "Remarque sur l'idée de complementarité," *Dialectica* **2**, 413–420 (1948).

[7]Cf. P. Alexander, "Complementary description," *Mind* **65**, 145–165 (1956); D. M. Mackay, "Complementary description," *Mind* **66**, 390–394 (1957); T. Bergstein, "Complementarity and philosophy," *Nature* **222**, 1033–1035 (1969), and T. Bergstein, *Quantum Physics and Ordinary Language* (Macmillan, London, 1972); N. Brody and P. Oppenheim, "Application of Bohr's

tion, Charles Alfred Coulson,[8] F. R. S. and Rouse Ball professor of mathematics at Oxford, viewed religion and science as two alternative approaches which, though apparently irreconcilable, are both true, being complementary to each other.

In our study we shall discuss the notion of complementarity only as far as physics is concerned. But even in this context it covers a whole spectrum of doctrines; for complementarity can be conceived, for example, from a purely ontological viewpoint, as many popularizations of modern physics suggest, or from an epistemological point of view, as Bohr advocated, or from a logical viewpoint, as his pupil C. F. von Weizsäcker proposed. A historical analysis of the notion of complementarity is further complicated by the fact that it is not only a homonym for different, though naturally related, conceptions, as expressed by different writers, but has also often undergone considerable semantic variations in the writings of one and the same author. This applies, as we shall see, to Bohr himself.

By concentrating our attention therefore on Bohr's early writings on this subject let us first find out, as precisely as possible, what Bohr meant when he spoke about "complementarity" in atomic physics. That this is no easy task can be illustrated by the following facts. In his "Reply to Criticisms"[9] in which Einstein refers to Bohr's ideas of complementarity as expressed in Bohr's article "Discussion with Einstein on Epistemological Problems in Atomic Physics,"[10] Einstein, writing at the end of January 1949, complained that "despite much effort which I have expended on it, I have been

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principle of complementarity to the mind-body problem," *Journal of Philosophy* **66**, 97–113 (1969). On the relation between complementarity and the philosophy of Kant see C. F. von Weizsäcker, "Das Verhältnis der Quantenmechanik zur Philosophie Kants," *Die Tatwelt* **17**, 66–98 (1941); "Atomtheorie und Philosophie," *Die Chemie* **55**, 99–104, 121–126 (1942); reprinted in C. F. von Weizsäcker, *Zum Weltbild der Physik* (Hirzel, Zurich, 4th ed., 1949), pp. 80–117; *The World View of Physics* (Routledge and Kegan Paul, London; University of Chicago Press, Chicago, 1952), pp. 92–135. On the relation between complementarity and the philosophy of Hegel see Max Wundt, *Hegels Logik und die moderne Physik* (Westdeutscher Verlag, Cologne, 1949). Cf. also G. Kropp, "Zum Begriff der Komplementarität," *Philosophia Naturalis* **1**, 446–462 (1950–1952).

[8] C. A. Coulson, *Christianity in an Age of Science* (Oxford University Press, London, 1953).

[9] P. A. Schilpp, ed., *Albert Einstein: Philosopher-Scientist* (Library of Living Philosophers, Evanston, Ill., 1949; Harper and Row, New York, 1959), pp. 663–688; *Albert Einstein als Philosoph und Naturforscher* (Kohlhammer, Stuttgart, 1955).

[10] N. Bohr, "Discussion with Einstein on epistemological problems in atomic physics," in Ref. 9 (1949, pp. 199–241), reprinted in Ref. 5 (Wiley, 1958, pp. 32–66); "Diskussion mit Einstein über erkenntnistheoretische Probleme in der Atomphysik," in Ref. 9 (1955, pp. 115–150), reprinted in Ref. 5 (Vieweg, 1958, pp. 32–67); "Discussion avec Einstein sur des problèmes epistemologiques de la physique atomique," in Ref. 5 (1961, pp. 35–66); "Diskussion med Einstein," in Ref. 5 (1957, pp. 45–82).

unable to achieve the sharp formulation of Bohr's principle of complementarity!" If for Einstein, the anticomplementarist, as one may claim, it was perhaps difficult to find a clear-cut definition of "complementarity," the second illustration, referring to an enthusiastic advocate of the complementarity interpretation, shows even more clearly how difficult our task is.

On the occasion of Bohr's seventieth birthday (October 7, 1955) Carl Friedrich von Weizsäcker, working then at the Max Planck-Institute for Physics in Göttingen, wrote a comprehensive article[11] on complementarity and logic. Although von Weizsäcker was not present at the Como meeting in 1927, his assistantship to Werner Heisenberg in Leipzig soon brought him in close touch with Bohr's conceptions of complementarity. For the purpose of the just-mentioned article, as explicitly stated there, von Weizsäcker re-read most carefully Bohr's early papers on this notion and came to the conclusion that for over 25 years he had misinterpreted Bohr's notion of complementarity, the real meaning of which he now thought he had discovered. But when he asked Bohr whether his interpretation (which we shall soon discuss in greater detail) accurately presents what Bohr has in mind, Bohr gave him a definitely negative answer.[12] This historical fact should be a warning to us to be particularly careful in our analysis of Bohr's original conception of complementarity.

Bohr's point of departure in his Como lecture was the statement that the "essence [of the quantum theory] may be expressed in the so-called quantum postulate, which attributes to any atomic process an essential discontinuity, or rather individuality, completely foreign to the classical theories and symbolized by Planck's quantum of action." Since, according to this quantum postulate, energy exchanges proceed only in discrete steps of finite size, the postulate of the indivisibility of the quantum of action demands "not only a finite interaction between the object and the measuring instrument but even a definite latitude in our account of this mutual action," as Bohr explained two years later in a talk[13] about his basic conceptions. For, since the interaction between the object and the instrument, contrary to classical physics, cannot be neglected, "an independent

[11]C. F. von Weizsäcker, "Komplementarität und Logik," *Die Naturwissenschaften* **42**, 521–529, 545–555 (1955).

[12]Letter from Bohr to von Weizsäcker, dated March 5, 1956, in reply to von Weizsäcker's letter of January 17, 1956.

[13]N. Bohr, "The atomic theory and the fundamental principles underlying the description of nature," translation of a lecture delivered at the 18th Scandinavian Congress of Scientists in Copenhagen, August 26, 1929. Reprinted in Ref. 1 (1934, pp. 102–119); "Die Atomtheorie und die Prinzipien der Naturbeschreibung," *Die Naturwissenschaften* **18**, 73–78 (1930) reprinted in Ref. 1 (1931, pp. 67–77).

reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observations."[14]

This result, according to Bohr, has far-reaching consequences. "On one hand, the definition of the state, of a physical system, as ordinarily understood, claims the elimination of all external disturbances. But in that case, according to the quantum postulate, any observation will be impossible, and, above all, the concepts of space and time lose their immediate sense. On the other hand, if in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible, and there can be no question of causality in the ordinary sense of the word." This passage, already quoted above, thus leads to the conclusion that space-time coordination and the claim of causality are complementary features in the description of physical observation.

As the preceding quotations clearly show, Bohr's 1927 conception of complementarity referred, above all, to the impossibility of carrying out a causal description of atomic phenomena which, at the same time, is also a space-time description. As early as 1925, in a lecture[15] to the Scandinavian Mathematical Congress at Copenhagen, Bohr had pointed out that the development of physics had led to the recognition that a coherent causal description of atomic phenomena is impossible. Although the notions of stationary states, of the transitions between them, and the notions associated with the then newly established Kramers-Heisenberg scattering theory were at that time still at the forefront of his interest, he already declared in August 1925 that "in contrast to ordinary mechanics, the new quantum mechanics does not deal with a space-time description of the motion of atomic particles."[16] In the fall of 1926, it seems, he must have arrived at the critical step that led from the correspondence principle to the complementarity principle. As mentioned previously, it is a mistake to assert, as did John Whitt-Hansen,[17] that "with point of departure in Heisenberg's relation of indeterminacy he [Bohr] formulated in 1927 his famous Principle of Complementarity," or as Bedau and Oppenheim[18] declared, "the discovery of the uncertainty principle motivated Bohr's

[14]Ref. 1 (1934, p. 54).

[15]N. Bohr, "Atomic theory and mechanics," reprinted in Ref. 1 (1934, pp. 25–51; 1929, pp. 20–39; 1931, pp. 16–33).

[16]*Loc.cit.*, p. 48.

[17]J. Whitt-Hansen, "Some remarks on philosophy in Denmark," *Philosophy and Phenomenological Research* **12**, 377–391 (1952).

[18]H. Bedau and P. Oppenheim, "Complementarity in quantum mechanics—A logical analysis," *Synthese* **13**, 201–232 (1961).

proposal of complementarity." The ascribing of unqualified classical attributes to micro-objects, Bohr continued in his Como lecture, involves a certain ambiguity, as illustrated, for example, by the well-known dilemma concerning the undulatory and corpuscular properties of light or electrons. "The two views of the nature of light," said Bohr, "are [indeed] to be considered as different attempts at an interpretation of experimental evidence in which the limitation of the classical concepts is expressed in complementary ways."

Whereas the ordinary electromagnetic theory provides a satisfactory description of the propagation of light in space and time, the conservation of energy and momentum during any interaction between radiation and matter—the causal aspect of optical phenomena, in Bohr's view—"finds its adequate expression just in the light quantum idea put forward by Einstein."[19] The physics of elementary particles of matter (electrons, etc.) presents an analogous situation.

In the sequel of his Como lecture Bohr discussed the problem of how far, in spite of the renunciation of a space-time and, at the same time, causal description in microphysics, the classical mode of description can be applied, which involves, of course, the Heisenberg relations. Starting this discussion with a reference to the well-established Planck-de Broglie-Einstein equations

$$E = h\nu \tag{1}$$

$$p = h\sigma \tag{2}$$

which connect the energy E and the momentum p with the frequency ν and the wave number σ, respectively—or in Bohr's notation

$$E\tau = p\lambda = h \tag{3}$$

where τ and λ are the period of vibration and wave length, respectively—Bohr remarked: "While energy and momentum are associated with the concept of particles, and, hence, may be characterized according to the classical point of view by definite space-time co-ordinates, the period of vibration and wave-length refer to a plane harmonic wave train of unlimited extent in space and time." A connection with the ordinary mode of description, Bohr continued, can be established with the aid of the superposition principle since it enables us to identify wave packets with particles, in view of de Broglie's well-known result[20] according to which the

[19] Ref. 1 (1934, p. 55).

[20] $v_{\text{group}} = d\omega/dk = d\nu/d\sigma = dE/dp = d(p^2/2m)/dp = p/m = v$.

group velocity of the wave field is equal to the translational velocity of the particle associated with the field. The association of a particle with a wave packet, Bohr pointed out, demonstrates the complementarity character of the description almost *ad oculos*; for "the use of wave groups is necessarily accompanied by a lack of sharpness in the definition of period and wave-length, and hence also in the definition of the corresponding energy and momentum" as given by the relation (3).

Referring to plane waves $A\cos 2\pi(\nu t - x\sigma_x - y\sigma_y - z\sigma_z)$ and to the equations

$$\Delta t \cdot \Delta\nu = \Delta x \cdot \Delta\sigma_x = \Delta y \cdot \Delta\sigma_y = \Delta z \cdot \Delta\sigma_z = 1, \tag{4}$$

well known from the classical theory, Bohr derived from (3) the Heisenberg relations

$$\Delta t \cdot \Delta E = \Delta x \cdot \Delta p_x = \Delta y \cdot \Delta p_y = \Delta z \cdot \Delta p_z = h, \tag{5}$$

which express a general reciprocal relation between the maximum sharpness of definition of the space-time variables (t,x,y,z) and the energy-momentum variables (E,p_x,p_y,p_z) associated with the micro-object. "This circumstance," continued Bohr, "may be regarded as a simple symbolical expression for the complementary nature of the space-time description and the claims of causality."

It will have been noted that Bohr's derivation of the indeterminacy relations (for reasons to be explained later, the following remark is particularly important for the time-energy relation) differs fundamentally from Heisenberg's derivation. To bring this fact into full relief for later reference we analyze Bohr's approach in greater detail. Bohr examined the time dependence of the interference of the Fourier components $\exp[2\pi i(px - Et)/h]$ into which he resolved the wave function of the particle. The time lapse between constructive interference and destructive interference at a fixed point in space must at least be equal to the reciprocal of the frequency spread $\Delta E/h$ between the components to produce the required changes in the relative phases. Assuming that the particle passes through the fixed point under discussion at any instant within the time interval during which the interference at this point is constructive, Bohr concluded that the spread Δt of the values of the possible transit times satisfies the relation $\Delta t = h/\Delta E$ where ΔE is the spread of energy values in the Fourier decomposition of the wave function.

This, then, was the original version of the complementarity interpretation of quantum mechanics as presented by Bohr in the fall of 1927. It is which Born, Kramers, Heisenberg, Fermi, and Pauli participated, the real issue of Bohr's paper was hardly touched upon. For those who heard Bohr's ideas for the first time it was probably too difficult to comprehend their full significance. Thus, for example, Léon Rosenfeld, who later became one of the most eloquent proponents of complementarity, said of Bohr's lecture: "I did not see, I did not feel, any of the subtlety that was in it";[22] Wigner, having heard about the Como lecture, remarked that Bohr's talk "will not induce any one of us to change his own opinion about quantum mechanics".[23] And von Neumann is said to have remarked: "Well, there are many things which do not commute and you can easily find three operators which do not commute."[24]

Von Neumann's criticism was, of course, prompted by Bohr's contention that the complementarity of the wave and particle pictures is adequately reflected in the noncommutativity of the corresponding variables (or operators). And it raised the question why "complementarity" had been confined to two properties only and had not—perhaps in extension to its purely linguistic use—been generalized to three or more components. A priori it is of course conceivable that, say, among three components each one "complements" the combination of the other two. This question was explicitly studied by C. F. von Weizsäcker,[25] who declared that the wave-particle duality constitutes a *complete* disjunction: physical reality is either pointlike concentrated or else spread out in space; the former is described by the model of a particle, the latter by that of a field or wave.

But let us return to Bohr's Como lecture. To say that this lecture "stirred the Congress as the Mistral sometimes rolls the ordinarily calm waters of Como"[26] is certainly an exaggeration. In fact, it took some time until the real significance of Bohr's conceptions was fully understood and until they were hailed as "opening a new chapter in our understanding of the universe we live in"[27] or as "the most revolutionary philosophical conception of our day."[28]

[21]"Discussione sulla comunicazione Bohr," Ref. 1 (Zanichelli, 1928, pp. 589–598).

[22]Interview with L. Rosenfeld, July 1, 1963 (Archive for the History of Quantum Physics).

[23]Ibid.

[24]Interview with E. P. Wigner, November 21, 1963 (Archive for the History of Quantum Physics).

[25]C. F. von Weizsäcker, "Zur Deutung der Quantenmechanik," *Zeitschrift für Physik* **118**, 489–509 (1941).

[26]R. Moore, *Niels Bohr* (Knopf, New York, 1966), p. 162.

[27]Ref. 8.

[28]J. A. Wheeler, "A septet of Sibyls," *American Scientist* **44**, 360–377 (1956).

4.2. CRITICAL REMARKS

Thus far we have given an almost verbatim report of Bohr's first version of his complementarity interpretation and part of its historical background. Let us now review the situation from a more critical point of view.

First it should be recalled that Bohr never gave a clear-cut explicit definition of the term "complementarity." Among all his statements probably the nearest to such a definition was made in 1929, when he declared that the quantum postulate "forces us to adopt a new mode of description designated as *complementary* in the sense that any given application of classical concepts precludes the simultaneous use of other classical concepts which in a different connection are equally necessary for the elucidation of phenomena."[29] According to this statement modes of description or descriptions are complementary.

This tallies perfectly well with his use of this term in his Como lecture where he used the adjective "complementary" 15 times, always in combinations like "the complementary character (nature, features) of the description," and the noun "complementarity" three times, once when he pointed out that "the quantum postulate presents us with the task of developing a 'complementarity' theory," once when he spoke of the "complementarity of the possibilities of definition," and, in the third instance, which deserves special consideration, when he concluded his talk with the words: "I hope, however, that the idea of complementarity is suited to characterize the situation, which bears a deep-going analogy to the general difficulty in the formation of human ideas, inherent in the distinction between subject and object." In almost all cases the complementary features of the description are explicitly mentioned as "the space-time coordination" (or description) and "the claim (or demand) of causality," the latter being generally understood or explicitly stated as referring to the conservation theorems of energy and momentum.

Let us for the time being ignore the last sentence of the Como lecture and instead consider other statements made by Bohr at various occasions such as the following: "Indeed, every experimental arrangement permitting the registration of an atomic particle in a limited space-time domain demands fixed measuring rods and synchronized clocks which, from their very definition, exclude the control of momentum and energy transmitted to them. Conversely, any unambiguous application of the dynamical conservation laws in quantum physics requires that the description of the phenomena involve a renunciation in principle of detailed space-time

[29] "Introductory survey," in Ref. 1 (1929). Cf. Ref. 1 (1934, p. 10).

coordination."[30] Then the following observations may be made.

Whenever, in the history of physics, a new fundamental principle has been proposed, it was established on the basis of a standard thought-experiment. For the principle of the conservation of momentum in classical mechanics it was the collision of two ideally elastic spheres in empty space, for the principle of equivalence in general relativity it was Einstein's well-known *Gedankenexperiment* of the laboratory in an elevator. For Bohr's principle of complementarity it was the experimental setup of a micro-object (photon, electron) passing through a slit in a diaphragm. If in this arrangement the diaphragm is rigidly connected with the frame of the local coordinate system of scales and clocks, the position of the micro-object (up to the, in principle, arbitrarily small width of the slit) is ascertainable, whereas any information concerning the exact energy or momentum exchange between the micro-object and the diaphragm is lost owing to the rigid connection of the diaphragm with the frame. If, on the other hand, the diaphragm with its slit is suspended by weak springs, the momentum transfer (manifested by the motion of the diaphragm of known mass) is ascertainable, whereas any information concerning the exact position of the passing micro-object is forgone owing to the indeterminate location of the diaphragm.

Generalizing this result Bohr contended that descriptions in terms of space-time coordinates and descriptions in terms of energy-momentum transfers or, more briefly, spatiotemporal and causal descriptions cannot both be operationally significant at the same time, since they require mutually exclusive experimental arrangements. The mutual exclusiveness of the experimental arrangements in the case of Bohr's standard thought-experiment was warranted by the fact that the existence or absence of a rigid connection between the diaphragm and the coordinating frame are logically contradictory to each other.[31] Such experimental arrangements may be called "complementary," for, although mutually exclusive, they are jointly necessary—or complement each other—for an exhaustive description of the physical situation.

[30] N. Bohr, "Atoms and human knowledge," Lecture delivered at the Royal Danish Academy of Sciences in Copenhagen, October 1955, reprinted in *Daedalus* **87**, 164–175 (1958), and in Ref. 5 (1957, pp. 101–114; Wiley, 1958, pp. 83–93; Vieweg, 1958, pp. 84–95; 1961, pp. 81–90).

[31] This statement of Bohr's supplies the answer to a question raised by Béla Fogarasi: Why does the simultaneous use of complementary attributes lead to contradictions whereas a nonsimultaneous use of such attributes leads only to complementarity? Cf. B. Fogarasi, "Ist der Komplementaritätsgedanke widerspruchsfrei?" *Proceedings of the Second International Congress of the International Union for the Philosophy of Science* (Zürich, 1954) (Editions du Griffon, Neuchatel, 1955), pp. 46–52. Cf. also B. Fogarasi, *Kritik des Physikalischen Idealismus* (Aufbau-Verlag, Berlin, 1953).

The term "complementary" may subsequently be transferred to the modes of description associated with complementary experimental arrangements so that spatiotemporal and causal descriptions are complementary to each other. Finally, the term "complementary" may be carried over to the parameters or variables themselves in terms of which complementary descriptions are formulated so that a position coordinate and a momentum variable are called complementary to each other. To avoid confusion it should never be forgotten that this last use of the term "complementary" is justified only if the variables, so-called, are used in descriptions corresponding to complementary experimental arrangements.

Having thus clarified the meaning of Bohr's central conception, let us return to the Heisenberg relations. As shown by most textbooks on quantum mechanics, it is an easy matter to derive Heisenberg's indeterminacy formulae from a mathematical consideration of the very experimental arrangements which served as the starting-point for our clarification of the concept of complementarity. In many textsbooks, in fact, Heisenberg's famous gamma-ray microscope thought-experiment serves as an illustration and often as a demonstration of the Heisenberg formulae. Conceptually speaking, however, Heisenberg's experiment may be regarded as but a variation or modification of Bohr's experiment, although, historically speaking, Heisenberg conceived his experiment, independently of Bohr, in an early discussion with his Göttingen friend Borchert, the son of Paul Drude.

In what respect, then, did Bohr and Heisenberg differ, if they both started from an identical situation? Clearly, it was not a dispute about experimental facts or about the mathematical formalism. It was a difference of opinion as to how far interpretation was called for by the situation evolved. Heisenberg agreed with Bohr that any interpretation should make use of the terminology of classical physics. But whereas Heisenberg was satisfied with the fact that either the particle language or the wave language—and either of them independently of the other—could be used for optimal description, although with certain limitations which found their mathematical formulations in the very indeterminacy relations, Bohr insisted on the necessity of using both. For Bohr the indeterminacy relations indicated that not the classical concepts but the classical conception of explanation has to be revised. "Thus, according to the view of the author," wrote Bohr[32] in 1929, "it would be a misconception to believe that the difficulties of the atomic theory may be evaded by eventually replacing the concepts of classical physics by new conceptual forms." And in the same year he declared: "We must, in general, be prepared to accept the

[32]Ref. 29 (1934, pp. 15–16).

fact that a complete elucidation of one and the same object may require diverse points of view which defy a unique description."[33]

It was the breakdown of the classical ideal of explanation Bohr had in mind when, referring to the wave-particle duality of light, he rejected Heisenberg's approach. The fact that, on the one hand, Heisenberg, was able to derive his indeterminacy formulae as a deduction from the formalism of quantum mechanics and that, on the other hand, an independent demonstration of these formulae could be given through the analysis of an imaginary experimental setup if combined with a consideration of the duality aspect, was for Bohr, as we have mentioned already, a proof of the consistency of his complementarity interpretation of microphysics with the mathematical formulation of quantum mechanics.

For Bohr, the quantum mechanical inderterminism was a consequence of the wave-particle dualism and hence ultimately of the utilization of different pictures for the complementary modes of description or of the absence of a unified account of motion and change. As he declared in his Como lecture, "The measurement of the positional co-ordinates 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 the description of atomic phenomena...."[34] In other words, in Bohr's view, the indeterminism of quantum mechanics has its origin in the unavoidable "rupture" of description; for what Heisenberg later called the "reduction of the wave packet" was for Bohr, so to say, the switching over from one mode of description to its complementary mode.

The early formulations of the complementarity interpretation contained a number of ambiguities of expression. In fact, it was probably due to this vagueness of expression and the conceptual flexibility associated with it that the complementarity interpretation managed to survive serious crises. Most of these inconsistencies refer to the epistemological and ontological consequences involved. Probably the most serious inconsistency in Bohr's own presentation of his ideas concerned the just-mentioned complementarity explanation of the "reduction of the wave packet" or "rupture" of description, as Bohr called it. For apart from explaining this feature on the basis of complementarity, Bohr repeatedly justified it by pointing out that "every observation introduces a new uncontrollable element",[35] "that the

[33]Ref. 1 (1934, p. 96).
[34]Ibid. (p. 68).
[35]Ibid. (p. 68).

magnitude of the disturbance caused by a measurement is always unknown,"[36] and that "we cannot neglect the interaction between the object and the instrument of observation."[37] It is this "interference with the course of the phenomena, which is of such a nature that it deprives us of the foundation underlying the causal mode of description,"[38] declared Bohr. Here, as we see, quantum mechanical indeterminism is not regarded as a result of switching from one picture to its complementary mode but as the outcome of an operational physical feature. It was precisely this insistence on the operational explanation of "the reduction of the wave packet" that made Bohr's interpretation most vulnerable, as we shall see later.

A second conceptual difficulty that eventually became an argument against Bohr, or rather against the original interpretation of the Heisenberg relations, concerns the apparent retrodictability of exact joint values of the position and momentum variables of a micro-object. Such a possibility was admitted by Bohr in his Como lecture but denied any predictive significance. "Indeed," he said, "the position of an individual at two given moments can be measured with any desired degree of accuracy; but if, from such a measurement, we would calculate the velocity of the individual in the ordinary way, it must be clearly realized that we are dealing with an abstraction, from which no unambiguous information can be obtained." In his thought-experiment Bohr imagined that an exact position measurement at time t_1 shows a free micro-object of mass m to be at point x_1 and that a similar measurement at time $t_2=t_1+\Delta t$ shows it to be at point $x_2=x_1+\Delta x$. Hence, he claimed, the position x_1 and the momentum $m\cdot\Delta x/\Delta t$ has been accurately determined for $t=t_1$.

In his Chicago lecture[39] Heisenberg discussed a similar thought-experiment: He replaced the first position measurement of Bohr's arrangement by a velocity measurement and concluded that for any time between the two measurements position and momentum can be calculated with arbitrary precision. But, he added, "it is a matter of personal belief whether such a calculation concerning the past history of the electron can be ascribed any physical reality or not." It has generally not been noted, apparently not even by Heisenberg himself, that the gamma-ray microscope thought-experiment already shows the possibility of unlimited precision in retrodictive calculation. For, reverting to our exposition of the experiment in Chapter 3, if it is assumed that p_x was accurately known, λ could be taken arbitrarily small (we ignore of course high-energy

[36]Ibid. (p. 11).
[37]Ibid. (p. 93).
[38]Ibid. (p. 115).
[39]Ref. 3–19.

phenomena) so that the position uncertainty Δx becomes as small as desired. The enormous Δp_x, caused by the collision, is now irrelevant since it refers to the time after the collision. The path prior to the collision can be retrodicted with arbitrary accuracy.

Another inconsistency concerns Bohr's conclusion from the role of measurements in quantum mechanics which is often quoted as a proof that Bohr's complementarity interpretation is based on a intersubjective idealism. Referring to the fact that a subsequent measurement deprives to a certain degree the information obtained through a previous measurement of its predictive significance, Bohr concluded that these facts "not only set a limit to the *extent* of the information obtainable by measurement, but they also set a limit to the *meaning* which we may attribute to such information. We meet here in a new light the old truth that in our description of nature the purpose is not to disclose the real essence of the phenomena but only to track down, so far as it is possible, relations between the manifold aspects of our experience."[40] If Bohr rejected in this statement the possibility of a realistic account of microphenomena, it was certainly an inconsistency to maintain, as he did, "that we have been forced step by step to forego a causal description of the behavior of individual atoms in space and time, and to reckon with a free choice on the part of nature between various possibilities to which only probability considerations can be applied."[41]

Bohr realized very early that his complementarity interpretation presupposes the necessity of the use of classical terminology. "Only with the help of classical ideas is it possible to ascribe an unambiguous meaning to the results of observation," he wrote in an introduction to a Danish edition of some of his early writings, and "it lies in the nature of physical observation, that all experience must ultimately be expressed in terms of classical concepts,"[42] he declared in 1929. The necessity of using the language of classical physics when discussing observational facts followed for Bohr from our inability to forgo our usual forms of perception; it also makes it unlikely that the fundamental notions of classical physics could ever be abandoned for the description of physical experience.[43]

At that time Bohr did not discuss the logical problem his insistence upon the use of classical physics gave rise to, that is, whether the use of classical

[40]Ref. 1 (1934, p. 18).

[41]Ref. 1 (1934, p. 4).

[42]Ref. 1 (1934, p. 94).

[43]Cf. on this point C. F. von Weizsäcker, "Niels Bohr and complementarity: The place of the classical language," in *Quantum Theory and Beyond*, Ted Bastin ed. (Cambridge University Press, Cambridge, 1971), pp. 23–31.

physics as a necessary prerequisite for the description (or measurement) of quantum phenomena is logically consistent with the recognition that quantum mechanics supplants classical physics. Or in other words, following von Weizsäcker: "Classical physics has been superseded by quantum theory; quantum theory is verified by experiments; experiments must be described in terms of classical physics."[44] We shall discuss this consistency problem, one of the major conceptual difficulties of Bohr's complementarity interpretation, in our account of the quantum mechanical measurement theory.

It will be useful at this stage to review the preceding exposition of Bohr's early notion of complementarity.

Bohr's reasoning may be schematically summarized by the following chain of logical deductions:[45]

1. Indivisibility of the quantum of action (quantum postulate).
2. Discontinuity (or individuality) of elementary processes.
3. Uncontrollability of the interaction between object and instrument.
4. Impossibility of a (strict) spatiotemporal and, at the same time, causal description.
5. Renunciation of the classical mode of description.

But, as we have just mentioned, any account of experimental evidence must be expressed, according to Bohr, in classical terms; however far the phenomena under discussion transcend the scope of classical physical explanation. This insistence obviously contradicts requirement 5 above. The only way to avoid such a contradiction would be by imposing certain limitations on the use of classical terms: The contradiction will be avoided if and only if the use of the classical terminology is delimited in such a way that it never encompasses a *complete* classical mode of description.

This condition is clearly satisfied if the experimental experience obtained under different arrangements can be described exhaustively only in terms of mutually exclusive sets of classical conceptions. Moreover, the indeterminacies, expressed by Heisenberg's relations, are precisely the price we have to pay if we nevertheless attempt to apply such mutually exclusive sets of classical conceptions simultaneously. The most important sets of classical conceptions are compatible in classical physics but are mutually exclusive in quantum physics, although both are necessary for an exhaustive account; these are the space-time and causal descriptions (the latter involving conservation theorems of energy and momentum).

[44] Ibid. (p. 26).

[45] In an interview on July 26, 1971, Heisenberg confirmed that this scheme is an accurate presentation of Bohr's reasoning in 1927.

4.3. "PARALLEL" AND "CIRCULAR" COMPLEMENTARITY

The preceding exposition of Bohr's complementarity interpretation—we shall call it briefly the Pauli version (of the Copenhagen interpretation), in view of its plausibility and intuitive clearness perhaps the most widely held view—agrees with most statements made by Bohr in his Como lecture and in his later discussions on this subject. But it can hardly be borne out by the last statement of the Como lecture, quoted above, and by the fact that nowhere in this lecture did Bohr refer to position and momentum as complementary quantities, although on various occasions he could easily have done so.

For this reason and particularly because of Bohr's intimation that a situation characterized by the idea of complementarity "bears a deep-going analogy to the general difficulty in the formation of human ideas, inherent in the distinction between subject and object" complementary descriptions, as conceived by Bohr, seem also to be associated with different object-subject relations, that is, are descriptions made from different vantage points. This difference may not only find its expression in the diversity of the experimental arrangements underlying the observation, in conformance with the Pauli version of the Copenhagen interpretation; this difference may also be the result of a change in the very structure of the relation between object and subject, for instance, if the perceiving subject itself forms part of the observed object.

Thus the mode of space-time description which presupposes an interaction between the object and the observer necessarily includes in its description some features of the observing subject, whereas the mode of causal description deliberately avoids such references. Just as "*in certain persons*, at least, *the total possible consciousness may be split into parts which coexist but mutually ignore each other*, and share the objects of knowledge between them,"[46] so in physics certain cognitions which coexist and harmonize in the classical theory are split, in quantum mechanics, into mutually exclusive views complementary to each other.

"A complete elucidation of one and the same object may require diverse points of view which defy a unique description. Indeed, strictly speaking, the conscious analysis of any concept stands in a relation of exclusion to its immediate application."[47]

The logical relation between the usual conception of complementarity as exemplified in the complementarity between position and momentum

[46]W. James, *The Principles of Psychology* (Holt, New York, 1890; Dover, New York, 1950), Vol. 1, p. 206. Concerning James' influence on Bohr see Ref. 1-1 (pp. 176–179).
[47]Ref. 1 (1934, p. 96).

(Pauli's version), on the one hand, and Bohr's just-mentioned characterization of the epistemological structure of complementarity, on the other hand, was a subject in which C. F. von Weizsäcker became greatly interested. After a study of Bohr's writings he came to the conclusion[48] mentioned previously that the complementarity between position and momentum is something completely different from the complementarity between space-time description and causal description or a description in terms of the Schrödinger function. The former he called "parallel complementarity" because it holds between two concepts (position, momentum) which both belong to the same intuitive picture of the physical process and both have to have definite values in classical physics if the state of the system is to be completely defined. In contrast, the relation between a space-time description and the Schrödinger function is called by von Weizsäcker "circular complementarity"; these two, never combined in any classical model, condition each other mutually in the sense that the space-time description is needed for the description of observations on the basis of which the Schrödinger function can be set up in each given situation and the Schrödinger function is needed to make the best possible statistical predictions of classical measurement results.

According to von Weizsäcker's conclusion, Bohr's original idea of complementarity was the circular conception; when Heisenberg found his indeterminacy relations, Bohr interpreted them, on the basis of circular complementarity, as an indication that the classical model of a particle cannot strictly be applied to microphysics since its mechanical behavior can be predicted only by referring to the complementary Schrödinger function. So far, no complementarity of position and momentum is involved. Their complementarity ("parallel complementarity") belongs to a different context of ideas.

The situation, according to von Weizsäcker, is even more complicated by the fact that Bohr associated energy and momentum misleadingly with the wave picture, obviously on the basis of the Planck-de Broglie equations, so that one is tempted to identify the complementarity between particles and waves with that between position and momentum. If, however, a plane wave is regarded as the Schrödinger field of a free single particle there corresponds a definite value for the momentum of a particle; however, if the wave has practically the form of a delta-function, it represents in the same interpretation as before a particle which has a definite position at the time under discussion. The complementarity between particle and wave is consequently a third kind of complementarity and cannot be logically reduced to the former two.

[48]Ref. 11.

Bohr[49] rejected categorically von Weizsäcker's differentiation between various kinds of complementarity on the ground that the mathematical formalism of quantum mechanics with the inclusion of the Schrödinger function is merely an algorithm which provides "an exhaustive description of quantum phenomena in a wide area of experience" but, not being itself a physical phenomenon, cannot stand in the relation of complementarity to directly recorded observations; complementarity can hold between phenomena alone. Bohr's answer did not satisfy von Weizsäcker, for whom, as we shall see in Chapter 8, the formalism of quantum mechanics was much more than just an algorithm. In a letter[50] to Pauli von Weizsäcker compared Bohr with Columbus, who sailed on his westbound voyage in accordance with a correct theory but arrived at an unexpected continent without noticing the error. Von Weizsäcker was well aware of the boldness of such a statement made about someone "of whom he is inclined to believe, more than of any other living thinker, that he knows what he says."

4.4. HISTORICAL PRECEDENTS

Although it was not easy, as we see, to define Bohr's notion of *complementarity*, the notion of *complementarity interpretation* seems to raise fewer definitory difficulties. The following definition of this notion suggests itself. A given theory T admits a complementarity interpretation if the following conditions are satisfied: (1) T contains (at least) two descriptions D_1 and D_2 of its substance-matter; (2) D_1 and D_2 refer to the same universe of discourse U (in Bohr's case, microphysics); (3) neither D_1 nor D_2, if taken alone, accounts exhaustively for all phenomena of U; (4) D_1 and D_2 are mutually exclusive in the sense that their combination into a single description would lead to logical contradictions.

That these conditions characterize a complementarity interpretation as understood by the Copenhagen school can easily be documented. According to Léon Rosenfeld,[51] one of the principal spokesmen of this school, complementarity is the answer to the following question: What are we to do when we are confronted with such a situation, in which we have to use two concepts that are mutually exclusive, and yet both of them necessary for a complete description of the phenomena? "Complementarity denotes

[49]Ref. 12.

[50]Letter from von Weizsäcker to Pauli, dated August 27, 1956.

[51]L. Rosenfeld, "Foundations of quantum theory and complementarity," *Nature* **190**, 384–388 (1961).

the logical relation, of quite a new type, between concepts which are mutually exclusive, and which therefore cannot be considered at the same time—that would lead to logical mistakes—but which nevertheless must both be used in order to give a complete description of the situation." Or to quote Bohr himself concerning condition (4): "In quantum physics, evidence about atomic objects by different experimental arrangements...-appears contradictory when combination into a single picture is attempted."[52] Born once summarized Bohr's view by saying: "There is no unique image of our whole world of experience." In fact, Bohr's Como lecture with its emphasis on the mutual exclusiveness but simultaneous necessity of the causal (D_1) and the space-time description (D_2), that is, Bohr's first pronouncement of his complementarity interpretation, forms an example which fully conforms with the preceding definition. Bohr's discovery of complementarity, it is often said, constitutes his greatest contribution to the philosophy of modern science.

However, having defined the idea of a complementarity interpretation in rather general terms and without any commitments as to the nature of U, we can easily find in the history of human thought much earlier examples of conceptual structures that satisfy all the conditions (1) to (4). One of the earliest instances was probably the treatment of the concept of motion in the famous paradoxes with which Zeno of Elea in the fifth century B.C. confounded his contemporaries (and posterity as well). A modern writer once summarized their essence as follows: "The human mind, when trying to give itself an accurate account of motion, finds itself confronted with two aspects of the phenomenon. Both are inevitable but at the same time they are mutually exclusive."[53]

Another classical example is the medieval doctrine of "double truth" [*duplex veritas*] which originated in the writings of the twelfth-century philosopher Ibn-Rushd (Averroes)[54] or was at least ascribed to him by his contemporary adversaries. This doctrine was taught by the Latin

[52]N. Bohr, "Quantum physics and philosophy," in *Philosophy in the Mid-Century*, R. Klibansky, ed. (La Nuova Italia Editrice, Florence, 1958), Vol. 1, pp. 308–314, quotation on p. 311; reprinted in N. Bohr, *Essays 1958–1962 on Atomic Physics and Human Knowledge* (Interscience, London, 1963), pp. 1–7; "Erkenntnisfragen der Quantenphysik" in *Max-Planck-Festschrift* (Deutscher Verlag der Wissenschaften, Berlin, 1958), pp. 169–175. "Kvantovaya fizika i filosofia," *Uspekhi Fizičeskih Nauk* **67**, 37–42 (1959); *Voprosy Filosofii* **1964** (8), 53–58; *Atomfysik og Menneskelig Erkendelse* (Schultz, Copenhagen, 1964), pp. 11–18.

[53]H. Fränkel, "Zeno of Elea's attacks on plurality," *American Journal of Philology* **63**, 1–25, 193–206 (1942), quotation on p. 8.

[54]Ibn-Rushd, *Kitâb facl el maqâl wataqrîr ma bain ech-charîk wal'hikma min el-ittical* [Treatise on the Accord between Religion and Philosophy]. Cf. J. Rosenfeld, *Die doppelte Wahrheit* (Scheitlin, Bern, 1914).

Averroists and was discussed by Duns Scotus and Siger of Brabant.[55] It declared that two different theses, such as a theological account (D_1) and a philosophical account (D_2) of the same substance-matter—for example, the Biblical teaching of the world's creation and the Aristotelian contention of the world's eternity—may both be true even if their logical conjunction leads to a "flat contradiction."

Finally, an analogy may also be drawn, though perhaps only in very general terms, between the Copenhagen approach and a very ancient conception of the nature of physical experimentation. To understand this point, let us recall the following. Bohr's complementarity philosophy had its point of departure, as we have seen, in the idea that spatiotemporal and simultaneously causal descriptions of microphysical processes exclude each other, an idea which was based, in turn, on the recognition that an experimental procedure leading to one kind of description is incompatible with an experimental procedure leading to the other kind of description. The fact that every experimental act or measurement precludes the possibility of obtaining additional (complementary) information may also be expressed by saying that every experiment is an interference with nature which effaces some of nature's (otherwise realizable) potentialities. The idea that the normal course of nature, regulated in its time development by the Schrödinger equation, is brusquely interfered with by the act of observation (experiment or measurement) and is thus diverted into enforced channels—an idea which was stressed especially by Pascual Jordan as we shall see in Section 6.1—has often been expressed by theorists outside the Copenhagen school.

In brief, an experiment is a violent interference with the regular course of nature. This idea is as old as physical thought. In fact, it was precisely this idea which discouraged, or rather deterred, the ancient Greeks from developing a systematic experimental method in their study of physical nature. True, sociological and other reason may also have played an important role in this respect. But it cannot be doubted that in ancient Greece the experiment was regarded as an interference with nature or its course as ordained by the Deity: to perform an experiment was an act of "insolence" [*hybris*] for which punishment has to be paid, as illustrated by the stories of Prometheus, Daedalus and Icarus, and others. For the Greeks it was a matter of religious retribution, for Bohr a matter of epistemological renunciation.

The feature that distinguishes Bohr's complementarity interpretation from all such historical precedents and gives it a unique place in the

[55]Cf. P. Mandonnet, *Siger de Brabant et l'Averroisme Latin au XIIIe Siècle* (Fribourg, 1899, 2nd ed. Louvain, 1908–1911).

history of human thought is, of course, the fact that Bohr's ideas were not merely abstract speculations but were firmly surrounded by empirical findings. In fact, they were conceived precisely in order to cope with the paradoxes of experimental observation.

In Section 6.5, dealing with Bohr's relational conception of the quantum mechanical state, the reader's attention will be drawn to further historical precedents analogous to Bohr's ideas.

The BOHR-EINSTEIN Debate

Chapter Five

5.1. THE FIFTH SOLVAY CONGRESS

The first enunciation of the complementarity interpretation at the Como Congress was regarded by Bohr more as a program for further elaboration than as a definite statement of an immutable dogma and he welcomed any opportunity for a critical discussion of both the foundations and the implications of his ideas. Regarding it as "a welcome stimulus to clarify still further the role played by the measuring instruments,"[1] he accepted the invitation to participate at the Fifth Physical Conference of the Solvay Institute which was to convene in Brussels, under the chairmanship of Lorentz, from October 24 to 29, 1927.

Born, Bragg, Brillouin, de Broglie, Compton, Debye, Dirac, Ehrenfest, Fowler, Heisenberg, Kramers, Pauli, Planck, Richardson, and Schrödinger also accepted the invitation, for they immediately realized that the official program of the Congress, "Electrons and Photons,"[2] was but to set the stage for a top-level discussion on one of the most pressing problems of the time: the meaning of "the new quantum theory." It also became known that Einstein, would attend this meeting. Bohr had met Einstein in 1920 during a visit to Berlin and knew of his reluctance to renounce continuity and causality. Yet, Bohr cherished the hope that the positivistic element in the complementarity interpretation would make Einstein change his mind, for Bohr was convinced, as were so many others later, that Einstein's philosophy of science, in its early stages undoubtedly influenced by Mach, continued to be primarily positivistic.[3] Bohr did not expect to fare with Einstein as Einstein fared with Mach.

It was therefore in the mood of great expectations that on Monday morning, October 24, 1927, the world's leading physicists assembled to

[1]Ref. 4-10.

[2]*Electrons et Photons—Rapports et Discussions du Cinquième Conseil de Physique tenu à Bruxelles du 24 au 29 Octobre 1927 sous les Auspices de l'Institut International de Physique Solvay* (Gauthier-Villars, Paris, 1928).

[3]Philipp Frank, in his biography of Einstein, *Einstein—His Life and Times* (Knopf, New York, 1947), p. 215, described how only in 1929 he learned suddenly at a congress of German physicists in Prague of Einstein's "partly antagonistic attitude toward the positivistic position" and its possible connection with his attitude toward Bohr's conception of atomic physics. Cf. also p. 99 in C. Lanczos, "Die neue Feldtheorie Einsteins," *Ergebnisse der exakten Naturwissenschaften* **10**, 97–132 (1931) and L. Rosenfeld, "The epistemological conflict between Einstein and Bohr," *Zeitschrift für Physik* **171**, 242–245 (1963), where Einstein's epistemological development is described as having proceeded from Machian positivism through Poincaré's conventionalism to an idealistic conception of physical determinism bordering on "mystification." The historical roots of Einstein's anti-positivistic attitude have been studied by G. Holton, "Influences on Einstein's early work in relativity theory," *Organon* **3**, 225–244 (1966); "Where is reality? The answers of Einstein," *Talk at UNESCO meeting "Science et Synthese"* (December 1965, Paris).

exchange their views on a question that was embroiling all of physics. After an opening address by Lorentz, W. L. Bragg lectured on the reflection of x-rays and A. H. Compton on the disagreement between experiment and theory of electromagnetic radiation. The first speaker on the main topic of the meeting was Louis de Broglie, who delivered a lecture entitled "La nouvelle dynamique des quanta." After reviewing Schrödinger's work on wave mechanics and Born's probabilistic particle interpretation of the ψ-function and thus emphasizing the success of both the wave and particle conceptions he asked: "Comment peut-on concilier l'existence d'éléments ponctuels d'énergie avec le succés des théories qui en visagent les ondes ψ?"

"Quel lien doit-on établir entre les corpuscules et les ondes? Ce sont les questions capitales qui se posent dans l'état présent de la mécanique ondulatoire."

As an answer de Broglie advanced a theory based on the guidance formula of his "theory of the double solution" but not on the singularity solution u of that theory. Considering the case of a single particle in an electromagnetic field defined by the potentials U and A, and writing $\psi = a\exp(2\pi i\varphi/h)$, where the real function φ corresponds to Jacobi's function S (as noted previously), de Broglie obtained for the velocity of the particle the guidance formula

$$v = -c^2 \frac{\nabla\varphi + (e/c)A}{\partial\varphi/\partial t - eU} \tag{1}$$

which in the absence of an electromagnetic field reduces to the formula $v = -\nabla\varphi/m$.

Clearly, de Broglie emphasized, this formula determines completely the motion of a particle if its initial position is known; if it is not, the probability of the particle's presence at a given position in space can be calculated with the help of ψ. The wave function ψ, de Broglie pointed out, thus plays a twofold role: it is a *probability wave*, but it is also a *pilot wave* [*onde pilote*], for via the guidance formula it determines the trajectory of the particle in space. The "pilot-wave theory," as de Broglie called this truncated version of his original "theory of the double solution," is therefore a deterministic or causal theory of microphysical phenomena. The theory, as de Broglie explained for the case of the hydrogen atom in one of its stable states, indeed ascribes precise values of position and velocity to particles even in atomic systems.

De Broglie's theory of the pilot wave found little favorable acceptance among the participants of the meeting. In fact, it was hardly discussed at all. The only serious reaction came from Pauli,[4] who remarked that de Broglie's conception, though perhaps compatible with Born's theory of elastic collisions as far as the statistical results of experiments are concerned, becomes untenable as soon as inelastic collisions are considered. In view of its historical importance in the present context—for it was primarily because of Pauli's criticism of similar arguments that de Broglie's causal theory was thought to have been definitely disproved until the early 1950s—and in view of its theoretical importance in a later context (hidden-variable theories), Pauli's objection will be discussed in some detail.

Pauli based his objection on an analysis of the collision between a particle and a plane rigid rotator which Fermi[5] carried out only a few weeks after Born had published his pioneer work on collision phenomena. It concerns a particle of mass m, moving along the x-axis (Fermi considered the more general case of motion in the xy-plane), and a plane rotator at the origin, with moment of inertia J and characterized by the azimuthal angle ϕ. The interaction potential $U(x,\phi)$, periodic in ϕ with the period 2π, differs from zero only for small x. The state function $\psi(x,\phi)$ of the total system satisfies the Schrödinger equation

$$-\left(\frac{\hbar^2}{2m}\right)\frac{\partial^2\psi}{\partial x^2}-\left(\frac{\hbar^2}{2J}\right)\frac{\partial^2\psi}{\partial\phi^2}+U(x,\phi)\psi=E\psi \tag{2}$$

where E is the total energy. Introducing $\xi=\sqrt{m}x$, $\zeta=\sqrt{J}\,\phi$, $V=\sqrt{E/2}$, and $\nu=E/h$ for the frequency of the wave, Fermi obtained

$$\frac{\partial^2\psi}{\partial\xi^2}+\frac{\partial^2\psi}{\partial\zeta^2}-\left(\frac{1}{V^2}\right)\frac{\partial^2\psi}{\partial t^2}=\left(\frac{2}{\hbar^2}\right)U(\xi,\zeta)\psi. \tag{3}$$

Since U, now periodic in ζ with period $d=2\pi\sqrt{J}$, differs from zero only for small ξ, the ζ-axis in the $\xi\zeta$-configuration space plays the role of a grating with the grating constant d and diffracts the incoming wave ψ. The initial wave of the particle, moving with velocity v_0 and kinetic energy $E_1=\frac{1}{2}mv_0{}^2$ is obviously

$$\psi_{\text{particle}}=A_1\exp\left[\frac{i\left(E_1t-\sqrt{2E_1}\,\xi\right)}{\hbar}\right],$$

[4]Ref. 2 (pp. 280–282).

[5]E. Fermi, "Zur Wellenmechanik des Stossvorganges," *Zeitschrift für Physik* **40**, 399–402 (1926).

while the initial wave of the rotator, rotating with angular velocity ω_0 and kinetic energy E_2, is

$$\psi_{\text{rotator}} = A_2 \exp\left[\frac{i\left(E_2 t - \sqrt{2E_2}\,\zeta\right)}{\hbar}\right]. \tag{4}$$

Because of the periodicity condition

$$E_2 = \frac{n^2\hbar^2}{2J} \qquad (n \text{ integral}). \tag{5}$$

With $\cos\alpha = (E_2/E)^{1/2}$, $\sin\alpha = (E_2/E)^{1/2}$, where $E = E_1 + E_2$ and $\lambda = h/(2E)^{1/2}$, the total incident wave ψ_0 can be written in the form

$$\psi_0 = A \exp\left[2\pi i\left(\frac{Et}{h} - \frac{\xi\sin\alpha + \zeta\cos\alpha}{\lambda}\right)\right], \tag{6}$$

that is, as a plane monochromatic wave of wave length λ and frequency $\nu = E/h$, making an angle α with the ξ-axis.

By this ingenious trick Fermi reduced the collision process to a diffraction phenomenon in the $\xi\zeta$-configuration space. If β denotes the diffraction angle (with respect to the grating plane) of the diffracted wave of maximum intensity, a simple calculation shows that β satisfies the condition

$$2\pi\sqrt{J}\,(\cos\beta - \cos\alpha) = k\lambda \qquad (k \text{ integral})$$

so that the wave function after the interaction (diffraction) is the following superposition of plane waves:

$$\psi_f = \sum_\beta a_\beta \exp\left[2\pi i\left(\frac{Et}{h} - \frac{\xi\sin\beta + \zeta\cos\beta}{\lambda}\right)\right] \tag{7}$$

or, equivalently,

$$\psi_f = \sum_\beta a_\beta \exp\left[\frac{2\pi i}{h}(Et - p_\beta x - \pi_\beta \phi)\right] \tag{8}$$

where $p_\beta = \sqrt{2mE}\,\sin\beta$ and $\pi_\beta = \sqrt{2JE}\,\cos\beta$. If the final state of the total system is observed, reduction of the wave packet shows that p_β and π_β are the momentum of the particle and of the rotator, respectively, after the collision. Clearly, $p_\beta{}^2/2m + \pi_\beta{}^2/2J = E$ for all β (conservation of energy).

From the diffraction condition it follows that

$$\cos\beta = \cos\alpha + \frac{k\hbar}{(2JE)^{1/2}} \tag{9}$$

and since

$$\cos\alpha = \left(\frac{E_2}{E}\right)^{1/2} = \frac{n\hbar}{(2JE)^{1/2}} \tag{10}$$

we obtain

$$\cos\beta = \frac{(n+k)\hbar}{(2JE)^{1/2}} \tag{11}$$

so that the energy of the rotator after the collision turns out to be

$$\frac{\pi_\beta{}^2}{2J} = E\cos^2\beta = (n+k)^2\frac{\hbar^2}{2J}\,. \tag{12}$$

For $k=0$ the collision is "elastic," for $k>0$ "inelastic of the first kind," and for $k<0$ "inelastic of the second kind." So much about Fermi's calculation.

Pauli now pointed out that if, as de Broglie suggested, the final state function ψ_f were written in the form $\psi_j = a\exp(2\pi i\varphi/h)$, a and φ being real functions, the phase φ would be a most complicated function and the corresponding motion of the system in the configuration space would be incompatible with the final quantized state of the rotator, a state well confirmed by experiment. "La manière de voir de M. de Broglie ne me semble donc pas compatible avec l'exigence du postulat de la théorie des quanta, que le rotateur se trouve dans un état stationnaire aussi bien avant le choc qu'après," concluded Pauli.

De Broglie tried to rebut Pauli's criticism by arguing that just as in classical optics one cannot speak of a grating diffracting a beam in a given direction unless both the grating and the incident wave are laterally limited, so in the present case the ψ_f wave must likewise be regarded as being laterally limited in configuration space; if this is granted, the velocity of the representative point of the system will be constant and will correspond to a stationary state of the rotator. But de Broglie's argumentation sounded too much ad hoc to convince any of the participants. In fact, even de Broglie himself now began to doubt whether the ψ-wave could be conceived as a real physical field, propagating as it usually is in a multidimensional, and hence fictitious, configuration space, and whose coordinates seem to represent positions that are not *actually* but only *potentially* occupied by the particle. Thus when he was invited in early 1928 to lecture at the University of Hamburg,[6] de Broglie publicly embraced for the first time the complementarity interpretation. And when in the fall of that year he assumed his position at the Paris *Faculté des Sciences*, he thought it unjustifiable to teach in his course[7] a theory whose validity he doubted. He thus joined the ranks of the adherents to the orthodox interpretation which was accepted by the overwhelming majority of the participants at the Solvay meeting.

After de Broglie, Born and Heisenberg presented their paper on matrix mechanics, the transformation theory, and its probabilistic interpretation. Referring to the indeterminacy relations they commented that "the real meaning of Planck's constant h is this: it constitutes a universal gauge of the indeterminism inherent in the laws of nature owing to the wave-particle duality." In concluding their lecture they made the provocative statement: "We maintain that quantum mechanics is a complete theory; its basic physical and mathematical hypotheses are not further susceptible of modifications." The next speaker was Schrödinger, who gave a paper on wave mechanics and, in particular, on the treatment of a many-body system in terms of this theory. The climax of the meeting was the general debate at the end of the conference.

The discussion was opened by Lorentz with some introductory remarks in which he expressed his dissatisfaction with the rejection of determinism in atomic physics, as proposed by the majority of the speakers. Although admitting that Heisenberg's indeterminacy relations impose a limitation on observation, he objected to regarding the notion of probability as an axiom a priori at the beginning of the interpretation instead of putting it at the end as a conclusion of theoretical considerations. "Je pourrais toujours garder ma foi déterministe pour les phénomènes fondamentaux, dont je n'ai pas parlé," he declared. "Est-ce qu'un esprit plus profond ne pourrait pas se rendre compte des mouvements de ces électrons. Ne pourrait-on pas garder le déterminisme en faisant l'objet d'une croyance? Faut-il nécessairement éxiger l'indéterminisme en principe?"

[6]Cf. L. de Broglie, "Souvenirs personnels sur les débuts de la mécanique ondulatoire," *Revue de Métaphysique et de Morale* **48**, 1–23 (1941).

[7]The substance of this course is contained in L. de Broglie, *Introduction à l'Etude de la Mécanique Ondulatoire* (Hermann, Paris, 1930).

After these provocative questions Lorentz called upon Bohr to address the meeting. Bohr accepted the invitation and spoke on his interpretation, repeating the substance of his Como lecture. It was clear that his words were addressed primarily to Einstein, who now heard Bohr's ideas on complementarity for the first time. As the *Rapports et Discussions* show, Einstein had not taken part in any of the discussions on the quantum theory during the official sessions of the Conference. Even now, at the conclusion of Bohr's talk, he remained silent.

The first to participate in the discussion of Bohr's ideas were Brillouin and de Donder, who drew attention to the agreement between Bohr's exposition and certain observations relating to the cell structure of phase space (Brillouin) and to the relativistic theory of the gravitational field (de Donder). The third speaker in the discussion was Max Born. "Mr. Einstein," he said, "once considered the following problem: a radioactive element emits alpha-particles into all directions; these are made visible by means of the Wilson chamber; if, now, a spherical wave is associated with every act of emission, how can it be understood that the trace of each alpha-particle appears as an (almost) straight line? In other words: how can the corpuscular character of a phenomenon be reconciled with its representation in terms of waves?" Born then referred to the usual answer to this question in terms of a "reduction of the wave packet" but also to an alternative approach, envisaged by Pauli, which describes the process without this "reduction" by having recourse to a multidimensional space, but, as Born cautiously—and correctly—added "this does not lead us very far concerning the basic problem."

It was only thereafter that Einstein rose to speak. "I have to apologize," he said, "for not having gone deeply into quantum mechanics. Nevertheless, I would like to make some general remarks." One may consider, he continued, the theory of quanta from two different viewpoints. To make his point clear, he referred to the following experiment.

A particle (photon or electron) impinges normally on a diaphragm with slit 0 so that the ψ-wave associated with the particle is diffracted in 0. A scintillation-screen (or photographic film) in the shape of a hemisphere is placed behind 0 so as to show the arrival of a particle (Figure 3), an event whose probability of occurrence is measured by the "intensity" of the diffracted spherical waves at the point under consideration.

According to *viewpoint I*, Einstein declared, the de Broglie-Schrödinger waves do not represent one individual particle but rather an ensemble of particles distributed in space. Accordingly, the theory provides information not on an individual process but rather on an ensemble of them. Thus $|\psi(r)|^2$ expresses the probability (probability density) that there exists at r *some* particle of the ensemble [une certaine particule du nuage].

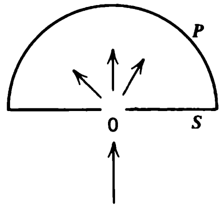

Figure 3.

According to *viewpoint II* quantum mechanics is considered as a complete theory of individual processes [une théorie complète des processus individuels]; each particle moving toward the screen is described as a wave packet which, after diffraction, arrives at a certain point P on the screen, and $|\psi(r)|^2$ expresses the probability (probability density) that at a given moment one and the same particle shows its presence at r. Although Einstein thought (erroneously) that the conservation laws for elementary processes, the results of the Geiger-Bothe experiments, and the almost continuous lines formed by alpha-particles in a Wilson chamber support only viewpoint II, he objected to it on the following grounds: If $|\psi|^2$ is interpreted according to II, then, as long as no localization has been effected, the particle must be considered as potentially present with almost constant probability over the whole area of the screen; however, as soon as it is localized, a peculiar action-at-a-distance must be assumed to take place which prevents the continuously distributed wave in space from producing an effect at *two* places on the screen ["mais l'interpretation d'après laquelle $|\psi|^2$ exprime la probabilité que cette particule se trouve à un endroit déterminé, suppose un mécanisme d'action à distance tout particulier, qui empêche que l'onde continûment répartie dans l'espace produise une action en deux endroits de l'écran"].

"It seems to me," Einstein continued, "that this difficulty cannot be overcome unless the description of the process in terms of the Schrödinger wave is supplemented by some detailed specification of the localization of the particle during its propagation. I think M. de Broglie is right in searching in this direction. If one works only with Schrödinger waves, the interpretation II of $|\psi|^2$, I think, contradicts the postulate of relativity."

Einstein concluded his remarks with two further arguments against interpretation II. It makes use, he argued, of multidimensional configuration spaces in which two systems of identical particles differing merely in the permutation of the latter are represented by two different points, a conclusion hardly reconcilable with the new statistics. Finally, he pointed out, the principle of contact forces, that is, the assumption that forces act only over small distances in space, cannot be adequately formulated in configuration space ["la particularité des forces de n'agir qu'à de petites distances spatiales trouve dans l'espace de configuration une expression moins naturelle que dans l'espace à trois ou quatre dimensions].

Prima facie it sounds somewhat strange that Einstein, who had been so successful in reducing gravitation to geometry, should have voiced the last-mentioned objection and rejected a new formalism on the grounds that it runs counter to a conventional force-principle. It was, however, not the *dynamical* notion of force as such but precisely the *geometrical* property of acting at a distance that he refused to accept. The argument was primarily directed against Schrödinger—in spite of the fact that Schrödinger's formulation of quantum mechanics as a field theory in terms of continuous waves and the ensuing attempted elimination of discontinuities seemed to Einstein less repugnant than any other formulation of the theory.

In his lecture at the meeting Schrödinger had emphasized the fundamental difference between de Broglie's wave mechanics dealing with waves only in the three-dimensional space or rather in the four-dimensional space-time continuum, and his own "multidimensional wave mechanics" [mécanique ondulatoire polydimensionnelle] in which a system of N particles is not represented by $3N$ separate functions $q_k(t)$ of the time t, as in de Broglie's theory, but as *one single* function ψ of $3N$ variables q_k, or $x_1 y_1 z_1, \ldots, x_N, y_N, z_N$, in addition to the time variable t, that is, in a $(3N+1)$-dimensional space.

It became clear that the function which served Schrödinger well in solving various many-body problems is determined by a partial differential equation, involving derivatives with respect to the q_k as independent variables; it also became apparent that this equation, in the case of two electrons, for example, implies some interaction among the ψ-values over an infinitesimal domain in the $x_1 y_1 z_1 x_2 y_2 z_2 t$-continuum, the configuration space-time for two particles, although $[(x_2-x_1)^2+(y_2-y_1)^2+(z_2-z_1)^2]^{1/2}$ may well be even a macroscopically large distance.

That Einstein pondered much about this question is known from a number of remarks made by his friend Paul Ehrenfest, with whom he discussed this matter repeatedly. Thus, for example, in an interesting footnote to a paper published in 1932, Ehrenfest declared: "If we recall what an *uncanny theory of action-at-a-distance* is represented by Schrödinger's wave mechanics, we shall preserve a healthy nostalgia for a four-dimensional *theory of contact-action*!"[8] Ehrenfest added: "Certain thought-

[8]"Wir sollten uns immer wieder daran erinnern, eine wie *unheimliche Fernwirkungstheorie* also die Schrödingersche Wellentheorie ist, um unser Heimweh nach einer vierdimensionalen Nahwirkungstheorie wach zu halten!" P. Ehrenfest, "Einige die Quantenmechanik betref-

experiments, designed by Einstein, but never published, are particularly suited for this purpose."

Since the notion of an action-at-a-distance, as we shall see later, plays an important role in connection with a number of other interpretive attempts, some further details on this issue seem to be appropriate.

In answer to Ehrenfest's article Pauli, in a paper[9] written under the same title four months later, proposed a formulation of the theory in terms of contiguous actions. For this purpose he recalled the procedure of transforming the classical theory of electrostatics, based on Coulomb's law of actions at a distance, into a theory of contiguous action by the introduction of the concept of an electrostatic field obeying a certain differential equation, $\operatorname{div} E = 4\pi\rho$, and asked whether something analogous could be done in quantum mechanics. Considering only electrostatic interactions Pauli suggested replacing the usual formulation of Coulomb's law, if incorporated into Schrödinger's equation, by the expression

$$\nabla E(x) = 4\pi \sum_{s=1}^{n} e_s \delta(x - X^{(s)}) \tag{13}$$

so that Schrödinger's equation can be written

$$i\hbar \frac{\partial\psi}{\partial t} = \left[-\sum \left(\frac{\hbar^2}{2m_s} \right) \Delta_s + \frac{1}{2} \int E^2(x)\, dx_1\, dx_2\, dx_3 \right] \psi(t, X^{(s)}) \tag{14}$$

where

$$\Delta_S = \sum_{k=1}^{3} \frac{\partial^2}{\partial X_k^{(s)^2}},$$

$X^{(s)}$ are the $3N$ coordinates of the N particles ($s = 1, \ldots, N$),

$$E(x) = \sum \frac{e_s}{r_s^2} \frac{x - X^{(s)}}{r_s}, \quad r_s = |x - X^{(s)}|,$$

and x the coordinates of the field point.

For a generalization of this approach which takes account of the radiative retardation and of magnetic interactions Pauli referred to his

fende Erkundigungsfragen," *Zeitschrift für Physik* **78**, 555–559 (1932).

[9]W. Pauli, "Einige die Quantenmechanik betreffenden Erkundigungsfragen," *Zeitschrift für Physik* **80**, 573–586 (1933), esp. pp. 584–586.

forthcoming article in the *Handbuch*.[10] Concerning the problem of the self-energy and that of a relativistic generalization of this approach, however, he had to admit that only a modification of the prevailing conception of space-time could lead to a solution. Nor did Pauli's proposal alleviate the main difficulty under discussion which was most strikingly expressed in Einstein's first objection: the instantaneous collapse of the ψ-wave, associated with an individual particle, at the moment of the latter's localization. Such a collapse, Einstein declared, would imply "a very peculiar mechanism of an action-at-a-distance."

In fact, such a process would not only violate the principle of relativity, because of the instantaneousness or simultaneity assumed, it could also never be subjected to an experimental analysis because by definition it involves only one particle with the detection of which the process has run its course. It is therefore not surprising that Heisenberg's conception of the "reduction of a wave packet" which is but a mathematical version of this collapse of the ψ-function, or von Neumann's projection postulate, which, again, is but another mathematical formulation of this idea, conveniently adapted for its application in the theory of measurement, became the target of serious objections.

Einstein, as we see, seems to have favored viewpoint I or, as it was later called, the ensemble interpretation of the Schrödinger function or, more generally, the ensemble interpretation of quantum mechanics, according to which quantum mechanics, as developed in the late 1920s, does not describe the behavior of an *individual* system but rather that of an *ensemble* of identical systems. Although both points of view may lead to the same experimental predictions, both being essentially statistical theories, they differ in their conception of the underlying notion of probability. Viewpoint II or Bohr's interpretation incorporates Born's probabilistic hypothesis according to which the theory predicts the probabilities of the results of a single experiment on a single system, a probability conception which Heisenberg, as we have mentioned, compared with the Aristotelian notion of potentiality.

An ensemble interpretation, as envisaged by Einstein, identifies the quantum mechanical probabilities as relative frequencies of the results of an ensemble of identical experiments, a notion probably more palatable to the majority of physicists. Although, as far as we know, Einstein did not explicitly refer to this difference concerning the notion of probability at the 1927 Brussels Conference, it is most likely that he had it in mind; in any case, his subsequent writings on this subject clearly state this difference.

The difference in the two interpretations of probability as used in

[10] Ref. 3–13.

quantum mechanics is extremely important in view of the following fact. Although it does not lead to *experimental consequences*—for in both cases the confirmation of predictions requires the performance of an ensemble of identical experiments—it does lead to *interpretative consequences*: The Einstein frequency interpretation opens the way to a hidden-parameter theory, which reduces quantum mechanics to a branch of statistical mechanics; the Bohr-Born probabilistic interpretation precludes this possibility.

To return to the Solvay Congress, de Broglie, as we have seen, soon abandoned his ideas in view of the unfavorable reaction they encountered. Einstein was practically alone in his opposition to the generally accepted interpretation of the quantum mechanical formalism. His objections, however, gave rise to heated discussions between the official sessions. His aim obviously was to refute the Bohr-Heisenberg interpretation by designing thought-experiments which show that the indeterminacy relations can be circumvented and that, in particular, the transfer of energy and momentum in individual processes can be given a fully detailed description in space and time.

Bohr's masterly report[11] of his discussions with Einstein on this issue, though written more than 20 years after they had taken place, is undoubtedly a reliable source for the history of this episode. It is, however, most deplorable that additional documentary material on the Bohr-Einstein debate is extremely scanty. For it was one of the great scientific debates in the history of physics, comparable, perhaps, only to the Newton-Leibniz controversy of the early eighteenth century. In both cases it was a clash of diametrically opposed philosophical views about fundamental problems in physics; in both cases it was a clash between two of the greatest minds of their time; and as the famous Leibniz-Clarke correspondence (1715–1716)—"*peut-être le plus beau monument que nous ayons des combats littéraires*" (Voltaire)—was only a brief manifestation of the profound divergence of opinions between Newton and Leibniz, so were the discussions between Bohr and Einstein in the lobby of the Hotel Metropole in Brussels only the highlight of a debate which went on for many years, though not in the form of a direct dialogue. In fact, it went on even after Einstein died (April 18, 1955), for Bohr admitted repeatedly that he continued, in his mind, to argue with Einstein, and whenever pondering over a fundamental issue in physics he asked himself how Einstein would have thought about it. Indeed, Bohr's last drawing on the blackboard in his study at Carlsberg Castle, made the evening before his death (November 18, 1962), was the sketch of the Einstein photon box, associated with one

[11]Ref. 4–10. Bohr wrote part of this essay during a visit to Princeton in 1948.

of the major problems raised in his discussions with Einstein.

5.2. EARLY DISCUSSIONS BETWEEN BOHR AND EINSTEIN

The beginning of the Bohr-Einstein debate can be traced back to the spring of 1920 when Bohr visited Berlin and met Einstein, Planck, and James Franck. With Franck Bohr developed on this occasion a warm friendship as a consequence of which Franck became one of the first foreign scientists to visit the new institute at Blegdamsvej in Copenhagen. Although Bohr greatly admired Einstein for his contributions to statistical molecular theory, his work on relativity, and especially his ingenious derivation of Planck's radiation law, it was still difficult for him to reconcile himself to Einstein's concept of light quanta. Thus in his talk before the Berlin Physical Society on April 27, 1920, about "the present state of the theory of spectra and the possibilities of its development in the near future"[12]—a subject intimately connected with the photon theory—Bohr referred only on one single occasion to the notion of "radiation quanta," and this probably only out of respect to Einstein, who attended the lecture; Bohr immediately added: "I shall not here discuss the familiar difficulties to which the 'hypothesis of light quanta' leads in connection with the phenomena of interference, for the explanation of which the classical theory of radiation has shown itself to be so remarkably suited."

The discussion between Bohr and Einstein in April 1920, if viewed in the perspective of later developments, may prima facie give the impression that their roles at that time were diametrically the reverse of what they were thereafter: Einstein maintained that a complete theory of light must somehow combine undulatory and particulate features, whereas Bohr, defending the classical wave theory of light, insisted that the "frequency" ν appearing in the energy $h\nu$ of the quantum is defined by experiments on interference phenomena "which apparently demand for their interpretations a wave constitution of light" and the photon theory thus made nonsense of its own basic equation. On closer analysis, however, their future characteristic antithetical positions were already recognizable. Bohr stressed the need for a profound break with the ideas of classical mechanics while Einstein, though endorsing the wave-particle duality of

[12]N. Bohr, "Über die Serienspektren der Elemente," *Zeitschrift für Physik* **2**, 423–469 (1920), reprinted in N. Bohr, *Drei Aufsätze über Spektren und Atombau* (Vieweg, Braunschweig, 1922); "On the series spectra of the elements," in *The Theory of Spectra and Atomic Constitution* (Cambridge University Press, Cambridge, London, 1924), pp. 20–60. *Tri Statji o Spektrach i Strojenni Atomov* (G.I.T.T.L., Moscow, 1923); reprinted in N. Bohr, *Izbrannije Naučnije Trudy* (Collected Scientific Works), Vol. 1 (Essays, 1909–1925) (see Ref. 4–1).

light, was convinced that these two aspects can be causally related with each other.

For Bohr classical physics and quantum theory, although asymptotically connected by the correspondence principle, seemed irreconcilable; Einstein, on the other hand, had already in 1909 suggested[13] that Maxwell's equations might yield pointlike singular solutions in addition to waves—an idea which he later (1927) successfully applied to the field equations of general relativity and which had prompted him, as we have seen, to support de Broglie's theory of pilot waves at the Fifth Solvay Congress—and thus he was a firm believer in a unified causal theory of all physical phenomena. How repugnant to his mind Bohr's dichotomic approach must have been is seen from a letter he wrote to Born on June 4, 1919: "The quantum theory provokes in me quite similar sensations as in you. One ought really to be ashamed of the successes, as they are obtained with the help of the Jesuitic rule: 'One hand must not know what the other does.'"[14]

In another letter (dated January 27, 1920) to Born, written before his meeting with Bohr, Einstein remarked: "That question of causality worries me also a lot. Will the quantum absorption and emission of light ever be grasped in the sense of complete causality, or will there remain a statistical residue? I have to confess, that I lack the courage of a conviction. However I should be very, very loath to abandon *complete* causality. . . ."

A few weeks later, on March 3, 1920, Einstein wrote to Born: "In my spare time I always brood about the problem of the quantum theory from the point of view of relativity. I do not think the theory can work without the continuum. But I do not seem to be able to give tangible form to my pet idea, which is to understand the structure of the quanta by redundancy in determination, using differential equations." [Es will mir aber nicht gelingen, meiner Lieblingsidee, die Quantenstruktur aus einer Überbestimmung durch Differentialgleichungen zu verstehen, greifbare Gestalt zu geben.][15] In view of this statement it is not difficult to understand why Einstein later became so "enthusiastic" about Schrödinger's work of 1926.

How Einstein in 1923 approached the problem of incorporating the

[13]A. Einstein, "Über die Entwicklung unserer Anschauungen über das Wesen und die Konstitution der Strahlung," *Physikalische Zeitschrift* **10**, 817–825 (1909); *Verhandlungen der Deutschen Physikalischen Gesellschaft* **11**, 482–500 (1909).

[14]M. Born, "Physics and reality," *Helvetica Physica Acta* (Supplementum 4), 244–260 (1956), quotation on p. 256; M. Born, *Albert Einstein-Max Born, Briefwechsel* (Nymphenburger Verlagshandlung, Munich, 1969), p. 29; *The Born-Einstein Letters* (Walter and Co., New York; Macmillan, London, 1971), p. 10; *Einstein-Born Correspondance 1916–1955* (Seuil, Paris, 1972), p. 28.

[15]Ref. 14 (1969, pp. 48–49; 1971, p. 26; 1972, p. 41).

theory of quanta into a general field theory, based on the principles of causality and continuity, is outlined in his essay "Does Field Theory Provide Possibilities for Solving the Problem of Quanta?"[16] In ordinary mechanics, he maintained, only the temporal evolution of the initial state of the system is subjected to specific laws, namely the differential equations of the laws of motion, whereas the initial state can be chosen at will; in quantum physics the initial state too, as the quantum conditions indicate, is governed by definite laws. This fact suggests that the problem be solved by "overdetermining" [*Überbestimmung*] the equations; that is, the number of the differential equations must exceed the number of the field variables involved. Einstein proposed such an overdetermination but failed to deduce from it the quantum conditions.

In spite of diversity of views, Einstein was greatly impressed by Bohr's personality. Shortly after Bohr's return from Berlin to Copenhagen Einstein wrote to him: "Not often in life was I so delightfully impressed already by the mere presence of somebody as by yours. Now I understand why Ehrenfest is so fond of you."[17] Bohr in his answer (June 24, 1920) called his visit to Einstein "one of the greatest events in my life." To Ehrenfest Einstein wrote: "Bohr was here and I am just as keen on him as you are. He is a very sensitive lad and goes about this world as if hypnotized." In a letter to Sommerfeld, dated December 20 (?), 1920, Einstein expressed his admiration of Bohr's intuition.[18]

That Einstein really admired Bohr as a person—although in later years he criticized his views often in rather sharp terms—seems beyond doubt. When thanking him for his congratulations on the occasion of the Nobel prize award which was made public when Einstein was on a trip to the Far East, Einstein addressed Bohr with the words "Dear or rather beloved Bohr!"[19]—and he meant it.

[16]A. Einstein, "Bietet die Feldtheorie Möglichkeiten für die Lösung des Quantenproblems?," *Berliner Berichte* **1923**, 359–364. Cf. also Einstein's letter to his friend Michele Besso, dated January 5, 1924: "The idea I am toiling with in order to reach full understanding of quantum phenomena refers to an over-determination of the laws by having more differential equations than field variables. For in this way the inarbitrariness of the initial conditions could be overcome without renouncing field theory. Although this approach may well turn out to be a failure it has to be attempted for, after all, it is logically possible.... The mathematics is exceedingly complicated and the relation to experience is even more indirect. But it remains a logical possibility, to do justice to reality, without any *sacrificium intellectus.*" Cf. *Albert Einstein, Michele Besso, Correspondance 1903–1955,* P. Speziali, ed. (Hermann, Paris, 1972), p. 197.

[17]Letter from Einstein to Bohr, dated May 2, 1920.

[18]"Seine Intuition ist sehr zu bewundern." *Albert Einstein-Arnold Sommerfeld: Briefwechsel,* A. Hermann, ed. (Schwabe & Co., Basel, Stuttgart, 1968), p. 75.

[19]Letter written on board S/S Haruna Maru, January 10, 1923.

The conflict between Bohr and Einstein reached its first peak after the discovery of the Compton effect, which seemed to lend unqualified support to the particulate theory of light and consequently called for drastic steps on the part of Bohr. To meet this challenge Bohr wrote in 1924 with Kramers and Slater the famous paper, "The Quantum Theory of Radiation,"[20] which completely abandoned Einstein's idea of a quantum structure of radiation, replacing it by a thoroughly probabilistic approach based on only a statistical conservation of energy and momentum.

On April 29, 1924, Einstein wrote to Born: "Bohr's opinion of radiation interests me very much. But I don't want to let myself be driven to a renunciation of strict causality before there has been a much stronger resistance against it than up to now. I cannot bear the thought that an electron exposed to a ray should by its *own free decision* [*aus freiem Entschluss*] choose the moment and the direction in which it wants to jump away. If so, I'd rather be a cobbler or even an employee in a gambling-house than a physicist. It is true, my attempts to give the quanta palpable shape have failed again and again, but I'm not going to give up hope for a long time yet."[21]

In a letter dated May 1, 1924, to Paul Ehrenfest, Einstein listed a number of reasons why he rejected Bohr's suggestions, the principal reason being that "a final abandonment of strict causality is very hard for me to tolerate."

In December 1925 Bohr and Einstein met again, this time in Leiden, on the occasion of the celebration of Lorentz' golden anniversary of his doctorate. Ehrenfest, who had been in Leiden since 1912, becoming Lorentz' successor in 1923, had friendly relations with Einstein ever since he had visited him in Prague in 1912; but he was also a great admirer of Bohr, with whom he had frequent contact since May 1918. Kramers, Bohr's long-time collaborator, was a student of Ehrenfest.

Ehrenfest thus served as a kind of mediator, a role which he played with great skill. Although the general problem of the nature of the quantum theory, as far as it was developed at that time, was undoubtedly a subject of their discussion, the debate seems to have focused on an experiment which Einstein had proposed in 1921. It was designed as an *experimentum crucis* between the classical wave theory of light and Bohr's quantum theory. This experiment was to decide between the undulatory Doppler

[20]N. Bohr, H. A. Kramers, and J. C. Slater, "The quantum theory of radiation," *Philosophical Magazine* **47**, 785–802 (1924); "Über die Quantentheorie der Strahlung," *Zeitschrift für Physik* **24**, 69–87 (1924). C. Ref. 1-1 (pp. 182–188).

[21]M. Born, "In memory of Einstein," *Universitas* **8**, 33–44 (1965), quotation on p. 39; and Ref. 14 (1969, p. 118; 1971, p. 82; 1972, p. 98).

formula $\nu = \nu_0(1 + v\cos\theta / c)$ applied to the radiation from canal rays, and the quantum-theoretical Bohr formula $E_2 - E_1 = h\nu$, according to which every elementary act of emission, including that caused by an atom in motion, was supposed to produce a unique frequency. In the wave theory the optical beam, after its passage through a dispersive medium, was expected, according to Einstein's theory, to suffer a deviation of a few degrees whereas according to the Bohr theory it was not.[22] A few weeks later, following Ehrenfest's suggestion that the group velocity rather than the phase velocity should be taken into consideration since the problem deals with a finite wave train, Einstein revised his theory of the propagation of light through dispersive media and came to the conclusion that the wave theoretical and the corpuscular treatments of the problem lead to the same result. Although the experiment had thus lost its "cruciality," it clearly touched on a number of issues of great concern for both Einstein and Bohr.[23]

Although very little is known about the conversation between Bohr and Einstein in Leiden, it seems certain that Bohr, having meanwhile accepted Einstein's theory of light quanta, put much emphasis on the difficulties of applying the notions of classical physics to quantum mechanics. In a letter of April 13, 1927, to Einstein, Bohr referred to their meeting in Leiden which he described as having given him "great pleasure"; and, as if continuing the discussion, he reiterated that the concepts of classical physics "give us only the choice between Scylla and Charybdis, depending on whether we direct our attention to the continuous or the discontinuous features of the description" [Diese Begriffe geben uns ja nur die Wahl zwischen Charybdis und Scylla je nachdem wir unsere Aufmerksamkeit auf die kontinuierliche oder diskontinuierliche Seite der Beschreibung richten.]

At the request of Heisenberg, Bohr enclosed in this letter to Einstein a preprint of Heisenberg's article on the indeterminacy relations. Connecting its contents with their discussion in Leiden, Bohr wrote that, as shown by Heisenberg's analysis, inconsistencies can be avoided only because of the fact that the limitations of our concepts coincide with the limitations of our observational capabilities—a clear indication that Bohr already envisaged his complementarity interpretation in April 1927.

Turning to the problem of light quanta, Bohr wrote: "In view of this

[22]For details see M. J. Klein, "The first phase of the Bohr-Einstein dialogue," *Historical Studies in the Physical Sciences*, R. McCormmach, ed. (University of Pennsylvania Press, Philadelphia, 1970), Vol. 2, pp. 1–39.

[23]Cf. A. Einstein, "Interferenzeigenschaften des durch Kanalstrahlen emittierten Lichtes," *Berliner Berichte* **1926**, 334–340.

new formulation [Heisenberg's relation] it becomes possible to reconcile the requirement of energy conservation with the implications of the wave theory of light, since according to the character of the description the different aspects of the problem never manifest themselves simultaneously." [*Durch die neue Formulierung ist die Möglichkeit gegeben, die Forderung der Erhaltung der Energie mit den Konsequenzen der Wellentheorie des Lichtes in Einklang zu bringen, indem nach dem Charakter der Beschreibung die verschiedenen Seiten des Problems nie gleichzeitig zum Vorschein kommen.*]

The last-quoted passage of this historically important letter clearly shows how Einstein's conception of the photon—to use this term which had been introduced by G. N. Lewis in 1926—and its ultimate acceptance by Bohr within the framework of the wave-particle duality were instrumental for Bohr's formation of his complementarity ideas.

Informed by Bohr of Einstein's reservations about the indeterminacy relations, Heisenberg wrote to Einstein on May 19, 1927, inquiring whether Einstein had designed an experiment which contradicts the indeterminacy principle. In another letter to Einstein on June 10, 1927, Heisenberg analyzed the thought-experiment of a particle diffracted by a grating whose grating constant (the separation between two consecutive lines) is much larger than the size of the slowly moving free particle:

> According to your theory the particle will be reflected in a definite discrete direction in space. If you knew the path of the particle, you could consequently compute where it hits the grating and place there an obstacle which reflects it in an arbitrarily chosen direction, independent of the other lines of the grating.... But in reality the particle will be reflected in that definite discrete direction. This inconsistency could be avoided only by relating the motion of the particle with its de Broglie wave [which, because of the assumed small velocity of the particle is of the order of magnitude of the grating constant]. This would mean, however, that the size of the particle, that is, the range of its interacting forces, is assumed to depend on its velocity. This amounts to abandoning the term "particle" and does not tally, I think, with the fact that in the Schrödinger equation or in the Hamilton function of matrix mechanics the potential energy is represented by the simple expression e^2/r. If you use the term "particle" in such a liberal way, I deem it well possible that the path of a particle can be defined. But then the great simplicity with which statistical quantum mechanics describes the motion of a particle, as far as one can speak of such a motion, is in my opinion lost. If I understand you correctly, you would be readily prepared to sacrifice this simplicity to save the principle of causality.

The basic problem of the debate between Einstein and Bohr at the Fifth Solvay Congress in Brussels—a problem at the forefront of foundational research ever since—was the question of whether the existing quantum mechanical description of microphysical phenomena should, and could, be carried further to provide a more detailed account, as Einstein suggested, or whether it already exhausted all possibilities of accounting for observable phenomena, as Bohr maintained. To decide on this issue, Bohr and Einstein agreed on the necessity of reexamining more closely those thought-experiments by which Heisenberg vindicated the indeterminacy relations and by which Bohr illustrated the mutual exclusion of simultaneous space-time and causal descriptions.

To understand correctly the views of the two disputants it should be recalled that for Bohr these thought-experiments were not the reason but the necessary consequence of a much more profound truth underlying the quantum mechanical description and, in particular, the uncertainty relations. Bohr consequently had the advantage that, from his point of view, he was justified in extending the chain of reasoning until he could appropriately resort to the indeterminacy relations to support his thesis. Einstein, on the other hand, had the advantage that if he could disprove the Heisenberg relations by a closer analysis of the mechanics of one single thought-experiment, Bohr's contention of the incompatibility of a simultaneous causal and space-time description of phenomena and with it his whole theory would be refuted.

Einsteins attack was therefore directed toward demonstrating that it is possible to provide an exact space-time specification of an individual process together with a detailed account of the balance of the energy and momentum transfer involved. If we recall that it was precisely Einstein's philosophy which had led Heisenberg to formulate his principle, we must conclude that, paradoxical as it sounds, Einstein now concentrated his efforts on disproving certain ideas that he had fathered, a situation not unusual in the history of physics.

The single-slit diffraction experiment leading to the first Heisenberg relation (3.1), as explained above, was obviously unsuitable for such a purpose since the diaphragm, being rigidly connected with the terrestrial measuring system, does not lend itself to any energy transfer calculations. But soon as the diaphragm was assumed to carry a shutter which opens the slit for a short time interval Δt, it became possible to apply the laws of energy and momentum conservation to the two-body system composed of the incident radiation (or particle) and the moveable shutter. In fact, in the case of radiation (of light), classical physics, with its theory of radiation pressure, predicts a transfer of momentum between the edges of the moving shutter and the incident wave.

If it were possible, reasoned Einstein, to calculate this momentum transfer, one could predict the accurate value of the component, parallel to the plane of the slit, of the momentum of the particle leaving the slit; since, in addition, the width of the slit defines with arbitrary precision the position coordinate, in the same plane, of the particle, Heisenberg's first relation would be disproved. That the momentum transfer between the particle and the shutter is an uncontrollable and not further analyzable disturbance, subject to Heisenberg's second relation, and that consequently Einstein's thesis was untenable, Bohr showed by the following argument.

The shutter, exposing the slit of width Δx for a time interval Δt, moves with the velocity $v \approx \Delta x/\Delta t$. A momentum transfer Δp therefore involves an energy exchange with the particle, amounting to $v\Delta p \approx (1/\Delta t)\Delta x\,\Delta p \approx h/\Delta t$ where use has been made of the first Heisenberg relation; Since $v\Delta p = \Delta E$, Heisenberg's second relation $\Delta E \Delta t \approx h$ holds and shows that the energy-momentum transfer is not further analyzable.

Accepting Bohr's counterargument and admitting the impossibility of a precise measurement of the momentum transfer by means of the very system which defines the position coordinate, Einstein assigned separate instrumental components to the two measurements involved, one for that of position and one for that of momentum. He thus proposed the following double-slit thought-experiment (Figure 4). Midway between a stationary

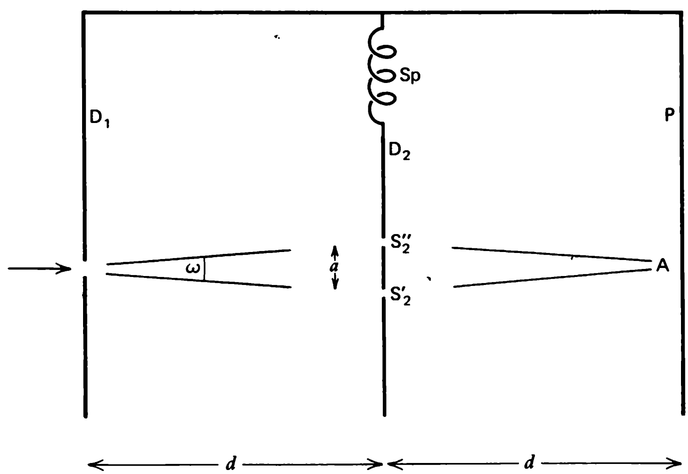

Figure 4.

diaphragm D_1 with a single slit S_1 and the screen (or photographic plate) P a second moveable diaphragm D_2 is suspended by a weak spring Sp. D_2 has two slits S_2' and S_2'', which are separated from each other by a distance

a, small compared to the distance d between D_1 and D_2. If D_2 were stationary, an interference would be observed on P, being an accumulation of individual processes if the intensity of the incident beam is so weak that only one particle passes through the apparatus at a time. Since the momentum imparted to D_2 depends on whether the particle passes through S_2' or S_2''—if, for example, the particle passes through the lower slit S_2' to reach A, the entire diaphragm must slightly recoil downward—Einstein suggested that by measuring the momentum imparted to D_2, the path of the particle (position and momentum) can be described with an accuracy greater than that allowed by the Heisenberg relations; for in addition to our knowledge of the momentum obtained from the analysis of the diffraction pattern the measurement would also show whether the particle passes through S_2' or through S_2''.

In his rebuttal Bohr pointed out that the difference of momentum transfer in the two alternative cases, corresponding to the passage of the particle either through S_2' or through S_2'', is given by $\Delta p = \omega p = h\omega/\lambda$, where ω is the angle subtended by a at S_1. Regarding D_2 now as a microphysical object, Bohr argued that any measurement of its momentum with an accuracy sufficient to measure Δp must involve a position indeterminacy of at least $\Delta x = h/\Delta p = \lambda/\omega$, which, however (as known, for example, from the analysis of the Young diffraction experiment in optics), is the reciprocal of the number of fringes per unit length; a momentum determination of D_2 sufficiently accurate to decide which slit has been passed through therefore involves a position indeterminacy of D_2 of the same order of magnitude as the distance between the interference fringes and hence obliterates completely the diffraction pattern.

Particle paths and interference patterns, concluded Bohr, are therefore complementary conceptions. In fact, the preceding double-slit thought-experiment, either resulting in a diffraction pattern on the screen and exhibiting the *wavelike* nature of the incident radiation or, if some detecting device registers which slit has been passed, exhibiting the *corpuscular* nature of the incident radiation, has since then become a standard paradigm for the illustration of the wave-particle dualism and the operational impossibility of measuring complementary observables. We have referred to it already in discussing the reason for rejecting Born's original version of his probabilistic interpretation of the ψ-function. The double-slit experiment is a phenomenon "which has in it the heart of quantum mechanics; in reality, it contains the *only* mystery" of the theory which cannot be explained in any classical way, as Richard P. Feynman said much later.[24]

[24]R. P. Feynman, R. B. Leighton, and M. Sands, *The Feynman Lectures on Physics* (Addison-Wesley, Reading, Mass, 1965), Vol. 3, p. 1–1. See also R. P. Feynman and A. R. Hibbs,

It is also well known how Bohr's complementarity interpretation, if applied in the analysis of this thought-experiment, avoids the paradoxical conclusion that the behavior of the particle should depend on the opening or closing of a slit through which it does not pass.

The Bohr-Einstein debate at the Fifth Solvay Congress ended, as we see, with the result that Bohr succeeded in defending the logical consistency of the complementarity interpretation. But he did not convince Einstein of its logical necessity. Einstein saw in Bohr's thesis more a dogmatic conviction, cleverly devised, than a scientific theory. In a letter to Schrödinger on May 31, 1928, Einstein characterized Bohr's view as follows: "The Heisenberg-Bohr tranquilizing 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 very easily be aroused. So let him lie there."[25]

The next phase of the Bohr-Einstein controversy had its origin in the following circumstances. Arnold Berliner, a physicist with an exceptionally broad knowledge in other natural sciences and author of what for many years was considered to be one of the best physics textbooks in German, left his position with Allgemeine Elektrizistätsgesellschaft to become editor of *Die Naturwissenschaften*.[26] In 1929 Berliner decided to dedicate an issue of his journal to Max Planck in commemoration of the golden anniversary

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Quantum Mechanics and Path Integral (McGraw-Hill, New York, 1965), pp. 2–13. The double-slit experiment was used also by Arthur Fine as the experimental basis for his analysis "Some conceptual problems of quantum theory," in *Paradigms and Paradoxes*, R. G. Colodny, ed. (University of Pittsburgh Press, 1972), pp. 3–31. Concerning the experimental evidence cf. G. I. Taylor, "Interference fringes with feeble light," *Proceedings of the Cambridge Philosophical Society* **15**, 114–115 (1909); A. J. Dempster and H. F. Batho, "Light quanta and interferences," *Physical Review* **30**, 644–648 (1927); L. B. Biberman, N. Sushkin, and V. Fabrikant, "Difratzia pootzeredno letjashikh elektronov" ("Diffraction of individually proceeding electrons") *Doklady Academii Nauk SSSR* **66**, 185–186 (1949), in which interference effects were verified with the time interval between two consecutive passages of an electron through the diffractor, a crystal of magnesium oxide, being about 30,000 times longer than the time needed by a single electron to pass the system. J. Faget and C. Fert, "Diffraction et interférences en optique électronique," *Cahiers de Physique* **11**, 285–296 (1957). A modern variation is the experiment performed by L. Mandel and R. L. Pfleegor who used two single-mode lasers, independently operated, (instead of the two slits) and obtained interference as if one laser, though not being the source of a photon in the apparatus, cooperated in producing the interference of the photon emitted from the other laser. Cf. R. L. Pfleegor and L. Mandel, "Interference effects at the single photon level," *Physics Letters* **24A**, 766–767 (1967); "Interference of independent photon beams," *Physical Review* **159**, 1084–1088 (1967). See also the discussion of this experiment in support of a subjectivistic version of the Copenhagen interpretation in R. Schlegel, "Statistical explanation in physics: The Copenhagen interpretation," *Synthese* **21**, 65–82 (1970).

[25]Ref. 2–13 (1967, p. 31).

[26]In 1935 Berliner was forced by the Nazis to relinquish his editorship and in 1942 he committed suicide.

of his doctorate;[27] he asked Sommerfeld, Rutherford, Schrödinger, Heisenberg, Jordan, A. H. Compton, F. London, and Bohr to contribute papers and his request was answered in all cases.

Bohr used this opportunity to expound in greater detail the epistemological background of his new interpretation of quantum mechanics. In his article[28] he compared in three different respects his approach with Einstein's theory of relativity. Planck's discovery of the quantum of action, he maintained, has confronted us with a situation similar to the discovery of the finiteness of the velocity of light; for just as the smallness of ordinary velocities in macromechanics makes it possible to separate sharply our conception of space from that of time, so the smallness of Planck's quantum of action makes it possible to provide simultaneously a space-time and a causal description of ordinary macroscopic phenomena. But in the treatment of microphysical processes the reciprocity or complementarity of the measuring results cannot be ignored, just as in high-speed phenomena the relativity of observation cannot be neglected in questions concerning simultaneity.

The restrictions expressed by Heisenberg's relations guarantee the consistency of quantum mechanics just as the impossibility of a super-light-velocity for the transmission of signals safeguards the consistency of the relativity theory. And just as the theory of relativity, "through a profound analysis of the problem of observation, was destined to reveal the subjective character of all the concepts of classical physics," so also quantum theory, through its cognizance of the indivisibility of the quantum of action, led to a further revision of the conceptual means for the description of nature. When writing these arguments, Bohr was obviously addressing himself primarily to Einstein.

Was not Einstein's rejection of Newtonian time based on the absence of any operational definition of absolute simultaneity? Should not the operational impossibility of defining simultaneously conjugate variables with arbitrary accuracy similarly lead to a rejection of the simultaneous validity of such conceptions? When Philipp Frank visited Einstein not long after the publication of Bohr's paper in the *Naturwissenschaften*, he discussed this issue with him and pointed out that the Bohr-Heisenberg approach "was invented by you in 1905," whereupon Einstein retorted: "A good joke should not be repeated too often."[29]

[27]Planck submitted his thesis "De secunda lege fundamentali doctrinae mechanicae caloris" in 1879 at the University of Munich.

[28]N. Bohr, "Wirkungsquantum und Naturbeschreibung," *Die Naturwissenschaften* **17**, 483–486 (1929), reprinted in Ref. 4-1 (1931, pp. 60–66; 1934, pp. 92–119; 1929, pp. 69–76).

[29]Ref. 3 (1947, p. 216).

Concerning the first two points of comparison Bohr was certainly right. But as to the third point of comparison, based on the assertion that relativity reveals "the subjective character of all concepts of classical physics" or, as Bohr declared again in the fall of 1929 in an address in Copenhagen, that "the theory of relativity reminds us of the subjective character of all physical phenomena, a character which depends essentially upon the state of motion of the observer,"[30] Bohr erroneously generalized the relativity or reference-frame dependence of metrical attributes, such as length or duration, which in Newtonian physics are invariants, to *all* concepts of classical physics, including such invariants as rest-mass, proper-time, or charge. Bohr overlooked that the theory of relativity is also a theory of invariants and that, above all, its notion of "events," such as the collision of two particles, denotes something absolute, entirely independent of the reference frame of the observer and hence logically prior to the assignment of metrical attributes. It was probably for this reason that Einstein thought the "repetition of the joke" unwarranted and that he remained unyielding toward Bohr's epistemological arguments.

5.3. THE SIXTH SOLVAY CONGRESS

The Einstein-Bohr debate was resumed at the Sixth Solvay Congress which convened in Brussels from October 20 to 25, 1930, and was presided over (after the demise of Lorentz) by Paul Langevin. It was devoted to the study of the magnetic properties of matter,[31] but, as at the Fifth Solvay Congress, the problem of the foundations of quantum mechanics was, at least between the official sessions, a major subject of discussion.

In view of Bohr's 1929 article in the *Naturwissenschaften*, citing relativity in support of his thesis, it would have been a *coup-de-maitre* to show that just the theory of relativity disproves Bohr's contention. This was precisely what Einstein had in mind when he set out to refute Heisenberg's relation $\Delta E \Delta t \geqslant h/4\pi$.

Einstein's attempt was prompted by the following consideration (Figure 5). Facing a stationary diaphragm with a slit a second diaphragm with a slit is set into motion by a clockwork so that part of a light beam is "chopped" through the two slits for a sharply determined time interval. The energy of the light pulse passing through the two slits, if absorbed by a received, can be determined with arbitrary accuracy. The apparent viola-

[30]N. Bohr, "Atomteorien og grundprincipperne for naturbeskrivelsen," *Fysisk Tidsskrift* **27**, 103–114 (1929); "Die Atomtheorie und die Prinzipien der Naturbeschreibung," *Die Naturwissenschaften* **18**, 73–78 (1929); Ref. 4-1 (1931, pp. 67–77; 1934, pp. 102–119).

[31]*Le Magnétisme—Rapports et Discussions du Sixième Conseil de Physique sous les Auspices de l'Institut International de Physique Solvay* (Gauthier-Villars, Paris, 1932).

tion of Heisenberg's energy-time indeterminacy relation is, however, not serious since the knowledge obtained refers to the past only and cannot be exploited for predictive statements.

To change this retrodictive measurement to a predictive one, the energy content of the emitted pulse must be determined before it is absorbed; this is possible only if for a source of radiation of precisely known energy the energy exchange with the moving shutter is taken into consideration. If the shutter moves with the velocity v, the indeterminacy Δp in its momentum change during the interaction with the radiation leads to the indeterminacy $\Delta E = v\Delta p$ in the energy transfer. Since the position of this energy interchange is determined by the width d of the stationary slit, the momentum indeterminacy is at least equal to h/d and hence $\Delta E \gtrsim hv/d$. To increase the accuracy in the energy determination, the fraction v/d has to be made as small as possible by either decreasing v or increasing d. In both cases, however, the accuracy of the time determination deteriorates since $\Delta t \approx d/v$. Obviously, $\Delta E \Delta t \gtrsim h$.

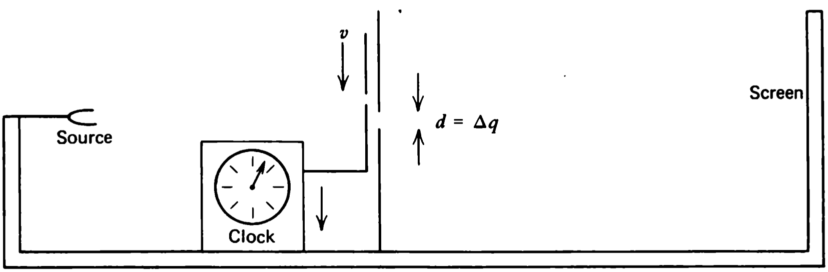

Figure 5.

To avoid the need of reducing the ratio v/d for an increase in the accuracy of the energy determination Einstein proposed the following stratagem. He considered a box with ideally reflecting walls, filled with radiation and equipped with a shutter which was operated by a clockwork enclosed in the box. He assumed that the clock was set to open the shutter at $t = t_1$ for an arbitrarily short interval $t_2 - t_1$ so that a single photon could be released.

Einstein then pointed out that by weighing the whole box before and after the emission of the accurately timed radiative pulse of energy, the difference in the energy content of the box could be determined with an arbitrarily small error ΔE from the mass-energy relation $E = mc^2$ of the theory of relativity. This energy difference, in accordance with the principle of energy conservation, would then be the energy of the emitted photon. Thus the energy of the photon and its time of arrival at a distant screen can be predicted with arbitrarily small indeterminacies ΔE and Δt in contradiction to the Heisenberg relation.

After a sleepless night over this argument Bohr rebutted Einstein's challenge with Einstein's own general theory of relativity. Only a few days earlier, on October 17, Bohr had delivered a lecture at the Royal Danish Academy of Science and Letters on "The Use of the Concepts of Space and Time in Atomic Theory."[32] There Bohr had dealt only with the nonrelativistic use of these concepts, hardly anticipating that, to counter Einstein's challenge, he would have to resort to the relativistic red-shift formula

$$\Delta T = T\frac{\Delta\varphi}{c^2}, \tag{15}$$

which expresses the change of rate ΔT during a time interval T for a clock displaced in a gravitational field through a potential difference $\Delta\varphi$.

Now, having realized during that sleepless night that Einstein had overlooked this important conclusion of his own general theory of relativity, Bohr was able to answer the challenge by pointing out that the very weighing process on which Einstein had based his argument contained the key to its refutation. Let us suppose, said Bohr early next morning at the Solvay Congress, that the box is suspended "in a spring-balance and is furnished with a pointer to read its position on a scale fixed to the balance support." To illustrate his point he made a rough sketch on the blackboard, similar to Figure 6, and continued:

> The weighing of the box may thus be performed with any given accuracy Δm by adjusting the balance to its zero position by means of suitable loads. The essential point is now that any determination of this position with a given accuracy Δq will involve a minimum latitude Δp in the control of the momentum of the box connected with Δq by the relation [$\Delta q\,\Delta p \approx h$]. This latitude must obviously again be smaller than the total impulse which, during the whole interval T of the balancing procedure, can be given by the gravitational field to a body with a mass Δm, or
>
> $$\Delta p \approx \frac{h}{\Delta q} < Tg\,\Delta m \tag{16}$$
>
> where g is the gravity constant. The greater the accuracy of the reading q of the pointer, the longer must be the balancing interval T, if a given accuracy Δm of the weighing of the box with its content shall be obtained.
>
> Now, according to general relativity theory, a clock, when displaced in the

[32]Cf. Note in *Nature* **127**, 43 (1931).

direction of the gravitational force by an amount of Δq, will change its rate in such a way that its reading in the course of a time interval T will differ by an amount ΔT given by the relation

$$\frac{\Delta T}{T} = g\frac{\Delta q}{c^2}. \tag{17}$$

By comparing (16) and (17) we see, therefore, that after the weighing procedure there will in our knowledge of the adjustment of the clock be a latitude

$$\Delta T > \frac{h}{c^2 \Delta m}. \tag{18}$$

Together with the formula $E = mc^2$ this relation again leads to

$$\Delta T \Delta E > h, \tag{19}$$

in accordance with the indeterminacy principle. Consequently, a use of the

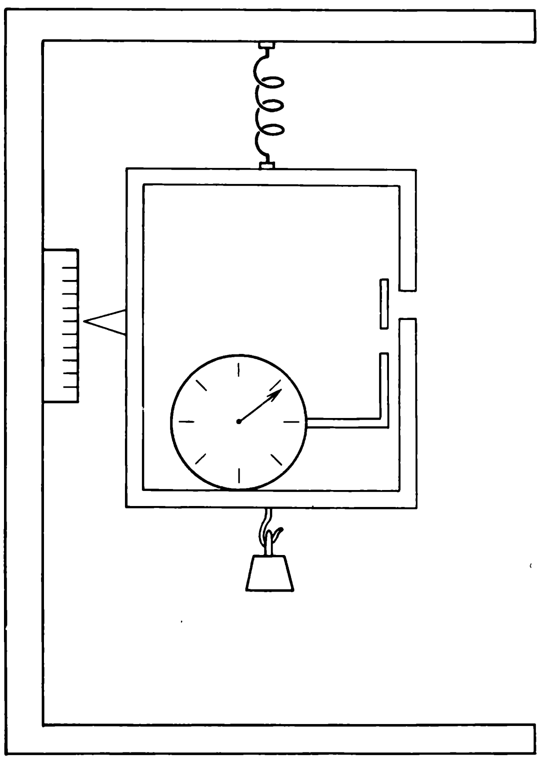

Figure 6.

apparatus as a means of accurately measuring the energy of the photon will prevent us from controlling the moment of its escape.[33]

As we see, the essential point in Bohr's reasoning which, though expounded only in the case of a spring-balance, applies to any method of weighing was simply that the very act of weighing a clock, according to general relativity, interferes with its rate. Einstein's appeal to relativity in order to refute the Heisenberg relation turned out to be a boomerang!

Indeed, this episode was one of the highlights of the Bohr-Einstein debate—and this not only because of the dramatic features involved. It was also a turning-point in Einstein's attitude toward quantum mechanics. Accepting Bohr's counterargument—for what could have been nearer to his heart than his own red-shift formula?—he gave up any hope of refuting the quantum theory on the grounds of an internal inconsistency. Instead, as we shall see after a lengthy digression on the implications of the photon box experiment, after the 1930 Solvay Congress Einstein concentrated on demonstrating the *incompleteness*, rather than the *inconsistency*, of quantum mechanics.

5.4. LATER DISCUSSIONS ON THE PHOTON BOX EXPERIMENT AND THE TIME-ENERGY RELATION

Bohr's account of his discussion with Einstein has been called "one of the great masterpieces of modern scientific reporting." According to Abraham Pais "nowhere in the literature can a better access to [Bohr's] thinking be found, and it is a must for all students of quantum mechanics, now or later."[34] V. A. Fock called it "a remarkably clear exposition of the physical foundations on which a correct interpretation of quantum mechanics must be based."[35] Recently Arthur Komar recommended the reading of Bohr's report as indispensable for getting full insight into the intricacies and subtleties of the problems discussed.[36] According to Léon Rosenfeld[37] it was the clearest exposition ever written by Bohr about his philosophy of quantum mechanics.

Bohr's answer to Einstein's photon box argument, if hailed as an

[33]Ref. 4–10 (1949, pp. 226–228).

[34]A. Pais, "Reminiscences from the post-war years," Ref. 3–24 (p. 225).

[35]V. A. Fock, "Zamechanija k statje Bora o evo diskussijakh s Einsteinom," *Uspekhi Fisičeskikh Nauk* **66**, 599–602 (1958); "Remarks on Bohr's article on his discussions with Einstein," *Soviet Physics Uspekhi* **66**, 208–210 (1958).

[36]A. Komar, Qualitative features of quantized gravitation," *International Journal of Theoretical Physics* **2**, 157–160 (1969).

[37]Private communication, January 17, 1971.

especially clear analysis of the intricacies of quantum mechanics, was of course also regarded as logically invulnerable. This judgment, however, was not universal.

Josef Agassi, when studying under Karl Popper, regarded Bohr's argument as "invalid"[38]—and still does so today[39]—on the ground that Bohr, by having recourse to general relativity, "changed illegitimately the rules of the game." The issue involved has been discussed at some length by Popper.

In his view the mass-energy relation "can be derived from special relativity, and even from non-relativistic arguments," whereas the red-shift formula (15) is part of Einstein's gravitational theory. Thus for Popper Bohr's recourse to the gravitational theory to meet Einstein's argument amounts "to the strange assertion that quantum theory contradicts Newton's gravitational theory, and further to the still stranger assertion that the validity of Einsteins's gravitational theory (or at least the characteristic formulae used, which are part of the theory of the gravitational field) can be derived from quantum theory."

Popper's statement seems to have been motivated by the following consideration. Although Einstein's original derivation of the mass-energy relation was based on the principle of relativity, some later demonstrations of the theorem of the inertia of energy, such as Max Born's popular proof, seem to make use only of classical principles, such as the conservation of linear momentum, as in Born's demonstration.[40] On closer examination, however, it will be realized that such derivations apply conceptions like the momentum of radiation (E/c) that find their justification only in Maxwell's theory of the electromagnetic field, which is itself a relativistic theory. Zero rest mass particles can be understood only within the framework of relativistic dynamics. Thus Born's derivation—or any other derivation, for that matter—of the inertia of energy is basically a relativistic consideration.

In short, Einstein's argumentation involves relativistic considerations and its disproof by relativistic counterarguments only shows the consistency of the Heisenberg relations with relativity but does not show their inconsistency with Newton's theory of gravitation. Since the logical chain of deductions, leading from (15) to (19), is not reversible—no equality can ever be derived from an inequality—the validity of Einstein's gravitational theory or any part of it cannot be said to follow from the quantum theory.

More precisely, the fallacy of the Popper-Agassi thesis, accepted also by

[38]See footnote 10 on p. 447 of Ref. 3–15 (1959).

[39]Private communication, June 18, 1972.

[40]M. Born, *Atomic Physics* (Blackie & Sons, London, 6th ed., 1957), pp. 55–56.

Alfred Landé, namely that Bohr's counterargument implies "that the relativistic redshift could be derived as a consequence of quantum indeterminacy, or vice versa,"[41] can be exposed as follows. Einstein's suggested determination of the energy E by means of a spring balance is based not only on the equivalence between energy and inertial mass, but also on the equivalence between inertial and gravitational mass which implies the red-shift formula (15). In other words, the time-dilation is a feature belonging to the particular *method* of measurement proposed but not to the object (viz. the quantities subject to the Heisenberg relation) of the measurement. Bohr's appeal to equation (15) was therefore prompted not by the quantum indeterminacy as such but rather by the specific mechanism proposed to test its validity.

In fact, as Schrödinger remarked in a conversation with Popper, since Einstein referred to weighing as the procedure of measuring the inertial mass of the box, any rebuttal of his challenge should be based on the best available theory of gravitation, that is, on the general theory of relativity.

That the Popper-Agassi thesis is untenable may be shown also by the following consideration. Einstein could equally well have proposed as a means of measuring the inertial mass of the box an elastic collision experiment between the box and another body or particle. In such a measurement of the momentum the necessary indeterminacy Δx in the position of the box would lead, during the time T of the measurement, to an indeterminacy $\Delta v = T\Delta x$ in the velocity of the box and hence, on account of the Lorentz transformation between the time variables, to an indeterminacy in the reading of the enclosed clock, for it would not be known exactly into what inertial system the clock would have been transferred. However, the Heisenberg relation could thus be retrieved on the basis of special relativity alone.

Let us also recall that the red-shift formula can be established[42] without any reference to Einstein's theory by merely using Newton's laws and the conception of the photon as a particle of mass $h\nu/c^2$.

In spite of his spectacular victory over Einstein in 1930, it seems that Bohr was never fully satisfied with his resolution of the photon box objection; time and again he returned to this problem. He also discussed this matter frequently with his colleagues in Copenhagen. In one of these discussions, for example, it was pointed out that the inequality $\Delta E > hc^2/$

[41] A. Landé, *New Foundations of Quantum Mechanics* (Cambridge University Press, Cambridge, 1965), p. 123.

[42] See, e.g., J. C. Gravitt and P. Waldow, "Note on gravitational red shift," *American Journal of Physics* **30**, 307 (1962), or A. J. O'Leary, "Redshift and deflection of photons by gravitation: A comparison of relativistic and Newtonian treatments," *ibid.*, **32**, 52–55 (1964).

$(Tg\Delta q)$, a consequence of (16) and $\Delta E = c^2 \Delta m$, shows that ΔE can be made arbitrarily small—that is, the apparatus allows an arbitrarily accurate determination of the emitted energy—if only with decreasing Δq the time interval T is taken sufficiently large. Even the red-shift formula $\delta T / T = g\delta q / c^2$, where δq denotes the difference in height, would not impair the accuracy obtained if only the correction δT, associated with δq, could be determined accurately. But to this end δq would have to be measured exactly. Any error Δq in measuring δq would impart to δT an indeterminacy $\Delta T = Tg\Delta q / c^2$, which increases with T and leads, if combined with the above $\Delta E > hc^2 / (Tg\Delta q)$, again to the Heisenberg relation. In particular, if T were defined as starting, in the second weighing process, at the moment t_2 when the shutter is closed again, t_2 could be known only with an indeterminacy ΔT such that $\Delta T \Delta E > h$.

Bohr's answer to Einstein's objection continued to be a subject of critical studies after Bohr's death. A severe criticism of Bohr's refutation was leveled by the Polish physicist Zygmunt Chyliński,[43] who thought it necessary to split the Einstein-Bohr experiment into two independent processes: (1) the weighing of the mass of the box, and (2) the photon emission from the box. Chyliński attempted to show without resorting to relativity that each process separately satisfies the Heisenberg relation; for, as he declared, it is obscure "why relativity or more generally finite light-velocity should have been invoked for saving the uncertainty relations." His own alternative resolution of the problem, however, abounds with intricacies and seems to be highly questionable.

Bohr's answer was also the subject of a series of investigations carried out during a number of years by Otto Halpern,[44] an Austrian-born physicist who has served since 1930 at universities and research laboratories in the United States. In his view Einstein's original conclusion that $\Delta E \Delta t < h/2\pi$, where ΔE refers to the energy (mass) loss of the system measurable with arbitrary accuracy and Δt to the opening time of the shutter, which can be made as small as we like, is "obviously correct, but it has nothing to do with the uncertainty principle. It is true that the energy lost, if any, was lost during the opening time of the shutter; but perhaps there was no loss of energy at all, due to fluctuations in the vessel. Whether energy was lost, and how much, can only be established by a time-

[43] Z. Chyliński, "Uncertainty relation between time and energy," *Acta Physica Polonica* **28**, 631–638 (1965).

[44] O. Halpern, "On the Einstein-Bohr ideal experiment," *Acta Physica Austriaca* **24**, 274–279 (1966); "On the uncertainty principle," *ibid.*, 280–286; "On the Einstein-Bohr ideal experiment II," *ibid.*, **28**, 356–358 (1968); "On the uncertainty principle II," *ibid.*, 353–355; "On the uncertainty principle III," *ibid.*, **30**, 328–333 (1969); "On the uncertainty principle IV," *ibid.*, **33**, 305–316 (1971).

consuming energy measurement. Only after the measuring time has elapsed can we know how much energy, if any, has escaped, and then we can expect an uncertainty relation to hold true between the energy lost and the time consumed during the measurement of this energy lost."

With respect to this latter measurement Halpern agreed with Bohr's conclusion that the Heisenberg relation is valid but not with Bohr's argumentation. He claimed that "even if one neglects the inaccuracies which Bohr derived with the aid of the formula for red-shift phenomena" the Heisenberg relation is not violated in this part of Einstein's experiment. Following Bohr Halpern deduced $\Delta ET \geqslant hc^2/g\Delta q$ but claimed that since $c^2/g\Delta q$, according to general relativity, is the reciprocal of the deviation of $\sqrt{g_{44}}$ from 1 and hence for weak gravitational fields a very large number, $\Delta E \cdot T > h$ has been proved without recourse to the red-shift formula. It should be noted, however, that Halpern's reference to $\sqrt{g_{44}}$ implies, implicitly at least, the red-shift formula.

The critical reader of the literature on the $\Delta E \Delta t$ relation will notice that the Δt in this relation has been differently interpreted by the various authors. In view of the widespread confusion on this issue the following historical remarks on the various theories about the role of time in quantum mechanics seem to be germane.

The basic question behind this issue is the problem whether the time coordinate t should be regarded, just like the position coordinate q, as an operator or (in Dirac's terminology) q-number or whether it should merely play the role of an ordinary parameter or c-number. The earliest discussion of this problem is found in Dirac's attempt,[45] made in the spring of 1926, to extend his method of quantization, based on the well-known analogy with the Poisson bracket of the classical theory, to systems for which the Hamiltonian involves the time explicitly.

Starting from the principle of relativity which "demands that the time shall be treated on the same footing as the other variables, and so it must therefore be a q-number," Dirac pointed out that in classical physics the canonical conjugate momentum to the time variable is minus the energy $(-E)$. Thus if t and $-E$ are taken as a new pair of variables in addition to the $2n$ variables q_k, p_k $(k=1,2,\ldots,n)$ the Poisson bracket $[x,y]$ of two dynamical variables for a system of n degrees of freedom with an explicitly time-dependent Hamiltonian, defined by the equation

$$[x,y]=\sum_k\left(\frac{\partial x}{\partial q_k}\frac{\partial y}{\partial p_k}-\frac{\partial x}{\partial p_k}\frac{\partial y}{\partial q_k}\right)-\frac{\partial x}{\partial t}\frac{\partial y}{\partial E}-\frac{\partial x}{\partial E}\frac{\partial y}{\partial t}, \tag{20}$$

[45]P. A. M. Dirac, "Relativity quantum mechanics with an application to Compton scattering," *Proceedings of the Royal Society* **A111**, 405–423 (1926).

is invariant under any contact transformation of the $(2n+2)$ variables. According to Dirac the dynamical system is now determined, not by a *function* of $2n$ variables, but by an equation $H-E=0$ between the $(2n+2)$ variables; and the equations of motion for any function X of the $(2n+2)$ variables reads $\dot{X}=[X,H-E]$.

Taking these results directly over into quantum theory Dirac managed to treat t as an operator. In fact, his method of not identifying energy with the Hamiltonian—because the former does commute with the time variable while the latter does not—and his imposition of the condition $E-H=0$ may be regarded as an anticipation of a method used later in quantum electrodynamics. For, when faced with a similar difficulty for quantum electrodynamics one introduces the auxiliary condition that the application of the operator $E-H$ on a state vector gives zero, that is, one admits only those solutions of the wave equation which satisfy this auxiliary condition.

When shortly after the publication of his paper Dirac read Schrödinger's first communication in the *Annalen der Physik* (Vol. 79, 1926) he published a second article[46] on the Compton effect in which he virtually withdrew his former approach, calling it "rather artificial." According to Léon Rosenfeld the development of quantum electrodynamics vindicates Dirac's original method, which makes it possible to put the $\Delta E \Delta t$ relation on the same footing as the $\Delta p \; \Delta q$ relation.

The generally accepted view, however, was that these two relations differ fundamentally. The position-momentum relation, it was pointed out, is a straightforward consequence of the commutation relation $[q,p]=i\hbar$ between the Hermitian operators representing these variables. On the other hand it was claimed that an analogous derivation of the time-energy relation could not be established, since, as Pauli[47] showed, time cannot be represented by a Hermitian operator T satisfying with the energy operator (Hamiltonian) H a relation $[T,H]=i\hbar$. Such a relation, generalized in the usual way to $[f(T),H]=i\hbar\partial f/\partial T$ and applied to the unitary operator $f(T)=\exp(i\alpha T)$, α being a real number, would imply that if ψ_E is an eigenfunction of H with eigenvalue E, $\exp(i\alpha T)\psi_E$ is also an eigenfunction of H, but with eigenvalue $E+\alpha\hbar$. Hence, since α is arbitrary, the eigenvalues of H would necessarily cover the real line from $-\infty$ to $+\infty$, in contradiction to the existence of discrete energy spectra.

For such reasons—in addition to the alleged fact that time "does not 'belong' (refer) to the system concerned"—Mario Bunge[48] recently sug-

[46] P. A. M. Dirac, "The Compton effect in wave mechanics," *Proceedings of the Cambridge Philosophical Society* **23**, 500–507 (1927).

[47] Ref. 3–13.

[48] M. Bunge, "The so-called fourth indeterminacy relation," *Canadian Journal of Physics* **48**,

gested that the formula $\Delta t \Delta E \geqslant \hbar$ "should be dropped from all treatments of this theory [quantum mechanics]."

If we call a pair of Hermitian operators A, B which satisfy a commutation relation of the type $[A,B]=i\hbar$ "canonical conjugates" we conclude that the Hamiltonian H has no canonical conjugate. Nor has any component of the angular momentum, such as L_Z, a canonical conjugate, a fact which, known for a long time, has raised a considerable amount of discussion.[49] The only operators that do have canonical conjugates in Hilbert space are p and q and their linear combinations.

This distinctive difference between the position-momentum and time-energy relations was undoubtedly also one of the reasons that prompted Bohr to reach further clarification on this matter. In addition, the newly developed relativistic generalization of quantum mechanics and the quantum theory of the electromagnetic field, which in those years scored their first successes, raised a number of problems such as the simultaneous measurability of field quantities which again focused attention on the Heisenberg relations.

It was in this context that Rudolf Peierls, who obtained his Ph.D. in 1929 under Pauli and was subsequently his assistant, and Lev Davidovich Landau, who between 1919 and 1931 visited West European universities, decided—when they met in 1929 at the Swiss Federal Institute (E.T.H.) in Zürich—to study the implications of the Heisenberg indeterminacies for a relativistic generalization of quantum mechanics, that is, the question whether, and to what extent, the definitions and methods of measurement

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1410–1411 (1970).

[49] W. Pauli, Ref. 3–1. P. Jordan, "Über eine neue Begründung der Quantenmechanik II," *Zeitschrift für Physik* **44**, 1-25 (1927). B. Podolsky, "Quantum-mechanically correct form of Hamiltonian function for conservative systems," *Physical Review* **32**, 812–816 (1928). D. Judge, "On the uncertainty relation for L_z and φ," *Physics Letters* **5**, 189 (1963). D. Judge and J. T. Lewis, "On the commutator $[L_z, \varphi]$," *ibid.*, 190. W. H. Louisell, "Amplitude and phase uncertainty relations," *Physics Letters (Holland)* **7**, 60–61 (1963). D. Judge, "On the uncertainty relation for angle variables," *Nuovo Cimento* **31**, 332–340 (1964). L. Susskind and J. Glogower, "Quantum mechanical phase and time operator," *Physics* **1**, 49–61 (1964). J. H. Rosenbloom, "The uncertainty principle for angle and angular momentum," (Noltr 63-207, U.S. Ordnance Laboratory, White Oak, Maryland), mimeographed. M. Bouten, N. Maene, and P. van Leuven, "On an uncertainty relation for angular variables," *Nuovo Cimento* **37**, 1119–1125 (1965). A. A. Evett and H. M. Mahmoud, "Uncertainty relation with angle variables," *ibid.*, **38**, 295–301 (1965). L. Schotsmans and P. van Leuven, "Numerical evaluation of the uncertainty relation for angular variables," *ibid.*, **39**, 776–779 (1965). K. Kraus, "Remark on the uncertainty between angle and angular momentum," *Zeitschrift für Physik* **188**, 374–377 (1965); "A further remark on uncertainty relations," *ibid.*, **201**, 134–141 (1967). P. Carruthers and M. M. Nieto, "Phase and angle variables in quantum mechanics," *Reviews of Modern Physics* **40**, 411–440 (1968). H. S. Perlman and G. J. Troup, "Is there an azimuthal angle observable?," *American Journal of Physics* **47**, 1060–1063 (1969).

of quantum mechanical quantities can be retained if quantum mechanics is relativistically generalized.[50]

In the course of proving that in a relativistic treatment the indeterminacy relations are much more restrictive and impose severe limitations on the applicability of the methods usually employed, the authors began their considerations with an analysis of the theory of measurement, of the meaning of the Heisenberg relations, and, in particular, of the time-energy relation. They pointed out that this relation," so often quoted but correctly interpreted only by Bohr," does not at all assert that energy cannot be exactly known or measured at a given time but rather refers to the difference between the value of the energy obtained as the result of a (predictable) measurement [*voraussagbare Messung*] and the value of the energy of the state of the system after the measurement.

Having demonstrated that the existence of *predictable measurements*, that is, measurements which assure that for every possible measurement result there exists a state of the system in which this measurement yields with certainty the result obtained, does not imply the existence of *reproducible measurements*, that is, measurements which assure that a repeated performance yields the same result, Landau and Peierls showed that the state of the system after the measurement is not necessarily identical with the state associated with the obtained measurement result.[51] Cognizant of this fact, they pointed out that the time-energy relation affirms that this difference of states leads to an energy indeterminacy of the order of magnitude $h/\Delta t$, so that within a time interval Δt no measurement can be performed for which the energy indeterminacy is less than $h/\Delta t$.

To substantiate their contention, Landau and Peierls considered a system of known energy E which interacts weakly with a measuring device of known energy ϵ; the measured energies E' and ϵ' after the interaction generally are different from the initial values. Referring to Dirac's method of the variation of constants (perturbation theory), according to which the probability of a transition of the system in the time interval Δt is proportional to

$$\frac{\sin^2\left[\pi(E'-E)\dfrac{\Delta t}{h}\right]}{(E'-E)^2}, \tag{21}$$

[50]L. Landau and R. Peierls, "Erweiterung des Unbestimmtheitsprinzips für die relativistische Quantentheorie," *Zeitschrift für Physik* **69**, 56–69 (1931).

[51]For detailed proofs of these statements see also L. de Broglie, *Sur une Forme plus restrictive de Relations d'Incertitude* (Hermann, Paris, 1932).

they concluded that the most probable value of the difference $E'-E$ is of the order of $h/\Delta t$ and hence independent of the strength of the perturbation. Two consecutive measurements, separated by the time interval Δt, consequently can verify the energy conservation law only with an accuracy of the order of $h/\Delta t$. Applying this result to the above-mentioned interaction they obtained for the difference between two exactly measured values of the total energy $E+\epsilon$ at two different instants the relation

$$|E+\epsilon-E'-\epsilon'|\approx\frac{h}{\Delta t} \tag{22}$$

or, if $\Delta E,\ldots,$ denotes the error in the measurement of $E,\ldots,$ and if in the most favorable case ϵ and ϵ' are exactly measurable ($\Delta\epsilon=\Delta\epsilon'=0$),

$$\Delta(E-E')\approx\frac{h}{\Delta t}. \tag{23}$$

In sharp contrast to the position-momentum relation, which denies the existence of exactly measurable values of these variables at the *same instant*, the time-energy relation, according to Landau and Peierls, relates the difference between exactly measurable values *at two different instants* with the time interval between these instants.

By applying this result to a single thought-experiment concerning a momentum measurement Landau and Peierls were able to demonstrate with particular clarity their thesis of the non-repeatability of measurements. To this end they considered a particle of initial momentum P and initial energy E to collide perpendicularly with a perfectly reflecting plane mirror whose momentum p and energy ϵ before the impact, and momentum p' and energy ϵ' after the impact, were supposed to be exactly measurable ($\Delta p=\Delta p'=\Delta\epsilon=\Delta\epsilon'=0$). To determine the momentum P of the particle (P' is its momentum after the reflection from the mirror) both the law of conservation of momentum and the law of conservation of energy must be used:

$$p+P-p'-P'=0,$$

$$|\epsilon+E-\epsilon'-E'|\approx\frac{h}{\Delta t}.$$

Under the above assumptions these imply for the inaccuracies the relations

$$\Delta P=\Delta P',$$

$$\Delta E-\Delta E'\approx\frac{h}{\Delta t}.$$

Since $\Delta E = v\,\Delta P$, where v is the velocity of the particle before the collision, and similarly $\Delta E' = v'\,\Delta p'$, they obtained

$$(v - v')\Delta p \approx \frac{h}{\Delta t}, \tag{24}$$

which shows that the momentum measurement of the particle involves a change in its velocity, a change which increases the shorter the duration of the measurement process. Short-time momentum measurements are therefore not reproducible and the measured value of the variable differs from the value of the variable after the measurement.

The far-reaching implications of these results for relativistic quantum mechanics are not our present concern.[52] Suffice it to mention that when Peierls and Landau (as a citizen of a country with which Switzerland at that time had no diplomatic relations) had to leave Zürich and went to Copenhagen, they discussed the manuscript of their paper with Bohr, who expressed reservations, just as he had done four years earlier when Heisenberg showed him his paper on the indeterminacy relations.

The conclusion which Landau and Peierls derived from perturbation theory could also be reached by considering the decay of a system into, say, two decay components under the action of some perturbation. Let τ denote the life time of a system of energy E_0, calculated without allowance for decay, E and ϵ the energies of the decay products so that $E + \epsilon$ gives an estimate of the system's energy before its decay and $\Gamma = |E_0 - E - \epsilon|$ defines the width of the energy level, then it could be shown that $\Gamma\tau \geqslant h/2\pi$.

The connection between lifetime and energy width, probably the most important application of the time-energy relation, had been known before the advent of modern quantum mechanics.[53] In fact, it was classically explained as a consequence of the damping of the atomic oscillator because of loss of energy by radiation. The earliest quantum mechanical derivation was based on Dirac's quantum theory of radiation[54] and was carried out by Victor Weisskopf and Eugene Wigner,[55] who showed in

[52]For these implications see, e.g., V. B. Berestetskii, E. M. Lifshitz, L. P. Pitaevskii, *Relativistic Quantum Theory* (Pergamon Press, Oxford, 1971), pp. 1–4.

[53]For historical details cf. W. Pauli, "Quantentheorie," in *Handbuch der Physik*, H. Geiger and K. Scheel, eds. (Springer, Berlin, 1926), Vol. 23, pp. 1–278, especially pp. 68–75. See Ref. 3–13.

[54]P. A. M. Dirac, "The quantum theory of the emission and absorption of radiation," *Proceedings of the Royal Society of London (A)* **114**, 243–265; "The quantum theory of dispersion," *ibid.*, 710–728.

[55]V. Weisskopf and E. Wigner, "Berechnung der natürlichen Linienbreite auf Grund der Diracschen Lichttheorie," *Zeitschrift für Physik* **63**, 54–73 (1930); "Über die natürliche Linienbreite der Strahlung des harmonischen Oszillators," *Zeitschrift für Physik* **65**, 18–29

exact mathematical terms how the natural broadening of spectral lines or, equivalently, the diffuseness of energy levels is complementary to the finite lifetime of excited states.

Another interpretation of the Heisenberg relation was proposed by Leonid Issakovich Mandelstam, shortly before his death (November 27, 1944), and Igor Tamm, at that time head of the Lebedev Physical Institute in Moscow. First they[56] pointed out that if energy is considered an observable in Dirac's sense, corresponding to the Hamiltonian of the dynamical system under discussion, it cannot be identified with the frequency of a monochromatic vibration multiplied by h. Consequently, they continued, Bohr's derivation[57] based on the elementary relation $\Delta\nu\,\Delta T \sim 1$, connecting the "uncertainty $\Delta\nu$ in the measurement of the frequency of the vibration with the time interval ΔT, during which this measurement is carried out," becomes invalid and the relation itself becomes meaningless.

Mandelstam and Tamm therefore thought it necessary to base their interpretation of the time-energy relation on another consideration, the fact that the total energy of an isolated quantum mechanical system in a nonstationary state, in contrast to a classical one, has no definite and constant value, and that only the probability of obtaining in a measurement any specified energy value is a constant in time. In a stationary state, on the other hand, energy is exactly determined, but then the distribution functions of *all* dynamical variables are constant in time; thus the definiteness of energy entails the constancy in time of all dynamical variables.

This conclusion suggested to them that there exists some correlation between energy dispersion and time variation of dynamical variables and that the quantitative formulation of this correlation is the meaning of the time-energy relation. To obtain this quantitative formulation the authors considered for any dynamical variable represented by the Hermitian operator R which is not a constant of motion and does not contain t explicitly, the well-known relations $\Delta R\,\Delta E \geqslant \frac{1}{2}|\langle[R,H]\rangle|$ and $(h/2\pi)\,d\langle R\rangle/dt = i\langle HR - RH\rangle$ and derived from them the inequality

$$\Delta R\,\Delta E \geqslant \left(\frac{h}{4\pi}\right)\left|\frac{d\langle R\rangle}{dt}\right| \tag{25}$$

(1930). Cf. also F. Hoyt, "The structure of emission lines," *Physical Review* **36**, 860–870 (1930) for partially identical results.

[56]L. Mandelstam and Ig. Tamm, "Sootnoshenie neopredelionnosti energii-vremeni v neotnositelnoy kvantovoy mekhanike," *Izvestiia Akademia Nauk* **9**, 122–128 (1945); "The uncertainty relation between energy and time in non-relativistic quantum mechanics," *Journal of Physics (USSR)* **9**, 249–254 (1945).

[57]See Section 4.1.

connecting the standard deviation ΔE of the energy with that of the other dynamical variable R by the time rate of change of the expectation value of R. By integrating from t to $t+\Delta t$ (E is constant since H is a constant of motion) Mandelstam and Tamm obtained $\Delta t \Delta E \geqslant (h/4\pi)|\langle R\rangle_{t+\Delta t} - \langle R\rangle_t| / \overline{\Delta R}$, where $\overline{\Delta R}$ denotes the average value of ΔR during the interval Δt. With Δt as the time interval during which the expectation value of R changes by an amount equal to $\overline{\Delta R}$, they finally obtained $\Delta t \Delta E \geqslant h/4\pi$.

The Mandelstam-Tamm interpretation has been adopted by several authors of modern textbooks[58] and simplified as follows. The inequality $\Delta R \Delta E \geqslant (h/4\pi)|d\langle R\rangle/dt|$, derived as before, is written

$$\frac{\Delta R}{|d\langle R\rangle/dt|}\Delta E \geqslant \frac{h}{4\pi} \tag{26}$$

and $\Delta t = \Delta t_R$ is defined as *the time during which the expectation value of R changes by an amount equal to its indeterminacy* ΔR, so that, again, $\Delta t \Delta E \geqslant h/4\pi$. Clearly, in a stationary state $d\langle R\rangle/dt = 0$, but also $\Delta E = 0$.

Now it will also be understood that the conventional derivation of the time-energy relation in terms of the transit time of a wave packet of length (position indeterminacy) Δq and velocity v is but a special case of the preceding considerations: if R is the position operator q. For if $\Delta t = \Delta q/v$, $\Delta E = \Delta(p^2/2m) = v\Delta p$ and hence $\Delta t \Delta E = \Delta q \Delta p \geqslant h/4\pi$, Δt has obviously been defined as the time interval during which the mean value of q changes by Δq, for v is the velocity of the mean value of q and, in fact, $\Delta t = \Delta q / |d\langle q\rangle/dt|$.

As Mandelstam and Tamm emphatically pointed out, Δt has an unambiguous meaning only with respect to a given dynamical variable (observable) R, for it denotes not the duration of the measurement of R, as often erroneously stated, but rather the time interval during which the expectation value of the observable R changes by an amount equal to the (averaged) indeterminacy of R. If this fact is disregarded, no consistent comprehension of the various applications of the time-energy indeterminacy relation can be reached.

[58] A. Messiah, *Mécanique Quantique* (Dunod, Paris, 1959), Vol. 1, pp. 269–270; *Quantum Mechanics* (North-Holland Publishing Company, Amsterdam, 1961), Vol. 1, pp. 319–320. E. Fick, *Einführung in die Grundlagen der Quantentheorie* (Akademische Verlagsgesellschaft, Leipzig, 1969), pp. 211–212. K. H. Ruei, *Quantum Theory of Particles and Fields* (University Press, Taipei, 1971), p. 101. O. Hittmair, *Lehrbuch der Quantentheorie* (K. Thiemig, Munich, 1972), pp. 43–44. B. G. Levich, V. A. Myamlin, and Yu. A. Vdovin, *Kurs Teoretičeskoj Fiziki*, Vol. 3 (Nauka, Moscow, 1971); *Theoretical Physics*, Vol. 3 (*Quantum Mechanics*) (North-Holland Publishing Company, Amsterdam, London, 1973), pp. 117–120. R. McWeeny, *Quantum Mechanics: Principles and Formalism* (Pergamon Press, Oxford, New York, 1972), p. 85.

Krylov and Fock[59] subsequently criticized both the Landau-Peierls and the Mandelstam-Tamm interpretations of the time-energy relation. Against the former they objected that the time of collision should be determined merely kinematically by the movement of the particles under discussion, one of them serving as a clock, and not by means of a (time-dependent) perturbation since no time-dependent potential is involved. Essentially, by reduction to the position-momentum relation Krylov and Fock obtained $\Delta t \geqslant h/\Delta E$, where Δt is the indeterminacy in the time the particle passes a fixed point. From the exact (previously mentioned) momentum conservation formula they then deduced that $\Delta(p'-p)=\Delta(P'-P)$ and pointed out that for a given $\Delta t \geqslant h/V\Delta P$ and a sufficiently large V the ΔP and $\Delta P'$ can be made arbitrarily small and with them also $\Delta(p'-p)$. The exact energy conservation formula therefore yields

$$\Delta(\epsilon'-\epsilon)=\frac{1}{2m}[\Delta(p'+p)(p-p)+(p'+p)\Delta(p'-p)]$$

$$=(v'-v)\Delta p=\Delta(E-E')\geqslant\frac{h}{\Delta t},$$

as obtained by Landau and Peierls.

Against the Mandelstam-Tamm derivation Krylov and Fock objected that its recourse to the wave function and operator calculus gives it only a statistical significance and prevents its application to an individual measurement; with this proviso the relation connecting the lifetime of the state of a system with the indeterminacy of its energy content is acknowledged as valid.

The interpretations of the time-energy relation as proposed by Bohr, Landau and Peierls, Krylov and Fock have in spite of minor differences with respect to its derivation one aspect in common: they all agree that the shorter the duration of an energy measurement the greater the indeterminacy in the energy transfer, or, more precisely, any measurement of energy, performed within a time interval Δt, necessarily involves a minimum indeterminacy in energy transfer to the observed system $\Delta(E'-E) \geqslant h/\Delta t$.

This conclusion was rejected in 1961 by Yakir Aharonov and David Bohm,[60] primarily on two grounds: (1) since it was obtained only on the

[59]N. S. Krylov and V. A. Fock, "Dve glavnye interpretatsii sootnosheniia neopredelennosti dlia energii i vremeni," *Zurnal Eksperimentalnoj i Teoretičeskoj Fiziki* **17**, 93–96 (1947); "On the uncertainty relation between time and energy," *Journal of Physics USSR* **11**, 112–120 (1947).

[60]Y. Aharonov and D. Bohm, "Time in the quantum theory and the uncertainty relation for time and energy," *Physical Review* **122**, 1649–1658 (1961).

basis of thought-experiments it violated the general principle that all indeterminacy relations should be derivable from the mathematical formalism as well; (2) the examples of measurement processes adduced as arguments were not sufficiently general and hence misleading. In fact, Aharonov and Bohm proposed the following scheme as an example for a precise energy measurement in an arbitrarily short time interval Δt. Denoting the variables of the observed system by x,p_x and those of the apparatus by y,p_y they considered the Hamiltonian

$$H=\frac{p_x{}^2}{2m}+\frac{p_y{}^2}{2m}+yp_xg(t)=H_x+H_y+H_{\text{interaction}} \tag{27}$$

where $g(t)$ is zero except for $t_0 \leqslant t \leqslant t_0+\Delta t$, where it is a constant. The equations of motion $\dot{x}=p_x/m+yg(t)$, $\dot{y}=p_y/m$, $\dot{p}_x=0$ and $\dot{p}_y=-p_xg(t)$ show that p_x is a constant of the motion and that $p_y=p_y{}^0-p_xg(t)\Delta t$. Thus p_x can be measured by observing $p_y-p_y{}^0$, provided the change of deflection $\Delta(p_y-p_y{}^0)$ of the apparatus is greater than the indeterminacy $\Delta p_y{}^0$ in the initial state of the apparatus, or $\Delta p_xg(t)\Delta t \geqslant p_y{}^0$. But this can be achieved for arbitrarily small Δp_x and Δt if $g(t)$ is chosen sufficiently large. Translated into experimental language, $g(t)$ corresponds to the action of a force during Δt or to a double collison. Aharonov and Bohm thus concluded that energy can be measured reproducibly in an arbitrarily short time interval.

In his criticism of the Aharonov-Bohm challenge Fock[61] pointed out that the use of a discontinuous function of time, corresponding to an instantaneous switching on and off of the interaction, amounts to the introduction of a field whose structure violates the indeterminacy relation; by taking as their premise the very proposition to be proved Aharonov and Bohm, according to Fock, *committed a petitio principii*. In an attempt to defend their thesis, Aharanov and Bohm[62] explained that the occurrence of indeterminacies in the energy of the "field" $g(t)$ need not necessarily introduce equal indeterminacies in the energy of the observed particle. In a letter to the editor of the (Soviet Physics) *Uspekhi* Fock[63] replied that the

[61]V. A. Fock, "O sootnoshenni neopredelnnosti dlia energii i vremeni i ob odnoj popytke evo oprovergnut," *Zurnal Eksperimentalnoj i Teoretičeskoj Fiziki* **42**, 1135–1139 (1962); "Criticism of an attempt to disprove the uncertainty relation between time and energy," *Soviet Physics JETP* **15**, 784–786 (1962).

[62]Y. Aharonov and D. Bohm, "Answer to Fock concerning the time energy indeterminacy relation," *Physical Review* **134B**, 1417–1418 (1964).

[63]V. A. Fock, "Yesho raz sootnoshenni neopredelnnosti dlia energii i vremeni," *Uspekhi Fizičeskikh Nauk* **86**, 363–365 (1965); "More about the energy-time uncertainty relation," *Soviet Physics Uspekhi* **8**, 628–629 (1966).

denial of the transferability of the field quanta of infinite energy to the particle signifies a denial of the applicability of the energy conservation law to the object-instrument system, whereas this law serves as a premise for the discussion of the change in the energy of the observed system.

A more sympathetic evaluation of the Aharonov-Bohm refutation of the time-energy relation in the sense of Bohr and his followers has been given by G. R. Allcock,[64] who accepted the conclusion of the reproducibility of accurate energy measurements in arbitrarily short time intervals; Allcock confined his objection to the Aharonov-Bohm paper merely to their neglect of the effect of photon emission on the energy balance but admitted that this effect can be ignored in nonrelativistic measurements since the total energy emitted by photons is on the average proportional to c^{-3}.

The difficulties encountered in the derivation and interpretation of the Heisenberg-Bohr type of the time-energy relation, in contrast to that of the Mandelstam-Tamm type, have their sources, as we have seen, in the fact that "time" in quantum mechanics plays the role of an extraneous topologically ordering parameter t and not of a dynamical variable representable by a Hermitian (hypermaximal) operator. The fact that although all other quantities (especially those x,y,z closely connected with t by the Lorentz transformation) are represented by operators, there corresponds to the time an ordinary number-parameter t, had been called by von Neumann "an essential,...in fact, the chief weakness of quantum mechanics."[65]

If one could introduce an operator T satisfying with the Hamiltonian H the commutation relation $[T,H]=ih/2\pi$, the time-energy and position-momentum relations would have the same logical status. Motivated by the relativity requirement of treating time and position coordinates as well as energy and momentum components on a common footing, Schrödinger,[66] in 1931, had explored the possibilities of introducing four-dimensional multiplicative Hermitian operators for the four-vector (t,x,y,z) in the same Hilbert space, but he had no success.

The problem was taken up again in 1958 by Folker Engelmann and Eugen Fick[67] on the grounds that Dirac's theorem concerning the eigenval-

[64] G. R. Allcock, "The time of arrival in quantum mechanics," *Annals of Physics* **53**, 253–285, 286–310, 311–348 (1969); cf. also M. Razavy, "Time of arrival operator," *Canadian Journal of Physics* **49**, 3075–3081 (1971).

[65] Ref. 1–2 (1932, p. 188; 1955, p. 354).

[66] E. Schrödinger, "Spezielle Relativitätstheorie und Quantenmechanik," *Berliner Berichte* **1931**, 238–248.

[67] F. Engelmann and E. Fick, "Die Zeit in der Quantenmechanik," *Nuovo Cimento, Supplement* **12**, 63–72 (1959).

ues of conjugate operators does not necessarily hold if the unitary transformation involved in the proof does not lead to an automorphism between the original and the transformed eigenfunctions. Having thus shown how the major objection against the introduction of a quantum mechanical time operator may be overcome, the authors seem to have been prompted by the following correspondence considerations. If at the (Newtonian) time t_0, defined by a macroscopic clock, the state of a dynamical system is characterized by (p_0, q_0), the laws of mechanics determine the state (p,q) at the clock reading t. It is, however, also possible, conversely, to determine the time $t - t_0$ by inspecting the state (p,q), in which case the system itself serves as a clock; for according to the Hamilton-Jacobi theory there exists a function $T(p,q)$ for which $T(p,q) - T(p_0, q_0) = t - t_0$.

If it were thus possible to define a Hermitian operator corresponding to $T(p,q)$, which is also hypermaximal, it would provide—in accordance with von Neumann's axiomatization of the quantum mechanical formalism—an observable representing the behavior of a quantum mechanical clock. This "inner time," in contrast to the conventional time parameter t, the "external time," would be subject to the typically quantum mechanical fluctuations being measured on a microscopic system. For the state of a sharp value of the energy, for example, the indeterminacy or spread of this time observable could be infinitely great, while its expectation value should be equal to t.

Elaborating on these ideas Harry Paul[68] constructed such a time operator T for a system composed of a free particle moving along the one-dimensional q-axis: $T = \frac{1}{2}m\ (qp^{-1} + p^{-1}q)$. This time operator satisfied with the Hamiltonian operator $H = p^2/2m$ the commutation relation $[T,H] = ih/2\pi$, and the relation $d\langle T\rangle/dt = (2\pi/ih)[T,H]$, as can be seen without difficulty; hence, in fact, $\langle T\rangle = t$. Paul also succeeded in constructing T for the case of a one-dimensional linear harmonic oscillator and was able to define "clock-states" φ_τ of the system which satisfy (approximately) the eigenvalue equation $T\varphi_\tau = \tau\varphi_\tau$. However, upon closer examination of the problem, he came to the conclusion that the expectation value of T can be established only for a very restricted category of states so that the derivation of the time-energy relation in this context similarly is valid only in exceptional cases.

The question of introducing a quantum mechanical time operator, as shown by Fick and Engelmann[69] and by Allcock, seems to yield a physically significant answer only if the conventional formalism of quan-

[68]H. Paul, "Über quantenmechanische Zeitoperatoren," *Annalen der Physik* **9**, 252–261 (1962).

[69]E. Fick and F. Engelmann, "Quantentheorie der Zeitmessung," *Zeitschrift für Physik* **175**, 271–282 (1963); **178**, 551–562 (1964).

tum mechanics is appropriately generalized, for instance, by relaxing the condition that only hypermaximal operators represent observables; or by working in a "super Hilbert space" as defined, for example, by David M. Rosenbaum.[70]

Another interesting attempt to cope with this problem was the representation of quantum mechanics in which position, momentum, energy, and time are treated alike, each being represented by a measurable function on an appropriate topological measure space S, as worked out by Bayard Rankin[71] of the Case Institute in Cleveland, Ohio. In this theory each observable, including time, has a probability distribution which makes it possible to interpret the time-energy relation as an expression of the fact that the smaller the energy variation the greater is the randomness of time as a function (operator) on the normalized measure space S.[72]

Prompted by the controversy about the interpretation of Δt in Heisenberg's relation, Hans-Jürgen Treder, since 1963 Professor at Humboldt University and subsequently director of the Institute for Pure Mathematics at the East Berlin German Academy of Sciences, recently discussed again the Einstein photon box experiment. According to Treder[73] it has become evident that the relation $\Delta t \Delta E \geqslant h$ does not apply to indeterminacies in energy and time measurements on stationary systems; rather, by asserting that in a determination of an energy difference $E_2 - E_1$ with an indeterminacy ΔE the two energy measurements must be separated in time by an interval $\Delta t \geqslant h/\Delta E$, it associates the lifetime (half-life) Δt of nonstationary states with their energy-spread (line-breadth): if the initial state of a nonstationary system has a sharp energy, the energy of the final state has a statistical line-breadth $\Delta E \approx h/\Delta t$.

To apply this result to the Einstein-experiment, Treder argued, we have to consider an ensemble Σ of boxes, each of equal initial energy and equal

[70]D. M. Rosenbaum, "Super Hilbert space and the quantum mechanical time operator," *Journal of Mathematical Physics* **10**, 1127–1144 (1969).

[71]B. Rankin, "Quantum mechanical time," *Journal of Mathematical Physics* **6**, 1057–1071 (1965).

[72]For a novel but elementary reinterpretation [*dans un esprit nouveau*] of the time-energy relation in terms of time averages of operators see E. Durand, *Mécanique Quantique* (Masson, Paris, 1970), Vol. 1, pp. 122–132. For another unconventional derivation (in conformity with the metatheorem according to which the formalism of quantum mechanics is capable of yielding its own interpretation) see I. Fujiwara, "Time-energy indeterminacy relationship," *Progress of Theoretical Physics* **44**, 1701–1703 (1970).

[73]H. J. Treder, "Das Einstein-Bohrsche Kasten-Experiment," *Monatsberichte der Deutschen Akademie der Wissenschaften zu Berlin* **12**, 180–184 (1970); "The Einstein-Bohr box experiment," in *Perspectives in Quantum Theory*, W. Yourgrau and A. van der Merwe, eds. (MIT Press, Cambridge, London, 1971), pp. 17–24.

tension of the suspending spring. Prior to the emission of a particle each system "box + spring" is in a stationary equilibrium state. As soon as a particle of mass dm is emitted, the tension of the spring and the weight of the box no longer balance and each system begins to oscillate with an amplitude dq proportional to the force acting on the system (α is the spring-constant)

$$\alpha \cdot dq = g \cdot dm. \tag{28}$$

If there were no damping, the vibrational energy would be given for each system by

$$\alpha \cdot (dq)^2 = g \cdot dm \cdot dq \tag{29}$$

and the variation dq of the spring-elongation and hence also dm would not be ascertainable. Due to inner friction, however, this motion is damped and the energy given by (29) dissipates as heat into the environment (under loss of information). The amount of dissipated energy varies from system to system with a fluctuation ΔE inversely proportional to the mean lifetime Δt of the oscillation-process

$$\Delta E \approx \frac{h}{\Delta t}. \tag{30}$$

Once the energy has dissipated, the position and hence final energy content of each system of Σ can be sharply determined, the spread in position, due to (28) and (30), being given by

$$\alpha \Delta q = \Delta E \cdot \frac{g}{c^2} = \frac{g^h}{c^2 \Delta t} \tag{31}$$

which yields

$$\Delta t \Delta E = h. \tag{32}$$

The shorter the mean time Δt necessary to reach equilibrium, the larger the spread ΔE in the energy loss. Hence to measure $E_2 - E_1$ with accuracy ΔE the final position reading must be separated from the initial position reading at least by the time interval $t = h/\Delta E$. This condition shows that for a given ΔE, even if $t_2 - t_1$ (the opening time of the shutter in Einstein's box) were made arbitrarily small, the reading of the final pointer position (to determine E_2) must lag behind t_1 at least by the time interval $\Delta t = h/\Delta E$, a restriction not envisaged by Einstein.

According to Treder, as we see, it is the quantum mechanical behavior of the spring and not any gravitational effect which saves Heisenberg's time-energy relation from Einstein's attack. To corroborate this conclusion

Treder modified the thought-experiment by replacing the (vertical) gravitational field by an electrostatic field F and assuming that the box, supposed to be practically massless, contains indistinguishable electrically charged particles (e.g., protons) of specific charge e/m_p. The total charge Q, the total mass M, and total energy E are then related by the equation

$$Q=\frac{e}{m_p}\sum m_p=\frac{e}{m_p}M=\frac{e}{m_p c^2}E \tag{33}$$

and

$$\alpha q=FQ. \tag{34}$$

Bohr's reasoning, Treder declared, would lead us to

$$\frac{h}{\Delta q}=\Delta p<\Delta Q\cdot F\cdot T=\frac{e}{m_p c^2}\,\Delta E\cdot F\cdot T. \tag{35}$$

But since, according to Treder, the red-shift time dilatation does not apply to this modified situation and since Δq therefore does not imply any indeterminacy ΔT in T, it would follow that, even if Bohr's counterargument were valid for the gravitational box experiment, it would break down for the electrostatic analog and Heisenberg's relation would be violated. Moreover, Treder pointed out, it is incomprehensible how, in Bohr's derivation, the indeterminacy in the momentum of the box, caused by the interference of the observer when reading q, should increase with the increase of the time T of the balancing process which is not related to this interference.

Concerning Treder's conclusion based on his modification of the thought-experiment we wish to point out that, contrary to Treder's view, Einstein's general equivalence principle also applies to energy changes in electrostatic fields. Finally, Treder's deviation of the Heisenberg relation from the quantum properties of the spring seems to us logically unassailable, provided one accepts the statistical objective interpretation of indeterminacy relations, as proposed by Popper and others, as well as the Mandelstam and Tamm interpretation of the time-energy relation.

After this lengthy digression let us return to the history of the Bohr Einstein debate. The result of the 1930 phase of the Einstein-Bohr debate may be best summarized by saying, as Bohr admitted himself, that Einstein was defeated but not convinced. Einstein, in spite of his failure to disprove the Heisenberg relations, refused to accept statistical statements as final laws in physics and thus did not change his personal credo concerning the validity of quantum mechanics, which he so pointedly formulated in a famous letter to Born, written on December 4, 1926: "The quantum mechanics is very imposing. But an inner voice tells me that it is still not the true Jacob. The theory yields much, but it hardly brings us nearer to the secret of the Old one. In any case I am convinced that he does not throw dice... I am toiling at deriving the equations of motion of material particles regarded as singularities from the differential equations of general relativity...."[74]

How arduously Einstein was toiling to connect quantum mechanics with general relativity and how, in particular, he might have thought to reconcile the indeterminacy relations with a causal and continuous field theory may be seen from his article "Unified Theory of Gravitation and Electricity,"[75] which he published in collaboration with Walter Mayer shortly after the Sixth Solvay Congress. This paper was prompted by Theodor Kaluza's[76] attempt to formulate a unified field theory in a five-dimensional space-time to account for both gravitation and electromagnetism in terms of a *single metric*, in contrast to Hermann Weyl's well-known approach which associated with the Einstein metric tensor $g_{\mu\nu}$ an additional gauge vector field permitting a path dependence for the transference of length.

It seems that Einstein by introducing five-vectors into a four-dimensional space-time cherished the hope that a unified field theory would dispense with the Heisenberg indeterminacies, for they could then be regarded as merely projections onto a world of four-vectors, and their statistical implications could be regarded as the result of the suppression of the fifth component, which is necessary for a complete strictly deterministic description of five-dimensional physical processes. It would then be clear that the Bohr-Heisenberg formulation of the quantum theory offers only an incomplete description of physical reality; at the same time, the puzzle—how this theory in spite of its incompleteness could be so successful—would have found a satisfactory solution. However Einstein's attempt to solve the "quantum problem" through such a generalization of the differential geometry of space-time turned out to be abortive.

[74]Ref. 14 (1956, p. 258; 1969, pp. 129–130).

[75]A. Einstein and W. Mayer, "Einheitliche Theorie von Gravitation und Elektrizität," *Berliner Berichte* **1931**, 541–557.

[76]Th. Kaluza, "Zum Unitätsproblem der Physik," *Berliner Berichte* **1921**, 966–972. Concerning the specific point under discussion Einstein could have been influenced also by Oskar Klein's remark: "Es ist bekanntlich immer weniger wahrscheinlich geworden, dass die Quantenerscheinungen eine einheitliche raumzeitliche Beschreibung zulassen, wogegen die Möglichkeit, diese Erscheinungen dürch ein System von fünfdimensionalen Feldgleichungen darzustellen, wohl nicht von vornherein auszuschliessen ist." O. Klein, "Quantentheorie und fünfdimensionale Relativitätstheorie," *Zeitschrift für Physik* **37**, 895–906 (1926), quotation on pp. 905–906.

This failure, combined with the outcome of the discussions at the 1930 Solvay Congress, led Einstein to admit the logical consistency of the Heisenberg relations and of Bohr's point of view. In fact, Einstein's tactics, as we shall see in the next chapter, changed from now on: not the *inconsistency* but rather the *incompleteness* of Bohr's approach became the object of Einstein's criticisms. This change in Einstein's position, which roughly coincided with his emigration from Europe to the United States of America, may be taken as the turning point that separates the earlier phases of the Bohr-Einstein debate, as treated in the present chapter, from its later phases, which will be discussed in the next chapter. But before proceeding to these later issues whose impact, as we shall see, is still felt today, let us conclude this chapter with a short resumé of some general evaluations of the Bohr-Einstein debate.

5.5. SOME EVALUATIONS OF THE BOHR-EINSTEIN DEBATE

In a study of the epistemological significance of the debate Hans Naumann[77] of the Technical University of Dresden (German Democratic Republic) explained the conflict between Einstein and Bohr as a consequence of the irreconcilability of materialism and idealism. Contrary to this view, Boris Kouznetsov,[78] vice-chairman of the Einstein Committee of the USSR Academy of Sciences and author of an Einstein biography (1962, 1963, 1965) which became a best-seller in the Russian language, viewed the controversy as a symptom, not of philosophical or ideological differences, but rather of the failure of current physics to reach a consistent synthesis of the concepts of relativity with those of quantum mechanics. A similar conclusion, though on completely different grounds, was reached by D. Bohm and D. L. Schumacher[79] in their analysis of what they claimed to be the characteristic feature of this debate, the failure to communicate. This failure, which in their view was more significant than the contents debated, "led physics to split into mutually irrelevant fragmentary parts which tended to develop fixed forms, rather than to engage in a genuine dialogue in which each would change, permitting something new to emerge." This fragmentation, according to Bohm and Schumacher, is the ultimate source of the absence of a full harmony of quantum

[77] H. Naumann, "Zur erkenntnistheoretischen Bedeutung der Diskussion zwischen Albert Einstein und Niels Bohr," *Deutsche Zeitschrift für Philosophie* **7**, 389–411 (1959).

[78] B. Kouznetsov, "Einstein and Bohr," *Organon* **2**, 105–121 (1965).

[79] D. Bohm and D. L. Schumacher, "On the failure of communication between Bohr and Einstein" (preprint, 1972).

mechanics with relativity. "What is customarily called 'relativistic quantum theory,' whatever its detailed form, *is* the failure of communication between Bohr and Einstein." Similarly, C. A. Hooker,[80] in a recent essay on quantum mechanical reality and the Bohr-Einstein debate, regarded the issues of this debate not only as far from dead, but even as a stimulus to "make relevant contributions to lines of research today."

According to C. F. von Weizsäcker, however, the Bohr-Einstein debate was merely the result of a serious misunderstanding (which has nothing to do with a failure to communicate). Although Einstein, for whom physical concepts were free creations of the human mind, never espoused the position of naive realism, he rightfully opposed, von Weizsäcker contended, any attempt at eliminating the notion of reality from physics. But it was a "tragic error"[81] on his part to believe that Bohr did just that. For Bohr, as von Weizsäcker emphasized, never rejected the notion of reality, he only modified it; what he rejected was merely the absolute separation between object and subject which characterizes classical physics. Although Bohr's philosophy shared with Mach's positivism its denial of the naive realistic dogma, it did not share its denial of physical reality.

In contrast to von Weizsäcker Kurt Hübner[82] viewed the Bohr-Einstein debate as merely a reflection of two different or even diametrically opposed principles: one (espoused by Einstein) according to which physical reality consists of substances which possess properties independently of their relations to other substances, and the other (espoused by Bohr) according to which reality is essentially a relation between substances, measurement being a special case of such a relation. Moreover, Hübner declared, "for Einstein relations are defined by substances; for Bohr substances are defined by relations." Hübner also claimed that neither Bohr nor Einstein succeeded in proving his own principle or in disproving that of the opponent for each based his argumentation on his own principle. According to Hübner, as we see, a consensus was not reached, not because the dispute was based on a failure to communicate or on a

[80] C. A. Hooker, "The nature of quantum mechanical reality: Einstein versus Bohr," in *Paradigms and Paradoxes* (Ref. 24), pp. 67–302.

[81] "Sein [Einsteins] tragischer Irrtum scheint mir darin zu liegen, dass er meint, dies [den Begriff der Wirklichkeit aus der Phyzsik zu entfernen] geschehe in der Quantenmechanik." "Einstein und Bohr," in C. F. von Weizsäcker, *Voraussetzungen des naturwissenschaftlichen Denkens* (Hanser Verlag, Munich, 1971; Herder, Freiburg im Breisgau, 1972), pp. 41–50, quotation on p. 48.

[82] K. Hübner, "Über die Philosophie der Wirklichkeit in der Quantenmechanik," *Philosophia Naturalis* **14**, 3–24 (1973), especially section 1 ("Der Streit zwischen Einstein und Bohr und ihre philosophischen Prinzipien"). (A German translation of a talk delivered at the University of Pennsylvania on September 10, 1971.)

misunderstanding, but simply because the disputants never really came to grips with the fundamental issue of their dissension.

It is interesting to note that according to some philosophers of science[83] the previously mentioned Leibniz-Clarke (Newton) debate had fared precisely the same way.

[83]Cf., e.g., F. E. L. Priestley, "The Clarke-Leibniz controversy," in *The Methodological Heritage of Newton*, R. E. Butts and J. W. Davis, eds. (Basil Blackwell, Oxford, 1970), pp. 34–56.

The INCOMPLETENESS OBJECTION and Later Versions of the Complementarity Interpretation

Chapter Six

6.1. THE INTERACTIONALITY CONCEPTION OF MICROPHYSICAL ATTRIBUTES

Bohr's point of departure in developing his complementarity interpretation was, as we have seen, the quantum postulate, which ascribes to every atomic or elementary process an essential discontinuity and thus prevents an exhaustive causal and spatiotemporal description. Its ontological basis lies in the wave-particle duality and its operational implications are manifested in the indeterminacy relations. Clearly, the operational incompatibility of exact values of conjugate dynamical variables was not the ultimate foundation of Bohr's theory.[1] However, the fact that the early textbooks[2] on quantum mechanics based their exposition of the theory on the Heisenberg relations, as well as certain ambiguous statements concerning the inseparability of the object and the observer made by Bohr and Heisenberg themselves, led to the widespread view that it is the disturbance of the object by the observation that entails the principle of indeterminacy and thus eventually constitutes the foundation of the whole theory.[3]

That the principle of indeterminacy is not a *consequence* of the unattainability of exact measurements but rather, in conjunction with the assumption of the unavoidability of a disturbance by observation, the *cause* of it had been pointed out as early as 1929 by Hans Reichenbach,[4] who at that time taught philosophy of physics at the University of Berlin. The separation of physical measurements or observations into an observer-independent occurrence and an observing device, said Reichenbach, is an idealization that emerged in macroscopic physics without being strictly true even in classical physics; though expedient for obtaining a simple description of natural phenomena, it is not an indispensable premise for scientific cognition. Similarly, in microphysics the disturbance of an electron by its illumination as in the Heisenberg gamma-ray microscope experiment would be inconsequential, if it were possible to infer the electron's position and momentum from observational data by a theory which takes into consideration all relevant factors, such as the pressure of light. The meaning of the principle of indeterminacy is precisely the

[1]Arguments for the thesis that the mere operational incompatibility of such values is neither a necessary nor sufficient condition for their lack of theoretical meaning were advanced by A. Grünbaum, "Complementarity in quantum physics and its philosophical generalizations," *Journal of Philosophy* **54**, 713–727 (1957).

[2]See Section 3.1.

[3]This claim has also been made by philosophers; see, e.g., Nicolai Hartmann, *Philosophie der Natur* (W. de Gruyter, Berlin, 1950), p. 374.

[4]H. Reichenbach, "Ziele und Wege der physikalischen Erkenntnis," in *Handbuch der Physik*, Vol. 4, H. Geiger and K. Scheel, eds. (Springer, Berlin, 1929), p. 78.

statement that such a corrective theory is not possible.

That the indeterminacy principle expresses the impossibility of establishing such a corrective theory of errors for microphysics as has been established for macrophysics has also been emphasized by Edgar Zilsel,[5] a lecturer at the *Volkshochschule* in Vienna. Unavoidable disturbances, according to Zilsel, are not confined to quantum physics; every measurement of temperature by a thermometer, of current intensities by an ammeter, of potentials by a voltmeter, and even every measurement of length by comparison with a meterstick whose mass, strictly speaking, changes the surrounding gravitational field is accompanied by such disturbances.

As Zilsel illustrated by the example of a temperature measurement, one need not even take care that these disturbances be as small as possible, for it is always possible in classical physics to correct for them with arbitrary accuracy on the basis of laws whose application never leads to conflict with experience. The fact that similar laws are unavailable in microphysics —and not that every measurement causes disturbance—is the reason for the validity of the indeterminacy relations. In other words, indeterminacy and with it quantum mechanics in its present form is, according to Zilsel, the result of an empirical methodological situation, namely, the unavailability of certain fine-grained corrective theories in microphysics.

If Zilsel's view may be called a *minimum* formulation of Bohr's interpretation, for it retains a minimum of ontological assumptions, Pascual Jordan's position, which was the subject of Zilsel's essay, may be called a *maximum* formulation of that interpretation. Jordan declared,[6] with emphasis, that observations not only *disturb* what has to be measured, they *produce* it! In a measurement of position, for example, as performed with the gamma-ray microscope, "the electron is forced to a decision. We compel it *to assume a definite position*; previously it was, in general, neither here nor there; it had not yet made its decision for a definite position.... If by another experiment the *velocity* of the electron is being measured, this means: the electron is compelled to decide itself for some exactly defined value of the velocity; and we observe *which* value it has chosen. In such a decision the decision made in the preceding experiment concerning position is completely obliterated." According to Jordan, every observation is not only a disturbance, it is an incisive enchroachment into the field of observation: "we ourselves produce the results of measurement" [*Wir selber rufen die Tatbestände hervor*].[7]

[5]E. Zilsel, "P. Jordans Versuch, den Vitalismus quantenmechanisch zu retten," *Erkenntnis* **5**, 56–64 (1935).
[6]Ref. 4-4 (1934).
[7]*Op. cit.*, p. 228.

The thesis to which Jordan gave so eloquent expression—that microphysical properties or determinations such as position or momentum (velocity) are not attributes possessed by the particle in the classical sense but are the result of interactions with the measuring device or instrument of observation—became in the early 1930s the characteristic feature of the complementarity interpretation. It was now thought that Bohr's complementary experimental arrangements signify mutually exclusive methods of *producing* measurement results or, as Jordan would have said, mutaully effacing constraints to enforce decisions. It was an absolute renunciation of any realistic conception of nature.

One of the earliest to challenge this view was Paul Jensen (1868–1952), who at that time had just retired from his professorship of physiology at the University of Göttingen. In his opinion a logical elaboration of such reasoning would lead to the far-reaching conclusion that, not only in microphysics but quite generally, any physical state of affairs would be the outcome only of observation; furthermore, an objective set of facts, independent of the sense organs and brains of men or manlike creatures, would not exist.[8]

Rejecting such a radically positivistic or idealistic exposition of the complementarity interpretation Jordan pointed out[9] that the act of observation of a macrophysical object differs in principle from that of a microphysical object. For the observation, for example, of the position of the moon at a certain moment t determines something which can be ascertained independently of this particular act of observation by observation of other kinds or at times before or after t; the results of these other observations are not affected by whether the observation at t was carried out or not. The epistemological situation in quantum mechanics therefore cannot be generalized for physics as a whole.

That, indeed, the "interactionality" of microphysical attributes is intimately related to the "mutual exclusiveness" of complementary notions can best be understood by the following consideration. Imagine an ensemble A of micro-objects which can be in a state ψ_1 so that all members of A have property x, or in a state ψ_2 so that all members of A have property y, where x and y are incompatible properties, that is, no system can simultaneously be of property x and of property y. As is well known, the formalism of quantum mechanics admits all linear superpositions of ψ_1 and ψ_2 as possible states. Let ψ_3 be such a superposition. As borne out by experience, it is possible that when measuring x for A in state ψ_3, we find that, say, 70% of all members of A have property x, and when measuring y

[8] P. Jensen, "Kausalität, Biologie und Psychologie," *Erkenntnis* **4**, 165–214 (1934).
[9] Ref. 4-4 (1934).

for A in state ψ_3, we find that likewise 70% of all members of A have property y. But x and y are incompatible.

Were x and y objectively existing properties that had been revealed but not disturbed or produced by these two measurements, a serious contradiction would have arisen, especially if it is further assumed that the two measurements could be performed simultaneously. In fact, a simple calculation would then show that at least 40% of the members of A would have both properties x and y. Logical consistency will be preserved if and only if (1) two such measurements can never be performed simultaneously and (2) every measurement is an interaction with the micro-object and affects its properties. Condition 1 expresses the mutual incompatibility or exclusiveness of the experimental arrangements, which was a necessary condition for their being complementary, and 2 denies the instrument-independency of measured observables.

The denial of an autonomous status of complementary attributes, such as position and momentum, need not necessarily imply a denial of the objective reality of the micro-object itself to which such attributes can be ascribed. Although many positivistically inclined proponents of the complementarity interpretation in the early 1930s contended, on these grounds, that a micro-object is nothing but "a bundle of appearances" or a linguistic-computational link between reproducible experimental arrangements and their observable consequences, the complementarity interpretation does not enforce this instrumentalist conclusion. Thus, for example, Philipp Frank's[10] claim that "the 'electron' is a set of physical quantities which we introduce to state a system of principles from which we can logically derive the pointer readings on the instruments of measurements" is not warranted by the complementarity interpretation, for the simple reason that this interpretation does not make any statements concerning noncomplementary variables such as the mass or the charge of an electron. It has been repeatedly stressed by Max Born[11] that apart from complementarity properties micro-objects may exhibit other properties which are invariants of observation. "Though an electron does not behave like a grain of sand in every respect, it has enough invariant properties to be regarded as just as real."

[10] P. Rank, "Foundations of physics," in *International Encyclopedia of Unified Science*, Vol. 1, No. 7 (University of Chicago Press, Chicago, 1946), p. 54.

[11] M. Born, *Natural Philosophy of Cause and Chance* (Oxford University Press, London, 1949; Dover, New York, 1964), pp. 104–105; "Physical reality," *Philosophical Quarterly* **3**, 139–149 (1953), reprinted in M. Born, *Physics in my Generation* (Pergamon Press, London, New York, 1956), pp. 151–163. "Physikalische Wirklichkeit," *Die Pyramide* **3**, 82–87 (1953); *Physikalische Blätter* **10**, 49–61 (1954); *Physik im Wandel meiner Zeit* (Vieweg, Braunschweig, 1957), pp. 145–159.

In this context it should be noted that the assumption that a particle has simultaneously well-defined values of position and momentum, even though these may be unknown and unobservable, can be refuted independently of the complementarity interpretation, as Dimitrii I. Blokhintsev[12] showed for the case of electrons in the helium atom. Instead of Blokhintsev's treatment let us discuss the simpler case of hydrogen atoms and show that the assumption of the simultaneous existence of such values is untenable.

First it should be recalled that by the scattering of x-rays or electrons the distribution of electrons in the atom, corresponding to $|\psi(r)|^2$, can be determined by experiments which are found to be in excellent agreement with the theory. As is well known, the total energy of hydrogen atoms in the ground state $(n=1, l=0, m=0)$

$$|\psi_{100}|=\left(\frac{a^3}{\pi}\right)^{1/2}\exp(-ar), \tag{1}$$

where $a=1/a_0$ and $a_0=0.53\text{Å}$ is the Bohr radius, is

$$E_0=-13.55\,\text{eV}. \tag{2}$$

The potential energy $U(r)$ of an electron in this atom is given by $-e^2/r$ and increases with the distance r from the nucleus. Solving $U(r)=E_0$, we find that if $r>r_1\approx 2\text{Å}$, then

$$U(r)>E_0. \tag{3}$$

To find the percentage P of electrons for which $r>r_1$, we calculate

$$P=4\pi\int_{r_1}^{\infty} r^2|\psi_{100}|^2\,dr \tag{4}$$

[12]D. I. Blokhintsev, *Osnovy Kvantovoi Mekhaniki* (G.I.T.T.L., Moscow, Leningrad, 1949; Vysshaya Shkola, 1964); *A Kvantummechanika Alapjai* (Tankönyvkiadó, Budapest, 1952); *Grundlagen der Quantenmechanik* (Deutscher Verlag der Wissenschaften, Berlin, 1953, 1961); *Základy Kvantové Mechaniky* (Československa Akademie Vied, Prague, 1956); *Principles* of Quantum Mechanics (Reidel, Dordrecht; Allyn and Bacon, Boston, 1964); *Mécanique Quantique* (Masson, Paris, 1969). "Kritika filosofskich vozzrenje tak nazyvajemoj 'Kopenhagenskoj shkoly' v fizike" in *Filosofičiskij Voprosy Sovremennoj Fizike* [Philosophical Problems of Modern Physics] (Moscow, 1952), pp. 358–395; "Kritik der philosophischen Anschauungen der sogenannten 'Kopenhagener Schule' in der Physik," *Sowjetwissenschaft (Naturwissenschaftliche Abteilung)* **6**, 545–574 (1953). The history of this problem can be traced back to Heisenberg's Chicago lecture [Ref. 3-19 (1930, pp. 33–34)]. Heisenberg's solution was criticized by Hans Reichenbach [Ref. 8-83 (1944, 1965, p. 165; 1949, pp. 180–181)].

and obtain $P=0.23$. Hence almost 25% of the electrons in unexcited hydrogen atoms have potential energies exceeding E_0. Had such an electron both a position coordinate r and a momentum variable p, then we could write

$$E_0 = \frac{p^2}{2m} + U(r). \tag{5}$$

Hence for such electrons

$$\frac{p^2}{2m} = E_0 - U(r) < 0 \tag{6}$$

and p would assume an imaginary value, an unacceptable result.

As Eino Kaila[13] pointed out, the complementarity view that a micro-object in itself has neither a sharp position coordinate q nor a sharp momentum coordinate p and that it is meaningless [*sinnlos*] to speak of such accurate simultaneous values as compatible with the Heisenberg relation invalidates Heisenberg's reasoning in deriving this relation from his thought-experiments. In this derivation, as we have seen, precisely such accurate values have been presupposed when it was shown that they lose their precision due to an uncontrollable disturbance caused by their interaction with the experimental arrangement. Mentioning also the fact that, apart from Dirac's theory, "there are relatively elementary cases that suggest the idea of a negative 'kinetic energy,'"[14] Kaila criticized the early version of the complementarity interpretation as not having consistently and distinctly differentiated between the language of classical physics and the language of quantum mechanics. Since, as Kaila maintained, terms like position, momentum, and energy are subjected in quantum mechanics to axioms that differ essentially from those of classical physics, their meaning similarly must differ from what they denote in classical physics. Although he seemed therefore to be inclined to ascribe a quantum mechanical meaning to the concept of a negative kinetic energy, he did not discuss the question whether such an admission implies the assumption of simultaneous sharp values of conjugate variables. Instead, Kaila concentrated on finding the correct relation between the two languages and showed that their systematic distinction leads to the result that Bohr's conception of the "indivisible quantum of action," on which Bohr has based his whole approach, does not provide a consistent and sufficient foundation for the quantum theory.

[13]E. Kaila, "Zur Metatheorie der Quantenmechanik," *Acta Philosophica Fennica* **5**, 1–98 (1950).
[14]Kaila, Op. cit., p. 82.

If it has been shown that a micro-object, such as an electron in the hydrogen atom, cannot have both a position and a momentum variable in certain regions of space where its existence is experimentally assured, there is no reason whatever to assume that this state of affairs is confined to such regions only. One must instead conclude that a micro-object never has both a position and a momentum coordinate simultaneously or rather has neither and that such variables or attributes are produced only through the very process of their respective measurements.

The state description of quantum mechanical systems as conceived by the proponents of the complementarity interpretation in the early 1930s was, as we see, based on the notion of physical interactions. To assert, for example, that a particle has a definite momentum p implied that the particle had been subjected de facto to a momentum-measuring device which recorded the value p. This "interactional" state description, as we may call it for brevity, underwent within the next few years a subtle but philosophically important modification whereby the state description became "relational" in a sense to be explained in due course.

Einstein's photon box seemed to be the catalyst of this conceptual development within the complementarity interpretation. It was the photon box experiment which led Einstein, as we shall see presently, to the basic idea of the so-called Einstein-Podolsky-Rosen (EPR) paradox, which, in turn, forced Bohr and his school to modify their conception of state descriptions as indicated.

6.2. THE PREHISTORY OF THE EPR ARGUMENT

As stated, the outcome of the 1930 discussion between Bohr and Einstein about the indeterminacy relations was that "Einstein was defeated but not convinced." His lack of conviction, as we shall see presently, referred from now on less to the validity of these relations than to the soundness of the quantum theory as a whole. In fact, as the following episode indicates, Einstein now fully accepted the validity of the Heisenberg relations—in a sense, even more so than their originator himself (if we recall that Heisenberg admitted exact retrodictions).[15]

Having received in 1930 an invitation to give a series of lectures at the California Institute of Technology in Pasadena, Einstein spent a few weeks of the following winter in California (as he did also in the two subsequent winters, residing at the Athenaeum). In spite of the inevitable sightseeing tours (e.g., an Indian tribe in Arizona adopted him as a member with the

[15] See Section 4.2.

honorary title Chief Great Relative), Einstein found time in Pasadena for scientific work. Together with the noted physicist Richard Chase Tolman, who from 1922 until his death served also as dean of the Institute's Graduate School, and with Boris Podolsky, a young Russian physicist who had obtained his Ph.D. there under Paul S. Epstein only two years earlier, Einstein wrote a paper entitled "Knowledge of Past and Future in Quantum Mechanics."[16] Although not containing one single mathematical formula, it must have been regarded as a unique contribution, for *Science*, in its News Supplement of March 27, 1931, gave it great publicity by previewing its contents and stating: "Professor Einstein laid one of the foundations of the quantum theory, building on the work of Professor Max Planck.... Now Professor Einstein adds the latest building block to our conception of matter and energy by telling us that the past as well as the future is uncertain."[17]

The Einstein-Tolman-Podolsky paper opened with the remark that it has sometimes been supposed that quantum mechanics admits of an exact description of the past path of a particle. "The purpose of the present note," it continued, "is to discuss a simple ideal experiment which shows that the possibility of describing the past path of one particle would lead to predictions as to the future behaviour of a second particle of a kind not allowed in the quantum mechanics. It will hence be concluded that the principles of quantum mechanics actually involve an uncertainty in the description of past events which is analogous to the uncertainty in the prediction of future events."

For this purpose the authors considered (Figure 7) a small box B, containing identical particles in thermal agitation to such an amount that cases arise in which—by releasing the shutter S of two small openings for a short time—one particle traverses the direct path SO, and a second particle the longer path SRO, being elastically reflected at the ellipsoidal reflector R. If the observer at O measures the momentum, using, for example, the low-frequency Doppler effect of the directly approaching first particle and subsequently its time of arrival at O, the time when the shutter was released can be exactly calculated from the known distance BO and the computed velocity of the first particle. If it is further assumed that the box was weighed before and after the shutter was opened, the total energy released and hence—after knowing the momentum of the first particle—also the energy and velocity of the second particle can be determined. Assuming the total distance BRO to be sufficiently large compared to BO,

[16]A. Einstein, R. C. Tolman, and B. Podolsky, "Knowledge of past and future in quantum mechanics," *Physical Review* **37**, 780–781 (1931).
[17]*Science* **73**, (1891), Supplement: Science News, 10 (1931).

"it would then seem possible to predict beforehand both the energy and the time of arrival of the second particle" with arbitrary accuracy, in contrast to the energy-time indeterminacy relation.

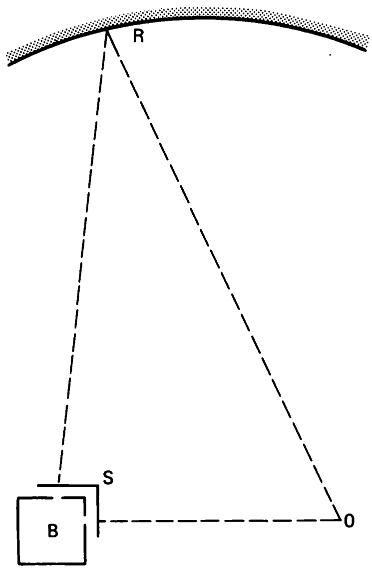

Figure 7.

The solution of this paradox, according to Einstein, Tolman, and Podolsky, was in the fact "that the past motion of the first particle cannot be accurately determined as was assumed." And they continued:

> Indeed, we are forced to conclude that there can be no method for measuring the momentum of a particle without changing its value. For example, an analysis of the method of observing the Doppler effect in the reflected infrared light from an approaching particle shows that, although it permits a determination of the momentum of the particle both before and after collision with the light quantum used, it leaves an uncertainty as to the time at which the collision with the quantum takes place. Thus in our example, although the velocity of the first particle could be determined both before and after interaction with the infrared light, it would not be possible to determine the exact position along the path SO at which the change in velocity occurred as would be necessary to obtain the exact time at which the shutter was open.

Generalizing these considerations the authors concluded that the indeterminacy relations apply to predictive and retrodictive measurements alike or, as they expressed it, "the principles of the quantum mechanics must involve an uncertainty in the description of past events which is analogous to the uncertainty in the prediction of future events."

It is undoubtedly an interesting historical testimony for the prevailing obscurity of the logical status of the indeterminacy relations that precisely four weeks later, on March 27, 1931, another visiting professor from Europe, Charles Galton Darwin of the University of Edinburgh, in a lecture delivered at the Lowell Institute in Massachusetts, made the following statement, which was published in the same volume of *Science*[18] as the Einstein-Tolman-Podolsky paper: "The uncertainty principle is essentially only concerned with the future; we can install instruments which will tell us as much about the past as we like." To illustrate his statement, Darwin described the following thought experiment:

> Suppose, for example, that we have two shutters, each provided with a very small hole, and a source of electrons to the left of both. The holes are usually blocked up, but for a very short space of time I first open the one in the left shutter, and at a definite time later I do the same for the one on the right. I look for electrons to the right of both shutters. If I see one, I can be quite certain that it went along the line between the holes and took a definite time in doing so; that is to say, I can know its position and speed precisely. What the principle asserts is that this knowledge is no use in predicting what is going to happen later, for it gives no knowledge of how the electron will be diffracted on emerging from the second hole.

Darwin's thought-experiment is, of course, only a repetition of Heisenberg's imaginary experiment discussed in his 1929 Chicago lecture and coincides with the case (a) of Popper's determination of the "path" of a particle.[19] As such it refers, at best, only to a limited section or part of the particle's "past," the time interval after the passing of the first hole and before the passage through the second hole. If determinism is understood as the unambiguous determination of an interminable sequence of evolving events, the Einstein-Tolman-Podolsky thesis that the indeterminacy relations preclude the thesis of determinism with respect both to the future and the past is not refuted by this experiment.

Darwin's two-shutter experiment had been proposed at the Nashville meeting of the American Physical Society on December 30, 1927, by Arthur Edward Ruark,[20] at that time assistant professor at Yale University,

[18]C. G. Darwin, "The uncertainty principle," *Science* 73, 653–660 (1931).
[19]Ref. 3-15 (1959, p. 219). See also below.
[20]Ref. 3-45.

as a possible refutation of the Heisenberg relations. A few weeks later, at the New York meeting of the Society on February 24–25, 1928, Ruark[21] analyzed this experiment in great detail and concluded: "The velocity of the particle changes when it passes the first slit, for it may be considered as a group of waves and the frequency of each harmonic train in the group is changed by the modulation due to the shutter. This involves a change of energy, that is a modified velocity."

The problem was further investigated by Earle Hesse Kennard[22] who, after his return from Copenhagen to Cornell, where he served as Professor of Physics from 1927 to 1946, studied in detail what he called the "rapid-shutter" effect. If a shutter is opened for a period τ, Kennard pointed out, the position of a particle approaching with velocity v and passing through the shutter is known up to an uncertainty $v\tau$. By regarding the particle as a wave packet and subjecting it to a Fourier analysis, he showed that its momentum is known with an uncertainty $mv/2\nu\tau$, where the de Broglie frequency ν is given by $mv^2/2h$. Hence $\Delta p = h/\tau v$ and $\Delta p\,\Delta x = h$ as required.

Einstein, as we know from his correspondence with Paul Ehrenfest and Paul S. Epstein, did not lose interest in the photon box experiment in spite of the fiasco in Brussels. In 1931 he adopted a new attitude toward this experiment: instead of using it as a weapon for a frontal attack on the Heisenberg relation he tried to derive from it a logical paradox. This was the trend of his ideas. Assume the box with the clock and the photon has been weighed and the photon subsequently released; one then has the choice either of repeating the weighing of the box—and then one would know the exact energy emitted—or of opening the box, and reading the clock and comparing the reading which had been disturbed by the weighing process with the standard time scale—and then one could predict the exact moment of its arrival after it has been reflected by a fixed mirror at a known distant position:

> Without in any way interfering with the photon between its escape and its later interaction with suitable measuring instruments, we are, thus, able to make accurate predictions pertaining *either* to the moment of its arrival *or* to the amount of energy liberated by its absorption. Since, however, according to the quantum-mechanical formalism, the specification of the state of an isolated particle cannot involve both a well-defined connection with the time scale and an accurate fixation of the energy, it might thus appear as if this

[21] A. E. Ruark, "Heisenberg's uncertainty relation and the motion of free particles," *Physical Review* **31**, 709 (1928).

[22] E. H. Kennard, "Note on Heisenberg's indetermination principle," *Physical Review* **31**, 344–348 (1928).

formalism did not offer the means of an adequate description.

It is obvious that Einstein's reasoning along these lines already contained the central idea of what later became known as the Einstein-Podolsky-Rosen paradox.

That such considerations did, in fact, engage Einstein's attention in 1931 can be seen from a letter[23] written by Ehrenfest to Bohr, dated July 9, 1931. Telling Bohr that he expected Einstein to visit Leiden for a few days at the end of October, Ehrenfest invited Bohr to come at the same time so that they could *quietly* (Ehrenfest wrote *RUHIG* in capital letters!) exchange their views. Einstein, continued Ehrenfest in his letter to Bohr, no longer intends to use the box experiment as an argument "against the indeterminacy relations" but for a completely different purpose: to construct a "machine" which ejects a projectile. *After* the projectile has left the machine, an "interrogator" [*Frager*] asks the "machinist" to inspect the machine and to predict *either* what value a of a magnitude A *or* what value b of a magnitude B the interrogator will obtain upon subjecting the projectile to an A measurement or to a B measurement, respectively, when the projectile returns after a rather long period of time, having been reflected by a distant reflector; A and B are assumed to be noncommutative quantities. Ehrenfest now informed Bohr that Einstein believed the photon box was just such a machine, the energy of the light quantum (projectile) and its time of arrival playing the roles of A and B.

At the end of his letter Ehrenfest described in full detail how according to Einstein such an experiment could be performed:

1. Set the clock's pointer to time 0 hour and arrange that at the pointer position 1000 hours the shutter will be released for a short time interval.
2. Weigh the box during the first 500 hours and screw it firmly to the fundamental reference frame.
3. Wait for 1500 hours to be sure that the quantum has left the box on its way to the fixed reflector (mirror), placed at a distance of $\frac{1}{2}$ light-year away.
4. Now let the interrogator choose what prediction he wants: (α) *either* the exact time of arrival of the reflected quantum, *or* (β) the color (energy) of it. In case (α) open the still firmly screwed box and compare the clock reading (which during the first 500 hours was affected, due to the gravitational red-shift formula) with the standard time and find out the correct standard time for the pointer position "1000 hours;" then the exact time of arrival can be computed; in case (β) weigh the box again for 500 hours;

[23]Bohr-Archive, Copenhagen.

then the exact energy can be determined.

In a postscript addressed to Mrs. Bohr, Ehrenfest wrote that he mailed the letter to *her* with the request not to show the letter to Bohr should he be too tired(!), but if she does hand the letter to Bohr, to tell him that there is absolutely no need for an answer (dass er ABSOLUT NICHT NOETIG HAT ZU ANTWORTEN—in capital letters and underlined).

Einstein did not keep these ideas secret. In fact, when invited by his colleague Max von Laue, shortly after returning from a visit to Pasadena (to be discussed), to give a "colloquium" on November 4, 1931, at the Berlin University, Einstein chose the indeterminacy relation as the subject of his talk.[24] Discussing again the thought-experiment of a box containing a clock and an automatic shutter which opens momentarily to release a radiative pulse of about 100 waves to be reflected from a remote mirror, Einstein pointed out that by weighing the box the color (energy) could be ascertained, or by inspecting the clock the time of release could be accurately ascertained—but not both. He emphasized that what to predict, energy or time, could be decided upon well after the radiation left the box.

That in 1933 Einstein was in possession of almost the complete physical contents of the Einstein-Podolsky-Rosen paradox can also be seen from a testimony given by Léon Rosenfeld. Einstein spent the time between his emigration from Nazi Germany and his embarkation, in Southampton, for the United States in the beautiful Belgian resort Le Cocque sur Mer (north of Oostende). Rosenfeld, at that time a lecturer at the University of Liège, had just completed the paper with Bohr in which it was shown that the quantum-electrodynamic measurements of the electromagnetic field strengths are consistent with the indeterminacy relations, and he gave a lecture in nearby Brussels on this subject.

As Rosenfeld recalled,[25] Einstein attended the lecture and followed its arguments with the closest attention. Though voicing no doubt about the logic of the argumentation, Einstein, in the discussion, expressed "uneasiness" (Einstein used the work *Unbehagen*) about the whole matter. Frankly he asked Rosenfeld:

> What would you say of the following situation? Suppose two particles are set in motion towards each other with the same, very large, momentum, and that they interact with each other for a very short time when they pass at known

[24]Cf. "Über die Unbestimmtheitsrelation," *Zeitschrift für Angewandte Chemie* **45**, 23 (1932). (Abstract)

[25]L. Rosenfeld, "Niels Bohr in the thirties" in Ref. 3-24 (pp. 114–137).

> positions. Consider now an observer who gets hold of one of the particles, far away from the region of interaction, and measures its momentum; then, from the conditions of the experiment, he will obviously be able to deduce the momentum of the other particle. If, however, he chooses to measure the position of the first particle, he will be able to tell where the other particle is. This is a perfectly correct and straightforward deduction from the principles of quantum mechanics; but is it not very paradoxical? How can the final state of the second particle be influenced by a measurement performed on the first, after all physical interaction has ceased between them?

Rosenfeld had the impression, when listening to Einstein, that at that time Einstein regarded the case as just "an illustration of the unfamiliar features of quantum phenomena." It is, however, clear that at that time Einstein had already begun to modify his photon box experiment and its paradoxical consequences, as mentioned before, into the imaginary experiment of two temporarily interacting particles.

How this modification proceeded through various stages in Einstein's mind can be traced in great detail from a letter which Einstein wrote to Paul Epstein in 1945.[26] Referring to what was later called the Einstein-Podolsky-Rosen paradox, Einstein wrote: "I myself arrived at these ideas starting from a simple thought-experiment." And now he described an ideally reflecting photon box B, which contains a clock operating a shutter and a quantum of radiation of low, but unknown, frequency. In contrast to the 1930 experiment he assumed this box to be moveable not in a vertical but rather in a horizontal direction along a frictionless rail which serves as a reference system K; and at one end S of the rail either an absorbing screen or a reflecting mirror can be mounted. An imaginary observer sitting on the box B and in possession of certain measuring devices was supposed to release the shutter at a precisely determinable moment, so that a photon would be emitted in the direction of S. Thereupon the observer could *either* immediately establish a rigid connection between B and K to measure the position of B—in which case he could predict the time of arrival of the photon at S—*or* he could measure, using the Doppler effect method with arbitrarily low frequency, the momentum of B relative to K in accordance with the recoil formula, momentum of $\mathrm{B} = h\nu/c$—in which case he could predict the energy of the photon arriving at S.

Since thus *either* the energy (or momentum) *or* the exact time of arrival (or position) of the photon may be predicted by choice, *both* attributes have to be ascribed to the photon, for, after all, the photon is a physical reality whose properties cannot depend on the discretion of a distant

[26]Letter from Einstein to P. Epstein, dated November 10, 1945, *Einstein Estate*, Princeton, N. J.

observer. The only logical alternative—that a subsequent measurement performed on B can *physically* affect the photon receding from B—seemed to Einstein unacceptable, for such an assumption would imply an action-at-a-distance or an action propagated with a velocity larger than c. Such an assumption, he wrote to Epstein, though logically possible, was against his physical intuition to such an extent that he could not take it seriously—quite apart from the fact that one cannot form any clear conception about the structure of such a process. Einstein's own narrative shows in detail how the idea of a gravitating photon box gradually changed into that of a system of two interacting particles, as later used in the Einstein-Podolsky-Rosen paradox. For the time being (1933), however, the role of one of these particles was still, on the whole, played by the photon box. The final elimination of the box itself and its replacement by a second "particle" may have been prompted by the following development.

Karl Popper (since 1965 Sir Karl Raimund Popper), one of the great humane thinkers of our time for whom "it makes all the difference in the world whether we put Truth in the first or in the second place," as E. C. G. Boyle once stated, started his academic career with "a mistaken thought-experiment," as he later called it himself. After obtaining his Ph.D. in Vienna and publishing some pedagogical essays in *Die Quelle* (1927, 1931, 1932) and a paper in *Erkenntnis* (1933), his scientific début—while he was still earning his living as an elementary school teacher—was a paper in *Die Naturwissenschaften* (1934) which contained, as he later called it, "a gross mistake for which I have been deeply sorry and ashamed ever since." It is not impossible that it was precisely this "mistake" which prompted Einstein (who immediately recognized the error) to publish, together with Podolsky and Rosen, the argument against the completeness of quantum mechanics.

Popper's point of departure was an indictment of Heisenberg for not having carried out his announced program of ridding the quantum theory of experimentally inaccessible quantities of "unobservables" and thus not having purged the theory of its metaphysical elements. To prove this contention Popper analyzed the relation between the concept of a path of a particle and the indeterminacy relations. Defining "path" as the set of position *and* momentum coordinates for a given interval of time, or in symbols $\{q(t),p(t)\}$ for $t_0 \leqslant t \leqslant t_1$, Popper pointed out that by combining the results of (a) two consecutive measurements of position or (b) a measurement of momentum followed by that of position, or (c) a measurement of position followed by that of momentum, the "path" of a particle for the whole period between the two measurements can be accurately established in spite of the indeterminacy relations. Such measurements he called "nonpredictive measurements" [*nichtprognostische*

Messungen]. In case *b* he was of the opinion that even the path prior to the first measurement could, in certain circumstances, be determined. He reasoned that contrary to the case of an observation of *position*, in which the unavoidable high-frequency radiation strongly interacts with the particle and disturbs its momentum, in the case of observing the *momentum* the possible use of arbitrarily low frequency leaves the momentum practically unchanged; hence it does not affect the position either, even though it fails to disclose it. By the second measurement, however, the position may be inferred and the path of the particle may be determined not only between the two measurements, but even *before* the first.

Popper now continued his proof as follows. There are only two alternatives:

1. Either the particle has both an exact position *and* an exact momentum —but then, since both of them (at least for the time after the second measurement) are not simultaneously ascertainable according to Heisenberg, "nature is still bent on hiding certain physical magnitudes from our eyes... [namely] the 'position-cum-momentum,' or the 'path.'" In this case the indeterminacy principle asserts a limitation of knowledge and is *subjective*.

2. According to what may be called the *objective* interpretation of the principle, a particle has "only either an exact position combined with an inexact momentum, *or* an exact momentum combined with an inexact position"—but then the formalism which, as we have seen, enables us to calculate exactly the "path" between the two measurements contains metaphysical elements, for such a "path" cannot be tested by observation.

3. It is puzzling that in this logical analysis Popper completely ignored the remaining logical possibility: The particle has *neither* an exact position *nor* an exact momentum. This assumption, though also leading to the conclusion obtained from assumption 2, would have brought him most closely to the Copenhagen view according to which "exact positions" or "exact momenta" are the results of certain (complementary) measurement procedures. Even Popper's conclusion (his statistical interpretation of the Heisenberg relations) could be reconciled with assumption 3.

The foregoing difficulties, continued Popper, can be removed if the Heisenberg relations are not regarded as statements relating to the behavior of an individual micro-object but rather as statistical statements or scatter relations. The fact that the indeterminacy relations can be logically derived from the formalism cannot disprove this contention, for —first of all—it is only the formula of these relations and not their interpretation that is derived, and—second—the ψ-function, on which the formalism is based, has itself only a statistical meaning (Born's interpretation of ψ).

Instead of the subjective interpretation, which reads "The more precisely the position of a particle is measured the less is known about its momentum and *vice versa*," Popper thus advocated what he called the *statistical objective interpretation*. Given an ensemble (aggregate of particles or sequence of experiments performed with *one* particle which after each experiment is reprepared to its original state) of particles from which, at a certain moment and with a given precision Δx those having a certain position x are selected; the momenta p of the latter will then show a random scattering with a range of scatter Δp where $\Delta x \Delta p \geqslant h$, and vice versa.

In short, the Heisenberg formulae have to be interpreted as relations holding between certain ranges of statistical dispersions.[27] In fact, not only do they not rule out the possibility of performing measurements in individual cases with arbitrary accuracies, they even require them in order to test the relation between the statistical dispersions; otherwise the Heisenberg formulae would be merely metaphysical statements. Such precision measurements are made possible by resorting to the kind of nonprognostic experiments described under (*a*), (*b*) and (*c*). Nor would the attainment of such precisions refute the validity of quantum mechanics, continued Popper; for the Bohr-Heisenberg interpretation of the indeterminacy formulae, as delimiting the attainable precision in each individual instance, is an *additional* assumption, not contained in the formalism of the theory.

Moreover, Popper thought it possible to prove his contentions by the explicit construction of a thought-experiment[28] which describes a predictive measurement performable with an accuracy that contradicts the Bohr-Heisenberg interpretation of the indeterminacy formulae. Intent on singular predictions and hence uninterested in statistical considerations, Popper resorted to an idealization of the Compton-Simon and Bothe-Geiger experiments on collisions between particles and photons, obeying the nonstatistical conservation laws of momentum and energy. Letting a beam of (monoenergetic) particles (*A* particles) of known momentum a_1 cross with a diverging pencil of (monochromatic) light radiation of given wavelength and hence known magnitude of momentum $|b_1|$ (*B* particles), Popper considered two narrow beams of these particles which intersect at the point

[27]That Born's interpretation of the wave function neither implies nor contradicts Popper's thesis has been pointed out by P. Feyerabend, "Problems in microphysics," in *Frontiers of Science and Philosophy*, R. G. Colodny, ed. (University of Pittsburgh Press, Pittsburgh, 1962), pp. 189–283.

[28]K. Popper, "Zur Kritik der Ungenauigkeitsrelationen," *Die Naturwissenschaften* **22**, 807–808 (1934); Ref. 3-15 (1935, pp. 178–181, 222–224; 1959. pp. 243–264, 303–305).

S [*Schnittpunkt*]. As soon as a definite direction for the beam of B particles has been selected, b_1 (and not just $|b_1|$) can be computed. Choosing now a direction SX and attending to those A particles within their narrow beam which, after collision, proceed in the direction SX, Popper employed the conservation laws to calculate a_2 and b_2, the momenta of the respective particles after their collision. To every A particle scattered into SX with momentum a_2 corresponds a B particle, scattered with momentum b_2 into the calculable direction SY.

> We now place an apparatus at X—for instance a Geiger-counter or a moving film strip—which records the impact of particles arriving from S at the arbitrarily restricted region X. Then we can say: as we note any such recording of a particle, we learn at the same time that a second particle must be travelling from S with the momentum b_2 towards Y. And we also learn from the recording where this second particle was at any given moment; for we can calculate from the time of the impact of the first particle at X, and from its known velocity, the moment of its collision at S. By using another Geiger-counter at Y (or the moving film band) we can test our predictions for the second particle.

The central idea of Popper's imaginary experiment was, as we see, to obtain, by virtue of the conservation laws, from a nonpredictive measurement of the path of one particle (the A particle) a predictive measurement of the path of its partner (the B particle), with which it had collided.

The editorial board of *Die Naturwissenschaften*, composed of Max Hartmann, Max von Laue, Carl Neuberg, Arthur Rosenheim, and Max Volmer, aware of the crucial importance of Popper's paper but skeptical about its scientific soundness, asked von Weizsäcker to comment on it. In a postscript[29] to Popper's paper von Weizsäcker pointed out that Popper's argument concerning the exact reconstruction of the path of the A particle preceding the momentum measurement at X is untenable, for such a measurement, for instance by means of a Doppler effect using low-frequency radiation, requires—precisely because of the low frequency—a relatively long duration of time so that the average velocity during this time interval is not accurately knowable. Popper's proposed ideal experiment, von Weizsäcker thus concluded, does not allow any relaxation of the

[29]K. F. von Weizsäcker, *Die Naturwissenschaften* **22**, 808 (1934). Popper, it seems, was not satisfied with von Weizsäcker's comments. In a letter to Einstein, dated August 29, 1935, Popper referred to his discussions with Viktor Weisskopf (at that time Pauli's assistant) on this matter and added: "trotzdem sind wir in langen und ausführlichen Diskussionen zu keiner stichhaltigen Widerlegung gekommen."

restrictions imposed by the Heisenberg relations.

Shortly after November 30, 1934, when the paper was published, Popper, encouraged by Einstein's friend the violinist Adolf Busch whom Popper knew through Busch's son-in-law, Rudolf Serkin, sent an offprint of the article together with a copy of his just published book, *Logik der Forschung*, to Einstein for comments.

Einstein replied that the experiment could not be carried out since, to predict position and momentum of the *B* particle, both time and energy of the *A* particle have to be measured simultaneously at *X*, which is impossible.

Popper's mistaken thought-experiment, of which he had sent an offprint to Einstein in December 1934, discussed an interaction between two particles and, after their separation, the performance of a measurement on one of them to obtain a prediction on the other. In these general lines it bears a striking resemblance to the Einstein-Podolsky-Rosen thought-experiment, as we shall see. It is therefore only natural to raise the question whether Popper's letter might not have had some influence on Einstein and his collaborators in their approach to the problem, even if Einstein, according to Rosenfeld's report, had been thinking in 1933 about such an experimental setup.[30]

For the sake of historical accuracy we make the following digression. Although we do not share the view that the main objective of investigations in the history of science is the search for priorities which, if pursued to their extreme, amount to showing, as somebody once pungently remarked, that "nothing has ever been discovered," we feel it our duty to point out that an essential point of the Einstein-Podolsky-Rosen argument can be found in a paper written by von Weizsäcker in 1931. Working for his Ph.D. (1933) under Heisenberg at the University of Leipzig, he was asked by Heisenberg to carry out a mathematically rigorous treatment,

[30]According to Professor Nathan Rosen, it might have been possible that Popper influenced Einstein (private communication, April 22, 1967); but according to Mrs. Polly Podolsky, who kept in close touch with her (in 1966 deceased) husband's work, it was unlikely that Popper's work could have reached Einstein before the first draft of the Einstein-Podolsky-Rosen paper was written (letter from Mrs. Polly Podolsky, dated August 1, 1967). Sir Karl Popper expressed his opinion on this issue in a letter dated April 13, 1967, as follows: "Now about your 'hunch.' This I find extremely flattering; but I must say that it never occurred to me before reading your letter: the possibility that a *gross mistake* made by a nobody (like myself) may have had any influence on a man like Einstein never entered my head. From a purely temporal view your hunch cannot be entirely ruled out..." (letter to the author). That from a purely logical view his reasoning was closely related to the Einstein-Podolsky-Rosen argument was clearly recognized by Popper when he wrote in a footnote to p. 244 of Ref. 3-15 (1959): "Einstein, Podolsky, and Rosen use a *weaker* but *valid* argument." In fact, one may say that Popper's is an overdetermined Einstein-Podolsky-Rosen argument.

within the Heisenberg-Pauli formulation of quantum electrodynamics, of the determination of the position of an electron by means of a microscope.

In the first part of his paper[31] von Weizsäcker subjected the famous gamma-ray microscope thought-experiment to a close examination in the course of which he arrived at the following conclusion. If a photon is diffracted by an electron and the momenta of both were known before their collision, from a measurement of the momentum of the diffracted photon, by virtue of the conservation laws, the *momentum* of the electron after the impact can be inferred; according to quantum mechanics the electron must consequently be represented by a plane monochromatic wave. If, on the other hand, the photon, after the impact, is directed, in the optical system, not to the focal plane but to the image plane of the system, the position of its collision with the electron, and not its momentum, is determined; hence the *position* of the electron at the moment of collision can be ascertained and, according to quantum mechanics, the electron must be represented by a spherical wave, originating from this position. Although not explicitly stated in the paper, it follows from these considerations that whether to describe the electron by a plane wave (sharp momentum) or by a spherical wave (sharp position) depends on the decision of the observer as to what kind of measurement (i.e., where to place the photographic plate) he wants to perform, a decision which, in principle, can be made even after the photon has ceased to interact with the electron.[32]

Asked whether he was aware of these conceptual implications when writing his paper, Professor von Weizsäcker declared:[33]

> The problem which led to this paper was certainly closely related to that raised by Einstein, Rosen and Podolsky. Except that Heisenberg, who suggested it to me, and I as well regarded this state of affairs not as a paradox, as conceived by the three authors, but rather as a welcome example to illustrate the meaning of the wave function in quantum mechanics. For this reason the matter did not carry such weight for us as it did for Einstein and his collaborators on the grounds of Einstein's philosophical intentions. The purpose of my paper was not to bring into full relief facts which were for us self-evident, but rather to examine, by means of a quantum-field-theoretical computation, the consistency of the underlying assumptions. The work, thus, properly speaking, was rather an exercise in quantum field theory and its

[31] K. F. von Weizsäcker, "Ortsbestimmung eines Elektrons durch ein Mikroskop," *Zeitschrift für Physik* **70**, 114–130 (1931).

[32] For a nontechnical description of this thought-experiment cf. W. Büchel, *Philosophische Probleme der Physik* (Herder, Freiburg, Basel, Vienna, 1965), pp. 406–421, 456–458.

[33] Letter to the author, dated November 13, 1967.

purpose, for the sake of which Heisenberg had proposed it to me, was rather a test of whether quantum field theory is a good quantum theory, than an additional analysis of the quantum theory itself.

It may well be that Heisenberg and von Weizsäcker were fully aware of the situation without regarding it as a problem. But as happens so often in the history of science, a slight critical turn may open a new vista with far-reaching consequences. As the biochemist Albert Szent-Gyorgyi once said: "Research is to see what everybody has seen and to think what nobody has thought." In fact, even if it was only a slight turn in viewing a well-known state of affairs, the work of Einstein and his collaborators raised questions of far-reaching implications and thus had a decisive effect on the subsequent development of the interpretation of quantum mechanics.

When in the late fall of 1933, on the invitation of Abraham Flexner, the founder and first director of the Institute for Advanced Study, Einstein entered upon his new position in Princeton, he brought Walter Mayer from Berlin. But Mayer soon obtained an independent position and Einstein was looking for the assistance of young mathematicians or physicists to continue his work. It happened that Boris Podolsky, who had already worked with Einstein and Tolman on the Pasadena paper[34] of 1931, and had left shortly afterward for the Ukranian Physico-Technical Institute in Kharkow to work with Vladimir A. Fock[35] (and Lev D. Landau) on quantum electrodynamics, had just returned on a fellowship to the Princeton Institute and Einstein became interested in him. At about the same time (in 1934) Nathan Rosen, a graduate of M.I.T., who had obtained his Ph.D. there under J. C. Slater, began to work at Princeton University. While still at M.I.T. Rosen had published a paper on a systematic method of calculating the interaction of two atoms each of which, as in the case of two hydrogen atoms, has one or two equivalent *S* electrons outside its closed shells,[36] and another paper[37] in collaboration with M. S. Vallarta, on spherically symmetrical statical fields in unified theory, a subject on which Einstein had been working for 10 years.

[34]Ref. 16.

[35]The best known result of their collaboration was the discovery that the Coulomb interaction can be derived from Dirac's quantum-electrodynamical field theory. Cf. V. A. Fock and B. Podolsky, "On the quantization of electromagnetic waves and the interaction of charges on Dirac's theory," *Physikalische Zeitschrift der Sowjetunion* **1**, 801–817 (1932).

[36]N. Rosen, "Calculation of interaction between atoms with *s*-electrons," *Physical Review* **38**, 255–276 (1931).

[37]N. Rosen and M. S. Vallarta, "Spherically symmetrical field in unified theory," *Physical Review* **36**, 110–120 (1930).

Venturing one day to enter Einstein's office, Rosen was surprised by the friendliness with which Einstein inquired about his work. When on the following day he met Einstein in the yard of the Institute, Einstein said to him: "Young man, what about working together with me?"[38] Shortly thereafter Rosen became a research fellow at Einstein's department. This then is the story of how Podolsky and Rosen joined Einstein.

6.3. THE EPR INCOMPLETENESS ARGUMENT

In the early spring of 1935 Einstein, Podolsky, and Rosen wrote their famous paper "Can Quantum-Mechanical Description of Physical Reality be Considered Complete?"[39] in the presentation of which we shall deliberately follow the original text as closely as possible.

The paper contains four parts: (A) an epistemological-metaphysical preamble; (B) a general characterization of quantum mechanical description; (C) the application of this description to a specific example; and (D) a conclusion drawn from parts (A) and (C).

Part A proposes (a_1) a necessary condition for the completeness of a physical theory: "every element of the physical reality must have a counterpart in the physical theory" ("condition of completeness"). It also proposes (a_2) a sufficient condition for physical reality: "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" ("condition of reality" or "criterion of reality"). Accordingly, elements of physical reality are not to be defined by a priori philosophical considerations, "but must be found by an appeal to results of experiments and measurements."

In part B the quantum mechanical description in terms of wave functions is summarized and it is pointed out that of two physical quantities which are represented by noncommuting operators the precise knowledge of one of them precludes such a knowledge of the other. It is also stated—in the sense of a complete (exhaustive) logical disjunction—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." For otherwise, completeness being assumed, the

[38]Interview with N. Rosen (Ma'ariv), March 5, 1954.

[39]A. Einstein, B. Podolsky, and N. Rosen, "Can quantum-mechanical description of physical reality be considered complete?" *Physical Review* **47**, 777–780 (1935). Reprinted in *Physical Reality*, S. Toulmin, ed., Harper and Row, Evanstone and London, 1970, pp. 122–142.

completeness condition would imply that these quantities would form part of the description and would hence both be predictable, contrary to the theory.

In part C it is shown that for a certain system composed of two particles, denoted by 1 and 2, a measurement of the momentum of 1 allows one to predict with certainty the momentum of 2 without in any way disturbing 2, and that a measurement of the position of 1 allows one equally well to predict with certainty the position of 2, again without in any way disturbing 2. Hence, in accordance with the reality criterion (a_2), to both the momentum and the position of particle 2 correspond elements of physical reality.

In part D, finally, the two alternatives of the disjunction in B are studied. If alternative (1) is denied, that is, if it is assumed that the quantum mechanical description is complete, then the result of part C leads to the conclusion that "two physical quantities, with noncommuting operators, can have simultaneous reality. Thus the negation of (1) leads to the negation of the only other alternative (2). We are thus forced to conclude that the quantum-mechanical description of physical reality given by wave functions is not complete."

This, then, is the logical structure of the Einstein, Podolsky, and Rosen (or EPR) paper. To complete its presentation the mathematical details of parts B and C still have to be discussed.

The authors considered a system composed of two particles 1 and 2, described by the variables x_1 and x_2, respectively, and assumed that these particles had been interacting from the time $t=0$ to $t=T$ and that after T no interaction takes place. Assuming further that the states of the two particles were known before $t=0$ they could calculate with the help of the Schrödinger equation the state of the combined system $1+2$ at any subsequent time. Thus for $t>T$ the state of the combined system can be described by the wave function

$$\Psi(x_1,x_2)=\sum_{n=1}\psi_n(x_2)u_n(x_1) \tag{7}$$

where $u_n(x_1)$ are the eigenfunctions of an operator, say, A_1 representing an observable $\mathcal{A}_1$ of particle 1. According to quantum mechanics, if the measurement of A_1 on particle 1 yields the eigenvalue a_k of A_1 belonging to $u_k(x_1)$, then the state of particle 2, after the measurement, is described by $\psi_k(x_2)$. Clearly in the case of continuous spectra (7) is replaced by

$$\Psi(x_1,x_2)=\int_{-\infty}^{\infty}\psi_y(x_2)u_y(x_1)\,dy \tag{8}$$

where y denotes the continuous eigenvalues of A_1.

The authors now supposed that the state of the system 1+2 is described by the wave function

$$\Psi(x_1,x_2)=\int_{-\infty}^{\infty}\exp\left[\frac{2\pi i(x_1-x_2+x_0)p}{h}\right]dp \tag{9}$$

where x_0 is an arbitrary constant, and they showed that the system thus described satisfies the conditions of part C. For $\Psi(x_1,x_2)$ can be expressed in two different but mathematically equivalent ways:

$$\Psi(x_1,x_2)=\int\exp\left[\frac{-2\pi i(x_2-x_0)p}{h}\right]\cdot\exp\left(\frac{2\pi i x_1 p}{h}\right)dp \tag{10}$$

or

$$\begin{aligned}\Psi(x_1,x_2)&=\int\left\{\int\exp\left[\frac{2\pi i(x-x_2+x_0)p}{h}\right]dp\right\}\delta(x_1-x)dx\\&=h\int\delta(x-x_2+x_0)\delta(x_1-x)dx.\end{aligned} \tag{11}$$

Case I. Comparison of (10) with (8), now written

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

shows that $u_p(x_1)=\exp(2\pi i x_1 p/h)$ is the eigenfunction of the linear momentum operator $(h/2\pi i)\partial/\partial x_1$ for particle 1 corresponding to the eigenvalue $p_1=p$ and that $\psi_p(x_2)=\exp[-2\pi i(x_2-x_0)p/h]$ is the eigenfunction of the lineaŕ momentum operator $P=(h/2\pi i)\partial/\partial x_2$ for particle 2 corresponding to the eigenvalue $p_2=-p$. Hence if a measurement of the momentum of particle 1 yields p [so that after the measurement $\Psi(x_1,x_2)$ is reduced to $\psi_p(x_2)u_p(x_1)$], it can be inferred without in any way disturbing particle 2 that its momentum is $-p$.

Case II. Comparison of (11) with (8), now written

$$\Psi(x_1,x_2)=\int\psi_x(x_2)u_x(x_1)dx, \tag{13}$$

shows that $u_x(x_1)=\delta(x_1-x)$ is the eigenfunction of the position operator x_1 for particle 1 corresponding to the eigenvalue $x_1=x$ and that $\psi_x(x_2)=h\delta(x-x_2+x_0)$ is the eigenfunction, multiplied by h, of the position operator $Q=x_2$ corresponding to the eigenvalue $x_2=x+x_0$. Hence if a measurement of the position of particle 1 yields x [so that after the measurement $\psi(x_1,x_2)$ is reduced to $\psi_x(x_2)u_x(x_1)$] it can be inferred without in any way disturbing particle 2 that its position is $x+x_0$. In accordance with the criterion of reality, in case I the quantity P and in case II the quantity Q must be considered elements of reality. But

$$PQ-QP=\frac{h}{2\pi i}. \tag{14}$$

It has thus been established that the system under discussion satisfies the conditions of part C.[40]

Einstein and his collaborators concluded this paper with the following statement: "While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. We believe, however, that such a theory is possible."

The authors did not declare whether, in their view, (1) the "complete description of physical reality" would merely extend the existing incomplete theory without altering it, or (2) the complete theory would be incompatible with the existing theory. We shall have occasion to come back to this point in due course.

It should be added that in their paper the authors already anticipated the possibility of the objection that their epistemological criterion of physical reality is not sufficiently restrictive. If one insisted, they said, that physical quantities can be regarded "as simultaneous elements of reality *only when they can be simultaneously measured or predicted,*" clearly the argument breaks down, for then, since either P or Q—but not both simultaneously—can be predicted, both can not be simultaneously real. However, according to such a restricted criterion, the reality of P and Q, referring to the second system, would then depend on the process of measurement carried out on the first system, which does not disturb the second system in any way. "No reasonable definition of reality," they contended, "could be expected to permit this."

The Einstein-Podolsky-Rosen argument for the incompleteness of quantum mechanics is based, as we see, on two explicitly formulated and two tacitly assumed—or only *en passent* mentioned—premises. The former are:

[40]Although not explicitly stated by the authors the following consideration justifies their treatment. Equation 9 describes the state of a system of two particles for which the difference of their x-coordinates x_1-x_2 and the sum of the x-components of their momenta, $p_{1x}+p_{2x}$ or briefly p_1+p_2, are well defined. That this assumption is not inconsistent with the formalism follows from the commutativity between x_1-x_2 and p_1+p_2, a fact explicitly stated for the first time by Bohr in Ref. 4-9 (1949, p. 233).

1. *The reality criterion.* "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.

2. *The completeness criterion.* A physical theory is complete only if "every element of the physical reality has a counterpart in the physical theory."

The tacitly assumed arguments are:

3. *The locality assumption.* If "at the time of measurement... two systems no longer interact, no real change can take place in the second system in consequence of anything that may be done to the first system."

4. *The validity assumption.* The statistical predictions of quantum mechanics—at least to the extent they are relevant to the argument itself—are confirmed by experience.

We use the term "criterion" not in the mathematically rigorous sense denoting necessary *and* sufficient conditions; the authors explicitly referred to 1 as a sufficient, but not necessary, condition of reality and 2 only as a necessary condition of completeness. The Einstein-Podolsky-Rosen argument then proves that on the basis of the reality criterion 1, assumptions 3 and 4 imply that quantum mechanics does not satisfy criterion 2, that is, the necessary condition of completeness, and hence provides only an incomplete description of physical reality.

We do not know precisely what part of the paper is due to each of the three authors or whether it was written by all three without "division of labor." Professor Rosen, when asked about this point, did not remember the details but had the impression that the latter was the case. There seems, however, to be no doubt that the main idea was due to Einstein. For in the letter to Epstein, it will be recalled, Einstein wrote explicitly: "I myself arrived at the ideas starting from a simple thought-experiment."[41] What was needed to produce the paper was to translate these ideas into the language of quantum mechanics and to illustrate them in terms of a fully calculated example.

Einstein and his collaborators used their argument to demonstrate that quantum mechanics is an *incomplete* theory. Let us remark that had they adopted the original Heisenberg position according to which the object prior to the measurement of one of its observables A has only the potentiality of assuming a definite value of A which is realized by the process of measuring A, they could equally well have used their argument

[41] "*Ich selber bin auf die Überlegungen gekommen, ausgehend von einem einfachen Gedankenexperiment.*" Letter from Einstein to P. S. Epstein, Ref. 26.

to demonstrate that quantum mechanics is an *overdetermined* theory. They could have argued as follows.

The measurement process performed on particle 1, through the interaction with the measuring device, brings into realization one of the potentialities of the observed system, for instance, as in case I, a sharp value of momentum. Since the formalism of quantum mechanics has, so to say, an inbuilt mechanism which implies that the other particle must then likewise have a sharp momentum though it is not interacted with, the theory is overdetermined. Its mathematical formalism would have to be revised in such a way that the puzzling correlation no longer forms part of the theory. The authors, it seems, did not propose this alternative because their conception of physical reality was incompatible with Heisenberg's philosophy of potentialities and realizations of physical properties, although Einstein reportedly envisaged the possibility "that the current formulation of the many-body problem in quantum mechanics may break down when particles are far enough apart."[42]

The reader will have noted that we have preferred to use the term EPR argument rather than EPR paradox, as it is usually called. The authors never regarded their thesis as a "paradox," neither in the sense of the medieval *insolubilia* nor in the more modern sense of syntactical or semantical antinomies. It was later referred to as a paradox because its claim—the state of one system depends on what the experimenter decides to measure on another (remote) system—was regarded as counterintuitive or contrary to common sense. It was precisely in this sense that Schrödinger called it, probably for the first time, a paradox: "It is rather discomforting that the theory should allow a system to be steered or piloted into one or the other type of state at the experimenter's mercy in spite of his having no access to it. This paper does not aim at a solution of the paradox, it rather adds to it, if possible."[43] Clearly, the assumption of such a strange unphysical mode of action seems unreasonable and

[42]Private communication to D. Bohm. Cf. D. Bohm and Y. Aharonov, "Discussion of experimental proof for the paradox of Einstein, Rosen, and Podolsky," *Physical Review* **108**, 1070–1076 (1957), quotation on p. 1071. This paper tried to show that the above-mentioned alternative is untenable in view of experimental evidence.

[43]E. Schrödinger, "Discussion of probability relations between separated systems," *Proceedings of the Cambridge Philosophical Society* **31**, 555–562 (1935), quotation on p. 556. Schrödinger apparently liked to use this term. In his treatise *Statistical Thermodynamics* (Cambridge University Press, Cambridge, 1952), pp. 72–73, he applied the term paradox to the easily understood Richardson effect; that is, to the well-known fact that thermoionic fermions of low "gas-pressure" behave like bosons in accordance with the Boltzmann "tail" of the Fermi-Dirac distribution. Cf. also W. Yourgrau, "A budget of paradoxes in physics," in *Problems in the Philosophy of Science* I. Lakatos and A. Musgrave, eds. (North-Holland Publishing Co., Amsterdam, 1968), pp. 178–199, and "Comments" (on this paper), by W. V. Quine, *ibid.*, pp. 200–204, where Quine suggested use of the term paradox for "any plausible

certainly "contrary to accepted opinion" [*para-doxa* = apart from general opinion].

The three authors themselves, we should note, always regarded their arguments as conclusive evidence for the incompleteness of the quantum mechanical description of physical reality—and never changed their mind on this matter. Nathan Rosen, the only surviving member of this team, who after a professorship at the University of North Carolina has served since 1952 at the Haifa Technion, has told the author on various occasions that he thinks the argument has never been disproved.[44] Boris Podolsky, who was professor at the University of Cincinnati from 1935 to 1961 and subsequently, until his death in 1966, at the Xavier University where he chaired the 1962 Conference on the Foundation of Quantum Mechanics, was of the same opinion. Einstein too never believed the difficulty raised in the paper had been satisfactorily overcome. In fact, as Einstein told Banesh Hoffmann[45] in 1937 or 1938, as soon as the paper was published he received quite a number of letters from physicists "eagerly pointing out to him just where the argument was wrong. What amused Einstein was that, while all the scientists were quite positive that the argument was wrong, they all gave different reasons for their belief!"

In his "Reply to Criticisms,"[46] written early in 1949, Einstein explicitly reaffirmed, notwithstanding the objections advanced by Bohr and others, the view expressed in the 1935 paper. Earlier, in a letter[47] to his lifelong friend Maurice Solovine, Einstein mentioned that there are some chances to work out "a supremely interesting theory, by which I hope to overcome the present mystic of probability and the abandonment of the concept of reality in physics." Later, in 1950, less than five years before his death, he wrote to Schrödinger that he felt sure that "the fundamentally statistical character of the theory is simply a consequence of the incompleteness of the description.... It is rather rough to see that we are still in the stage of our swaddling clothes, and it is not surprising that the fellows struggle against admitting it (even to themselves)."[48]

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argument from plausible premises to an implausible conclusion" and the term antinomy for "a crisis-engendering specific paradox."

[44]"One gets the impression that Einstein's opponents believed at the time that their arguments completely demolish the paper. However, it seems that its ghost continues to haunt those concerned with the foundations of quantum mechanics. The question raised thirty years ago is still being discussed." N. Rosen in *Einstein—The Man and his Achievement*, G. J. Whitrow, ed. (British Broadcasting Corporation, London, 1967), p. 81. (A series of broadcast talks)

[45]Ref. 44 (p. 79).

[46]Ref. 4-9 (1949, pp. 663–688, especially pp. 681–683).

[47]A. Einstein, *Lettres à Maurice Solovine* (Gauthier-Villars, Paris, 1956), p. 74.

[48]Ref. 2-13 (1967, p. 40; 1963, p. 37).

In the same year, in an interview with R. S. Shankland,[49] Einstein, referred to the disagreement between him and most of his colleagues on the quantum theory. He spoke of them as "not facing the facts," as "having abandoned reason," and he said that modern quantum physics "avoids reality and reason." Shankland, continuing his report of the interview of February 4, 1950, also mentioned that Einstein "spoke several times of Bohr whom he greatly likes and admires but with whom he disagrees in many fundamental ways. He said that Bohr's thinking was very clear but that when he begins to write he becomes very obscure and he thinks of himself as a prophet." Two years later, in another interview with Shankland, Einstein restated that in his view the "ψ-function does not represent reality" and said that "the quantum theory people all have a 'narrow view'"; but he admitted that it is correct to use the theory as long as it is useful, "even though it is not a complete description."

The present author who had long discussions with Einstein in August 1952 and June 1953 can only confirm that Einstein never abandoned the view that quantum mechanics, as presently formulated, is an incomplete description of physical reality.

One may well ask oneself why Einstein, whose own contributions had such a pronounced influence on the development of statistical methods in physics, opposed so strongly the current ideas in quantum mechanics. The answer, it seems, lies in his deep philosophical conviction that statistical methods, though useful as a mathematical device for dealing with natural phenomena that involve large numbers of elementary processes, do not give an exhaustive account of the individual process or, as he once wrote in a famous letter to Max Born, he "could not believe in a dice-throwing God." It appears that since about 1948 he even felt that in his quest for a causal foundation of the theory he would never succeed.

In another letter to Born, written in 1948, he said: "I can quite well understand why you take me for an obstinate old sinner, but I feel clearly that you do not understand how I came to travel my lonely way. It would certainly amuse you although it would be impossible for you to appreciate my attitude. I should also have great pleasure in tearing to pieces your positivistic-philosophical viewpoint. But in this life it is unlikely that anything will come of it."[50] Einstein had the courage to go his lonely way and to swim against the current, just as he had the courage to spend almost half of his lifetime in establishing a unified field theory, although he was

[49]Interviews with Einstein on February 4, 1950; November 17, 1950; February 2, 1952; October 24, 1952; December 11, 1954. R. S. Shankland, "Conversations with Albert Einstein," *American Journal of Physics* **31**, 47–57 (1963).
[50]Ref. 5-14 (1969, p. 221; 1971, p. 163; 1972, p. 178).

fully aware that the chances of success were very small indeed. He did so because he thought it his duty to take the risk, even if the whole community of physicists might regard him as a "renegade." "I still work indefatigably at science but I have become an evil renegade who does not wish physics to be based on probabilities."[51]

6.4. EARLY REACTIONS TO THE EPR ARGUMENT

The EPR paper, which had been received by the editors of the *Physical Review* on March 25, 1935, was published in the May 15 issue of this journal. But before it reached the scientific community it was given wide publicity by Science Service of Washington, D. C. And on May 4, 1935, the Saturday issue of *The New York Times* (Vol. 84, No. 28,224, page 11) carried a lengthy report under the impressive caption "Einstein Attacks Quantum Theory." "Professor Einstein," it read, "will attack science's important theory of quantum mechanics, a theory of which he was a sort of grandfather. He concludes that while it is 'correct' it is not 'complete.'" After a nontechnical summary of the main thesis of the paper an explanation is supplied by quoting the following statement allegedly made by Podolsky:

> Physicists believe that there exist real material things independent of our minds and our theories. We construct theories and invent words (such as electron, positron, &c.) in an attempt to explain to ourselves what we know about our external world and to help us to obtain further knowledge of it. Before a theory can be considered to be satisfactory it must pass two severe tests. First, the theory must enable us to calculate facts of nature, and these calculations must agree very accurately with observation and experiment. Second, we expect a satisfactory theory, as a good image of objective reality, to contain a counterpart for every element of the physical world. A theory satisfying the first requirement may be called a correct theory while, if it satisfies the second requirement, it may be called a complete theory.

Podolsky concluded his statement with the declaration that it has now been proved that "quantum mechanics is not a complete theory."

This article was followed by a report, "Raises Point of Doubt," concerning an interview with Edward U. Condon, who was at that time Associate Professor of Mathematical Physics at Princeton University. Asked to comment on the EPR argument Condon reportedly replied: "Of course, a great deal of the argument hinges on just what meaning is to be attached

[51] *Ibid.*

to the word 'reality' in connection with physics. They have certainly discussed an interesting point in connection with the theory." Condon, the report continued, then referred to Einstein's dissatisfaction with the indeterminism of conventional quantum mechanics and quoted his already famous aphorism that "the good Lord does not throw dice," and finally Condon declared: "For the last five years Einstein has subjected the quantum mechanical theories to very searching criticism from this standpoint. But I am afraid that thus far the statistical theories withstood criticism."

With the exception of a very short note on a problem in general relativity, written together with the Dutch astronomer Willem de Sitter and published 1932 in the *Proceedings of the National Academy of Sciences*, the EPR article was Einstein's second *scientific* paper published in the United States. The first was the 1931 Pasadena paper, written with Tolman and Podolsky, which, as will be recalled, was also given prepublication publicity. Detesting any kind of publicity and especially of this sort, Einstein expressed his indignation in a statement which appeared in *The New York Times* on May 7, 1935 (No. 28,227, page 21): "Any information upon which the article 'Einstein Attacks Quantum Theory' in your issue of May 4 is based was given to you without authority. It is my invariable practice to discuss scientific matters only in the appropriate forum and I deprecate advance publication of any announcement in regard to such matters in the secular press." A daily newspaper, even of a standard as high as that of *The New York Times*, was in Einstein's view not the appropriate forum for scientific discussions and he deplored the misuse which press reporters had made of their medium when interviewing Condon. But it remains a historical fact that the earliest criticism of the EPR paper—moreover, a criticism which correctly saw in Einstein's conception of physical reality the key problem of the whole issue—appeared in a daily newspaper prior to the publication of the criticized paper itself.

The first reaction to the EPR paper to appear in a scientific periodical was a letter written by Edwin Crawford Kemble[52] to the Editor of *The Physical Review* 10 days after the publication of the paper. Kemble had obtained his Ph.D. in 1917 at Harvard University under P. W. Bridgman, and he has been teaching quantum theory there since 1919 with an interruption for a visit to Munich and Göttingen in 1927. In his opinion the EPR argument is not sound. First, if it were indeed possible, as claimed by the three authors, "to assign two different wave functions...to the same reality," quantum mechanical description, Kemble claimed, would not

[52]E. C. Kemble, "The correlation of wave functions with the states of physical systems," *Physical Review* **47**, 973–974 (1935).

only be incomplete but even erroneous, "for each different wave function involves a different prediction regarding the future behavior of the system described and the authors...clearly intend the phrase 'the same reality' to refer to the same system in the same physical state."

Kemble continued: "...since β[particle II] has not been disturbed by the observation of α, Einstein, Podolsky and Rosen argue that it cannot be affected by that observation and must in all cases constitute 'the same physical reality.' In other words, they assume it to be in the same 'state' in all cases. Here lies a fallacy, however, for whenever two systems interact for a short time there is a correlation between the subsequent behavior of one system and that of the other." "To clear up the whole question" Kemble referred to the statistical ensemble interpretation of quantum mechanics which in his view had been most clearly presented by John C. Slater,[53] and which "seems the only way of solving the paradoxes of the average presentation of elementary quantum theory." In this interpretation where ψ describes only the properties of an assemblage of a very large number of similarly prepared systems, the EPR argument does not disprove the completeness of the quantum mechanical description of atomic systems. Kemble apparently completely overlooked the fact that the expansion of the wave function $\psi(x_1, x_2)$ of the combined system is not unique.

To understand the acridity of Kemble's criticism we should recall the state of mind in which he wrote this Letter to the Editor. When the Einstein-Podolsky-Rosen paper appeared he was well along in the preparation of the manuscript of his well-known book[54] in which he repeatedly argued "that the wave function is merely a subjective computational tool and not in any sense a description of objective reality."[55] Influenced by the operationalism of Bridgman, the positivism of Mach, and the pragmatism of Peirce, Kemble regarded the job of the physicist as merely "to describe the experimental facts in his domain as accurately and simply as possible, using any effective procedure without regard to such a priori restrictions on his tools as common sense may seek to impose."[56] In his view the problem raised by Einstein and his collaborators was a verbal question, completely irrelevant to the work of the physicist.

[53] J. C. Slater, "Physical meaning of wave mechanics," *Journal of the Franklin Institute* **207**, 449–455 (1929).

[54] E. C. Kemble, *The Fundamental Principles of Quantum Mechanics* (McGraw-Hill, New York, 1937; Dover, New York, 1958).

[55] Kemble *op. cit.*, p. 328.

[56] E. C. Kemble, "Operational reasoning, reality, and quantum mechanics," *The Journal of the Franklin Institute* **225**, 263–275 (1938). Cf. also E. C. Kemble, "Reality, measurement, and the state of the system in quantum mechanics," *Philosophy of Science* **18**, 273–299 (1951).

Challenged by Kemble's criticism and obviously hurt by his rejection of the argument as a "fallacy" Podolsky wrote a paper to be published as a Letter to the Editor of *The Physical Review*. It was entitled "States and Reality of Physical Systems." Since it has never been published it will be quoted in full:[57]

> In a letter to the editor[1] Kemble asserts that the argument of Einstein, Podolsky, and Rosen[2] is in his opinion unsound. Not being in a position to discuss Kemble's objections with Einstein and Rosen, I take the liberty of expressing my own reaction to them.
>
> The closer I examine Kemble's letter the more I am convinced that he does not prove his contention. His argument comes to a climax in the third paragraph, where he says: ": since β has not been disturbed by the observation of α, Einstein, Podolsky, and Rosen argue that it cannot be affected by that observation and must in all cases constitute 'the same physical reality.' In other words, they assume it to be in the same 'state' in all cases. Here lies a fallacy, however, for whenever two systems interact for a short time there is a correlation between the subsequent behaviour of one system and that of the other." Thus, it seems, our "fallacy" consisted in assuming that if a system is not *disturbed* it is also not *affected*, and thus remains in the same physical state.
>
> In the next paragraph, however, Kemble seems to assert that even this is not a fallacy, for he says: "It is quite true, as pointed out by E., P., and R., that the act of observing the system α *can hardly be supposed to change the state of a system* β which may be widely separated from it in space, but it can and does reveal something about the state of β which could not have been deduced... without such an observation." (Emphasis mine. B.P.) Kemble, therefore, agrees that, in spite of something being revealed by the observation, we are still dealing with the same *state* of the system, and therefore the same *reality*. I fail to see that after this anything remains of the fallacy asserted earlier.
>
> It is probably because of this anticlimax that Kemble adds a paragraph beginning with "These remarks would hardly clear up the whole question if no specific mention were made of the interpretation of quantum mechanics as a statistical mechanics of assemblages of like systems." This is an entirely different question. I believe that Einstein and Rosen would agree with me that quantum mechanics is, in view of our present knowledge, a correct and a complete statistical theory of such assemblages. A statistical mechanics, however, may not be a complete description of elementary processes—and it is of these that we speak. If its validity as a complete description is restricted to assemblages, then the difficulty mentioned by Kemble in the second paragraph of his paper does not arise; for in that case we are not dealing with the same reality.

[57]With the kind permission of Mrs. Polly Podolsky. It is not clear whether personal reasons prevented Podolsky from publishing this paper or whether he followed Einstein's counsel in this matter.

Kemble, however, goes further and says: "... we cannot know more about an individual electron than the fact that it belongs to a suitable potential assemblage of this kind." It is here that the real difference of opinion is revealed; the difference of opinion which led to the discovery of a fallacy where there was none.

If we cannot know more about a physical system than the fact that it belongs to certain potential (and therefore imaginary) assemblage, then a mere change in our *knowledge* of the system must result in the change of the assemblage, and thus of the *physical state* of the system. Since, as is the case with the system β, this knowledge can be altered without disturbing the system, the physical system loses the essential property of physical reality—that of existence independent of any mind. This point of view is in direct contradiction with the philosophic point of view explicitly stated in the first paragraph of our article.[2] If independent existence of physical reality is not accepted, there is no common ground for the discussion. What is more, the very question used as the title of our article loses all meaning.

1. *Phys. Rev.* **47**, 973 (1935).
2. *Phys. Rev.* **47**, 777 (1935).

Boris Podolsky

Graduate School
University of Cincinnati
July 6, 1935

Podolsky's concluding statement, "if independent existence of physical reality is not accepted, then there is no common ground for the discussion," poignantly underlined the whole issue involved, since for Kemble "the province of the physicist is not the study of an external world, but the study of a portion of the inner world of experience" and "there is no reason why the constructs introduced [such as the ψ-function] need correspond to objective realities."[58]

In a letter[59] to the present author Professor Kemble, whom the author had shown the unpublished paper by Podolsky, expressed his regret that Podolsky's rebuttal was never published, "for it would have brought out the real difference between my interpretation of quantum mechanics and that of Einstein, Podolsky, and Rosen." Admitting that "it was incorrect to accuse Einstein, Podolsky, and Rosen of a fallacy, i.e., an error in reason-

[58]Ref. 56 (1938, p. 274).
[59]Letter from E. C. Kemble, dated January 2, 1971.

ing," Kemble declared that his criticism should have been directed instead at their basic assumptions, that is, at their thesis that "on the quantum level the same sharp distinction between subjective and objective" can be maintained "that serves us so well on the everyday large-scale common-sense level of discourse."

The first reverberation to the EPR paper found in Europe was a Note,[60] published in the June 22 issue of *Nature*—only five weeks after the appearance of the paper—signed by H. T. F. (H. T. Flint, a reader in physics at the University of London). After giving a nonmathematical outline of the argument, Flint described the paper as an "appeal for a more direct description of the phenomena of physics. The authors seem to prefer the artists' portrayal of the landscape rather than a conventional representation of its details by symbols which bear no relation to its form and colour."

The one who thought it his duty to take up the cudgels immediately was, of course, Niels Bohr. The *Physical Review* carrying the EPR paper was issued on May 15 and on June 29 Bohr had already sent a Letter to the Editor[61] of *Nature* in which he rejected the criterion of physical reality as proposed by the three authors, claiming that it "contains an essential ambiguity" when applied to problems of quantum mechanics. And he declared that a closer examination, the details of which he would publish shortly in the *Physical Review*, reveals "that the procedure of measurements has an essential influence on the conditions on which the very definition of the physical quantities in question rests. Since these conditions must be considered as an inherent element of any phenomenon to which the term 'physical reality' can be unambiguously applied, the conclusion of the above-mentioned authors would not appear to be justified."

In the announced paper,[62] which carried the same title as its challenge and which was received by the editors of the *Physical Review* on the very day its announcement appeared in *Nature*, Bohr attempted to show, first, that the physical situation discussed by the three authors has nothing exceptional about it and that the problem they raised in connection with it may be found as being inherent in any other quantum mechanical phenomenon, if it is only sufficiently analyzed. In fact, as Bohr pointed out in a footnote at the beginning of his exposition, their deductions may be considered as straightforward consequences of the transformation theorems of quantum mechanics as follows.

[60] H. T. F., "Quantum mechanics as a physical theory," *Nature* **135**, 1025–1026 (1935).
[61] N. Bohr, "Quantum mechanics and physical reality," *Nature* **136**, 65 (1935).
[62] N. Bohr, "Can quantum-mechanical description of physical reality be considered complete?" *Physical Review* **48**, 696–702 (1935).

The two pairs of canonically conjugate variables (q_1,p_1) and (q_2,p_2), pertaining to two systems I and II, respectively, and satisfying the usual commutation relations, can be replaced by two pairs of new conjugate variables (Q_1,P_1) and (Q_2,P_2), which are related to the former by the orthogonal transformation

$$\begin{aligned} q_1 &= Q_1\cos\theta - Q_2\sin\theta \qquad & p_1 &= P_1\cos\theta - P_2\sin\theta \\ q_2 &= Q_1\sin\theta + Q_2\cos\theta \qquad & p_2 &= P_1\sin\theta + P_2\cos\theta \end{aligned} \tag{15}$$

where θ is an arbitrary angle of rotation. By inversing (15), that is,

$$\begin{aligned} Q_1 &= q_1\cos\theta + q_2\sin\theta \qquad & P_1 &= p_1\cos\theta + p_2\sin\theta \\ Q_2 &= -q_1\sin\theta + q_2\cos\theta \qquad & P_2 &= -p_1\sin\theta + p_2\cos\theta \end{aligned} \tag{16}$$

it is easy to also verify that $[Q_1,P_1]=ih/2\pi$ and $[Q_1,P_2]=0$, and so on. Hence not Q_1 and P_1, but Q_1 and P_2 may be assigned sharp values. But then, as the first and last equations of (16) clearly show, a subsequent measurement of either q_2 or p_2 will make it possible to predict the value of q_1 or p_1, respectively.

To clarify the relation of transformations of the type (15) to the EPR argument, Bohr considered the experimental arrangement of a rigid diaphragm with two parallel slits, which are very narrow compared with their separation. He assumed that through each slit one particle with given initial momentum passes independently of the other. "If the momentum of this diaphragm is measured accurately before as well as after the passing of the particles," he declared, "we shall in fact know the sum of the components perpendicular to the slits of the momenta of the two escaping particles, as well as the difference of their initial positional coordinates in the same direction," the latter quantity being the distance between the two narrow slits. A subsequent single measurement either of the position or of the momentum of one of the two particles will clearly enable us to predict, with any desired accuracy, the position or momentum, respectively, of the other particle. In fact, by taking $\theta = -\pi/4$, $P_2=0$ and $Q_1 = -X_0/\sqrt{2}$, the first and last equations in (16) yield

$$-X_0 = q_1 - q_2 \qquad \text{and} \qquad 0 = p_1 + p_2 \tag{17}$$

and the wave function representing this state is precisely the function $\psi(x_1,x_2)$ chosen by Einstein, Podolsky, and Rosen [see (9), with $x_1 \equiv q_1$, $x_2 \equiv q_2$].

Having thus associated the rather abstract mathematical formulation of the EPR argument with a concrete experimental setup, Bohr was in a position to point out that the freedom of choice, to measure *either* p_1 *or* q_1 —and to compute therefrom p_2 or q_2, respectively—involves a discrimination between different and mutually exclusive experimental procedures. Thus to measure q_1 means to establish a correlation between the behavior of particle I and an instrument rigidly connected with the support which defines the space frame of reference. The measurement of q_1 therefore also provides us with the knowledge of the location of the diaphragm, when the particles passed through its slits, and thus of the initial position of particle II relative to the rest of the experimental setup. "By allowing an essentially uncontrollable momentum to pass from the first particle into the mentioned support, however, we have by this procedure cut ourselves off from any future possibility of applying the law of conservation of momentum to the system consisting of the diaphragm and the two particles and therefore have lost our only basis for an unambiguous application of the idea of momentum in predictions regarding the behavior of the second particle."

Conversely, if we choose to measure the momentum p_1, the uncontrollable displacement inevitably involved in this measurement precludes any possibility of deducing from the behavior of particle I the location of the diaphragm relative to the rest of the apparatus and hence cuts us off from any basis whatever for a prediction of q_2. As these considerations show, argued Bohr, the very conditions which define the possible types of prediction depend on whether q_1 or p_1 is being measured during the last critical stage of the experiment. Hence the expression "without in any way disturbing a system," as used by Einstein, Podolsky, and Rosen in their criterion of reality, contains an ambiguity. True, no mechanical disturbance is exerted on particle II; but since the conditions defining the possible types of prediction concerning particle II constitute an inherent element of the description of any phenomenon to which the term "physical reality" can be properly attached, and since these conditions, as we have seen, depend on whether q_1 or p_1 is being measured, the conclusion of the three authors is not justified.

To make this point clear Bohr compared the quantum mechanical observation with that in classical physics. To endow classical laws with experimental significance we must be able to determine the exact state of all the relevant parts of the system. This requires a correlation between the system of interest and the measuring apparatus subject to the condition that the state of the system can be inferred by observing the large-scale measuring apparatus. Now, in classical physics, in spite of such an interaction between object and measuring apparatus the two systems can be distinguished by an appropriate conceptual analysis. In quantum physics, on the other hand, no such analysis is possible: object and measuring device form an unanalyzable unity. Whereas in classical physics the interaction between object and measuring device may be neglected or compensated for, in quantum mechanics it forms an inseparable part of the phenomenon. Hence, as Bohr expressed it in an essay which he regarded as a particularly lucid presentation of his view:

> The unambiguous account of proper quantum phenomena must, in principle, include a description of all relevant features of the experimental arrangement.... In the case of quantum phenomena, the unlimited divisibility of events implied in such an account is, in principle, excluded by the requirement to specify the experimental conditions. Indeed, the feature of wholeness typical of proper quantum phenomena finds its logical expression in the circumstance that any attempt at a well-defined subdivision would demand a change in the experimental arrangement incompatible with the definition of the phenomena under investigation.[63]

The result of any quantum mechanical measurement informs us consequently not of the state of the object as such but of the whole experimental situation in which it is immersed. The completeness of quantum mechanical description, challenged by Einstein and his collaborators, was thus saved, in Bohr's view, by the feature of wholeness.

6.5. THE RELATIONAL CONCEPTION OF QUANTUM STATES

Bohr, as we see, succeeded in defending his position by refuting the epistemological criterion of physical reality, as proposed by the three authors, on the grounds that the object under observation together with the observing apparatus form a single indivisible system not susceptible to any further analysis, at the quantum mechanical level, into separate distinct parts. The combination of a given particle with one particular experimental setup for observation differs essentially from the combination of the same particle with another experimental setup for observation. The basic problem in quantum mechanics is no longer (*a*) what is the probability π_n that a system S has the value q_n of a physical quantity Q, but rather (*b*) what is the probability π_n of obtaining the result q_n by measuring a physical quantity Q on a system S *by means of an experimental arrangement* A? Since the "state" of the system S is the sum total ("catalogue") of all the π_n, formulation (*b*) shows that the state of the system depends not solely on S, as asserted in the EPR argument, but also on A.

In other words, the description of the state of a system, rather than being

[63] Ref. 4-52.

restricted to the particle (or system of particles) under observation, expresses a relation between the particle (or particles) and all the measuring devices involved. On the basis of the relational conception of the state of a system it could of course well be possible that the state of $\mathfrak{S}$ might change without $\mathfrak{S}$ being mechanically interfered with at all. A macroscopic example is the state σ of a body $\mathfrak{S}$ being defined as "hotter than a body $\mathfrak{S}'$"; σ may be changed either by cooling $\mathfrak{S}$ or by heating $\mathfrak{S}'$—in the latter case without interfering with $\mathfrak{S}$.

The present situation was not the earliest conception in the history of human thought of the possibility that the state (in the most general sense of the word) of an object may change without the object itself being interfered with. Thus the reader familiar with the scholastic theories of space may be reminded that according to the philosophy of Thomas Aquinas and Bonaventura[64] the "ubiety" [*ubicatio*] of the extremities of a long, solid rod undergoes a radical change as soon as a part, say near the center, of the material rod is removed, leaving in its stead a vacuum. Suarez, in his *Disputationes Metaphysicae*,[65] it will be recalled, opposed the Thomistic view by ascribing an independent reality to the "*ubi*" (the state of locality) of the rod's ends. In this analogy Bohr may be compared to Thomas and Einstein to Suarez, the pre-Newtonian advocate of an absolute space! Since, in Bohr's view, quantum mechanics can be consistently regarded as a computational device for obtaining the probability of every measurement involving both the entity to be observed *and* the measuring experimental setup, no attributes of physical reality can be ascribed to the former alone.

In particular, a particle, even after having ceased to interact with another particle, can by no means be considered as an independent seat of "physically real" attributes and the EPR thought-experiment loses its paradoxical character.

What has been said about two-particle and many-particle systems applies equally well, according to Bohr, to the observation of single-particle systems. In fact, the wave-particle duality is only a particular case of this conception.

Bohr's reaction to the EPR argument led, as we see, to two important results. First, as far as quantum theory is concerned, the notion of a quantum mechanical state became a *relational* conception. Second—and this result is of cardinal importance from the viewpoint of the history of science and its philosophy—the notion of a "structure" in the sense of an

[64]St. John of Fidanza Bonaventure, *Commentarii in Quatuor Libros Sententiarum Petri Lombardi* (1248), dist. 37, p. 2, q. 3 ("*ubi est vacuum, non est distantia*").
[65]Francis Suarez of Granada, *Disputationes Metaphysicae* (Paris, 1619).

unanalyzable wholeness or form, a notion which from the decline of Aristotelian physics[66] in the times of Galileo and Newton to the rise of field theory with Faraday and Maxwell[67] had no place in physical thought, has been revived.

Bohr's relational conception of state has, in fact, a unique position in the history of physical thought, for it is a renunciation of the Baconian principle of the "*dissectio naturae*," that "it is better to dissect than to abstract nature" [*melius autem est naturam secare, quam abstrahere*][68]—and moreover in the very field of scientific research (microphysics) where this principle, owing to the nature of the object under study, not only scored overwhelming success, but came to be regarded as an indispensible method of investigation. The Lord Chancellor's declaration that "without dissecting and anatomizing the world most diligently" we cannot "found a real model of the world in the understanding, such as it is found to be, not such as man's reason has distorted"[69] became one of the most important and most successful guiding principles of the method of modern science.

Descartes' second "Rule for Investigation"[70] and Galileo's "*metodo resolutivo*" reverberate this maxim, and once it was combined with the appropriate mathematics, as in the hands of Newton, it led science to its greatest achievements. More than any other subject, atomic physics owed its development to a systematic application of Bacon's "principle of dissection." And yet it was precisely in atomic physics where Bohr's logical "analysis" of the most elementary processes conceivable in this field found the solution of the emerging difficulties in the adoption of a relational and holistic conception of the state of a physical system.

That Bohr was fully aware of the historic importance of his insistence on

[66]The priority of the whole over the parts, instead of treating the whole merely as the sum of its parts, for scientific investigation, whether of an animal or a couch, was stressed by Aristotle in *De Partibus Animalium*: "Just as discussing a house, it is the whole figure and form of the house which concerns us, not merely the bricks and mortar and timber; so in natural science, it is the composite thing, the thing as a whole, which primarily concerns us..." (645 a 33-36). Although because of its teleological component Aristotle's notion of "wholeness" [σuνόλον] is not entirely identical with Bohr's, Aristotle's insistence that the behavior of a particular element, such as earth, "must not be considered in isolation, but only as a part of the cosmos with its universal laws" (*De Caelo*, 294 b) may be regarded as an early analog to Bohr's position.

[67]Cf. E. Cassirer, *The Logic of the Humanities* (Yale University Press, New Haven, Conn., 1961), pp. 159–181.

[68]Francis Bacon, *Novum Organum* (1620), book 1, section 51.

[69]"*Etenim verum exemplar mundi in intellectu humano fundamus, quale invenitur, non quale cuipiam sua propria ratio dictaverit. Hoc autem perfici non potest, nisi facta mundi dissectione atque anatomia diligentissima.*" *Ibid.*, section 124.

[70]René Descartes, *Discourse de la Méthode* (1637), Second Part.

the holistic and relational character of state descriptions and its philosophic implications has been given clear evidence by his address to the Second International Congress for the Unity of Science, held in Copenhagen on June 21–26, 1936. In this talk on "Causality and Complementarity"[71] he spoke of the futility of analyzing elementary processes by "subdividing their course more closely." Concerning the relational aspect of state description he compared the dependence on the various experimental arrangements and the logical reconciliation of the apparent contradictions in the resulting regularities to the choice of different frames of reference in the theory of relativity and the removal of the discrepancies originating in this theory owing to the finite size of the velocity of light; just as the Lorentz transformation formulae resolve the paradoxes of relativity, contended Bohr, so do the Heisenberg indeterminacy relations make the various state descriptions compatible with each other.

The conceptual reduction of complementarity to a relativity of description, depending on the experimental setup, was also the major point raised by Philipp Frank[72] on this occasion. Said Frank: "I believe that, as a starting point for a correct formulation of the complementarity idea, one must retain as exactly as possible the formulation set forth by Bohr in his reply in 1935 to Einstein's objection against the present quantum theory. As stressed by Bohr also in his talk just delivered, quantum mechanics speaks neither of particles the positions and velocities of which exist but cannot be accurately observed, nor of particles with indefinite positions and velocities. Rather, it speaks of experimental arrangements in the description of which the expressions 'position of a particle' and 'velocity of a particle' can never be employed simultaneously."

It seems worthwhile to elaborate on Bohr's comparison between the quantum theory and relativity in order to avoid a possible misconception concerning the relational conception of quantum mechanical states. Bohr's interpretation of the formalism of quantum theory, as is well known, has been challenged in certain quarters as being an idealistic or subjectivistic approach, primarily in view of this relational conception. It will be recalled that a similar charge had been leveled, especially in the early 1920s, against the theory of relativity. This charge stemmed, at least to some extent, from a gross misinterpretation of the very name of the theory; it was claimed

[71] N. Bohr, "Kausalität und Komplementarität," *Erkenntnis* **6**, 293–303 (1936); "Causality and complementarity," *Philosophy of Science* **4**, 289–298 (1937); "Kausalitet og Komplementaritet," *Naturens Verden* **21**, 113–122 (1937).

[72] P. Frank, "Philosophische Deutungen und Missdeutungen der Quantentheorie," *Erkenntnis* **6**, 303–317 (1936); "Philosophical misinterpretations of the quantum theory," in P. Frank, *Modern Science and its Philosophy* (Harvard University Press, Cambridge, Mass., 1949), pp. 158–171.

that, because all laws of physics are "relative" to the observer, they "depend" on the observer and that thereby the human element plays an integral part in the description of physical data—whereas, quite to the contrary, the very tensor calculus on which the theory is based vouches, so to say, for the "standpointlessness" of the laws formulated in the theory.

In a sense, Bohr's theory of relational state description may indeed be compared with the theory of relativity, or it may even be regarded as some kind of generalization of the latter, not in contents but in method. The role of inertial frames of reference, always equipped with the identical inventory of measuring rods and clocks, relative to which the physical phenomena are observed, is taken over in quantum mechanics, according to Bohr's conception, by different experimental setups varying in their inventory of measuring instruments. And just as the choice of a different frame of reference in relativity affects the result of a particular measurement, so also in quantum mechanics the choice of a different experimental setup has its effect on measurements, for it determines what is measurable.

One may also regard the connection between Bohr's relational conception of state with Einstein's theory of relativity from the general point of view that in the historical development of physics attributes were gradually replaced by relations. In a sense, this replacement principle already applied to the transition from Aristotelian qualitative physics to Newtonian quantitative physics. If in the latter "length," "area," and so on, or "durations" of physical processes could still be regarded as attributes of individual objects, the special theory of relativity, as is well known, changed their status to that of relations. For on the question: "What is the length of a certain object?" an unambiguous answer can be given only with reference to a specified inertial system. Similarly, in Bohr's relational theory, the question "What is the position (or momentum) of a certain particle?" presupposes, to be meaningful, the reference to a specified physical arrangement.

To subsume both cases under one heading, one may formulate a theory of "perspectives," the term perspective denoting a coordinated collection of measuring instruments either in the sense of reference systems as applied in the theory of relativity or in the sense of experimental arrangements as conceived by Bohr. The important point now is to understand that although a perspective may be occupied by an observer, it also exists without such occupancy. In fact, one may even assert that perspectives, just as in optics, belong to the object of observation, being carried by it in great number. A "relativistic frame of reference" may be regarded as a geometrical or rather kinematical perspective; Bohr's "experimental arrangement" is an instrumental perspective. And just as the former relativized lengths or time intervals and deprived them of the attribute of being "possessed" by the object, so did the latter with regard to dynamical variables such as position or momentum.

One of the most trenchant and acclamatory formulations of this relational version of complementarity was given by Fock. In a paper[73] already referred to Fock wrote:

> The probabilities expressed by the wave function are the probabilities of some result of the interaction of the micro-object and the instrument (of some reading on the instrument). The wave function itself can be interpreted as the reflection of the potential possibilities of such an interaction of the micro-object (prepared in a definite way) with various types of instruments. A quantum mechanical description of an object by means of a wave function corresponds to the relativity requirement with respect to the means of observation. This extends the concept of relativity with respect to the reference system familiar in classical physics.

To interpret the wave function as a catalogue of potential interactions and to extend the concept of relativity to the means of observation, as expressed by Fock, is indeed one of the most articulate characterizations of Bohr's relational conception of quantum mechanical state description. There is, however, a profound issue on which Einstein's theory of relativity and Bohr's conception of complementarity differ considerably: the ontological status of the objects under discussion. In spite of its emphasis on relational aspects relativity rests on a matrix of ontologically real and absolute existents or occurrences; for the pointlike events which it discusses are thought of as being real in every sense of the word. In fact, it was this realism that made relativity so attractive to Max Planck, who "always regarded the search for the absolute as the loftiest goal of all scientific activity."

If we now ask whether Bohr in his conception of quantum phenomena espoused a similar realism, we raise a difficult question. First it has to be understood that the various proponents of the "Copenhagen interpretation," though concurring on the indispensability of complementarity, differed with respect to its ontological implications. Some of them, for example, Pascual Jordan, adopted a positivistic point of view according to which the role of quantum mechanics is not "to go beyond experience and to understand the 'essence' of things."[74]

[73]Ref. 5-61.

[74]"*Die wissenschaftlichen Begriffsbildungen und Theorien stellen also nicht etwa einen über die sinnliche Erfahrung* hinausgehenden *Erkenntnisvorstoss auf das 'Wesen' der Naturerscheinungen dar, sondern lediglich eine zur Registrierung und Ordnung unserer sinnlichen Erfahrungen nützliche*, von uns hinzugedachte Hilfskonstruktion, *analog etwa den geographischen Länge-und Breitengraden.*" P. Jordan, *Anschauliche Quantenmechanik* (Springer, Berlin, 1936), p. 277.

Bohr's philosophy of science, however, in spite of occasional statements which if uncritically interpreted seem to suggest a positivistic point of view, cannot be classified as positivism, certainly not in the usual sense of this term. Thus at the very beginning of the "Introductory Survey" to his published early essays[75] Bohr defined the task of science as "both to extend the range of our experience and to reduce it to order," a definition well compatible with the positivistic outlook. A few lines later, however, he declared that "all new experience makes its appearance within the frame of our customary points of view and *forms of perception*" (italics by the present author). And on a later occasion[76] he emphasized "that no content can be grasped without a formal frame," expressions reminiscent of Kant's celebrated "deduction of the categories," though without any explicit reference to a priori origins.[77]

If Bohr's insistence on such forms of perception—an assumption that clearly contradicts positivistic principles—is brought into association with his repeated assertion that "all experience must be expressed in classical terms," one may be tempted to conclude, with Paul K. Feyerabend, that since those forms of perception are, in Bohr's view, "imposed upon us and cannot be replaced by different forms, ... Bohr's point of view still retains an important element of positivism."[78]

Bohr's alleged point of view according to which not the subjective perceptions of the observer but rather his descriptions of the phenomena in terms of classical physics constitute the ultimate "data" for scientific construction was called by Feyerabend "a positivism of a higher order."[79] According to Feyerabend the answer to our question concerning the ontological issue in Bohr's complementarity interpretation should consequently be given as follows. Although the retention of the notion of a quantum mechanical object proves to be useful primarily because of its linguistic conciseness, it should not mislead into ignoring that "such an object is now characterized as a set of (classical) *appearances* only, without any indication being given as to its nature. The *principle of complementarity* consists in the assertion that this is the only possible way in which the concept of an object (if it is used at all) can be employed upon the microscopic level."[80]

According to Feyerabend's exegesis Bohr's "positivism of higher order"

[75]Ref. 4-1 (1934, p. 1).

[76]Ref. 4-10 (1949, p. 240).

[77]On the "a priori" and the epistemology of quantum mechanics cf. K. F. von Weizsäcker, *Zum Weltbild der Physik* Ref. 4-7.

[78]P. K. Feyerabend, "Complementarity," *Supplementary Volume 32 of the Proceedings of the Aristotelian Society*, 75–104 (1958).

[79]Feyerabend, *op. cit.*, p. 82.

[80]Feyerabend, *op. cit.*, 94.

regards microphysical concepts as merely stenographic descriptions of the functioning of macroscopic instruments and ascribes reality only to the latter, which in contrast with the former are always describable in classical terms. This view can indeed be supported by some of Bohr's statements and particularly, of course, by those made in his rebuttal of the Einstein, Podolsky, and Rosen argument where it offered itself as a convenient logical weapon: "...there can be no question of any unambiguous interpretation of the *symbols of quantum mechanics* other than that embodied in the well-known rules which allow to predict the results to be obtained by a given experimental arrangement described in a totally classical way."[81]

In 1948 Bohr wrote in a similar vein: "The entire formalism is to be considered as a tool for deriving predictions, of definite or statistical character, as regards information obtainable under experimental conditions described in classical terms."[82] Furthermore, this view suggests associating a typically quantum mechanical phenomenon precisely with those macroscopic situations for the account of which complementary descriptions are required. Thus the word "quantum," rather than referring to the content or object of the description, characterizes the type of description or, expressed in other words, "the aim of the complementarity principle is a definition of 'quantum description,' which does not mean 'description of quanta.'"[83]

It is clear that according to this view the very concept of a "quantum mechanical object" is nonsensical—for nothing real corresponds to it. Bohr himself seems to have accepted, at some time, such a radical point of view for, according to his long-time assistant Aage Petersen, Bohr once declared when asked whether the quantum mechanical algorithm could be considered as somehow mirroring an underlying quantum reality: "There is no quantum world. There is only an abstract quantum physical description. It is wrong to think that the task of physics is to find out how nature *is*. Physics concerns what we can say about nature."[84]

In fact, if pursued to its logical conclusion, this view implies a complete reversal of the classical reality conception. Let us explain this point in terms of the following historical digression.

The reality conception of classical physics has its ultimate origin in the atomic doctrine of Leucippus and Democritus. Their thesis that "only the

[81]Ref. 62 (p. 701).
[82]N. Bohr, "On the notion of causality and complementarity," *Dialectica* **2**, 312–319 (1948).
[83]D. L. Schumacher, "Time and physical language," in *The Nature of Time*, T. Gold, ed. (Cornell University Press, Ithaca, N. Y., 1967), pp. 196–213.
[84]A. Petersen, "The philosophy of Niels Bohr," *Bulletin of the Atomic Scientist* **19**, 8-14 (1963), quotation on p. 12.

atoms are real" and "everything else is merely thought to exist"[85] was adopted, with for our present context irrelevant modifications, by Gassendi, Boyle, Newton, and others, and it became the dominating ontological principle of classical physics. It implied that a macroscopic body *qua* macroscopic object has no independent reality; only the atoms of which it is composed are real. The complementarity doctrine, if interpreted as stated above, maintains in contrast that only the macroscopic apparatus is something real and that the atom is merely an illusion. That this nonclassical reality conception was part also of Heisenberg's philosophy of quantum mechanics can be seen from the following statement which he wrote in 1959, though in a different context and with a slightly different emphasis: "In the experiments about atomic events we have to do with things and facts, with phenomena that are just as real as any phenomena in daily life. But the atoms or the elementary particles themselves are not as real; they form a world of potentialities or possibilities rather than one of things or facts."[86]

The historian of science or of philosophy will probably notice some similarity between this present situation and the state of affairs after the advent of early Greek atomism. In fact, the extreme complementarity point of view may well be compared with the teachings of the Sophists in the second half of the fifth century B.C. In contrast to the atomists, for whom the atoms alone were real, the Sophists contended that the ordinary facts of life are the realities that count, whereas the theoretical world of science is but a phantasmagoria of no relevance to man. This similarity has recently been stressed by Kurt von Fritz.[87] He even claimed that Protagoras' famous dictum, "Man is the measure of all things, of things that are that they are, and of things that are not that they are not," is not, as often stated, the expression of a subjectivistic or sensualistic or relativistic philosophy but rather the denial of the validity of all those philosophies which, like Democritean atomism, ascribe reality to theoretical entities postulated by science and not to the facts in the world of human actions. After this historical digression let us return to Bohr's "positivism of higher order."

Attractive as it is, this phenomenalistic interpretation leads to serious

[85]Cf. Diogenes Laertius, IX, 42.

[86]W. Heisenberg, *Physics and Philosophy* (G. Allen and Unwin, London, 1958; Harper and Row, New York, 1959), p. 160; *Physik und Philosophie* (S. Hirzel, Stuttgart, 1959), p. 180; (Ullstein, Frankfurt-am-Main, Berlin, Vienna, 1970), p. 156; *Fisica e Filosofia* (Saggiatore, Milan, 1961), p. 162.

[87]K. von Fritz, *Grundprobleme der Geschichte der antiken Wissenschaft* (W. de Gruyter, Berlin, New York, 1971), p. 222.

difficulties. It is a basic assumption of the formalism of quantum mechanics that there exists, roughly speaking, a one-to-one correspondence between the observables of a given physical system and the (hypermaximal) Hermitian operators on the Hilbert space associated with it. But by 1935, in his third Louis Clark Vanuxem lecture at Princeton University, Percy Williams Bridgman[88] already questioned the operational feasibility of this correspondence when he asked: "What is the apparatus in terms of which any arbitrary 'observable' of Dirac acquires its physical meaning?" That, conversely, it would be hard to find the Hermitian operators corresponding to such common operations as the length measurement of a splinter of a diamond crystal or the measurement of the angle between two of the crystal's planes has been pointed out by Schrödinger.[89] Moreover, quantum mechanics, in spite of its original program (Heisenberg) of not admitting anything unobservable, is in this respect even inferior to classical mechanics, as emphasized by Feyerabend.[90] Shimony's speculation[91] of resolving this difficulty on the basis of the dispersion theory formulation of quantum field theory and its definition of quantum mechanical states in terms of (observable) S-matrix quantities, such as suggested by Cutkosky,[92] can at best, if at all, account for operations which are reducible to impulsive interactions.

Returning now to the foregoing questions concerning Bohr's conception of the ontological status of microphysical objects, we should expect to find the answer in his critique of the Einstein, Podolsky, and Rosen criterion of physical reality in the course of which he spoke of the need for "a radical revision of our attitude towards the problem of physical reality." But what precisely was Bohr's revised attitude toward this problem? In his "Discussion with Einstein"[93] he referred to the distinction between the object and the measuring instrument which defines in classical terms the conditions under which the phenomena appear. Bohr remarked that it is irrelevant that measurements of momentum or energy transfer from atomic particles to measuring devices like diaphragms or shutters would be difficult to perform. "It is only decisive that, in contrast to the proper measuring instruments, these bodies together with the particles would in

[88]Ref. 3-8 (1936, p. 119).
[89]E. Schrödinger, "Measurement of length and angle in quantum mechanics," *Nature* **173**, 442 (1954).
[90]Ref. 27.
[91]A. Shimony, Role of the observer in quantum theory, "*American Journal of Physics* **31**, 755–773 (1963).
[92]R. E. Cutkosky, "Wave functions," *Physical Review* **125**, 745–754 (1962).
[93]Ref. 4-10 (1949, pp. 221–222).

such a case constitute the system to which the quantum-mechanical formalism has to be applied."

This statement and similar passages, as has been pointed out,[94] suggest that Bohr, recognizing the insufficiency of the phenomenalistic position, regarded the measuring instrument as being describable both classically and quantum mechanically. By concluding that the macrophysical object has objective existence and intrinsic properties in one set of circumstances (e.g., when used for the purpose of measuring) and has properties relative to the observer in another set of circumstances, or, in other words, by extending complementarity on a new level to macrophysics, Bohr avoided committing himself either to idealism or to realism. Summarizing, we may say that for Bohr the very issue between realism and positivism (or between realism and idealism) was a matter subject to complementarity. In any case, also according to Bohr, the ontological preassumptions in quantum mechanics differed from those in relativity.

This disparity, however, does not affect the similarity of both theories as to the relational character of the objects of discussion.

Was Bohr really the first to express the relational nature of quantum processes? Digressing into the history of the complementarity interpretation we find that, to some extent at least, he was preceded by Grete Hermann. Having started her academic career with the study of mathematics under Emmy Noether in Göttingen, Grete Hermann became greatly influenced by the philosopher Leonard Nelson, the founder of the Neo-Frisian school, and in the spring of 1934 joined Heisenberg's seminar in Leipzig. Leipzig in the early 1930s was not only, next to Göttingen and Copenhagen, one of the foremost centers for the study of quantum mechanics and its applications (due to the presence of Felix Bloch, Lev Landau, Rudolf Peierls, Friedrich Hund, and Edward Teller), it also became famous for its study of the philosophical foundations and epistemological implications of the quantum theory, particularly after Carl Friedrich von Weizsäcker, at the age of only 18 years, joined the Heisenberg group. Convinced that the criticistic thesis of the a prioristic character of causality is essentially correct, and knowing that modern quantum mechanics allegedly implied a breakdown of universal causality, Grete Hermann went to Leipzig hoping that in Heisenberg's seminar she would find a solution of this contradiction.

Although not a specialist in physics by schooling, she was able, assisted by B. L. van der Waerden and C. F. von Weizsäcker, to participate most actively in the work of the seminar. As a result of these discussions Grete

[94]Ref. 91 (p. 769).

Hermann published in March 1935 a long essay[95] on the philosophical foundations of quantum mechanics, which still deserves attention today. Hermann's point of departure—which led her to the relational conception of quantum mechanical description—was the empirical fact of the unpredictability of precise results in the measurements on microphysical objects. The usual way out of such a situation by searching for a refinement of the state description in terms of additional parameters is denied by the theory.

Since Hermann rejected von Neumann's proof[96] of the impossibility of hidden variables—for reasons to be explained in due course she regarded this proof as a *petitio principii*—she raised the question of what justifies this denial. To reject the possibility of such a refinement of the state description merely on the basis of its present unavailability would violate the principle of the incompleteness of experience [*Satz von der Unabgeschlossenheit der Erfahrung*]. The sufficient reason for renouncing as futile any search for the causes of an observed result, she declared, can be only this: *One already knows the causes*. The dilemma which quantum mechanics faces is therefore this: Either the theory provides the causes which determine uniquely the outcome of a measurement—But then why should not the physicist be able to predict the outcome?—or the theory does not provide such causes—But then how could the possibility of discovering them in the future be categorically denied? Hermann—and now we come to the crucial point of our historical digression—saw the solution of this dilemma in the relational or, as she expressed it, the "relative" character of the quantum mechanical description, which she regarded as the "decisive achievement of this remarkable theory."

By renouncing the classical principle of objectivity and replacing it by that of the instrument-dependency. in conjunction with the idea that from the factual result of a measurement the physical process, leading to the result, can be causally reconstructed, Hermann explained why the theory prevents predictability without excluding a *post factum* identification of the causes of the particular outcome. How this can be achieved in detail has been described by Hermann for the case of the Weizsäcker-Heisenberg experiment,[97] which, as we have pointed out, was a forerunner of the Einstein-Podolsky-Rosen thought-experiment, which, in its turn, led Bohr to the relational conception of quantum mechanical states. Grete Hermann's resolution of the dilemma was, as she stated, approved by Heisen-

[95]G. Hermann, "Die naturphilosophischen Grundlagen der Quantenmechanik," *Abhandlungen der Fries'schen Schule* **6**, 75–152 (1935).

[96]Von Neumann's proof will be discussed in Chapter 7.

[97]Ref. 31.

berg with the words: "That's it what we have tried for so long to make clear!"[98]

Referring now not to the historical but to the philosophical significance of Hermann's essay we make the following observation. Hermann, as we saw, disproved the possibility of additional parameters on the grounds that quantum mechanics, though predictively indeterministic, is retrodictively a causal theory. In other words, since, with the final result of a measurement in sight, the physicist can reconstruct the causal sequence that led to the observed result, any additional causes (or parameters) would only overdetermine the process and thus lead to a contradiction. Causality and predictability, Hermann emphasized, are not identical. "The fact that quantum mechanics assumes and pursues a causal account also for unpredictable occurrences proves that an identification of these two concepts is based on a confusion."[99]

It seems, however, that Hermann's claim of retrodictive causality is unwarranted.[100] In the author's opinion she did not prove, as she claimed, that a retrodictive conceptual reconstruction of the measuring process provides a *full* explanation of the particular result obtained. Although such a reconstruction may prove the *possibility* of the result obtained, it does not prove its *necessity*. Thus in the Weizsäcker-Heisenberg experiment her reconstruction, starting from the observation, accounts for the fact that the photon *can* impinge on the photographic plate where it impinges, but not that it *must* impinge there.[101]

Hermann's view was later independently embraced by Norwood Russell Hanson in his discussion of the asymmetry between explanation and prediction in quantum mechanics as opposed to their symmetry in classical physics. "After a microphysical event X has occurred within our purview," he wrote, "we can give a complete explanation of its occurrence within the total quantum theory. But it is in principle impossible to predict in advance those features of X so easily explained *ex post facto*."[102] If

[98]"'*Das ist es, was wir schon lange zu sagen versuchen!' sagte mir Heisenberg damals,*" Letter from Grete Henry-Hermann, March 23, 1968.

[99]G. Hermann, "Die naturphilosophischen Grundlagen der Quantenmechanik (Auszug)," *Die Naturwissenschaften* **42**, 718–721 (1935). Cf. also G. Henry-Hermann, "Die Kausalität in der Physik," *Studium Generale* **1**, 375–383 (1947–1948).

[100]For other objections cf. M. Strauss' review in the *Journal of Unified Science (Erkenntnis)* **8**, 379–383 (1940), and W. Büchel, "Zur philosophischen Deutung des quantenmechanischen Indeterminismus," *Scholastik* **27**, 225–240 (1952).

[101]Ref. 95 (pp. 113–114).

[102]N. R. Hanson, "Copenhagen interpretation of quantum theory," *American Journal of Physics* **27**, 1-15 (1959).

Hanson's expression "within the total quantum theory" has to be understood as a qualification of the "completeness" of explanation, namely as far as it is consistent with the probabilistic tenet of the theory, Hanson's statement is certainly correct. But if that expression is not meant as such a qualification, his view, now identical with that professed by Grete Hermann, seems untenable, for "explanation" is a logical process and as such not dependent on temporal order.

The only alternative that would make this asymmetry acceptable would be to distinguish between "quantum mechanical explanation" and "classical explanation"—but this distraction would revert to the preceding qualification. If, however, we also recall that every observation of a quantum phenomenon involves inevitably an irreversible amplification process, without which the phenomenon could not be recorded, we must admit that this very irreversibility not only complicates the chain, if it existed, between cause and effect, but it precludes an unambiguous identification of the cause from the knowledge of the effect.

That Heisenberg's enthusiasm for Hermann's ideas must soon have abated can be seen from his lecture in Vienna,[103] held shortly after the publication of Hermann's essay. Heisenberg refuted in this lecture the possibility of hidden variables not for the reasons adduced by Hermann but on the grounds of the motility of the "cut" [*Schnitt*] between the observed object and the measuring device. Since on both sides of this cut, Heisenberg contended, the physical relations are uniquely (causally) determined, the statistical character of the theory refers precisely to the cut. Any insertion of additional "causes" (hidden variables) at this place would lead to contradictions as soon as the cut is shifted (in the sense of von Neumann's theory of measurement) to another place; for then the place where the cut had formerly been would be part of a causally determined development and the "causes" (hidden variables) inserted there would conflict with the causal laws which now, after the shifting of the cut, prevail at that place.

Heisenberg's argument against hidden parameters seems to lose its cogency if one insists that the so-called classical mode of action of the measuring apparatus should also be regarded as involving hidden parameters; on this assumption the cut between the "classical" and the quantum mechanical parts of the description is no longer uniquely definable as the seat where these parameters become operative but merely as the place where they are brought into the open within the overall description of the

[103] W. Heisenberg, "Prinzipielle Fragen der modernen Physik," in *Fünf Wiener Vorträge* (Deuticke, Leipzig and Vienna, 1936), pp. 91–102; reprinted in W. Heisenberg, *Wandlungen in den Grundlagen der Naturwissenschaft* (Hirzel, Stuttgart, 8th ed., 1949), pp. 35–46; *Philosophic Problems of Nuclear Science* (Faber and Faber, London, 1952), pp. 41–52.

process; any shift of the cut could no longer lead to inconsistencies.

In concluding our historical digression we must point out that Grete Hermann's relationalism was, strictly speaking, even more radical than Bohr's. In her view, this description, to become fully effective, has to take into consideration not only the experimental setup of the situation but also the precise outcome of the observation. The fact that the consequences she drew for the solution of the "dilemma" were erroneous does not disqualify her relational conception of the quantum mechanical description.

In a Letter to the Editor of the *Physical Review*[104] Arthur E. Ruark, at the time Professor of Physics at the University of North Carolina, expressed the idea that the EPR argument may be attacked on the grounds, mentioned in passing by the three authors themselves, that p_2 and q_2 possess physical reality only if both p_1 and q_1—and not merely one or the other—could be measured simultaneously. Against their claim that it would not be reasonable to suppose that the reality of p_2 and q_2 can depend on the process of measurement carried out on system I, he objected that "an opponent could reply: (1) that it makes no difference whether the measurements are direct or indirect"; and (2) that system I is nothing more than an instrument, and that the measurement of p_1 makes this instrument unfit for the measurement of q_1. In brief, according to Ruark's view, the issue in question appears to be primarily a matter of defining the notion of measurement.

6.6. MATHEMATICAL ELABORATIONS

The next to contribute to the EPR issue was Erwin Schrödinger. Having resigned in November 1933 from his position as professor of theoretical physics at the University of Berlin, where he had succeeded Planck in 1928, he accepted an invitation extended to him by F. A. Lindemann, later Lord Cherwell, who, as we recall,[105] was deeply interested in the foundations of quantum theory, to take residence in Oxford as Fellow of Magdalen College. His (five years older) friend and colleague Max Born had left Göttingen two moths earlier to become Stokes Lecturer in Applied Mathematics at Cambridge.

It was during his fellowship in Oxford, where due to the generosity of the Imperial Chemical Industries he was at leisure to study what he liked, that Schrödinger became interested in the EPR paper. On August 14, 1935,

[104] A. E. Ruark, "Is the quantum-mechanical description of physical reality complete?" *Physical Review* **48**, 466–467 (1935).

[105] Ref. 3-46.

only a few weeks after receiving the paper, Schrödinger submitted to the *Cambridge Philosophical Society* an essay which was communicated by Max Born and was read on October 28, 1935. Schrödinger's paper, to which reference[106] has already been made, is in many aspects the very antithesis of Bohr's article on the EPR argument: Schrödinger ignored the epistemology of complementarity; he did not go into any details about the instrumental setup but instead confined himself to a profound abstract investigation anchored firmly in the formalism of the theory. He not only reaffirmed the result obtained by the three authors but generalized it and regarded it as an indication of a serious deficiency of quantum mechanics.

Schrödinger began his paper with a statement of the following well-known facts: the wave function of a system of two particles, which have separated after a temporary interaction, is no longer the product of separate wave functions and hence the knowledge of ψ would not enable us to ascribe to each of the particles an individual wave function even if the interaction was known in all detail; in other words, the best possible knowledge of a whole does not generally include the best possible knowledge of its parts. In this *entanglement* Schrödinger saw *the* characteristic trait of quantum mechanics—"the one that enforces its entire departure from classical lines of thought." By performing an experiment on only one of the particles, not only can this particle's wave function be established, but that of its partner can be inferred without interfering with it. This procedure or "disentanglement," is, according to Schrödinger, of "sinister importance"; for, being involved in every measurement, it forms the basis of the quantum theory of measurement, "threatening us thereby with at least a *regressus in infinitum*, since it will be noticed that the procedure itself involves measurement." The disentanglement and, in particular, in its form underlying the EPR argument in Schrödinger's view, as mentioned before, has something paradoxical about it.

Following Schrödinger and only slightly deviating from his notation we shall call the particle whose state is (indirectly) inferred particle X and characterize it by the variable x; its partner with which it interacted in the past and on which an experiment is performed will be called particle Y and characterized by the variable y.

To understand the meaning of the theorem which Schrödinger proved in the first part of his paper—we may call it the theorem of the noninvariance of inferred state description—we make the following preliminary remarks. Particle Y may be subjected to different types of measurement or different "programs of observation" and in each case the state of particle X can be inferred. Since the latter particle, after its separation from the former, is

[106]Ref. 43.

not interfered with in any way whatever, it may seem natural to assume that the state inferred will always be the same irrespective of the chosen program of observation on particle Y. The noninvariance theorem declares that this assumption is false: The state inferred depends decidedly on what measurement one chooses to perform on the other particle.

To prove the theorem, following Schrödinger, let the experiment on particle Y measure a dynamical variable represented by an operator F whose eigenfunctions form the complete orthonormal set $\varphi_n(y)$ with corresponding eigenvalues y_n. In this case the state function $\psi(x,y)$ of the system to be disentangled must be expanded in terms of $\varphi_n(y)$:

$$\psi(x,y)=\sum_n c_n\gamma_n(x)\varphi_n(y). \tag{18}$$

The c_n have been introduced to assure that the $\gamma_n(x)$ are normalized:

$$\int \gamma_n^*(x)\gamma_n(x)\,dx=1. \tag{19}$$

Equation 19 and the equation

$$c_n\gamma_n(x)=\int \varphi_n^*(y)\psi(x,y)\,dy \tag{20}$$

determine the $\gamma_n(x)$ and c_n, apart from an irrelevant phase factor. If the result of measuring F is y_k (with probability $|c_k|^2$), then $\gamma_n(x)$ describes the state of particle X.

Though normalized, the $\gamma_n(x)$ need by no means be orthogonal to each other. Schrödinger studied the conditions which the $\varphi_n(y)$ have to satisfy in order that the $\gamma_n(x)$ form an orthogonal set. Ignoring questions of convergence and exceptional cases he arrived at the result that the $\varphi_n(y)$ and $|c_n|^{-2}$ must be the eigenfunctions and eigenvalues, respectively, of the homogeneous integral equation

$$\varphi(y)=\lambda\int \mathrm{K}(y,y')\varphi(y')\,dy', \tag{21}$$

where the Hermitian kernel $\mathrm{K}(y, y')$ is defined by the equation

$$\mathrm{K}(y,y')=\int \psi^*(x,y')\psi(x,y)\,dx. \tag{22}$$

In the general case all the reciprocal eigenvalues of the integral equation (21) will differ from each other; then the $\varphi_n(y)$ and the $\gamma_n(x)$ are uniquely determined and there exists, as a rule, one and only one biorthogonal expansion of $\psi(x,y)$. If and only if the eigenfunctions of a program of observations carried out on particle Y include the eigenfunctions of (21), the experiment will lead to the biorthogonal expansion and imply the $\gamma_n(x)$ as the other set.

If the inferred state of particle X were always the same for all programs of observation carried out on particle Y, the same $\gamma_n(x)$ would have to turn up. But these $\gamma_n(x)$ determine the biorthogonal expansion uniquely—just as the $\varphi_n(y)$ did—and hence also the $\varphi_n(y)$ for the other particle Y. Since, however, the program of observation carried out on particle Y was arbitrary, this need not be the case—which proves the noninvariance theorem.

If all $|c_n|^2$ differ from each other, the $\varphi_n(y)$ and the $\gamma_n(x)$ are uniquely determined by $\psi(x,y)$. In this case, which is the rule, the entanglement may be said to consist of the fact that one and only one observable of particle X is uniquely determined by a definite observable of particle Y and vice versa. In this case there exists only one single way to disentangle $\psi(x,y)$ and no paradox of the EPR type can be formulated. At the opposite extreme when *all* eigenvalues of (21) are equal and all possible expansions of $\psi(x,y)$ are biorthogonal the entanglement may be said to consist of the fact that every observable of particle X is determined by an observable of particle Y and vice versa. This is the case discussed by Einstein, Podolsky, and Rosen.

Having thus analyzed from the mathematical point of view the entanglement referred to in the EPR argument, Schrödinger generalized its conclusions. Like Bohr, but independently of him, he reformulated the entanglement under discussion in terms of a ψ-function of the eigenvalues belonging to the commuting observables

$$x = x_1 - x_2 \qquad \text{and} \quad p = p_1 + p_2, \tag{23}$$

which are supposed to have definite known numerical value x' and p', respectively. The eigenvalues of these operators satisfy the equations

$$x' = x_1' - x_2' \qquad \text{and} \quad p' = p_1' + p_2'. \tag{24}$$

Hence, corresponding to Case II (Section 6.3), x_2' can be predicted from x_1', or, corresponding to Case I, p_2' from p_1', and vice versa. Each single *one* of the four measurements in question, if carried out, disentangles the system and endows each of the two particles with a definite state function.

After reformulating the contents of the EPR argument Schrödinger showed that it could be considerably generalized by proving that the value of a Hermitian operator referring to particle I, if given by a "well-ordered" analytical function of the observables x_1 and p_1

$$F(x_1, p_1) \tag{25}$$

and not containing $\sqrt{-1}$ explicitly, is equal to the value of the observable

$$F(x_2+x',p'-p_2), \tag{26}$$

which obviously refers to system II. Hence the result of either observation can be predicted from the other one. In the EPR argument clearly F is identical with either its first argument (Case II) or with its second argument (Case I). As Schrödinger pointed out, the theorem can easily be proved by showing that—through simple algebraic operations performed on the terms of the minuends in the curved brackets—the following equation holds:

$$\{F(x_2+x',p'-p_2)-F(x_1,p_1)\}\psi=0. \tag{27}$$

An interesting example of such a generalization was given by Schrödinger in an essay he was working on at Oxford in the early fall of 1935 and which appeared in three parts in *Die Naturwissenschaften.*[107] Referring to the EPR argument, by which it was prompted, Schrödinger compared the particle II, the one assumed not to be interfered with mechanically, with "a scholar in examination" who is questioned as to the value of his position or momentum coordinate. He is always prepared to give the correct answer to the *first* question he is asked, although thereafter he "is invariably so disconcerted or tired out that all the following answers are wrong." But since he always provides the right answer to the first question without knowing which of the two questions—position or momentum—he is going to be asked first, "he must know both answers."

Turning now to the generalization Schrödinger started from (23) and assumed for simplicity that $x=p=0$ so that

$$x_1=x_2 \qquad \text{and} \quad p_1=-p_2. \tag{28}$$

For $F(x_2,p_2)$ of (25) he took the operator function $F_2=x_2{}^2+p_2{}^2$ whose eigenvalues are known from the theory of the harmonic oscillator to be $\hbar, 3\hbar,\ldots,(2n+1)\hbar,\ldots$.[108] The eigenvalue of F_2 for particle II, assumed to be the "scholar" (not being interfered with mechanically), must therefore be one of the terms of this series and must, as shown in the preceding paper, coincide with the eigenvalue of $F_1=F(x_1,p_1)=x_1{}^2+p_1{}^2$, pertaining to particle I on which the measurement is performed. In the same sense, the scholar always gives the correct answer as to the eigenvalue of the operator

[107]E. Schrödinger, "Die gegenwärtige Situation in der Quantenmechanik," *Die Naturwissenschaften* **23**, 807–812, 824–828, 844–849 (1935).

[108]For the Hamiltonian $(p^2/2m)+(k/2)x^2$ with $k=4\pi^2m\nu^2$ the eigenvalues are $(n+\frac{1}{2})h\nu$. In the above case $m=\frac{1}{2}$, $k=2$ and hence $\nu=1/\pi$.

$G_2 = a^2 x_2{}^2 + p_2{}^2$ where a is an arbitrary positive constant and the eigenvalues are $a\hbar, 3a\hbar, \ldots, (2n+1)a\hbar, \ldots$.

Each new value of a provides a new question to which the scholar, if "asked," provides the right answer. What is still more amazing is the fact that these answers are mathematically not interrelated, that is, they cannot be connected with each other as the formulae indicate, for if x_2' were the answer stored in the scholar's "mind" for the x_2 question, p_2' the answer for the p_2 question, then

$$\frac{(a^2 x_2'^2 + p_2'^2)}{a\hbar} \tag{29}$$

is *not* an odd integer for given values of x_2' and p_2' and *every* positive number a. And yet for every "question" imposed on particle II, we get the answer by performing the appropriate measurement on particle I, and every measurement on particle II informs us of the result which a corresponding measurement, if performed on particle I, would yield; the correctness of this inference can be tested by performing the measurement *de facto* on particle I. Once the two systems agree, in the sense of (28), in their position and momentum coordinates, they agree, roughly speaking, in all dynamical variables. But how the numerical values of all these variables at one and the same system are interrelated with each other, we do not know. It remains, according to Schrödinger, one of the mysteries of quantum mechanics.

In the sequel of the paper Schrödinger explained that such difficulties, and in particular the paradox exhibited in the EPR argument, cannot be resolved within the framework of conventional quantum mechanics by pointing out that measurements are temporally extended processes rather than instantaneous acts so that the tacitly assumed simultaneity of measurement results is open to question. He envisaged, however, the possibility that a modified theory which treats time not merely as a parameter but as a dynamical variable subject to an indeterminacy relation may well solve the antinomies. In fact, in his opinion, these conceptual difficulties make such a revision of quantum theory imperative.[109] Schrödinger also advanced in this paper an argument against the completeness thesis of quantum mechanics, which differs from the EPR argument. It resorts to what may be called "the principle of state distinction," or briefly PSD: *states of a macroscopic system which could be told apart by a macroscopic observation are distinct from each other whether observed or not*. Since in a quantum mechanical measuring process the state of the total system,

[109]Cf. Schrödinger's search for a time operator, Ref. 5-66.

composed of the quantum (micro) system characterized by the variable x and the (macro) measuring device characterized by the variable y, is described by a function $\psi(x,y)$ of the type (18), which remains "disentangled" as long as it is not observed, the states of the macrosystem, as described by quantum mechanics, do not satisfy the PSD; hence the quantum mechanical description of physical reality is not complete. Schrödinger explained this point, at the end of the first part (section 5) of the paper under discussion, in terms of a thought-experiment which has since become known as "the case of the Schrödinger cat."

He imagined a closed steel chamber containing a cat and a small amount of a radioactive element, the probability of disintegration of one atom of which per hour is exactly .5; the disintegration, if it occurs, activates a Geiger counter and closes a circuit, thereby electrocuting the cat. If the entire system is represented by a wave function ψ_1 denoting the state "cat alive" and ψ_2 the state "cat dead," then the state of the system at the end of the hour is described, according to quantum mechanics, by the (normalized) wave function

$$\psi = 2^{-1/2}(\psi_1 + \psi_2), \tag{30}$$

a superposition in which the two states "cat alive" and "cat dead" are "mixed or smeared together by equal amounts"—in blatant contrast to the PSD of macro-observables, as mentioned above. Only through the very act of observation, that is, looking at the cat, is the system thrown into a definite state. It would be "naive," said Schrödinger, to consider the ψ-function in (30) as depicting reality.

The situation depicted in this example has nothing exceptional in it. In fact, it describes a characteristic feature of every quantum mechanical measurement. The only trait which distinguishes "the case of the Schrödinger cat" from the majority of quantum mechanical measurements is the fact that in this case "the reduction of the wave packet," enacted by the process of observation, comes down to the selection between only two (alternative) states with mutually exclusive and contradictory properties (life and death): $\psi \rightarrow \psi_1$ or $\psi \rightarrow \psi_2$.

The reason that the practicing physicist is not seriously disturbed by this deficiency of the theory has been correctly stated by Hilary Putnam:[110]

> It must be admitted that most physicists are not bothered by the Schrödinger's cat case. They take the standpoint that the cat's being or not being electrocuted should itself be regarded as a measurement. Thus in their view, the

[110]H. Putnam, "A philosopher looks at quantum mechanics," in *Beyond the Edge of Certainty*, R. G. Colodny, ed. (Prentice Hall, Englewood Cliffs, N. J., 1965), pp. 75–101.

reduction of the wave packet takes place..., when the cat either feels or does not feel the jolt of electric current hitting its body. More precisely, the reduction of the wave packet takes place precisely when if it had not taken place a superposition of different states of some macro-observable would have been predicted. What this shows is that working physicists accept the principle that macro-observables always retain sharp values (by macroscopic standards of sharpness) and deduce when measurement *must* take place *from* this principle. But the intellectual relevance of the Schrödinger's cat case is not thereby impaired. What the case shows is that the principle that macro-observables retain sharp values at all times is not *deduced* from the foundations of quantum mechanics, but is rather dragged in as an additional assumption.

A similar objection against the quantum mechanical way of describing a situation which, as a macroscopic configuration, demands an unambiguous characterization was raised in 1953 by Einstein, though in a different context. In a contribution to a book dedicated to Max Born on the occasion of his retirement from the Tait Chair of Natural Philosophy in the University of Edinburgh Albert Einstein[111] considered the following problem. A ball of a diameter of about 1 mm rebounds elastically between two parallel walls placed normally on the x-axis at the points $x=0$ and $x=L$; if the ball always moves along the x-axis and has a well-determined energy, the wave function which describes this motion, according to quantum mechanics, is given by

$$\psi_n = a_n \sin\left(\frac{n\pi x}{L}\right)\exp\left(\frac{iE_n t}{\hbar}\right) \tag{31}$$

where

$$E_n = \frac{n^2h^2}{8mL^2} \tag{32}$$

and n is an integer.

Since $E_n = {p_n}^2/2m$ the momentum p_n turns out to be

$$p_n = \pm\frac{nh}{2L} \tag{33}$$

and ψ describes a superposition of two motions with opposite velocities whereas, macroscopically, the ball can have only one of such motions. The wave function ψ, Einstein concluded, therefore does not describe the

[111]A. Einstein, "Elementare Überlegungen zur Interpretation der Grundlagen der Quanten-Mechanik," in *Scientific Papers Presented to Max Born* (Oliver and Boyd, Edinburgh, London, 1953), pp. 33–40.

individual process but rather represents a statistical ensemble of particles with opposing motions and quantum mechanics as usually understood is incapable of describing the real behavior of an individual system [*Realbeschreibung für das Einzelsystem*].

At the end of his paper Einstein referred to the de Broglie and Bohm approach of hidden variables (which we shall discuss in the next chapter[112]) and to Schrödinger's conception of the ψ-field as the ultimate physical reality and his rejection of particles and of Born's probabilistic interpretation, and he showed that both these attempts at obtaining descriptions of individual processes are unsatisfactory.

In January 1953 Einstein sent a preprint of this paper to Bohm, who had left Princeton for São Paulo, without expecting that Bohm would agree with him. In fact, Bohm replied[113] that he accepts neither Einstein's criticism of his (and de Broglie's) causal interpretation nor even Einstein's conception of the usual interpretation of Born. In particular, Bohm declared: "I do not think that the Born theory fulfills the condition to contain as a limit case the behavior of macro-systems. I would therefore appreciate it very much, if you could give me a chance to have a few comments published alongside of your paper." Einstein,[114] of course, welcomed this idea but asked Bohm to send a copy of his own (Einstein's) paper together with Bohm's remarks to de Broglie so that de Broglie might also publish his comments if he would like to do so. Space does not allow us to discuss the interesting exchange of letters[115] between Einstein and Bohm on the issue raised. But the main argument brought forth by Bohm will be discussed later in the context of our analysis of his hidden variable theory.

A short time after Einstein wrote this paper Lajos Jánossy of the Central Research Institute of Physics in Budapest (Magyar Tudományos Akadémia Kötponti Fizikai Kutató Intézete) suggested a thought-experiment[116] involving a situation that sounded even more paradoxical than that of the Schrödinger cat. At the First Hungarian Congress of Physics, held in Budapest, Lajos Jánossy described it as follows. Consider

[112] Ref. 7-64, 7-65.

[113] Letter from Bohm to Einstein, dated February 4, 1953.

[114] Letter from Einstein to Bohm, dated February 17, 1953.

[115] On November 24, 1954, Einstein wrote to Bohm: "I do not believe in micro- and macro-laws, but only in (structural) laws of general validity [*nur an (Struktur) Gesetze*]. And I believe that these laws are logically simple, and that reliance on this logical simplicity is our best guide."

[116] L. Jánossy and K. Nagy, "Über eine Form des Einsteinschen Paradoxes der Quantentheorie," *Annalen der Physik* **17**, 115–121 (1956).

a diaphragm with two slits, each of which can be opened or closed by a shutter connected with a separate counter, and a weak α-particle emitter placed between the two counters. In the beginning of the experiment Jánossy imagined both slits to be closed; if an α-particle strikes one of the counters, the slit connected with this counter is opened, the counters cease to operate, and a light-source is turned on in front of the diaphragm to illuminate a photographic plate placed behind the diaphragm. If ψ_1 describes the state of the system when only slit I is open and ψ_2 the state when only slit II is open, the state of the whole system, if unobserved, is given, according to quantum mechanics, by the superposition

$$\psi = 2^{-1/2}(\psi_1 + \psi_2), \tag{34}$$

which corresponds to an interference pattern on the photographic plate. If, however, the slits are observed, the state is described by either ψ_1 or ψ_2. Jánossy thus came to a conclusion: "If we turn on the apparatus, wait until the exposure is finished, and only thereafter develop the plate, we shall obtain on it a system of interference fringes. If, however, we find out by observation, prior to the exposure, which slit is open, then this 'observation' enforces, in accordance with the usual principles of quantum mechanics, a reduction $\psi \rightarrow \psi_K$ ($K=1$ or $K=2$) and thereafter no interference should appear." Jánossy mentioned this thought-experiment which, though apparently easily performable in the laboratory, has never been performed, in connection with certain deliberations which we shall discuss later.

The climax of Schrödinger's contributions to the problems raised by the EPR argument was a sequel to his paper read before the Cambridge Philosophical Society. In this later paper,[117] which he completed in April 1936 and which, communicated by Dirac, was read before the Society on October 26, 1936, Schrödinger showed that his former result, according to which the inferred state of one particle depends decidedly on what measurement the experimenter chooses to perform on the other, can be given a much stronger form. According to the former result the experimenter, using the indirect method of noninterference, has a certain control over a given spectrum of states to be inferred. In the present paper Schrödinger showed that this control is much more powerful: "in general a sophisticated experimenter can, by a suitable device which does *not* involve measuring non-commuting variables, produce a non-vanishing probability of driving the system into any state he chooses; whereas with the ordinary direct method at least the states orthogonal to the original one are

[117]E. Schrödinger, "Probability relations between separated systems," *Proceedings of the Cambridge Philosophical Society* **32**, 446–452 (1936).

excluded." It would carry us too far to review the proof of this statement in its mathematical details. Schrödinger, with his characteristic modesty, did not claim any priority for the statement but called it merely a corollary to a theorem about "mixtures" (in contrast to "pure states").

How it is physically possible that an experimenter can indeed steer a far-away system, without interfering with it at all, into any state out of an infinity of possible states can be shown most simply for polarization states of annihilation photons.[118]

Summarizing Schrödinger's work on the issue under discussion we may say that he agreed with the assumptions underlying the view of Einstein and his collaborators, but instead of satisfying himself with the epistemological conclusions drawn by these authors, he pursued the mathematical aspects of these examples and showed in its elaborations that the conceptual situation is even more complicated than envisaged by Einstein, Podolsky, and Rosen. In fact, for Schrödinger, it was not only a matter of incompleteness of the theory but a manifestation of a serious flaw in its very foundations. As he intimated at the end of each of the three papers discussed, a possible source of this deficiency was, in his view, the particular role played by the concept or variable "time" in quantum mechanics and, in particular, in its theory of measurement which applies the nonrelativistic theory probably beyond its legitimate range.

The logical gap between the formalism of a theory and its interpretation is most vividly illustrated when two theoreticians base opposing interpretations on the same mathematical formalism. A case in point is Schrödinger's work on the EPR argument and especially his essays published in the *Proceedings of the Cambridge Philosophical Society*[119] and Wendell Hinkle Furry's "Note on the Quantum-Mechanical Theory of Measurement"[120] published at about the same time in the *Physical Review*. Furry, a graduate of the University of Illinois and from 1934 to 1937 an instructor in Kemble's department at Harvard University, wrote his paper independently of Schrödinger's work, arrived at almost identical results from the mathematical point of view, but gave them an interpretation precisely opposite to that given by the originator of wave mechanics. For Furry, the crucial issue in the dispute between Einstein, Podolsky, and Rosen and

[118] Cf. O. R. Frisch, "Observation and the quantum," in *The Critical Approach to Science and Philosophy*, M. Bunge, ed. (The Free Press of Glencoe, Collier-Macmillan, London, 1964), pp. 309–315.

[119] Ref. 43, 117.

[120] W. H. Furry, "Note on the quantum-mechanical theory of measurement," *Physical Review* **49**, 393–399 (1936); "Remarks on measurement in quantum theory," *Physical Review* **49**, 476 (1936).

Bohr was the fact that the former took it for granted that a system as soon as it is free from mechanical interference, has independent real properties and is in a definite quantum mechanical state. The irreconcilability of the ordinary quantum mechanical formalism with such an assumption is, in Furry's view, the content of the EPR argument.

To investigate in general and abstract terms the exact extent of this disagreement, Furry first formulated the opposing underlying assumptions explicitly for the case of two systems which had interacted with each other at some previous time. In the notation of (18) the combined system is described by the state function

$$\psi(x,y)=\Sigma w_n^{1/2}\gamma_n(x)\varphi_n(y) \tag{35}$$

where $|c_n|^2=w_n$ [the phase factor being absorbed by either $\gamma_n(x)$ or $\varphi_n(y)$], the $\gamma_n(x)$ are eigenfunctions of an observable G_x with eigenvalues g_n, and the $\varphi_n(y)$ are eigenfunctions of an observable F_y with eigenvalues f_n. For simplicity it is assumed that all g_n (and all f_n) are distinct. A measurement of G_x on the x-system or of F_y on the y-system must always result in a g_n or f_n, respectively, where g_n implies f_n (and vice versa). Each system may thus serve as a measuring instrument for the state of the other.

If G'_x is any observable of the x-system (with eigenfunctions γ'_n and eigenvalues g'_n), ψ can be expanded in the γ'_n with coefficients which are functions of y:

$$\psi=\sum_n \gamma'_n(x)\alpha_n(y). \tag{36}$$

From the orthonormality of the γ'_n it follows that

$$\alpha_k(y)=(\gamma'_k,\psi)=\sum_n w_n^{1/2}(\gamma'_k,\gamma_n)\varphi_n. \tag{37}$$

The probability that, after a measurement of G'_x on the x-system has given the value g'_k, the measurement of an observable F'_y of the y-system (with eigenfunctions φ'_n and eigenvalues f'_n) results in the value f'_r is according to the rules of quantum mechanics

$$|(\gamma'_k\varphi'_r,\psi)|^2. \tag{38}$$

But this expression, or

$$\left|\sum_n w_n^{1/2}(\gamma'_k,\gamma_n)(\varphi'_r,\varphi_n)\right|^2, \tag{39}$$

is also equal to

$$|(\varphi_r', \alpha_k)|^2. \tag{40}$$

Since this holds for all observables F_y', it follows that after a measurement on the x-system has resulted in the value g_k' for G_x', the state of the y-system is given by $\alpha_k(y)$, apart from normalization.

As shown earlier by von Neumann, as far as one system alone is concerned, the statistical information provided by (35) is represented by a mixture of the relevant states [$\gamma_n(x)$ or $\varphi_n(y)$, respectively] with the weights w_n.

Furry later compared the results of quantum mechanical calculations with those obtained on the assumption that a system, having ceased to interact, can be regarded as possessing independently real properties. This assumption, which he regarded as underlying Einstein's approach, he called briefly *Assumption A*: During the interaction each system has really made a transition to a definite state in which it remains once the interaction ceased (the ordinary time development according to the Schrödinger equation of motion being ignored); these transitions are not causally determined nor do we know, without making a suitable measurement, which transition occurred. All we know, in the absence of measurements, is merely that if the x-system is, say, in state γ_k, then the y-system is in the corresponding state φ_k, and the probability for this transition is w_k. This suffices to make all required predictions with the help of the theorems of ordinary probability theory (Method A). To investigate to what extent Method A leads to conclusions which conflict with those obtained by the usual quantum mechanical calculations (or, briefly, Method B), Furry studied four types of question, always assuming that the state of the combined system is given by (35):

1. What is the probability of obtaining f_x' for F_y' without any measurement having been made on the x-system? Both methods yield

$$\sum_n w_n |(\varphi_n, \varphi_k')|^2. \tag{41}$$

2. If g_n has been obtained in measuring G_x, what is the probability of obtaining f_x' for F_y'? Both methods yield

$$|(\varphi_n, \varphi_k')|^2. \tag{42}$$

3. If G_x' has been measured with result g_j', what is the probability of obtaining f_k for F_y? Both methods yield

$$\frac{w_k|(\gamma_k,\gamma_j')|^2}{\sum_n w_n|(\gamma_n,\gamma_j')|^2}. \tag{43}$$

Thus far no discrepancies between the two methods could be established.

4. If G_x' has been measured with result g_j', what is the probability of obtaining f_k for F_y'? Method A yields

$$\frac{\sum_n w_n|(\gamma_n,\gamma_j')|^2|(\varphi_n,\varphi_k')|^2}{\sum_n w_n|(\gamma_n,\gamma_j')|^2}, \tag{44}$$

whereas Method B yields

$$\frac{\left|\sum_n w_n^{1/2}(\gamma_n,\gamma_j')(\varphi_n,\varphi_k')\right|^2}{\sum_n w_n|(\gamma_n,\gamma_j')|^2}. \tag{45}$$

A comparison of (44) with (45) shows that Assumption A "resolves" the well-known phenomenon of interference between probability amplitudes. Since such a resolution of interference generally is accounted for as being the result of the mere introduction of an intermediate measuring device,[121] one may be easily misled to conclude that (44) and its underlying Assumption A are consistent with the theory. The discrepancy between (44) and (45) is a consequence of the fact that after a measurement of G_x' has been made on the x-system, the y-system is in a *pure state* which is in general not one of the $\varphi_n(y)$, whereas on the Assumption A its statistics is described by a *mixture* of these states which can never be reduced through any possible manipulation of the w_n to any pure state other than one of the $\varphi_n(y)$.

To show that such contradictions between Assumption A and the established theory of quantum mechanics are not restricted to abstract

[121]See, e.g, Ref. 3-19 (1930, pp. 59–62).

mathematics but have concrete physical implications, Furry described a thought-experiment, based on that proposed in the EPR argument, and showed by detailed computation that it leads, if treated according to Assumption A, to a contradiction with the position-momentum indeterminacy relation. Hence both mathematical arguments and concrete experiments, he concluded, prove "that the assumption, a system when free from mechanical interference necessarily has independent real properties, is contradicted by quantum mechanics."

Furry, as we have seen, regarded Assumption A as the basis adopted by Einstein and his collaborators for their argument. But strictly speaking their argument rests only on the existence of a correlation between the two components of the system and not on Assumption A. The importance of Furry's analysis consequently lies not in its attempt to refute the EPR argument, as so often alleged, but in the fact that it suggests a specific experiment—that described by question (4), by means of which Assumption A can be tested (See, e.g., the experiment (1950) of Wu and Shaknov which will be discussed in Section 7.9 below.)

6.7. FURTHER REACTIONS TO THE EPR ARGUMENT

A few weeks after the editors of *The Physical Review* received the first of Furry's papers they received a letter from Hugh C. Wolfe[122] of the City College of New York in which the following simple solution of the EPR paradox was proposed. Quantum mechanics, according to Wolfe, does not concern itself with the "state" of a physical system but rather with our knowledge of that state, an idea which on various occasions had been expressed by Heisenberg. Wolfe argued that the "measurements on the first system affect our knowledge of the accord and therefore affect the wave function which describes that knowledge." It is thus only natural, according to Wolfe, that different measurements performed on the first particle provide us with different information about the second and hence also with different wave functions; and it follows that different measurements on the first particle give rise to different predictions of the results of measurements on the second particle.

Wolfe's purely idealistic interpretations of the ψ-function as a description not of a state of a physical system but of our knowledge of that state had been resorted to before by many physicists to explain the reduction of the wave packet not as a physical process but as a sudden change in

[122]H. C. Wolfe, "Quantum mechanics and physical reality," *Physical Review* **49**, 274 (1936).

information. It was felt, however, by the majority of quantum theoreticians that such a view not only denies the objectivity of physical state description but makes physics part of psychology and threatens thereby the very existence of physics as a science of human-independent existents. Led to its logical conclusion this view would imply that the physicist is not investigating nature at all but only his own investigations.

Thus far all attempts to resolve the difficulty raised by the EPR argument concerned themselves with the interpretational part of the theory. The first to regard this difficulty as indicative of a fundamental defect in the formalism of the theory was Henry Margenau. Margenau obtained his Ph.D. in 1929 at Yale University where he became Eugene Higgins Professor of Physics and Natural Philosophy. At the end of 1935 he suggested[123] that several conceptual difficulties in the quantum mechanical description can be eliminated by abandoning the projection postulate from the formalism of quantum mechanics. According to this postulate any measurement performed on a physical system transforms the initial wave function, characterizing the state of the system before the measurement, into an eigenfunction of the operator representing the measured observable; in other words, the measurement "projects" the initial state function on that vector in Hilbert space whose eigenvalue is the result of the measurement ("reduction of the wave packet"). The denial of this postulate clearly invalidates one of the presuppositions necessary for the formulation of the EPR argument and thus dissolves the dilemma.

Margenau listed four major reasons for the rejection of the projection postulate:

1. It introduces a peculiar asymmetry of time into quantum mechanical description, since according to the postulate the state of the system is completely determined only *after* the measurement.

2. It contradicts the more fundamental Schrödinger equation of motion. If, for example, the position of a particle in a definite momentum state φ is measured by means of the coordinate measurement operator M, the state φ is suddenly converted into a δ-function ψ:

$$M\varphi = \psi. \tag{46}$$

Since, however, the measurement is undoubtedly a physical operation, the process must be describable as an interaction between physical systems in terms of the ordinary formalism. If H_0 denotes the interaction-free Hamiltonian, H_M the interaction with the measuring device, and $H = H_0 + H_M$,

[123] H. Margenau, "Quantum mechanical description," *Physical Review* **49**, 240–242 (1936).

then

$$H\varphi = \frac{h}{2\pi i}\frac{\partial\varphi}{\partial t}. \tag{47}$$

Assuming that the time Δt of the interaction, transforming φ into $\psi = \varphi + \Delta\varphi$ is small, we obtain

$$\Delta\varphi = \frac{2\pi i}{h}\Delta t H\varphi \tag{48}$$

and

$$\left[1 + \frac{2\pi i}{h}\Delta t(H_0 + H_M)\right]\varphi = \psi \tag{49}$$

or

$$\left[1 + \frac{2\pi i}{h}\Delta t(H_0 + H_M)\right] = M. \tag{50}$$

Since ψ in (46) is unpredictable, M cannot be a unique operator as usually encountered in the formalism. The left-hand side of (50), however, represents a unique operator whatever the specific form of H_M may be.

3. Since a wave function such as ψ is, strictly speaking, a probability distribution, its determination requires a very large number of observations and cannot be determined by a single measurement as the projection postulate contends.

4. The postulate is not only undesirable and in conflict with other axioms, it is also unnecessary, for, according to Margenau, no physically significant quantum mechanical calculation requires its validity.[124]

On November 13, 1935, Margenau sent a preprint of this paper to Einstein with a covering letter in which he expressed his dissatisfaction with all the "replies" made so far concerning the Einstein, Podolsky, and

[124]That not only the usual (or "strong") projection postulate has to be rejected, since it defines a state vector, i.e., a vector describing the state of an ensemble of systems, on the basis of only a single measurement, but that also the "weak" projection postulate according to which the state of the postmeasurement ensemble of all systems yielding a particular result has to be described by the eigenfunction belonging to this result is unnecessary, useless, and even unjustifiable has repeatedly been argued by Margenau's student James L. Park. Cf. J. L. Park, "Nature of quantum states," *American Journal of Physics* **36**, 211–226 (1968); "Quantum theoretical concepts of measurement," *Philosophy of Science* **35**, 205–231 (1968); J. L. Park and H. Margenau, Ref. 3-72.

Rosen argument. In his opinion they did not touch on the fundamental issue involved but applied "superficial means" to eliminate the difficulty. In his answer to Margenau Einstein[125] pointed out that the formalism of quantum mechanics requires inevitably the following postulate: "If a measurement performed upon a system yields a value m, then the same measurement performed immediately afterwards yields again the value m with certainty." He illustrated this postulate by the example of a quantum of light which, if it has passed a polarizer P_1, is known to pass with certainty a second polarizer P_2 with orientation parallel to P_1.

It was Einstein's reply and, in particular, the "if" in it that led Margenau to his distinction between "state preparation" and "measurement." For this was Margenau's reasoning: To remove the "if" from Einstein's proposition, one has to check whether the quantum did really pass through P_1; but for this purpose some second device (eye, photocell, etc.) would be necessary to register the presence of the photon. Thus only the combination P_1 plus checking device constitutes a measuring instrument, whereas P_1 alone merely prepares a state.

These ideas found further elaboration in an essay Margenau[126] published in 1937. In an attempt to clarify the logical structure of quantum mechanics and, in particular, its theory of measurement, Margenau—following in this respect the Aristotelian way of expounding a subject—first investigated, as impartially as possible, the consequences of different views on the nature of the theory. According to the *objective view* the ψ-function refers to the state itself, and according to the *subjective view* it refers only to the knowledge of the state. Margenau also emphasized their incompatible features and did not conceal his preference for the former. In his opinion only the objective view could be coupled with the empirical frequency interpretation of probability—in contrast to the subjective "a priori" conception of probability—and only the empirical interpretation of probability is not intrinsically unmeasurable.

This position, in turn, led Margenau to a statistical interpretation of quantum mechancis and, in particular, to a statistical interpretation of the indeterminacy relations as relations between the spreads in repeated measurements of conjugate observables on similarly prepared systems—an approach not much different from that of Popper. Distinguishing sharply

[125]Cf. H. Margenau, "Philosophical problems concerning the meaning of measurement in physics," *Philosophy of Science* **25**, 23–33 (1958); *Measurement—Definitions and Theories*, C. W. Churchman and P. Ratoosh, eds. (Wiley, New York; Chapman and Hall, London, 1959), pp. 163–176. An excerpt of Einstein's letter is quoted (1958, p. 29) (1959, p. 171).

[126]H. Margenau, "Critical points in modern physical theory," *Philosophy of Science* **4**, 337–370 (1937).

between "preparations of states," such as the injection of electrons through a magnetic field which endows the electrons with a new spin-state but does not yet measure the spin, and "measurements" proper which yield numerical results, Margenau rejected categorically the theory of acausal jumps and hence the projection postulate.

On the basis of the statistical interpretation of the wave function no single measurement—as mentioned before—admits enough information to determine a state. Moreover, a measurement, in contrast to a preparation, frequently annuls the system completely; for example, a position measurement of a particle by recording it on a photographic plate. "On the objective view," Margenau argued, "the assertion that a measurement produces an eigenstate is precisely as meaningful as the contention that, after cracking and eating a nut, I still have a nut, but in its cracked and eaten state."[127] At the conclusion of the article Margenau applied these ideas to the method of reduction of the wave packet in the case of two particles which have interacted before. He assumed that the state of the combined system could be written [as in (35)] as a biorthogonal expansion

$$\psi(x,y)=\sum c_n\gamma_n(x)\varphi_n(y) \tag{51}$$

where γ_n is an eigenfunction belonging to the eigenvalue of the operator G referring to the first particle, and $\varphi_n(y)$, f_n, and F, respectively, to the second. If a measurement of G yielded the result g_k, the usual "jump theory," as Margenau (like Schrödinger) derisively remarked, interprets the process as a sudden contraction of ψ into $\gamma_n(x)\varphi_k(y)$. According to Margenau what happens is only this: The probability for the occurrence of the combination of g_k and f_j is zero unless $j=k$. In fact, this probability, according to quantum mechanics, equals

$$\left|\int\int\psi(x,y)\gamma_k^*(x)\varphi_j^*(y)\,dx\,dy\right|^2=\left|\sum_n c_n\int\gamma_n(x)\gamma_k^*(x)\,dx\int\varphi_n(y)\varphi_j^*(y)\,dy\right|^2$$

$$=\left|c_j\int\gamma_j(x)\gamma_k^*(x)\,dx\right|^2=|c_j|^2\delta_{jk}. \tag{52}$$

"This is all that matters in any application of the method of resolving the wave packet. The conclusion that the state function makes an abrupt transition is quite irrelevant and avoidable," concluded Margenau at the end of his essay.

[127] *Op. cit.*, p. 364.

In 1936 Einstein published his *credo* concerning the philosophy of physics in an essay "Physics and Reality,"[128] which started with the remark that science is but a refinement of everyday thinking and showed how the ordinary conception of a "real external world" leads the scientist to the formation of the concept of bodily objects by taking, out of the multitude of his sense experiences, certain repeatedly occurring complexes of sense impressions. From the logical point of view, this concept of a bodily object is not to be identified with the totality of sense impressions but is "an arbitrary creation of the human mind." Although we form this concept originally on the basis of sense impressions, we attribute to it—and this is the second step in building up "reality"—a significance which is to a high degree independent of sense impressions and we thereby raise its status to that of an object of "real existence." This process, continued Einstein, finds its justification exclusively in the fact that "by means of such concepts and mental relations between them, we are able to orient ourselves in the labyrinth of sense impressions." That the totality of sense impressions can be put in order was for Einstein a fact "which leaves us in awe, for we shall never understand it." The eternal mystery of the world," he declared, "is its comprehensibility."

This, briefly, was the conceptual process on which, in conjunction with the principle of using a minimum of primary notions and relations, Einstein based the construction of the fundamental concepts of classical mechanics, field theory, and relativity. At the end of the essay, when discussing quantum mechanics, Einstein raised the question of how far the ψ-function describes a real condition of a mechanical system. To answer this question he considered a system, originally in state ψ_1 of lowest energy E_1, which is acted upon during a finite time interval by a small external force. Its state consequently becomes $\psi = \Sigma c_r \psi_r$, where $|c_1| \approx 1$ and $|c_k| \approx 0$ for $k \neq 1$. If the answer is an unqualified yes, Einstein declared, then we can hardly do other than ascribe to this condition a definite energy E which exceeds E_1 by a small amount ("because, according to a well established consequence of the relativity theory, the energy of a complete system [at rest] is equal to its inertia [as a whole]. This, however, must have a well defined value.") Pointing out that such a conclusion violates the Franck-Hertz experiments on electron collisions, according to which energy values of a state lying

[128] A. Einstein, "Physik und Realität," *Journal of the Franklin Institute* **221**, 313–347 (1936); translated into English by J. Picard, "Physics and reality," *ibid.*, 349–382. The German text is reprinted in *Zeitschrift für freie deutsche Forschung* **1** (1), 5–19, (2), 1–14 and in A. Einstein, *Aus Meinen Späteren Jahren* (Deutsche Verlags-Anstalt, Stuttgart, 1952), pp. 63–104; the English text is reprinted in A. Einstein, *Out of My Later Years* (Thames & Hudson, London; Philosophical Library, New York, 1950; Littlefield, Adams&Co., Totowa, N.J., 1967), pp. 58–94, and in A. Einstein, *Ideas and Opinions*, (Crown, New York, 1954), pp. 290–323.

between the quantum values do not exist, Einstein arrived at the result that the ψ-function "does not in any way describe a homogeneous condition of the body, but represents rather a statistical description...."

On the basis of this conclusion, that the ψ-function describes not a single system but rather an ensemble of systems, Einstein thought he was in a position also to resolve the difficulties which he, Podolsky, and Rosen had raised. Here the operation of measuring one of the conjugate quantities may be conceived of as a transition to a narrower ensemble of systems; a position measurement leads therefore to a subensemble that differs from that obtained by a measurement of the momentum, and hence also the state functions depend "upon the point of view according to which this narrowing of the ensemble of systems is made."

Intrigued by Einstein's essay and by the Einstein-Podolsky-Rosen paper, Paul Sophus Epstein, whose contributions to quantum mechanics, such as his explanation of the Stark effect, are well known, decided to put aside for some time his more technical work and study the problem of physics and reality. Epstein, who had obtained his Ph.D. in Munich under Arnold Sommerfeld in 1914, had met Einstein in Zürich and corresponded with him since 1919. Now at the California Institute of Technology Epstein wrote a paper[129] in which he analyzed the realist viewpoint according to which the world of things has a reality beyond the observing mind and independent of it, in contrast to the view of the phenomenologist or sensationalist for whom such a distinction is illusory. Distinguishing between a *philosophical* and a *physical* problem of reality—for the physicist the world is simply the "totality of all critically sifted observations, no matter how obtained"—Epstein admitted that the physicist may also interpret his observations as manifestations of a nature existing beyond him and his instruments and thus form a dualistic conception which consists of two opposing physical worlds, one directly observed and the other inferred. Epstein now raised the question of whether such a dualistic viewpoint is necessary or whether it may be replaced by the phenomenological restriction of recognizing observations only and ignoring any reality beyond. If the answer, he argued, depends on the criterion which of the two views is "better suited to the logical description of the accumulated scientific observations," the problem is not necessarily insoluble.

In quantum mechanics, Epstein declared, this problem can be given a concrete and definite formulation: Can one assign reality—and if so, to what extent—to unobservables, such as the position of a particle whose momentum is accurately known? To illustrate the relation of this problem

[129] P. S. Epstein, "The reality problem in quantum mechanics," *American Journal of Physics* **13**, 127–136 (1945).

to the Einstein-Podolsky-Rosen argument Epstein discussed an interesting thought-experiment based on the operation of an interferometer employing a half-silvered glass plate and moveable mirrors. Claiming that the analysis of this experiment shows that the very possibility of a measurement may constitute a sufficient reason for a sudden change of state, even in the absence of a physical disturbance of the system, he concluded that "the critical work of Einstein...was highly important in contributing to the clarification of the conceptions of quantum dynamics but was in no way fatal to these conceptions. In particular, it did not prove that the unobservables have more reality than the quantum theory attributes to them."

Shortly after the paper was published in June, Epstein sent a reprint to Einstein who read it with great interest was glad, as he wrote to Epstein, not to agree with him, "for this gives me an opportunity to write to you." To affirm, Einstein replied, that the ψ-function should be regarded as a description of a real factual situation [*eines realen Sachverhaltes*] "leads to conceptions which run counter to my intuitive feeling (space-time action at a distance of an implausible character)." "My private opinion," he concluded the letter, "is this: the quantum theory in its present form is a most successful attempt, carried out with insufficient means (concepts)."[130]

In his reply[131] to Einstein's letter, which also contained an outline of the Einstein-Podolsky-Rosen argument and pointed out, in this context, that the interaction under discussion is supposed to take place "in the vicinity of $t=0$," Epstein apologized for the delay of his answer. When critically studying the argument again, Epstein wrote, he was unable to understand certain points in it but hoped that Tolman would be able to explain them to him. When, however, even Tolman was unable to do so he saw no other way than to turn to Einstein himself. Epstein thus sent to Einstein his own mathematical analysis of the physical situation underlying the Einstein-Podolsky-Rosen argument.

Epstein did not start by describing the state of the system by (7)–(9), as proposed by Einstein and his co-workers, for he questioned the legitimacy of ignoring the time factor in this expression [Was mir Schwierigkeiten bereitet, ist der Umstand, dass sich der Zeitfaktor von diesem Ausdruck abspalten lässt, während dies für die gewöhnlichen Wellenpakete der Mechanik nicht der Fall ist.] Starting therefore *ab ovo* from the time-

[130] "Meine Privatmeinung ist die: Die Quantentheorie in ihrer gegenwärtigen Form ist ein höchst erfolgreicher Versuch, unternommen mit unzureichenden Mitteln (Begriffen)." A. Einstein, letter to P. S. Epstein (undated), copy in Einstein Estate, Princeton, N.J.

[131] Letter of P. S. Epstein to A. Einstein, dated November 4, 1945, Einstein Estate, Princeton, N.J.

dependent Schrödinger equation for a two-particle system

$$\left[H(x_1,x_2)+\frac{\hbar}{i}\frac{\partial}{\partial t}\right]\psi=0 \tag{53}$$

Epstein obtained the general solution

$$\psi(x_1,x_2,t)=\sum_E U_E(x_1,x_2)e^{-iEt/\hbar} \tag{54}$$

and in the special case of noninteraction where $H=H_1(x_1)+H_2(x_2)$, $E=E_1+E_2$,

$$\psi(x_1,x_2,t)=\sum_{E_1,E_2} u_{E_1}{}^{(1)}(x_1)u_{E_2}{}^{(2)}(x_2)e^{-i(E_1+E_2)t/\hbar} \tag{55}$$

where

$$[H_1(x_1)-E_1]u_{E_1}{}^{(1)}(x_1)=0 \qquad \text{and} \quad [H_2(x_2)-E_2]u_{E_2}{}^{(2)}(x_2)=0.$$

If instead of in terms of the energies E_1 and E_2 the states are described in terms of the momenta p_1 and p_2, then the state function is

$$\psi(x_1,x_2,t)=\sum_{p_1,p_2} u_{p_1}{}^{(1)}(x_1)u_{p_2}{}^{(2)}(x_2)\exp\left\{\frac{-it[E_1(p_1)+E_2(p_2)]}{\hbar}\right\}. \tag{56}$$

Finally, if only cases with $p_1=-p_2=p$ are considered, where p is a continuous variable, then

$$\psi(x_1,x_2,t)=\int_{-\infty}^{\infty} u_p^{(1)}(x_1)u_{-p}^{(2)}(x_2)\exp\left\{\frac{-it[E_1(p)+E_2(-p)]}{\hbar}\right\}, \tag{57}$$

which, if compared with the Ansatz (7)–(9) proposed by Einstein,

$$\psi(x_1,x_2)=\int_{-\infty}^{\infty}\exp\left[\frac{ip(x_1-x_2+x_0)}{\hbar}\right]dp, \tag{58}$$

shows that Einstein and his collaborators assumed

$$E_1(p)+E_2(-p)=\text{const.}, \tag{59}$$

that is, the total energy does not depend on p.

Condition (59), Epstein pointed out, would be satisfied, for instance, in the case of two electrons if one is in a positive and the other in a negative energy state. But the latter is not observable. In ordinary mechanical systems condition (59) is never satisfied. By calculating the time-dependent state function for such ordinary systems Epstein could prove that it is a delta-function only for $t=0$ so that a measurement of x_2 at a later time does not make it possible to infer a sharp value of x_1, the position coordinate of the first particle, and no conceptual difficulties arise.

Epstein concluded his exposition with the following remark: "It is possible that somewhere in my calculations a mistake has been made which I do not notice.... As mentioned, Tolman does not find any error in the preceding so that I thought it advisable to send you the whole calculation."

In his early reply[132] to Epstein, Einstein admitted that "it may be possible that our work on the subject under discussion may contain some gaps. But the crucial point is not affected thereby. Schrödinger has thoroughly examined the formal aspects of our paper shortly after its publication and fully confirmed its calculations. I myself am not sufficiently familiar with the formalism of quantum mechanics to check your argument without spending much time...." At the end of the letter Einstein restated his position: "If one is of the opinion that a theory of the structure of quantum mechanics is something final for physics he has either to renounce the space-time localization of the real or to replace the idea of a real state of affairs by the notion of probabilities for the result of all conceivable measurements. I think this is the view presently adopted by the majority of physicists. But I do not believe that it will prove in the course of time to be the correct way."

Curious to know how Professor Nathan Rosen would react to Epstein's criticism, which was unknown to him since he had left Princeton in 1936, the present author showed him a photostatic copy of Epstein's calculation. Rosen's answer was as follows: "In my opinion the question of the time dependence of the wave function is irrelevant. What counts is that at a certain moment a certain state with certain properties can exist. The fact that after this moment the state changes and assumes other properties cannot affect the results which can be drawn from the state at the moment under consideration. It may be added that on the basis of Schrödinger's wave mechanics one has a rather wide choice of selecting the wave in a certain moment and I think there is no reason to reject the wave function in terms of which the problem was originally formulated."[133]

[132]Letter from Einstein to P. S. Epstein, dated November 10, 1945, Einstein Estate, Princeton, N.J.

[133]Letter from N. Rosen to the author (in Hebrew), dated December 10, 1967.

Rosen's answer is supported by the fact that there exist other formulations of the EPR argument which lead to the same conclusion as the original formulation but do not involve any time dependence. The best known example of such reformulations of the EPR argument is David Bohm's in which the ordinary wave functions are replaced by spin functions. Designed to make the mathematics easier, it was presented by Bohm as follows.[134]

Consider a system composed of two spin one-half particles to be in a singlet state (total spin zero) and its two particles to move freely in opposite directions. The state will be described by the following function, which is invariant under spatial rotations:

$$\psi = 2^{-1/2}[\psi_+(1)\psi_-(2) - \psi_-(1)\psi_+(2)] \tag{60}$$

where $\psi_\pm(k)$, $k = 1,2$, represents the wave function of the state in which particle k has the spin $\pm h/2$ in the direction in which it is measured. Once the particles have separated without change of their total spin and ceased to interact, any desired spin component of particle 1 may be measured [e.g., the x-component $s_x(1)$]. The total spin being zero, one knows immediately, without in any way interfering with particle 2, that its spin component in the same direction is opposite to that of particle 1 [i.e., $s_x(2) = -s_x(1)$]. On the basis of Einstein's criterion of physical reality it must be concluded that the inferred value [in our case $s_x(2)$] presents an element of physical reality and must have existed even before the measurement had been carried out. But since any other direction could have been chosen equally well, all three spin components of particle 2 must have simultaneously definite sharp values after its separation from particle 1. Since, however, quantum mechanics (because of the noncommutativity of the spin operators) allows only one of these components to be specifiable at a time with complete precision, quantum mechanics does not provide a complete description of physical reality.

It is historically interesting to note that in 1951 Bohm made the following comment: "If this conclusion were valid, then we should have to look for a new theory in terms of which a more nearly complete description was possible. We shall see, however,... that [Einstein's] analysis involves in an integral way the implicit assumption... that the world is actually made up of separately existing and precisely defined 'elements of reality.' Quantum theory, however, implies a quite different picture of the structure of the world at the microscopic level. This picture leads, as we shall see, to a perfectly rational interpretation of the hypothetical experiment of ERP

[134] D. Bohm, *Quantum Theory* (Prentice-Hall, Englewood Cliffs, N.J., 1951), pp. 614–619.

within the present framework of the theory."[135]

The "quite different picture of the structure of the world at the microscopic level" to which Bohm here referred is Bohr's conception of the nature of microphysics which, as we shall see in the next chapter, Bohm soon rejected, though not *in toto* as is usually assumed.

To the 1949 issue of *Dialectica*, which was dedicated to discussions on the concept of complementarity and was edited by Wolfgang Pauli, Einstein contributed a paper[136] in which he pointed out that the assumption that objects once spatially separated from each other could still influence each other would make it impossible to formulate or test physical laws. In this context Einstein put special emphasis on the notion of bounded regions in space [*begrenzte Raumgebiete*] or spatial domains separated from each other. Whereas the existence of such spatially bounded regions served Einstein, on physical grounds, as an argument in support of the Einstein-Podolsky-Rosen thesis, it served Cooper, on mathematical grounds, as an argument against the thesis.

Jacob Lionel Bakst Cooper, an applied mathematician who had graduated from the University of Cape Town, gone to Oxford, and become reader and lecturer at Birkbeck College in London, was primarily interested in functional analysis. In 1943 he found a new proof[137] of the spectral resolution theorem by showing that the existence of exactly one resolution of unity can be proved if, instead of von Neumann's hypothesis that the operator H is hypermaximal, the physically more meaningful assumption is made that the Schrödinger time-dependent equation has a solution for all t if its Hamiltonian is a closed Hermitian operator (Cooper used the terminology of M. H. Stone according to which Hermitian operators with domains dense in Hilbert space are called "symmetric"). While elaborating on this proof in a later paper[138] Cooper became interested also in the philosophy of quantum mechanics and in particular in the EPR argument.

[135] D. Bohm, *op.cit.*, p. 615. In analogy to our remarks in Ref. 40 we can show that it is possible to prepare the state described by (60) which exhibits a complete correlation between the spins of the two particles, i.e., $s_n(1)+s_n(2)=0$ where n denotes x or y or z, by pointing out that the three operators $S_n=s_n(1)+s_n(2)$ commute with each other. For example, $[s_x(1)+s_x(2), s_y(1)+s_y(2)]=i[s_z(1)+s_z(2)]=0$, even though the individual spin components of a single particle do not commute.

[136] A. Einstein, "Quantenmechanik und Wirklichkeit," *Dialectica* **2**, 320–324 (1949).

[137] J. L. B. Cooper, "Symmetric operators in Hilbert space," *Proceedings of the London Mathematical Society* **50**, 11–55 (1949).

[138] J. B. Cooper, "The characterization of quantum-mechanical operators," *Proceedings of the Cambridge Philosophical Society* **46**, 614–619 (1950).

If Margenau had criticized the argument as being based on a too liberal use of the formalism of quantum mechanics (by relying on the projection postulate) and if Epstein had criticized it as being based on a too restrictive use of the formalism (by not allowing for time dependence), Cooper flatly assailed it as having made a faulty use of the formalism. To understand Cooper's reasoning[139] we have to recall that the expansion (7) assumes that the operator A_1 has a complete set of eigenfunctions and hence is valid if and only if A_1 is self-adjoint. It should also be recalled that an operator is defined only if, in addition to its mathematical structure, its domain is specified. In the space of quadratically integrable functions $f(x)$, for example, the operator $P=(\hbar/i)\partial/\partial x$ is self-adjoint if x can vary from $-\infty$ to $+\infty$; if, however, x is restricted to the half-line $x>0$, the structurally similar operator $P_1=(\hbar/i)\partial/\partial x$ with domain $\mathfrak{L}^2(0,\infty)$ is not self-adjoint (though Hermitian) unless the function $f(x)$ on which it operates satisfies the boundary condition $f(0)=0$. In fact, P_1 is in von Neumann's terminology maximal but not hypermaximal; it has no eigenfunctions at all, for otherwise these would include the eigenfunctions $\varphi(x)=\exp(ipx/\hbar)$ of P, but $\varphi(0)\neq 0$.[140]

The assumption of a separation in space between the particles after the interaction, Cooper argued, implies that the momentum operator is not P but rather P_1 and consequently the expansion (7) does not hold. "The arguments in the paradox of separated systems break down, either because systems in quantum mechanics cannot be completely separated, or because if they are so separated they no longer have self-adjoint representation for the momenta."

Like Epstein's so also Cooper's criticism seems never to have been countered on purely formal grounds. More than two months before sub-

[139]J. L. B. Cooper, "The paradox of separated systems in quantum theory," *Proceedings of the Cambridge Philosophical Society* **46**, 620–625 (1950).

[140]An elementary proof of the above statement is this:

$$(f,P_1 f)=(\hbar/i)\int_0^\infty f^*(x)f'(x)\,dx$$

$$=(\hbar/i)f^*(x)f(x)|_0^\infty-(\hbar/i)\int_0^\infty f^{*\prime}(x)f(x)\,dx$$

$$=(\hbar/i)|f(0)|^2+(P_1 f,f).$$

For a deeper discussion of this point cf. J. von Neumann, "Zur Theorie der unbeschränkten Matrizen," *Journal für Mathematik* **161** 208–236 (1926), especially p. 234 *et seq.*, *Collected Works,* Vol. 2, pp. 144–172, or M. H. Stone, *Linear Transformations in Hilbert Space* (Ref. 1-5), p. 435 (Theorem 10.8).

mitting his paper for publication Cooper had sent Einstein a draft of it.[141] In his reply Einstein[142] did not touch upon the mathematical aspects involved but rather pointed out that when he referred to the independent existence of two parts *A* and *B* of a system he did not mean that a potential barrier separated *A* from *B* but rather that "an action on *A* has no immediate influence on the part *B*." Only in this sense, Einstein declared, is the assumption (*a*) of an independent existence of spatially separated parts incompatible with the thesis (*b*) that the ψ-function offers "the complete description of an individual physical situation." "The majority of quantum theorists discard (*a*) tacitly to be able to conserve (*b*), I, however, have strong confidence in (*a*) so that I feel compelled to relinquish (*b*)," he wrote to Cooper. Unsatisfied with this answer Cooper[143] informed Einstein on additional reasons in support of his thesis that "the argument about separated systems can,...be applied only when there is a barrier which makes it impossible for the two to move together." But "in that case...the momentum operator has no representation theorem," that is, it is not self-adjoint. In his last letter[144] to Cooper Einstein argued that the alternative, either quantum mechanics offers an incomplete description or one has to assume some kind of action at a distance, cannot be invalidated by the mathematical considerations under discussion.

Whereas Cooper tried to solve "the paradox of separated systems" by restricting determinations in space, Olivier Costa de Beauregard,[145] who after his demobilization in 1940 had been working at the Centre National de la Recherce Scientifique under Louis de Broglie and obtained his Ph.D. in 1943, tried to resolve the difficulties by relaxing determinations in time. Classical reasoning concerning space and time, he claimed, tacitly postulates the exclusive admissibility of retarded actions (retarded potentials); but this is a prejudice rooted in macrophysical experience and no a priori reasons can be adduced to justify the necessity of this postulate for individual quantum phenomena. In fact, according to Costa de Beauregard, arguments such as that by Einstein, Podolsky, and Rosen precisely show that the individual quantum process obeys a symmetrical principle of

[141]Letter from Cooper to Einstein, dated October 11, 1949.

[142]Letter from Einstein to Cooper, dated October 31, 1949.

[143]Letter from Cooper to Einstein, dated November 19, 1949.

[144]Dated December 18, 1949.

[145]O. Costa de Beauregard, "Le 'paradoxe' des correlations d'Einstein et de Schrödinger et l'épaisseur temporelle de la transition quantique," *Dialectica* **19**, 280–289 (1965). A similar proposal of renouncing the validity of the macrophysical space-time order for microphysical processes has been suggested also by Wolfgang Büchel, "Eine philosophische Antinomie der Quantenphysik," *Theologie und Philosophie* **42**, 187–207 (1967).

retarded and advanced actions instead. As soon as advanced actions are admitted, the measurement process performed on one of the two particles may be conceived as producing an effect on the other particle at a time when the two particles were still interacting with each other, a conception which obviously would resolve the "paradox."

Costa de Beauregard suggested this conjecture in 1953 when he showed that advanced action does not necessarily imply strict determinism, for this action may affect the interaction between the two particles in such a way that of two conjugate dynamical variables only that one becomes determined which will be measured at the later time.[146] In the situation under discussion the measurement performed on particle 1, say, of its momentum p_1, is then supposed not to affect directly particle 2 but rather, in a time-reversed direction, the original interaction between the two particles in such a way that at the moment of the performed measurement of p_1 the momentum p_2 of particle 2 is determinate ($= -p_1$) but its position q_2 is completely indeterminate. The consistency of quantum mechanics would thus be saved without resorting to any action at a distance or superluminal velocities.

The suggestion to which J. L. B. Cooper alluded as a possible alternative to his own refutation of the incompleteness argument, that is, "systems in quantum mechanics cannot be completely separated," was elaborated in great detail about 10 years later by David H. Sharp,[147] a pupil of Hilary Putnam at Princeton University, in a multilateral attack on the EPR argument.

In his first assault (p. 229) against the argument of the incompleteness of quantum mechanical description Sharp pointed out that such an argument, to be relevant, must refer to the most complete description available in quantum mechanics; but according to Sharp, the authors did not employ the most complete description available when they used the formalism of correlated systems and, in particular, when they assigned "pure states to parts of the correlated system after measurement." For, in Sharp's view, "strictly speaking, only the entire system has a state function; separate 'parts' of the system will not be representable by pure states even after the measurement." Sharp's second objection (p. 229), which is mentioned but not "dwelt upon," criticized the mathematical representation of the state

[146]O. Costa de Beauregard, "Une réponse à l'argument dirigé par Einstein, Podolsky et Rosen contre l'interpretation bohrienne de phénomènes quantiques," *Comptes Rendus* **236**, 1632–1634 (1953).

[147]D. H. Sharp, "The Einstein-Podolsky-Rosen paradox re-examined," *Philosophy of Science* ***28**, 225–233 (1961).*

[148]On this point cf. Chapter 11.

after the measurement as proposed by Einstein et al. In his third criticism Sharp challenged Einstein's assumption that an exact measurement on system 1 of an observable can be carried out, for according to Wigner's elaboration[148] of von Neumann's theory of measurement, in a closed system including the measured object and the measuring apparatus, only quantities which commute with all conserved quantities are exactly measurable.

Finally—and this objection was fully endorsed by Putnam—Sharp contended that the interaction energy between two particles, however far they may be separated from each other, is never rigorously zero. "In particular one can think of two forms of interaction, believed to hold between any two charged material particles, that can never rigorously be assumed to vanish if the particles are a finite distance apart: the gravitational interaction and the electrostatic interaction." Concerning the argument that for sufficiently large distances such interactions are approximately zero Sharp pointed out that "whether or not an approximation is justified depends on the specific context in which it is made." Approximations are justified, he continued, only if the final results are substantially the same whether or not the approximation is made. Hence, contrary to most ordinary problems in classical dynamics or celestial mechanics, the situation of the EPR argument does not admit the assumption that the interaction is zero, for the mathematical implication of such an assumption—that is, the factorization of the wave function of the combined system into the product of wave functions for the individual components—differs substantially from the mathematical implication of the alternative assumption—that there exists an interaction, however weak it may be, in which case the wave function no longer separates. Sharp's criticism resembled Bohr's insofar as it also advocated a holistic character of the description of nature, though on entirely different grounds. It would therefore have been highly instructive if in the last section of Sharp's paper, which discussed Bohr's answer to the EPR argument, the difference between the two holistic approaches had been brought out in full relief.

Instead, Sharp criticized as inadequate Bohr's procedure of simply replacing the reality criterion of Einstein and his co-workers by a new criterion according to which two quantities are simultaneously real only if they are simultaneously measurable. For "their critique is not answered," said Sharp, "if it is *presumed* that the criterion for reality of a physical quantity ordinarily used in quantum mechanics is the correct one, and that a rival criterion is a priori incorrect if it is incompatible with it. The above line of argumentation would be quite satisfactory, of course, if it could be shown that quantum mechanics itself somehow contained the correct criterion for the reality of a physical quantity. Whether or not this is the case is at present unknown."

In his comments in support of Sharp's paper Hilary Putnam[149] placed the blame on serious inconsistencies in the conceptual structure of quantum mechanics and, in particular, in von Neumann's theory of measurement and made a final solution of the problem contingent upon the elaboration of a consistent and physically acceptable theory of measurement. His pungent critique provoked a sharp answer by Margenau and Wigner,[150] who defended von Neumann against what they called the "confusion in Professor Putnam's comments." At the same time, however, they admitted that not all problems in the interpretation of quantum mechanics had been solved. Since the quantum theory of measurement and its specific problems will be discussed only at the end of this book a detailed analysis here of Margenau and Wigner's comments and Putnam's comments[151] on these comments would interfere too much with the order of our presentation.

The reader interested in this controversy may also consult with great profit some recent articles in which Clifford A. Hooker[152] of the University of Western Ontario analyzed Sharp's arguments at great length in order to confute them point by point.

Sharp's criticism of Bohr's rebuttal of the incompleteness argument has lately been revived by Henry P. Krips[153] of the University of Adelaide in Australia. According to Krips Bohr's reality criterion, by insisting on measurement as a precondition for reality, is more complicated than its EPR counterpart; therefore it would have been Bohr's obligation to justify his preference on grounds other than by merely resolving the EPR problem. Moreover, Krips added, since the measurement process itself is a physical process involving object and apparatus, a state vector describing this situation will be real in Bohr's sense only if measured itself, a procedure which obviously leads to an infinite regress and is hence unacceptable. Having thus rejected Bohr's solution, Krips explained his

[149]H. Putnam, "Comments on the paper of David Sharp," *Philosophy of Science* **28**, 234–237 (1961).

[150]H. Margenau and E. P. Wigner, "Comments on Professor Putnam's comments," *Philosophy of Science* **29**, 292–293 (1962).

[151]H. Putnam, "Comments on comments on comments—A reply to Margenau and Wigner," *Philosophy of Science* **31**, 1–6 (1964).

[152]C. A. Hooker, "Sharp and the refutation of the Einstein, Podolsky, Rosen paradox," *Philosophy of Science* **38**, 224–233 (1971); "The nature of quantum mechanical reality," in *Paradigms and Paradoxes*, R. G. Colodny, ed. (University of Pittsburgh Press, 1972), pp. 67–302. Cf. also C. A. Hooker, "Concerning Einstein's, Podolsky's and Rosen's objection to quantum theory," *American Journal of Physics* **38**, 851–857 (1970).

[153]H. P. Krips, "Two paradoxes in quantum mechanics," *Philosophy of Science* **36**, 145–152 (1969); "Fundamentals of measurement theory," *Nuovo Cimento* **60B**, 278–290 (1969); "Defence of a measurement theory," *Nuovo Cimento* **1B**, 23–33 (1971).

own by pointing out that the injunction against assigning simultaneous eigenvectors for q and p as states of the same system, which would imply $\Delta q = \Delta p = 0$ and thus contradict the indeterminacy relations, "refers only to ensemble states and not to states on a particular occasion," since the indeterminacy principle restricts only the distribution of values over ensembles and not, as Einstein, Podolsky, and Rosen wanted to claim, the simultaneous values of an individual system. But with the admission of such a simultaneous assignment the EPR argument has lost its strongest feature.

Krips' resolution was rejected by Hooker,[154] for whom the EPR problem expresses not merely a formal difficulty in the quantum mechanical formalism but rather a deep-seated inconsistency in its physical interpretation, and must be regarded as a central clue in the search for a more adequate understanding of the theory; Krips' resolution was rejected also by Herman Erlichson[155] of Staten Island Community College, New York, as being fallacious on the grounds that Krips has not shown that a state associated with sharp values of position and momentum can be expressed in the language of quantum mechanics, for by resorting to "state on a particular occasion" as distinct from "quantum states" he abandons, in Erlichson's opinion, the quantum mechanical formalism which does not provide for such distinctions.

A short paper by Richard Schlegel[156] of Michigan State University which criticized Hooker's demand for a physical clarification of the EPR issue as superfluous on the ground that Bohr's reply provided just such a clarification gave rise to a similar, though less heated, debate. According to Schlegel the inference of sharp position and momentum values for particle 2 from corresponding measurements on particle 1 is physically meaningful only if it is associated, as Bohr insisted, with an experimental arrangement that allows one to determine these values; but such a determination would conflict with the indeterminacy restrictions and hence must be excluded. Hooker[157] rebuffed Schlegel's reasoning as restricting too much the physical significance of the quantum formalism. Erlichson[158] claimed that

[154] C. A. Hooker, "Against Krips' resolution of two paradoxes in quantum mechanics," *Philosophy of Science* **38**, 418–428 (1971).

[155] H. Erlichson, "The Einstein-Podolsky-Rosen paradox," *Philosophy of Science* **39**, 83–85 (1972).

[156] R. Schlegel, "The Einstein-Podolsky-Rosen paradox," *American Journal of Physics* **39**, 458 (1971).

[157] C. A. Hooker, "Re: Schlegel's Bohrian reply to EPR," *American Journal of Physics* **40**, 633–634 (1972).

[158] H. Erlichson, "Bohr and the Einstein-Podolsky-Rosen paradox," *ibid.*, 634–636.

Schlegel misinterpreted Bohr by imputing to him a denial of the possibility of inferring those values; Bohr's disagreement concerned only the definition of physical reality. In his defense Schlegel,[159] though admitting to having deliberately simplified his presentation, quoted Bohr's writings in support of his thesis.

Our discussion of the various reactions to the EPR argument is far from exhaustive, for to do full justice to all significant papers[160] on this subject would require a monograph even longer than that[161] recently published on the notorious clock paradox in relativity, with which it has perhaps more than merely historical features in common.

The great interest in the EPR problem among philosophically inclined physicists originated undoubtedly from the fact that the problem seems to relate a *prima facie* purely physical situation to fundamental metaphysical issues, somehow like Anselm of Canterbury's famous "ontological argument" to prove the existence of God which claimed to have established a linkage between thought and existence. The philosophical issue which is often, though not always,[162] regarded as ultimately responsible for a definite stance vis-à-vis the EPR problem is the alternative "realism versus idealism." Since we refrain from using philosophical labels we shall not distinguish the various categories of realism (naive, critical, structural) or of idealism (ontological, epistemological, subjective, transcendental) but shall confine ourselves to pointing out that for most authors the EPR criterion of reality presents, in the present context at least, the viewpoint of the realist (whatever that means), whereas Bohr's position on this issue is generally called idealist or positivist or philosophical-positivist (in contrast to methodological-positivist). However, the realist is also often defined as one who under certain conditions ascribes existence to unobserved entities whereas the positivist either categorically denies such existence or at least rejects any statement about such entities as scientifically meaningless. The

[159] R. Schlegel, "Reply to Hooker and Erlichson," *ibid.*, 636–637.

[160] Among these we mention are E. Breitenberg, "On the so-called paradox of Einstein, Podolsky and Rosen," *Nuovo Cimento* **38**, 356–360 (1964). J. M. Jauch, *Foundations of Quantum Mechanics* (Addison-Wesley, Reading, Mass., 1968), section 11-10. K. R. Popper, "Particle annihilation and the argument of Einstein, Podolsky, and Rosen," in *Perspectives in Quantum Theory*, W. Yourgrau and A. van der Merwe, eds. (M.I.T. Press, Cambridge, 1971), pp. 182–198. D. L. Reisler, "The epistemological basis of Einstein's, Podolsky's, and Rosen's objection to quantum theory," *American Journal of Physics* **39**, 821–831 (1971). H. Erlichson, "Einstein and the Einstein-Podolsky-Rosen criterion of reality," *ibid.*, **40**, 359–360 (1972). P. A. Moldauer, "Reexamination of the arguments of Einstein, Podolsky and Rosen" (to be published).

[161] L. Marder, *Time and the Space-Traveller* (George Allen & Unwin, London, 1971).

[162] H. Mehlberg, "Philosophical interpretations of quantum physics" (preprint).

question may be asked therefore whether the EPR reality criterion implies a realism also in the just mentioned sense.

The answer is affirmative if certain elaborations, discussed by Paul K. Feyerabend[163] and by Bernard d'Espagnat[164], for example, of the original EPR reality criterion are accepted as equivalent to, or at least as immediate consequences of, the original criterion. According to these elaborations the with certainty predicted or inferred value such as $-p$ in case I in the original EPR situation exists even prior to the moment of measurement and it does so because in the absence of any interaction between the two particles the measurement performed only on particle 1 cannot have created the inferred value of the momentum of particle 2.

The mathematical and physical implications of this stronger version of the EPR reality criterion had been studied earlier by W. H. Furry[165] (Assumption A) and been found in particular instances to be at variance with ordinary quantum mechanics. From the historical point of view it is interesting to note that Einstein, who once declared that "the belief in an external world independent of the perceiving subject is the basis of all natural science," had never committed himself to the stronger version of his reality criterion. In his view, only the incompleteness but not necessarily the incorrectness of ordinary quantum mechanics was a logical consequence of his reality criterion.

It has been claimed on various occasions that Einstein's argumentation had not reached its final logical conclusions in the sense that a closer examination could show that his reality criterion is even incompatible with the very assumption that quantum mechanics provides a *correct* description of physical reality and not merely with the assumption that this description is *complete*. A recent example of this claim is an argument advanced by Richard Friedberg[166] of Columbia University, which has the additional merit that it studies some implications of the reality criterion for physics in general[167] and not for quantum mechanics alone. The argument, with minor modifications, proceeds as follows.

Referring to the EPR formulation of the reality criterion (Section 6.3) we first raise this question: How do we know that the value or result we

[163]P. K. Feyerabend, Lecture (unpublished, 1965).

[164]B. d'Espagnat, *Conceptual Foundations of Quantum Mechanics* (Benjamin, Menlo Park, Calif., 1971), Chapter 7.

[165]Ref. 120.

[166]R. Friedberg, "Verifiable consequences of the Einstein-Podolsky-Rosen criterion for reality" (unpublished, 1969).

[167]It should be recalled that Einstein explicitly stated that the criterion is of course also "in agreement with classical...ideas of reality." Ref. 39 (p. 778).

predict with certainty without in any way disturbing system S_2 pertains indeed to system S_2? Clearly, only if the result can be measured also without disturbing anything *but* S_2 (for conciseness we ignore the disturbance on the measuring apparatus). We thus arrive at the first reformulation of the EPR criterion of reality: If a result is obtained either by a measurement which does not disturb anything but S_2 or by a measurement which in no way disturbs S_2, then there exists an element of physical reality which corresponds to this result and pertains to S_2, or, briefly, then the result is part of reality. Since it was tacitly assumed that the result obtained by either procedure is the same we may also say: If the result of a measurement which disturbs only S_2 agrees with the result of another measurement which in no way disturbs S_2, then the result is part of reality. Calling "system S_1" all that is disturbed by the second measurement mentioned in the preceding statement, we arrive, by a further obvious step, at the final reformulation (RF) of the EPR reality criterion:

RF. If the result of a measurement on S_1 which (i.e., the measurement) in no way disturbs S_2 certainly[168] agrees with the result of a measurement on S_2 which in no way disturbs S_1, then the result is part of reality—even if neither measurement is performed.

Now, if three numerical quantities x, y, z satisfy RF they possess simultaneously definite values, whether or not they can be measured simultaneously, and also $A=xy+yz+zx$, though perhaps unmeasurable, possesses a definite value. Since we can measure any two of the three quantities simultaneously, one on S_1 and the other on S_2, each product xy, yz, zx is measurable. By preparing repeatedly identical pairs S_1, S_2 the "product averages" or expectation values $\langle xy\rangle$, $\langle yz\rangle$, $\langle zx\rangle$ can also be "simultaneously" determined by measuring them on three independent sequences of such pairs. Therefore the sum of these product averages, $B=\langle xy\rangle+\langle yz\rangle+\langle zx\rangle$, is also a measurable quantity. RF, of which the last few steps were independent, then implies that $B=\langle A\rangle$, for RF implies that the product averages are derivable from a single probability distribution.

If, in particular, the numerical value of each quantity x, y, z is either $+1$

[168]The expression "certainly agrees" should imply that the information about the "agreement" need not necessarily be obtained by actually performed measurements but may also be a consequence of the theory itself (as in the EPR example). The positivist objection that "if neither measurement is performed" we do not know (or even do not know what it means) that the results agree may be answered by referring to a sequence of measurements on a series of pairs S_1, S_2 identically prepared.

or -1, we obtain without the use of RF the inequality

$$-3 \leqslant B \leqslant 3 \tag{61}$$

because each product average ranges from -1 to $+1$; but with the use of RF we obtain the more restrictive inequality

$$-1 \leqslant B \leqslant 3 \tag{62}$$

because A is either $+3$ (if $x=y=z=+1$) or -1 (otherwise) and $B=\langle A\rangle$. In other words, RF has the verifiable consequence

$$\langle xy\rangle+\langle yz\rangle+\langle zx\rangle \geqslant -1, \tag{63}$$

which is valid both in classical and in quantum physics being derived exclusively from RF and generally valid statistical assumptions. Classical physics will never conflict with this inequality, but quantum mechanics will!

Consider the two spin-$\frac{1}{2}$ particles 1 and 2 of a system in a singlet state (total spin zero) as discussed earlier in Bohm's version of the EPR argument and let $\mathbf{n}$ be an arbitrary unit vector. The spin component $\boldsymbol{\sigma}(1)\cdot\mathbf{n}$ of particle 1 satisfies all the conditions imposed on x: its numerical value, either $+1$ or -1, if obtained by a measurement on particle 1 will certainly agree with the result obtained by a measurement on particle 2 (under reversal of sign) while it is assumed that neither measurement disturbs the particle on which it is not performed. Similarly, for any three unit vectors $\mathbf{n}_1$, $\mathbf{n}_2$, and $\mathbf{n}_3$ the quantities $\boldsymbol{\sigma}(1)\cdot\mathbf{n}_1$, $\boldsymbol{\sigma}(1)\cdot\mathbf{n}_2$, $\boldsymbol{\sigma}(1)\cdot\mathbf{n}_3$ satisfy these conditions. Hence by (63)

$$\langle\boldsymbol{\sigma}(1)\cdot\mathbf{n}_1\boldsymbol{\sigma}(1)\cdot\mathbf{n}_2\rangle+\langle\boldsymbol{\sigma}(1)\cdot\mathbf{n}_2\boldsymbol{\sigma}(1)\cdot\mathbf{n}_3\rangle+\langle\boldsymbol{\sigma}(1)\cdot\mathbf{n}_3\boldsymbol{\sigma}(1)\cdot\mathbf{n}_1\rangle \geqslant -1 \tag{64}$$

where it is understood that a measurement of $\boldsymbol{\sigma}(1)\cdot\mathbf{n}_j\boldsymbol{\sigma}(1)\cdot\mathbf{n}_k$ is made by measuring $\boldsymbol{\sigma}(1)\cdot\mathbf{n}_j$ on particle 1 and $\boldsymbol{\sigma}(1)\cdot\mathbf{n}_k=-\boldsymbol{\sigma}(2)\cdot\mathbf{n}_k$ on particle 2.

Now, according to quantum mechanics

$$\langle\boldsymbol{\sigma}(1)\cdot\mathbf{n}_j\boldsymbol{\sigma}(1)\cdot\mathbf{n}_k\rangle=\mathbf{n}_j\cdot\mathbf{n}_k \tag{65}$$

so that

$$\mathbf{n}_1\cdot\mathbf{n}_2+\mathbf{n}_2\cdot\mathbf{n}_3+\mathbf{n}_3\cdot\mathbf{n}_1 \geqslant -1 \tag{66}$$

and

$$(\mathbf{n}_1+\mathbf{n}_2+\mathbf{n}_3)^2=3+2(\mathbf{n}_1\cdot\mathbf{n}_2+\mathbf{n}_2\cdot\mathbf{n}_3+\mathbf{n}_3\cdot\mathbf{n}_1) \geqslant 1, \tag{67}$$

an inequality which is *not* satisfied, for example, by three coplanar unit vectors each of which forms with each of the others an angle of 120°.

It follows that the EPR reality criterion, at least in its RF version, is incompatible with the statistical predictions of quantum mechanics, a conclusion which is much stronger than that obtained by Einstein, Podolsky, and Rosen. Sharp's concluding remark "whether or not...it could be shown that quantum mechanics itself somehow contained the correct criterion for the reality of a physical quantity...is at present unknown" has consequently to be qualified in the sense that it is known that quantum mechanics itself excludes certain almost self-evident criteria of reality. If one believes in the validity of quantum mechanics and its statistical predictions, one has to reject the EPR criterion of reality, at least in the RF version, or one may call into question, as Sharp has done, the very possibility of ever separating two particles to such an extent that a measurement can be made on one of them without disturbing the other; but then one must admit instantaneous actions which, without diminution with distance, could act in an appropriate Lorentz frame "backwards in time" as conjectured in this context by Costa de Beauregard and others. Of course, one may also embrace Bohr's complementarity interpretation which, though depurated by these developments, survived them without serious debilitation.

6.8. THE ACCEPTANCE OF THE COMPLEMENTARITY INTERPRETATION

Let us conclude this lengthy chapter with some historical remarks on the acceptance of Bohr's complementarity interpretation. In spite of the opposition to Bohr's views by some leading physicists like Einstein and Schrödinger the vast majority of physicists accepted the complementarity interpretation in general without reservations, at least during the first two decades after its inception. Impressed by the spectacular successes of quantum mechanics in all fields of microphysics they were interested primarily in its applications to practical problems and in its extensions to unexplored regions.

These pragmatic tendencies in research also had their effect on academic instruction: most textbooks concentrated on teaching how to solve problems and paid little attention to the meaning of the concepts involved. The need of acquiring new mathematical techniques left little room for philosophical analysis. Usually at the end of a discussion of the Heisenberg relations a statement was added to the effect that the logical relationship between the concepts of position and momentum is called complementarity and in a footnote reference was made to some of Bohr's writings. In any case, all texts written between 1930 and 1950—and this includes the books by Landé, de Broglie, and Bohm, who later all turned against the Copenhagen view—espoused the complementarity principle even if they did not name it. In fact, it would be difficult to find a textbook of that period which denied that "the numerical value of a physical quantity has no meaning whatsoever until an observation upon it is performed.[169]

In prewar Soviet Russia physical thought was predominantly influenced by the teachings of Bohr and Heisenberg, for most of Russia's leading physicists of that time had studied at the European centers of research where complementarity was the official philosophy. There was even a group of physicists, including M. P. Bronshtein, V. A. Fock, L. D. Landau, and I. E. Tamm, who became known as the "Russian branch" of the Copenhagen school.

True, some criticisms had already been voiced against Bohr already at that time. Ernst Mach's neopositivism, it will be recalled, was severely attacked in Lenin's *Materialism and Empirio-Criticism* (1909) as irreconcilable with dialectical materialism, which recognizes the existence of matter and of its physical properties as independent of, and separate from, the mind. A statement such as the one just quoted from Persico's book, for example, seemed incompatible with Marxist philosophy. The earliest attack along these lines within a physics journal proper was probably Konstantin Vjatseslavovits Nikolskii's 1936 paper "The Principles of Quantum Mechanics,"[170] which condemned complementarity as an idealistic misrepresentation of physical facts. Such criticisms, however, did not impress Fock, who defended the principle of complementarity on many occasions and most eloquently perhaps in his 1949 essay "The Fundamental Laws of Physics from the Viewpoint of Dialectical Materialism."[171] Nor did they discourage Nikolaij Serjevits Krylov from basing on this principle his *Studies in the Foundations of Statistical Physics*[172] or Lev Davidovich Landau and Elya Mikhailovich Lifshits from sympathizing with it in their

[169]E. Persico, *Fundamentals of Quantum Mechanics* (Prentice-Hall, Englewood Cliffs, N. J., 1950, 1957), p. 314.

[170]K. V. Nikolskii, "Printsipy kvantovoj mekhaniki," *Uspekhi Fisičeskikh Nauk* **16**, 537–565 (1936).

[171]V. A. Fock, "Osnovnye zakony fiziki v svete dialekticheskovo materializma," *Vestnik Leningradskovo Universiteta* **4**, 39–47 (1949).

[172]N. S. Krylov, *Raboty po Obosnovaniju Statističeskoj Fiziki* (USSR Academy of Sciences, Moscow, Leningrad, 1950).

well-known textbook[173] on quantum mechanics, published in 1948. Even the Ukrainian philosopher of science M. E. Omeljanovskii[174] who later became the acknowledged representative of Soviet philosophy of quantum mechanics, accepted in his early writings Bohr's principle of complementarity although he regarded it as the expression of an objectively existing duality in nature rather than merely an epistemological precept.

The official ideological campaign against complementarity was inaugurated with A. A. Zdanov's famous speech,[175] on June 24, 1947, in which the bourgeois influence on Soviet science was vigorously condemned. In fact, when in 1947 Moisey Aleksandrovich Markov, a research physicist at the Physics Institute of the USSR Academy, published an article[176] in which he sided with Bohr in the EPR debate he was severely reproached by Maksimov.[177] Aleksandr Aleksandrovich Maksimov, a philosopher who from 1922 had been a member of the editorial board of *Pod Znamenem Marksizma*, [*Under the Banner of Marxism*] an influential philosophy journal which ceased to appear in 1944, accused Markov of denying the existence of micro-objects between observations and of identifying "the physical reality of the micro-world" with the state of the macroscopic measuring instrument. As a result of this condemnation B. M. Kedrov, the chief editor of the *Voprosy Filosofii* in which Markov's paper was published, had to resign from his position and five of his colleagues were dismissed from the editorial board, among them Omeljanovskii. The 1952 edition of the semiofficial book *Philosophical Problems of Modern Physics*[178] severely censured the Copenhagen interpretation because its

[173]L. Landau and E. Lifshits, *Kvantovaja Mekhanika* (G.I.T.T.L., Moscow, Leningrad, 1948); *Quantum Mechanics* (Pergamon Press, Oxford; Addison-Wesley, Reading, Mass., 1958). Note, however, that the term "complementarity" is never used in this book.

[174]M. E. Omeljanovskii, *V. I. Lenin i Fizika XX Veka* [V. I. Lenin and the Physics of the 20th Century] (USSR Academy of Sciences, Moscow, 1947); *Filosofskie Problemy Kvantovoj Mekhaniki* [Philosophical Problems of Quantum Mechanics] (USSR Academy of Sciences, Moscow, 1956); *Philosophische Probleme der Quantenmechanik* (Deutscher Verlag der Wissenschaften, Berlin, 1962).

[175]A. A. Zdanov, "Vystuplenie na diskussii o knige G. F. Aleksandrova 'Bol'shevik'" (Speech at the discussion on G. F. Aleksandrov's book *The Bolshevik*), 1947. For details cf. L. R. Graham, "Quantum mechanics and dialectical materialism," *Slavic Review* **25**, 381–410 (1966); *Science and Philosophy in the Soviet Union* (Knopf, New York, 1972).

[176]M. A. Markov, "O prirode fizicheskogo znanija," [On the nature of physical knowledge], *Voprosy Filosofii* **1947**, 140–176.

[177]A. A. Maksimov, "Ob odnoi filosofikom Kentavre," [Concerning a philosophic centaur], *Literaturnaia Gazeta* **1948** (April 10), 3.

[178]*Filosofskie Voprosy Sovremenoj Fiziki*, A. A. Maksimov et al., eds. (USSR Academy of Sciences, Moscow, 1952).

doctrine of the importance of the role played by the observer was condemned as being absolutely incompatible with the two tenets of dialectical materialism, that nature exists independently of observation and is inexhaustible in the sense that every existing theory about nature is only a temporary stage in a never-ending progress in knowledge.

Papers written by Bohr's opponents like de Broglie, Vigier, Lochak, and Bohm now gained prominence in Soviet journals. In particular, the "theory of levels" in which Vigier and Bohm proposed their thesis of the qualitative inexhaustibility of matter was favorably accepted in official quarters.

However, after Fock's visit in 1957 to Bohr in Copenhagen and the subsequent publication in the *Uspekhi* of Bohr's paper "Quantum Physics and Philosophy"[179] the opposition against Bohr abated considerably.[180] Fock's comments on this paper in which he claimed that Bohr "considerably approaches the materialistic treatment of the fundamental principles of quantum mechanics" by stressing "the objectivity of the quantum-mechanical description and its independence of the knowing subject" paved the way for a *rapprochement* of Soviet philosophy of physics with the ideas of complementarity. The enthusiasm with which Bohr was received during his visit to the Soviet Union in May 1961 and the homage paid to him in the 1963 special issue of the *Uspekhi* dedicated to his memory were clear indications that Bohr's views ceased to be the object of ideological persecution in Russia.

In the early 1950s the almost unchallenged monocracy of the Copenhagen school in the philosophy of quantum mechanics began to be disputed in the West. The previous lack of widespread criticism in this field was explained in some quarters as the result of a somewhat dictatorial imposition of what was called "the Copenhagen dogma" or "orthodox view" upon the younger generation of physicists. The appearance in 1949 of the often quoted Einstein volume edited by Schilpp which contained Bohr's debate with Einstein, Einstein's self-written "obituary," and his candid "reply to criticisms" and which was widely read by philosophizing physicists contributed considerably to the creation of a more critical atmosphere toward the complementarity philosophy. "Heterodox" interpretations, though sometimes highly speculative or tentative and incomplete, were readily seized as alternatives to the authorized view. The extent to which this process was fomented and supported by social-cultural movements

[179]Ref. 4-52.

[180]Cf. S. Müller-Markus, "Niels Bohr in the darkness and light of Soviet philosophy," *Inquiry* 9, 73–93 (1966). Cf. also Edwin Levy, "The de Broglie program and Soviet dialectical materialism" (unpublished, University of British Columbia, Vancouver) and his other writings (unpublished).

and political factors such as the growing interest in Marxist ideology in the West deserves to be investigated just as diligently as the influence of the "Weimar culture"[181] on early quantum theory has recently been studied. The decisive factor, however, was probably the psychological factor which will be described in the opening statements of our chapter on hidden-variable interpretations, interpretations which supported, and were supported by, the development just described.

[181] P. Forman, "Weimar culture, causality, and quantum theory, 1918–1927" in *Historical Studies in the Physical Sciences*, R. McCormmach, ed. (University of Pennsylvania Press, Philadelphia, 1971), Vol. 3, pp. 1–115.

HIDDEN VARIABLE Theories

Chapter Seven

7.1. MOTIVATIONS FOR HIDDEN VARIABLES

Theoretical physicists, however revolutionary in their views professed *ex cathedra*, were brought up and live in a classical world. It has been claimed that even the most "progressive" theoretician believes at the bottom of his heart in a strictly deterministic, objective world even if his teachings categorically deny such a view. Whether this claim is true is a problem not of physics but rather of the psychology of human behavior. It explains, however, why some physicists rejected the prevailing probabilistic interpretation of quantum mechanics and tried to demonstrate that the existing theory in spite of its spectacular success is only a provisional approximation to a deeper scientific truth.

Quantum mechanics was too successful to be dismissed as *completely* false. Moreover, not the slightest idea of a radically different theory that could compete with it in microphysics was in sight. It was therefore natural to "refine" the conventional scheme and to regard ordinary quantum theory as a kind of statistical mechanics which yields only average values of measured quantities while at a more profound—but for the time being empirically inaccessible—level each individual system should be regarded as performing its motion in accordance with strictly deterministic laws.

Whereas classical particle mechanics, whether in the Newtonian, Lagrangian, or Hamiltonian formulation, as a theory about the behavior of individual systems logically as well as historically preceded its generalization to ensembles of systems—that is, classical statistical mechanics—the situation in quantum physics was the reverse: one had to construct a theory to explain the behavior of individual systems from the statistics of their ensembles, a task obviously much more complicated than its reverse.

Such a theory, it was hoped, would not only restore determinism and causality to the realm of microphysics, it would also dispense with the peculiar dichotomy of physics into classical and quantum phenomena and re-establish a unitary account of the physical world, a prospect of sometimes[1] greater incitement than the desire for determinism. A third, more

[1] "It is this possibility, of a homogeneous account of the world, which is for me the chief motivation of the study of the so-called 'hidden-variable' possibility." J. S. Bell, "Introduction to the hidden-variable question," in *Foundations of Quantum Mechanics* (International School of Physics "Enrico Fermi"—Course 49), B. D'Espagnat, ed. (Academic Press, New York, London, 1971), pp. 171–181, quotation on p. 172. For an example of how the desire to retain determinism motivates the belief in hidden variables the reader is referred to C. W. Rietdijk's unconventional critique of the fundamental principles of quantum mechanics *On Waves, Particles and Hidden Variables* (Van Gorcum, Assen, 1971), which concludes with the statement (p. 130): "Therefore, our only hope of survival, in the deepest sense of the word, the only hope of the truly religious man, has to be set on determinism, on hidden variables."

specific motivation to search for such a "completion" of the theory was the problem raised by the Einstein-Podolsky-Rosen argument. The correlation between the two measurement results obtained at separated locations suggested that these results were actually determined in advance, when the two systems still interacted with each other, by certain dynamical variables also correlating the states of the systems after their separation. If these variables, though hidden from our sight and beyond our control, thus correlate the states, it could be understood that the outcome of one measurement makes it possible to predict that of the other without the need of assuming that the very performance of the first measurement influences causally the outcome of the second.

Although the Einstein-Podolsky-Rosen incompleteness argument was undoubtedly one of the major incentives for the modern development of hidden variable theories, it would be misleading to regard Einstein, as some recent authors do,[2] as a proponent of or even as "the most profound advocate of hidden variables." True, Einstein was sympathetically inclined toward any efforts to explore alternatives, and as such also the ideas of de Broglie and of Bohm, but he never endorsed any hidden variable theory.[3] What he was groping for was a more radical departure from the conventional approach, somewhat analogous to the way in which his theory of general relativity supplanted Newton's theory of gravitation.[4] In fact, oral tradition, transmitted by Podolsky to Eugene Guth and others, ascribes to Einstein the belief that future research would reveal significant discrepan-

[2]A. Shimony, "Experimental test of local hidden variable theories," Ref. 1 (Academic Press), pp. 182–194.

[3]Referring to Bohm's theory of hidden variables Einstein wrote in 1953: "*Ich glaube aber nicht, dass diese Theorie sich halten lässt*" (letter from Einstein to M. Renninger, dated May 3, 1953). A few months later, again with reference to Bohm's ideas, he wrote to Aron Kupperman of Notre Dame University: "*Ich denke, dass man zu einer Beschreibung der individuellen Systeme überhaupt nicht durch blosse Ergänzung der gegenwärten statistischen Quantentheorie gelangen kann.*" One of the sources of erroneously listing Einstein among the proponents of hidden variables was probably J. S. Bell's widely read paper "On the Einstein Podolsky Rosen paradox," *Physics* **1**, 195–200 (1964), which opened with the statement: "The paradox...was advanced as an argument that quantum mechanics...should be supplemented by additional variables." It was obviously under Bell's influence that J. F. Clauser, M. A. Horne, A. Shimony, and R. A. Holt wrote in their paper "Proposed experiment to test local hidden-variable theories," *Physical Review Letters* **23**, 880–884 (1969): "Einstein, Podolsky and Rosen concluded that the quantum mechanical description of a physical system should be supplemented by postulating the existence of 'hidden variables,' the specification of which would predetermine the result of measuring any observable of the system." Einstein's remarks in his "Reply to Criticisms" (Ref. 4-9, p. 672), quoted by Bell in support of his thesis, are certainly no confession of the belief in the necessity of hidden variables.

[4]Cf. on this point M. Sachs, "Comment on 'Alternative to the orthodox interpretation of quantum theory,'" *American Journal of Physics* **36**, 463–464 (1968).

cies between quantum mechanics and experience. Whenever he ignored his more ambitious hope that a future unified field theory would also cover quantum phenomena, he seemed to have favored the statistical ensemble interpretation, which is neutral on the issue of hidden variables: it admits them as possible, but it does not demand them as necessary ingredients of the theory.

No doubt, Einstein's criticisms, and in particular his work with Podolsky and Rosen, greatly contributed to the development of hidden variable theories, just as Mach's ideas contributed to the rise of Einstein's relativity; but, as is not uncommon in the history of physics, the intellectual originator of a theory does not necessarily identify himself with its full-fledged development.

The hypothetical quantities introduced to "refine" or "modify" the theory were usually called "hidden parameters" and later "hidden variables" and the "refinements" or "modifications" were called "hidden variable interpretations" or "hidden variable theories." We prefer the former term if the usual formalism of quantum mechanics is retained and the latter term if it is modified, thus leading to a new theory. During the past decades much has been written about hidden variables, but it is hard to find in the literature a generally accepted definition of this notion. In fact, views on their nature seemed to differ considerably. David Bohm,[5] for instance, in an essay on such theories, characterized them as "a further set of variables, describing the state of new kinds of entities existing in a deeper subquantum mechanical level and obeying qualitatively new types of individual laws." He then added that such variables, though at present "hidden," may "be revealed in detail when we will have discovered still other kinds of experiments, which may be as different from those of the current type as the latter are from experiments that are able to reveal the laws of the large-scale level."

According to Peter Mittelstaedt,[6] on the other hand, hidden variables

[5]D. Bohm, "Hidden variables in the quantum theory," in *Quantum Theory*, D. R. Bates, ed. (Academic Press, New York, London, 1962), Vol. 3, p. 348.

[6]"*Da bei nicht klassischen Theorien die operativ-apriorische Grundlage nicht kompatibel ist mit der Forderung empirischer Realisierbarkeit der Theorie, bieten sich zwei mögliche Wege an, eine nichtklassische Theorie in befriedigender Weise zu formulieren: Entweder man verzichtet auf die operativ-apriorische Grundlegung oder man verzichtet auf die durchgängige empirische Realisierbarkeit der Theorie. Beide Möglichkeiten sind diskutiert worden. Die erste führt auf Theorien beobachtbarer Grössen, die zweite auf Theorien mit verborgenen Parametern.*" P. Mittelstaedt, "Verborgene Parameter und beobachtbare Grössen in physikalischen Theorien," *Philosophia Naturalis* **10**, 468–482 (1968); reprinted in P. Mittelstadt, *Die Sprache der Physik* (Bibliographisches Institut, Mannheim, Vienna, Zurich, 1972), pp. 33–50. Cf. also P. Mittelstaedt, *Klassische Mechanik* (B.I., Mannheim, 1970), pp. 13–18.

characterize a theory in the formulation of which one "dispenses with a pervasive realizability of the theory." The prescientific operative measurement procedures needed to define the fundamental notions of the theory, he argued, may or may not be consistent with the measurement prescriptions as derived from the body of the theory. In other words, not every theory necessarily satisfies what C. F. von Weizsäcker[7] called "the principle of semantic consistency," the requirement that "the rules by which we describe and guide our measurement, defining the semantics of the formalism of a theory, must be in accordance with the laws of the theory." If the principle is satisfied, the theory possesses self-consistency (as in the case of Newtonian mechanics) and it is called by Mittelstaedt a *classical* theory; otherwise it is called *nonclassical*. It is a historical fact that all physical theories which are called "classical" in the usual sense of the word are also *classical* in Mittelstaedt's terminology. In the case of a nonclassical theory, that is, when the operative a priori foundation of the theory is incompatible with the requirement of its empirical realizability, two possibilities exist: (1) either the operative a priori foundation, or (2) the requirement of empirical realizability has to be given up. The first possibility leads to theories of observable quantities (e.g., special relativity), the second to theories of hidden variables. In the latter the originally operatively defined fundamental concepts play the role of hidden variables, which, although employed in formulating the theorems of the theory,[11] *are by their very definition unobservable*."[8] Obviously, Mittelstaedt's conception of hidden variables differs considerably from Bohm's. Many other views on the nature, function, and purpose of hidden variables could be cited. The development of hidden variable theories (interpretations) suffered seriously from such equivocations.

To avoid ambiguities we begin our historical survey with a general definition of hidden variables, which will be followed by a more technical definition with special reference to quantum mechanics.

Definition I. In a given theory T about certain physical systems S certain variables v describe the states of S; in a theory T' about S certain variables v' (which may be dynamical quantities or other hypothetical

[7]C. F. von Weizsäcker, "The Copenhagen interpretation," in *Quantum Theory and Beyond*, T. Bastin, ed. (Cambridge University Press, Cambridge, 1971), pp. 25–31; *Die Einheit der Natur* (C. Hanser Verlag, Munich, 1971), pp. 231–232.

[8]"Verborgene Parameter sind auf Grund der Theorie, zu deren Formulierung sie definiert wurden, in keinem möglichen Experiment beobachtbar." *Op. cit.* p. 478. Cf. also P. Mittelstaedt, *Philosophische Probleme der Modernen Physik*, 3rd ed., (Bibliographisches Institut, Mannheim, 1968), p. 13.

entities) which are not experimentally detectable within the framework of T describe the states of S; if the values of v, or of explicitly defined functions (or functionals) associated with v as used in the state description in T, can be obtained by some averaging operation over the values of v', v' are called hidden variables (with respect to T) and T' is called a hidden variable theory (interpretation) (with respect to T).

Note that this definition does not stipulate that the embedding of T in T' entails the transformation of a statistical or probabilistic theory into a deterministic or causal theory.

7.2. HIDDEN VARIABLES PRIOR TO QUANTUM MECHANICS

The idea of hidden variables is as old as physical thought. It was applied in man's early attempts to explain the visible world in terms of a postulated invisible world. In fact, Empedocles' doctrine of the four elements, Anaxagoras' theory of matter with its principles of homoeomereity, universal mixture, and predominance, and—most important (for until the invention of the cloud chamber by C. T. R. Wilson in 1911 or of the scintillation counter by Sir William Crookes in 1903 but effectively used first by Ernest Rutherford atoms *were* hidden variables) the atomic theories of Plato, of Leucippus, and Democritus were all such theories T' in which the theory T of ordinary bodies was embedded. The averaging operation was perhaps not always strictly defined in mathematical terms, but the lack of quantitative determinations was characteristic of ancient physics in general. But was not Anaxagoras' theory of "universal mixture" according to which "in everything there is a portion of everything"[9] essentially a hidden variable theory dealing with ensembles that are never dispersion-free?

The earliest theory of hidden variables which by their very definition are undetectable by direct observation and which, moreover, were characterized by mathematical (geometric) properties was probably the theory of vision in terms of "optical rays" (radii) proposed by Archytas of Tarentum in the first half of the fourth century BC. Refining by geometrization the Pythagorean doctrine of vaporous effusions proceeding from the eye toward the visually perceived object, this theory was later adopted by the majority of Greek mathematicians who studied optics, such as Euclid, Hero, Hipparchus, Cleomedes, and Ptolemy. Since, according to Archytas,[10] the rectilinear rays (forming a cone with its vertex in the eye) are

[9]Simplicius, *In Aristotelis Physicorum Commentaria*, H. Diels, ed. (Reimer, Berlin, 1882), p. 164, line 23.

[10]Cf. Lucius Apuleius of Mădaura, *Apologia*, Chapter 15.

constitutive elements of the very process of vision, they can never be "seen"; nor can they, for that matter, be perceived otherwise for they are too tenuous for the other senses and do not interact with each other. They are, by definition, unobservable variables in a theory of observation!

The strictly deterministic atomism of Democritus, as is well known, was modified by Epicurus into an indeterministic theory of motion. According to Epicurus all atoms, though of unequal weight, fall through the void with equal speed. Since such atoms could never collide to form compounds so that "nature would never have created anything,"[11] Epicurus postulated the existence of sudden "swerves" [*clinamen*]: "When the atoms are travelling straight down through empty space by their own weight, at quite indeterminate times and places [*incerto tempore incertisque locis*] they swerve ever so little from their course, just so much that you can call it a change of direction."[12] Epicurus embedded the orthodox deterministic theory T in a more general indeterministic theory T', describing acausally the swerve. But now, the Stoics, it should be recalled, contended that "every event has a forerunner, the cause upon which it depends."[13] For them such an indeterministic description seemed unacceptable. They thought it necessary (Chrysippus) to postulate hidden causes in all cases where the equilibrium is perturbed without any external symptom of a cause being perceptible: "For there is no such thing as lack of cause or spontaneity; in the so-called accidental impulses which some have invented, there are causes hidden from our sight [*aitias adēlous, causas obscuras*] which determine the impulse in a definite direction."[14] Chrysippus thus embedded the Epicurean indeterministic (and hence hidden variable) atomic theory T' in a hidden-variable theory T'' by postulating "hidden causes."

Another interesting hidden variable theory is Abu'l Hudhayl's modification of Abu Ishaq Ibrahim Al-Nazzam's theory of the "leap" [*tafrah*]. Al-Nazzam[15] (775–846) attempted in his treatise on motion, *Kitāb fi al-haraka*, to solve Zeno's problems by advancing the idea that a body could move from a point A to a point C in space without traversing the intermediary point B. In contrast to the atomistic conception of the Kalam

[11]Carus Lucretius, *De rerum natura*, Book 2, lines 217–220.

[12]*Ibid.*

[13]Alexander of Aphrodisias, *De fato*, p. 192, line 6.

[14]Plutarch, *De Stoicorum Repugnantis*, line 1045c.

[15]For a general discussion of Al-Nazzam's philosophy of nature see J. van Essen, "Dirār b.'Amr und die 'Cahmīya'," *Der Islam* **43**, 241–279 (1967); **44**, 1-70, 318–320 (1968). Cf. also Abu Ridah, *Ibrāhīm 'ibn Saiyar al-Nazzām wa-ārā'uhu alkalāmiyah al-falsafīyah* (in Arabic) (Lagnat al-talif, Cairo, 1946).

on space, time, and existence, Al-Nazzam maintained that a body in motion, rather than annihilating itself and reappearing again, performs, so to say, a leap: "The mobile may occupy a certain place and then proceed to the third place without passing through the intermediate second place on the fashion of a leap."[16] In support of his idea he referred to certain skip phenomena reminiscent of the "rota Aristotelis" which still intrigued Galileo. In short, Al-Nazzam's kinematics reduced the apparently continuous motion of macroscopic bodies to a sequence of microscopic processes which by their very definition defy any exhaustive space-time description. In fact, Al-Nazzam's notion of leap, his designation of an unanalyzable interphenomenon, may be regarded as an early forerunner of Bohr's conception of quantum jumps. Abu'l Hudhayl (751–849), Al-Nazzam's uncle, attempted to reconcile his nephew's theory of discrete leaps with the Aristotelian doctrine of continuous motion by postulating the existence of "hidden states" [*waqafāt hafiyah*]. Here a discrete theory T of motion was embedded in a continuous theory T' of motion by the adjunction of hidden states.

It would lead us too far astray to show in detail that many of the notorious medieval doctrines of occult qualities, many theories in alchemy, the phlogiston theory at the dawn of modern chemistry, as well as the numerous so-called kinetic theories of gravitation in the eighteenth and nineteenth centuries were but hidden variable theories.[17] Better known as such are Maxwell's theories of the ether and Hertz' theory of hidden coordinates [*verborgene Koordinaten*], which specify the "hidden motions" [*verborgene Bewegungen*] of "hidden masses" [*verborgene Massen*], in terms of which Hertz tried to explain how bodies, though not in direct contact with each other, could mutually affect each other. J. J. Thomson's early work interpreting potential energy as a kind of kinetic energy of cyclic coordinates is another example. In general, whenever the attempt was made to reduce action-at-distance to contiguous action hidden variables

[16]Abul-Hasan Al-Ash'ari, *Kitāb Maqālāt al-Islāmiyyin* (in Arabic), H. Ritter, ed. (Istanbul, 1929–1930), p. 321.

[17]For details cf. M. Jammer, *Concepts of Force* (Harvard University Press, Cambridge, Mass., 1957; Harper and Brothers, New York, 1962), pp. 188–199; *Storia del' Concetto di Forza* (Feltrinelli, Milan, 1971), pp. 201–212. That also in certain modern Lorentz-invariant theories of gravitation, such as those described by W. E. Thirring in his papers "Lorentz-invariante Gravitationstheorien," *Fortschritte der Physik* **7**, 79–101 (1959), "An alternative approach to the theory of gravitation," *Annals of Physics* **16**, 96–117 (1961), the pseudo-euclidean intervals between events have to be regarded as hidden variables, at least in the sense defined by P. Mittelstaedt, has been pointed out by P. Mittelstaedt in his essay "Die Sprache der Physik" in *Quanten und Felder* (Festschrift für Werner Heisenberg), H. P. Dürr, ed. (F. Vieweg und Sohn, Braunschweig, 1971), pp. 27–51; reprinted in P. Mittelstaedt, *Die Sprache der Physik* (Bibliographisches Institut, Mannheim, Vienna, Zürich, 1972), pp. 84–115.

suggested themselves as a natural solution to the problem.

In quantum mechanics, as we shall see, the effect of introducing hidden variables seemed to lead into the opposite direction: The insistence on hidden variables seemed to entail the renunciation of locality.

Not only the concept of hidden variables, but also the attempt to disprove their possibility can be traced back to the dawn of scientific thought. Aristotle,[18] the foremost opponent of atomism in antiquity, was eager to recount time and again that the doctrine of atomism is logically untenable and methodologically unnecessary, the former because the idea of indivisible magnitudes is beset with internal contradictions, violating the principles of mathematics and of motion, its necessary presupposition of the existence of empty space being physically inadequate, the latter because its explanations of physical processes such as condensation, rarefaction, and chemical change are superficial and abortive. Equally well known are Zeno's arguments against the existence of atoms, although it may not be quite clear precisely against whom his famous paradoxes were directed.

Another interesting logical refutation of atoms qua hidden variables may be found in the writings of Caelius Firmianus Lactantius, an early Christian apologetic (c. 260–340) who was a pupil of Arnobius and tutor of Crispus, the sun of Constantine the Great. In his *De Ira Dei*[19] Lactantius tried to disprove the existence of hypothetical hidden atomic entities by the following *reductio ad impossibile*. "If atoms were round or smooth," he said, "they surely could not cohere and form a body... ; if alternatively they were rough and furnished with corners or hooks so as to cling together, they could certainly be divided and cut... If the invisible is composed of invisible parts, the visible must consequently consist of visible parts. Why, then, does nobody see them?"[20] That his arguments, and especially the latter connecting microscopic entities with macroscopic bodies, were taken seriously is shown by the fact that Peter Gassendi (1592–1665),[21] the innovator of modern atomism, thought it necessary to

[18]Aristotle, *Physics* line 231a, *De Caelo*, lines 303a *et seq.*

[19]Lactantius, *De Ira Dei ad Donatum liber unus*, Opera Omnia, 1786. Cf. the recent edition by H. Kraft and A. Wlosok (Latin and German) (Wissenschaftliche Buchgesellschaft, Darmstadt, 1957).

[20]"Si [illa semina] levia sunt et rotunda, utique non possunt invicem se adprehendere, ut aliquod corpus efficiant, utsi quis milium velit in unam coagmentationem constringere, lenitudo ipsa granorum in massam coire non sinat. Sin aspera et angulata sunt et hamata, ut possint cohaerescere, dividua ergo et secabilia sunt.... Si ex invisibilibus sunt quae non videntur, consequens est ut ex visibilibus sint quae videntur. Cur igitur nemo videt?" *Op.cit.* (1957), p. 26, p. 30.

[21]P. Gassendi, *Animadversiones in decimum librum Diogenis Laertii* (1649) Cf. also K. Marwan, *Die Wiederaufnahme der griechischen Atomistik durch Pierre Gassendi* (Kirsch, Beuthen, 1935)

refute them one by one. Many more examples could be cited—apart from the numerous impossibility proofs, such as those formulated by Dionysius of Alexandria in his *Peri Physeos* or by Aurelius Augustinus in his *Epistola ad Dioscorum* or *Epistola ad Nebridium*, which applied theological or teleological arguments.

The classical precedent for the controversy about hidden variables in quantum mechanics was, of course, the dispute that was fought out in the latter part of the nineteenth century between the proponents of a mechanistic kinetic interpretation of phenomenological thermodynamics, led by Ludwig Boltzmann, and their opponents, either on technical grounds, such as those advanced by Joseph Loschmidt or Ernst F. F. Zermelo, or on philosophical grounds, such as those brought forth by Ernst Mach. Loschmidt's *reversibility objection*[22] (1876) and Zermelo's *periodicity objection*[23] were attempts to refute the possibility of interpreting the laws of thermodynamics in terms of the invisible motions of molecules as postulated in the kinetic theory of gases or in statistical mechanics. The modern claim that hidden variables, since experimentally not vindicable, are metaphysical and scientifically useless embroideries had its analogy in Mach's[24] opposition to Boltzmann's ideas, on the grounds of simplicity and postulational economy. In fact, more than a formal analogy is involved; for the philosophy of the logical positivism with which Mach is identified was, as we have seen, quite important for the formation of the orthodox interpretation of quantum mechanics and its rejection of hidden variables. The fact that modern physicists almost unanimously follow Boltzmann and not Mach, primarily because of the striking experimental confirmations of the atomic theory since 1890, cannot serve as a logical argument in support of hidden variables for quantum theory.

7.3. EARLY HIDDEN VARIABLE THEORIES IN QUANTUM MECHANICS

Before turning to the history of hidden variables in quantum mechanics let us redefine the notion of hidden variables with special regard to quantum

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and B. Rochot, *Les Travaux de Gassendi* (J. Vrin, Paris, 1944).

[22]J. Loschmidt, "Über den Zustand des Wärmegleichgewichtes eines Systems von Körpern mit Rücksicht auf die Schwere," *Wiener Berichte* **73**, 128–142 (1876), **75**, 287–299 (1877), **76**, 204–225 (1877).

[23]E. Zermelo, "Über einen Satz der Dynamik und die mechanische Wärmetheorie," *Annalen der Physik* **57**, 485–494 (1896); trans. "On a theorem of dynamics and the mechanical theory of heat," in *Kinetic Theory*, S. G. Brush, ed. (Pergamon Press, Oxford, 1966), Vol. 2, pp. 208–217.

[24]E. Mach, *Die Prinzipien der Wärmelehre* (J. A. Barth, Leipzig, 1896).

mechanics. The following definition, though never explicitly formulated during the early stages of the development of hidden variable theories in quantum mechanics and hence anachronistic at this stage, is presented here primarily for the purpose of introducing certain terms whose use will make our exposition more accurate and concise.

Definition II. (1) Each individual quantum system described by the usual state function ψ is characterized by additional hidden states labeled by a parameter λ; the totality of all hidden states is the phase space Γ of hidden states; ψ and λ determine the result of measuring any observable on the system. (2) Each state function ψ is associated with a probability measure $\rho_\psi(\Lambda)$ on Γ such that if Λ is a measurable subset of Γ then $\rho_\psi(\Lambda)$ is the probability that the state, defined by ψ and λ, lies in Λ. (3) Each observable $\mathcal{A}$, represented by the self-adjoint operator A, is associated with a single-valued real-valued function $f_A:\Gamma\to R$ which maps Γ into the set R of all reals. (4) If M is a measurable subset of R and $\mu_\psi{}^A$ is the quantum mechanical probability measure such that $\mu_\psi{}^A(M)$ is the probability that the value of $\mathcal{A}$ lies in M, then

$$\mu_\psi{}^A(M)=\rho_\psi[f_A^{-1}(M)]$$

or equivalently

$$\langle A\rangle_\psi=\int_\Gamma f_A(\lambda)\,d\rho_\psi(\lambda).$$

In the case of discrete and nondegenerate spectra the last two conditions can be reformulated as follows: (3) Each observable $\mathcal{A}$, represented by the self-adjoint operator A, is associated with a single-valued, real-valued function $f_A(\lambda)$ that maps Γ onto the set of the eigenvalues of A. (4) If ψ_n is an eigenvector of A and a_n the corresponding eigenvalue, and if Γ_{a_n} is the inverse image $f^{-1}(a_n)$ of a_n in Γ, that is, $\Gamma a_n=\{\lambda|f_A(\lambda)=a_n\}$, then for all ψ

$$\int_{\Gamma_{a_n}} d\lambda\rho_\psi(\lambda)=|(\psi_n,\psi)|^2$$

where $\rho_\psi(\lambda)$ is the probability density associated with ψ according to (2). The parameters λ (or sometimes even the states characterized by them) are called (noncontextual) hidden variables; and a space Γ, a set of probability measures ρ, and a set of functions f satisfying the preceding conditions (1) to (4) is called a hidden variable theory or interpretation. If we knew the value of λ for an individual system in state ψ, we could predict with certainty the result of any measurement on it; and if we knew the probability distribution of the hidden variables, we could recover the statistics of quantum mechanics.

Whereas (1) and (2) in the preceding definition postulate the existence of dispersion-free hidden states and probability densities, (3) and (4), by qualifying (1) and (2), assure the reproduction of the statistical predictions of ordinary quantum mechanics. The qualification "noncontextual" (or "noncontextualistic") emphasizes that the result $f_A(\lambda)$ does not depend on what other observables, compatible with $\mathscr{A}$, are measured simultaneously with $\mathscr{A}$ on the same system. Since the early proponents of hidden variables in quantum mechanics always conceived (or tacitly assumed) this noncontextuality or independence of the surrounding experimental setup we shall omit this qualification in our discussion until an explicit reference to it is required. Strictly speaking, the same applies to another qualification, the "local" or "nonlocal" character of hidden variables: If the result $f_A(\lambda)$ of measuring $\mathscr{A}$ on a system S_1 in state ψ, λ does not depend on the kind of measurement (or its outcome) performed on a second system S_2, spatially separated from S_1, the hidden variables are called "local"; otherwise they are called "nonlocal." Again, in the early stages of the theory all hidden variables were tacitly assumed to be "local."

It is clear that an interpretation based on hidden variables as defined in Definition II is *eo ipso* a hidden variable extension T' of the ordinary quantum theory T also in the sense of Definition I.

With these definitions in mind let us now turn to the history of hidden variable theories in quantum mechanics. The earliest theories of this kind were designed to embed quantum mechanics in a strictly deterministic theory. In fact, in the very paper in which Max Born[25] proposed his probabilistic interpretation of the ψ-function according to which "the motion of particles conforms to the laws of probability" and in which only a statistical meaning was ascribed to ψ, the possibility of hidden variables was not ignored. Said Born: "But, of course, anybody dissatisfied with these ideas may feel free to assume that there are additional parameters not yet introduced into the theory which determine the individual event. In classical mechanics these are the 'phases' of the motion, such as the coordinates of the particles at a certain moment. I thought it at first unlikely that quantities corresponding to these phases could be reasonably incorporated into the new theory; but Mr. Frenkel informed me that this may still be feasible."[26]

Born wrote these lines in June 1926. A few weeks later, on August 10, at the 1926 Oxford meeting of the British Association Born again referred to hidden variables which he called "microscopic coordinates": "The classical theory introduces the microscopic coordinates which determine the in-

[25] M. Born, "Quantenmechanik der Stossvorgänge," Ref. 2-31.
[26] *Ibid.*, p. 825 (p. 256; p. 76).

dividual process, only to eliminate them because of ignorance by averaging over their values; whereas the new theory gets the same results without introducing them at all. Of course, it is not forbidden to believe in the existence of these coordinates; but they will only be of physical significance when methods have been devised for their experimental observation."[27]

Jakov Il'ich Frenkel, who was affiliated with the Leningrad Polytechnical Institute from 1921 until his death in 1952, spent the summer of 1926 in Göttingen on a Rockefeller Foundation fellowship which he had obtained on the recommendation of Paul Ehrenfest, working there as an assistant to Born. Unfortunately, no documentary material can be found which could throw some light on Frenkel's hidden variable theory.[28] The earliest hidden variable theory in quantum mechanics is apparently an irretrievable loss for the history of modern physics.

One may conjecture, however, what Frenkel could have had in mind. Any reconstruction of Frenkel's theory should be based on two considerations: (1) What would have been, at that time, the most natural way to establish the existence of hidden variables? (2) Would this way have been congenial with Frenkel's inclinations? In view of the fact that, apart from quantum mechanics, statistical mechanics was at the focus of his interest—the first edition of his treatise[29] on statistical physics had already appeared in 1932—this reconstruction, if formulated in modern terms, may perhaps read as follows.

Confining himself to nondegenerate discrete spectra or eigenvalues $\{a_j\}_{j=1}^{\infty}$ of a self-adjoint operator A such that $A\alpha_j = a_j\alpha_j$, Frenkel, following Born, knew that the probability of obtaining a_j in a measurement of A in state ψ is $p_j = |(\alpha_j, \psi)|^2$, where $\sum p_j = 1$. A hidden variable had to be defined whose statistical distribution $\rho_\psi(\lambda)$ for a given ψ reproduces the p_j. To this

[27]M. Born, "Physical aspects of quantum mechanics," *Nature* **119**, 354–357 (1927); reprinted in M. Born, *Physics in my Generation* (Pergamon Press, London, New York, 1956), pp. 6-13.

[28]The present author tried in vain to trace some documents on Frenkel's theory. Max Born, interrogated on this issue, did not recall any details (Interview with Born, Bad Pyrmont, July 12, 1965); the University of Minnesota, where Frenkel lectured during 1930–1931, has no records on this question; Frenkel's son, Professor V. J. Frenkel of the Physico-Technical Institute in Leningrad, has never seen any reference to this subject among the manuscripts of his father (Interview with V. J. Frenkel, Moscow, August 18, 1971). Nor can any such references be found in Frenkel's well-known textbooks on quantum mechanics, *Einführung in die Wellenmechanik* (J. Springer, Berlin, 1929), *Wave Mechanics—Part I: Elementary Theory* (Oxford University Press, 1932, 1936; Dover, New York, 1950), *Part II: Advanced General Theory* (Oxford, 1934; Dover, 1950).

[29]J. I. Frenkel, *Statističeskaya Fisika* (Nauk, Moscow, Leningrad, 1932); revised, 1947; *Statistische Physik* (Akademie-Verlag, Berlin, 1957).

end it would have sufficed to associate with each a_j a domain I_j of λ and to postulate that $\rho_\psi(\lambda)$ attributes p_j to I_j. The easiest way would then have been to divide the unit interval into subintervals I_j of length p_j and to stipulate, in terms of Definition II, that $f_A^{-1}(a_j)=I_j$. To generalize this procedure for the case of several observables, represented by $A^{(1)}, A^{(2)},\ldots$, all one had to do was to introduce corresponding hidden variables $\lambda^{(1)}$, $\lambda^{(2)},\ldots$, to postulate that $a_j^{(k)}$ depends merely on $\lambda^{(k)}$ and to associate with each $a_j^{(k)}$ a domain $I_j^{(k)}$ of $\lambda^{(k)}$ such that $a_j^{(k)}$ is obtained if $\lambda^{(k)}$ lies in $I_j^{(k)}$. If $\rho_\psi^{(k)}(\lambda^{(k)})$ is defined in analogy to the above $\rho_\psi(\lambda)$, then the general distribution is given by $\rho_\psi(\lambda^{(1)}, \lambda^{(2)}, \ldots)=\Pi\rho_\psi^{(k)}(\lambda^{(k)})$. For discrete spectra the number of hidden variables can be reduced to 1.

Although merely a simple mathematical trick which would hardly admit any assignment of a physical meaning to these hidden variables, the above procedure would have sufficed to show that the idea of hidden variables is not incompatible with the rules of quantum mechanics. But apparently this was not common knowledge at that time and was obviously also unknown to John von Neumann, whose famous "proof" of the impossibility of hidden variables we consider next.

7.4. VON NEUMANN'S "IMPOSSIBILITY PROOF" AND ITS REPERCUSSIONS

John von Neumann[30] was primarily engaged in the mathematical aspects of quantum mechanics when, during the late 1920s, he was privatdozent at the University of Berlin. But he also took great interest in the discussions on the philosophical implications of the new theory which were held frequently at the end of the famous Physics Colloquia of that University. The main problem that attracted von Neumann's attention was the precise nature of the statistical character of quantum mechanics, the question why, in spite of the unambiguous definition of the state of a system by the state function, only statistical statements could be made about the value of the physical quantities (observables) involved.

The fact that ensembles described by the same state function exhibit dispersion suggested to him two a priori conceivable interpretations: either

[30]For biographical details see S. Ulam, "John von Neumann, 1903–1957," *Bulletin of the American Mathematical Society* **64**, 1-49 (1958). L. van Hove, "Von Neumann's contributions to quantum theory," *ibid.*, 95-99. E. P. Wigner, "John von Neumann," *Yearbook of the American Philosophical Society* **1957**, 149–153. S. Ulam, H. W. Kuhn, A. W. Tucker and C. E. Shannon, "John von Neumann, 1903–1957," *Perspectives in American History*, D. Fleming and B. Bailyn, eds. (Charles Warren Center for Studies in American History, Cambridge, Mass., 1968), Vol. 2, pp. 235–269. It is regrettable that till now no full biography has appeared on von Neumann who was so influential in the development of modern science.

(1) the individual systems, though described by the same ψ, differ in additional hidden parameters whose values determine the precise outcome of the measurements, or (2) all individual systems of the ensemble are in the same state "but the laws of nature are not causal": dispersion results from nature's disregard of the principle of sufficient reason. In support of (1), he stated, it is claimed that nature could never violate the principle of sufficient reason because the principle is but a definition of equality [*Gleichheitsdefinition*]; states are equal (or identical) by definition if and only if they yield equal results for equal measurements. In the non-mathematical introduction to his "proof"[31] von Neumann declared that any attempt to introduce causal order on the basis of such a criterion is doomed to failure in quantum mechanics.

To show this failure in detail, von Neumann considered the measurement of a physical quantity R which, for simplicity, he assumed to be capable of yielding only two results r_1 and r_2, to be performed on an ensemble E of N systems. Let the measurement yield r_1 on the subensemble E_1 of E, and r_2 on E_2 of E, E_1 and E_2 being dispersion-free for an immediately repeated R measurement. Let us now measure on these subensembles a second quantity Q, incompatible with R and yielding only q_1 or q_2. Let q_1 be obtained on $E_{1,1}$ of E_1 and on $E_{2,1}$ of E_2 and let q_2 be obtained on $E_{1,2}$ of E_1 and on $E_{2,2}$ of E_2. Experience shows that $E_{1,1}$ and $E_{1,2}$, although subsets of E_1, as well as $E_{2,1}$ and $E_{2,2}$, although subsets of E_2, exhibit dispersion with respect to R, in contrast to E_1 and E_2. The attempt failed, von Neumann concluded, because "in the atom we are at the boundary of the physical world, where each measurement is an interference of the same order of magnitude as the object measured, and therefore affects it basically." An obvious example which von Neumann seems to have had in mind was the measurements of two spin-components of a spin-$\frac{1}{2}$ particle.[32] A sufficiently large number of such measurements should, according to von Neumann, narrow the range of hidden variables, if they existed, and finally lead to the result that the spin components would have definite values in all directions, a conclusion incompatible with quantum mechanics.

According to Eugene P. Wigner, who at that time was at the Technische Hochschule in Berlin and one of von Neumann's intimate friends and subsequently shared with him a professorship at Princeton University (1930–1933), considerations of this kind and not so much the explicit mathematical "proof" (soon to be discussed) were decisive for von Neu-

[31]Ref. 1-2, Chapter 4, sections 1 and 2.

[32]E. P. Wigner, "On hidden variables and quantum mechanical probabilities," *American Journal of Physics* **38**, 1005–1009 (1970), corroborates this assumption.

mann's rejection of hidden variables. Against Schrödinger's objection "that the spin measurement in one direction, though perhaps specifying one set of hidden variables, may restore a random distribution of some other set" von Neumann, according to Wigner, argued that this objection "presupposed hidden variables in the apparatus used for the measurement. Von Neumann's argument needs to assume only two apparata, with perpendicular magnetic fields, and a succession of measurements alternating between the two apparata. Eventually, even the hidden variables of both apparata will be fixed by the outcomes of many subsequent measurements of the spin component in their respective directions so that the whole system's hidden variables will be fixed."[33] This reasoning was recently criticized by John F. Clauser,[34] who constructed a simple model of hidden variables which allows for Schrödinger's restoration of randomness without requiring the existence of hidden variables in the measuring device and which reproduces the usual quantum mechanical predictions.

One may further object, von Neumann continued, that—even granted that no *physical* method exists of dividing E into dispersion-free ensembles —*conceptually* every dispersive ensemble is but the superposition of two or more dispersion-free subensembles which differ from each other. That even such a "fiction" in support of alternative (1) is untenable is the content of von Neumann's famous proof.[35]

It is based on the following four postulates:

P.I. If a quantity (observable) is represented by the operator R, then a function f of this quantity is represented by the operator $f(R)$.

P.II. If quantities are represented by the operators $R, S, \ldots$, then the sum of these quantities is represented by the operator $R+S+\cdots$, regardless of whether the operators commute or not.

P.III. If the quantity $\mathfrak{R}$ is by nature nonnegative, then its expectation value $\langle \mathfrak{R} \rangle$ is nonnegative.

[33] *Ibid.*

[34] J. F. Clauser, "Von Neumann's informal hidden-variable argument," *American Journal of Physics* **39**, 1095–1096 (1971). But cf. also E. P. Wigner, "Rejoinder," *ibid.*, 1097–1098, and J. F. Clauser, "Reply to Dr. Wigner's objections," *ibid.*, 1098–1099.

[35] Ref. 1-2, Chapter 4, sections 1 and 2. The logical consistency of von Neumann's postulates as formulated in these sections of his book has frequently been called into question. In particular, the acceptability of PI has been questioned. One of the first to draw attention to this issue was George Temple (then of King's College, London). In his paper "The fundamental paradox of the quantum theory," *Nature* **135**, 957 (1935), he showed that PI may lead to serious contradictions. Cf. also H. Fröhlich and E. Guth, "The fundamental paradox of the quantum theory," *ibid.*, **136**, 179 (1935), and R. Peierls, "The fundamental paradox of the quantum theory," *ibid.*, **136**, 395 (1935).

P.IV. If $\mathfrak{R}, \mathfrak{S}, \ldots$ are arbitrary quantities and $a, b, \ldots$, real numbers, then $\langle a\mathfrak{R} + b\mathfrak{S} + \cdots \rangle = a\langle \mathfrak{R} \rangle + b\langle \mathfrak{S} \rangle + \cdots$.

To keep his proof as general as possible von Neumann derived the statistical formula for the expectation value from first principles. To this end he introduced in Hilbert space $\mathcal{H}$ a complete orthonormal set of vectors φ_k and defined $R_{mn} = (\varphi_m, R\varphi_n)$, where the parentheses denote the inner product. He also defined the matrices $U^{(n)} = (e^{(n)})$, where $e^{(n)}_{\mu\nu} = 1$ if $\mu = \nu = n$ and zero otherwise, $V^{(mn)} = (f^{(m,n)}_{\mu\nu})$, where $f^{(m,n)}_{\mu\nu} = 1$ if $\mu = m$, $\nu = n$ or if $\mu = n$, $\nu = m$ and zero otherwise, $W^{(m,n)} = (g^{(m,n)}_{\mu\nu})$, where $g^{(m,n)}_{\mu\nu} = i$ if $\mu = m$, $\nu = n$, $g^{(m,n)}_{\mu\nu} = -i$ if $\mu = n$, $\nu = m$ and zero otherwise. With $A_{\mu\nu}$ defined by

$$A_{\mu\nu} = \sum_n R_{nn} e^{(n)}_{\mu\nu} + \sum_{\substack{m,n \\ m<n}} R_{mn} f^{(m,n)}_{\mu\nu} + \sum_{\substack{m,n \\ m<n}} R_{mn} g^{(m,n)}_{\mu\nu}$$

where square brackets denote the real and curved brackets the imaginary part of the enclosed number, it follows that $A_{\mu\nu} = R_{\mu\nu}$ and hence

$$R = \sum_n R_{nn} U^{(n)} + \sum_{\substack{m,n \\ m<n}} [R_{mn}] V^{(mn)} + \sum_{\substack{m,n \\ m<n}} \{R_{mn}\} W^{(mn)}.$$

By virtue of P.II and P.IV von Neumann obtained

$$\langle \mathfrak{R} \rangle = \sum_n R_{nn} \langle U^{(n)} \rangle + \sum_{\substack{m,n \\ m<n}} [R_{mn}] \langle V^{(mn)} \rangle + \sum_{\substack{m,n \\ m<n}} \{R_{mn}\} W^{(mn)} \qquad (1)$$

and after defining

$$u_{nn} = U^{(n)},$$

$$u_{mn} = \frac{1}{2}\langle V^{(mn)} \rangle + \frac{i}{2}\langle W^{(mn)} \rangle \qquad \text{for } m<n,$$

$$u_{nm} = \frac{1}{2}\langle V^{(mn)} \rangle - \frac{i}{2}\langle W^{(mn)} \rangle \qquad \text{for } m<n,$$

the result

$$\langle \mathfrak{R} \rangle = \sum_{m,n} u_{nm} R_{mn} = \mathrm{Tr}(UR) \qquad (2)$$

where U, defined by $(\varphi_n, U\varphi_m) = u_{nm}$, is the statistical operator. U, by construction independent of R, is nonnegative definite and depends only on the ensemble.

Having thus established the statistical formula $\langle \mathfrak{R} \rangle = \mathrm{Tr}(UR)$ von Neumann continued his proof by showing that no quantum mechanical ensemble is dispersion-free. A dispersion-free ensemble would satisfy for all R the equation $\langle R^2 \rangle - \langle R \rangle^2 = 0$ or $\mathrm{Tr}(UR^2) = (\mathrm{Tr}(UR))^2$. Choosing for R the (idempotent) projection operator $P_\varphi = |\varphi><\varphi|$, where φ is an arbitrary normalized vector, he obtained $\mathrm{Tr}(UP) = (\mathrm{Tr}(UP))^2$ or $(\varphi, U\varphi) = (\varphi, U\varphi)^2$, that is, either $(\varphi, U\varphi) = 0$ for all φ or $(\varphi, U\varphi) = 1$ for all φ, and hence either $U = 0$ or $U = 1$. Since both conclusions are unacceptable, the nonexistence of dispersion-free ensembles has been demonstrated.

Von Neumann now raised the question of whether at least homogeneous or pure ensembles [*einheitliche oder reine Gesamtheiten*] exist, these being ensembles E such that for every physical quantity $\mathfrak{R}$ and every pair of subensembles E_1 and E_2 the equation $\langle \mathfrak{R} \rangle_E = a\langle \mathfrak{R} \rangle_{E_1} + b\langle \mathfrak{R} \rangle_{E_2}$ with $a > 0$, $b > 0$, and $a + b = 1$ implies that $\langle \mathfrak{R} \rangle_E = \langle \mathfrak{R} \rangle_{E_1} = \langle \mathfrak{R} \rangle_{E_2}$. This condition of homogeneity—the property that the statistics of an ensemble is the same as that of any of its subensembles—can equivalently be expressed by the statement that $U = aU_1 + bU_2$ implies $U = U_1 = U_2$ (or they differ at most by constant factors). Von Neumann could easily prove that an ensemble E is homogeneous if and only if its statistical operator U is a projection operator $P_\psi = |\psi><\psi|$ (up to a constant factor). The necessity was proved by choosing a vector φ_0 such that $U\varphi_0 \neq 0$ and constructing the operators V and W as follows (φ is any vector):

$$V\varphi = \frac{(U\varphi_0, \varphi)}{(U\varphi_0, \varphi_0)} U\varphi_0; \qquad W = U - V,$$

which by their definition are Hermitian positive operators and satisfy $U = V + W$.[36] Hence by assumption V is proportional to U.

Von Neumann then defined the normalized vector ψ and the positive constant c by the equations

$$\psi = \frac{U\varphi_0}{\|U\varphi_0\|}, \qquad c = \frac{\|U\varphi_0\|^2}{(U\varphi_0, \varphi_0)}$$

and obtained $U\varphi \propto V\varphi = c\psi(\psi, \varphi) = cP_\psi\varphi$. In a similar vein he proved the sufficiency of the condition. If, in particular, ψ belongs to a complete orthonormal set of vectors φ_k, in Hilbert space, say $\psi = \varphi_n$, then $\langle \mathfrak{R} \rangle = \mathrm{Tr}(UR) = \mathrm{Tr}(P_{\varphi_n} R) = (\varphi_n, R\varphi_n)$, the usual formula for the expectation

[36]To prove the positive definiteness of W von Neumann used the following generalization of the Schwarz inequality: For any positive Hermitian Y and any φ, φ_0,

$$|(\varphi, Y\varphi_0)| \leq \sqrt{(\varphi, Y\varphi)(\varphi_0, Y\varphi_0)}\,.$$

value. Ordinary quantum mechanical states, such as φ_n, describe homogeneous ensembles.

To conclude his impossibility proof von Neumann pointed out that the assumption of hidden variables implies that no dispersive ensemble is homogeneous. However, since as shown above every ensemble is dispersive, no ensemble could be homogeneous; but since it has been demonstrated that homogeneous ensembles do exist, the assumption concerning the existence of hidden variables has been refuted.

Anticipating certain objections (which indeed were later raised) von Neumann added the following remarks which often have been ignored:

> The only formal theory existing at the present time which orders and summarizes our experiences in this area in a half-way satisfactory manner, i.e., quantum mechanics, is in compelling logical contradiction with causality. Of course it would be an exaggeration to maintain that causality has thereby been done away with: quantum mechanics has, in its present form, several serious lacunae, and it may even by that it is false, although this latter possibility is highly unlikely, in the face of its startling capacity in the qualitative explanation of general problems, and in the quantitative calculation of special ones. In spite of the fact that quantum mechanics agrees well with experiment, and that it has opened up for us a qualitatively new side of the world, one can never say of the theory that it has been proved by experience, but only that it is the best known summarization of experience. However, mindful of such precautions, we may still say that there is at present no occasion and no reason to speak of causality in nature—because no experiment indicates its presence, since the macroscopic are unsuitable in principle, and the only known theory which is compatible with our experiences relative to elementary processes, quantum mechanics, contradicts it.
>
> To be sure, we are dealing with an age old way of thinking of all mankind, but not with a logical necessity (this follows from the fact that it was at all possible to build a statistical theory), and anyone who enters upon the subject without preconceived notions has no reason to adhere to it. Under such circumstances, is it sensible to sacrifice a reasonable physical theory for its sake?[37]

Von Neumann was hailed by his followers and credited even by his opponents as having succeeded in bringing the foremost methodological and interpretative problem of quantum mechanics down from the realm of speculation into the reach of mathematical analysis and empirical decision. For the essence of his proof seemed to have been simply this: If experiments force us to base the formalism of quantum mechanics on postulates as formulated above, any conception of hidden variables, designed to

[37]Ref. 1-2, p. 173 (English edition, pp. 327–328).

embed, even only conceptually, the theory into a deterministic scheme is inconsistent with these postulates.

Independent of von Neumann's work and not based on the operator calculus in Hilbert space, another proof of the inconsistency of hidden variables with quantum mechanics was given in 1932 and published in 1933 by Jacques Solomon,[38] a son-in-law of Paul Langevin. Solomon had studied in Paris, Zurich, London, and Copenhagen and lectured in Berlin, London, Cambridge, Kharkov, and Moscow before his deportation and execution in 1943 by the Nazis. The principal idea of his proof was as follows.

If a time-dependent physical quantity [*grandeur*] $F(t)$ is conceived to depend also on certain hidden parameters u, the mean value of $F(t,u)$ will be given by $\bar{F}(t)=UF(t,u)$, where U is some averaging operator. If the kernel of U is $G(u)$, the preceding equation may be written

$$\bar{F}(t)=\int F(t,u)G(u)du$$

or, after a transformation of the integration variable,

$$\bar{F}(t)=\int F(t,w)dw.$$

Applying this relation to the nth power of the position coordinate $x=f(t,w)$, where w denotes the hidden variable, it follows that

$$\overline{x^n}=\int f^n(t,w)dw.$$

On the other hand, if $\rho(x,t)=|\psi(x,t)|^2$ is the usual probability density of finding the system within the interval from x to $x+dx$ at time t, the nth moment of x (or mean value of x^n) is given by

$$\overline{x^n}=\int x^n\rho(x,t)dx=\int w^n\rho(w,t)dw.$$

Consistency would require that for all t and all n, $f^n(t,w)=w^n\rho(w,t)$, which can hold only if $\rho(w,t)$ is identically unity, in contradiction to quantum mechanics. Since neither the nature of u nor that of the averaging procedure were specified Solomon claimed that his proof was of greatest generality: "Comme les hypothèses dont nous sommes partis pour définir la théorie des paramètres cachés nous semblent bien être les plus larges

[38] J. Solomon, "Sur l'indéterminisme de la mécanique quantique," *Journal de Physique* **4**, 34–37 (1933).

possibles, compatibles avec la théorie quantique, la démonstration qui précède nous semble être de nature à éliminer définitivement ce dernier espoir d'une théorie déterministe des phénomènes atomiques."[39]

Solomon's argumentation, which on closer examination raises a number of questions, has apparently never been commented upon. Von Neumann's proof, in contrast, became the subject of numerous discussions. Interestingly, however, von Neumann's treatise which—apart from the hidden variable proof—contained an enormous amount of thought-provoking material was, with the exception of two short notes written in 1933 (one by Felix Bloch and the other by Henry Margenau[40]) never reviewed prior to 1957, two years after the publication of the English translation by R. T. Beyer.[41] It seems that at that time, due perhaps to the political situation and the war, too few physicists had the mathematical knowledge, and too few mathematicians the physical knowledge, fully to assess this contribution. In fact, in the 1950s, when after the publication of various proposals of consistent hidden variable theories, von Neumann's proof became the subject of extensive discussions, opinions on its validity still were greatly divided.

Characteristic of this situation is a paper published in 1959 by H. Nabl,[42] the son of Boltzmann's long-time assistant J. Nabl. Pointing out how greatly Pauli, March, Bopp, Rosenfeld, de Broglie, Destouches, Feyerabend, Fenyes, and Zinnes, among others, differ in their evaluation of von Neumann's proof, Nabl complained that no clear-cut answer exists on the question whether this proof has ever been refuted. He concluded his paper with an appeal to all physicists and mathematicians to clarify this situation and with an expression of gratitude to any expert who could provide an unambiguous answer to his question.[43] It took quite some time until the situation was fully clarified.

The earliest criticism leveled against von Neumann's proof is contained in Grete Hermann's essay, mentioned earlier in another context.[44] Hermann charged von Neumann with having committed a *petitio principii* by

[39] *Op. cit.*, p. 37.

[40] F. Bloch, *Physikalische Zeitschrift* **34**, 183 (1933). H. Margenau, *Bulletin of the American Mathematical Society* **39**, 493–494 (1933).

[41] The English edition was reviewed by P. K. Feyerabend in *The British Journal for the Philosophy of Science* **8**, 343–347 (1957/8).

[42] H. Nabl, "Eine Frage an Mathematiker und Physiker—Wurde der Parameterbeweis J. v. Neumanns widerlegt?," *Die Pyramide* **3**, 96 (1959).

[43] "Welche Beurteilung des Neumannschen Beweises trifft tatsächlich zu?...Der Einsender wäre dankbar, wenn ihm diese Frage, auf die es offenbar eine eindeutige Antwort gibt, von sachverständiger Seite beanwortet würde." *Loc. cit.*

[44] Ref. 6-95.

introducing into the formal presuppositions of his proof a statement which is logically equivalent to the assertion to be proved. The point in question concerns the additivity of the expectation value, that is, the validity of the relation $\langle \mathfrak{R} + \mathbb{S} \rangle = \langle \mathfrak{R} \rangle + \langle \mathbb{S} \rangle$ for arbitrary ensembles. Von Neumann was of course well aware that for incompatible observables this relation requires special consideration. In fact, in footnote 164 of his treatise he explicitly referred to the energy operator H for an electron moving in an electric field, defined by the potential $V(x)$, $H = (p^2/2m) + V(x) = R(p) + S(x)$, where the first term is a momentum operator and the second term a coordinate operator; however, since the energy measurement requires an entirely different procedure, for example, the measurement of the frequency of the emitted radiation (in accordance with Bohr's frequency relation), von Neumann felt justified in declaring that nevertheless "under all circumstances" the foregoing relation is correct.

Grete Hermann now pointed out that since an arbitrary ensemble is a mixture of pure cases it presumably suffices to claim the additivity only for ensembles all elements of which are described by the same (pure state) wave function. For such ensembles, however, von Neumann, according to Hermann, resorted to the mathematical formalism $(\varphi,(R+S)\varphi) = (\varphi, R\varphi) + (\varphi, S\varphi)$ as a valid relation irrespective of whether R and S commute. Hermann now objected that, as long as the possibility of hidden variables has not yet been disproved, $(\varphi, R\varphi)$ denotes the expectation value of R for such ensembles E alone whose elements are described by φ. This does not imply that also subensembles E_1 of E, defined by perhaps not yet available criteria (hidden variables), have the same expectation value of R, nor that the latter satisfies the additivity condition. Thus an important step in von Neumann's proof is lacking. But to retain this assumption, as von Neumann did, is tantamount to assuming that the elements of an ensemble, described by φ, cannot be further differentiated by any criteria on which the result of an R measurement may depend. Since the denial of the existence of such criteria is precisely the thesis that has to be proved, Hermann concluded that von Neumann's proof is circular. If we call for a moment "complete" a theory T which cannot be embedded in a hidden variable extension T' (in the spirit of our Definition I), then Hermann's criticism may be summarized as follows: von Neumann has proved that quantum mechanics is a complete theory, but only as far as quantum mechanical states are concerned.[45]

[45]Not certain whether he had correctly understood the relevant passages in Grete Hermann's essay, the present author sent her this text and obtained the following statement: "Ihre Wiedergabe meines Gedankengangs hat mich sehr gefreut; Sie trifft genau, was ich sagen wollte. Längere Zeit meditiert habe ich über den Schluss Ihrer Zusammenfassung meiner

In addition Grete Hermann pointed out that one has to distinguish between the *physical* question of whether future research could lead to more accurate predictions and the *mathematical* question of whether such results could be formalized within the framework of the existing quantum mechanical calculus of operators; the fact that this formalism was successful in the past is no guarantee that all future experience could be accounted for by a theory of the present structure.

It is remarkable that Hermann's criticism touched precisely on one of the weakest points in the proof, the additivity postulate. If the subensembles mentioned in her criticism were meant to be of dispersion-free states for which expectation values and eigenvalues are identical, then clearly the addivity postulate, according to which the expectation value of a linear combination of observables is equal to the same linear combination of the expectation values, precludes the existence of dispersion-free states (or hidden variables), for the eigenvalues generally do not combine linearly. A simple example is the spin component, say of an electron, in the direction along the bisector between the x-axis and the y-axis, represented by the operator $2^{-1/2}(\sigma_x + \sigma_y)$ whose eigenvalues ± 1 differ obviously from $2^{-1/2}(\pm 1 \pm 1)$.

Kritik: 'von Neumann has proved that quantum mechanics is a complete theory, but only as far as quantum mechanical states are concerned." Hoffentlich verstehe ich den mir sehr wichtigen Zusatz richtig: '... aber nur soweit es um quantenmechanische Zustände geht', wobei 'quantenmechanische Zustände' solche physikalischen Zustände sind, die sich im bereits bewährten quantenmechanischen Formalismus bestimmen lassen. Wenn das der Sinn Ihrer Formulierung ist, dann muss ich sagen, dass Sie mein—damaliges und heutiges—Urteil über Neumanns Beweis knapper und schärfer ausgedrückt haben, als ich das in meiner eigenen Arbeit finde" (Letter from Mrs. Grete Henry-Hermann to the author, dated April 11, 1968). We agree with Grete Hermann's criticism that the proof did not achieve its declared objective of demonstrating that quantum mechanical ensembles cannot be decomposed into *any* kind of dispersion-free subensembles, and this for a reason to be explained later. But we do not dismiss the proof as nugatory. True, in view of von Neumann's excessively restrictive assumptions it is not an *impossibility proof* of any conceivable class of hidden variables, but it is a *completeness proof*, in this respect, of von Neumann's axiomatics (with the inclusion of postulate P.IV), since it shows that this formalism does not admit nonquantum mechanical ensembles. It may even be regarded as a *consistency proof* of this formalism with its usual interpretation. Louis de Broglie's statement that "von Neumann's demonstration did not add greatly to what was already known, since the conclusion is already implied in the uncertainty relations; but this observation in no way diminishes the soundness of his conclusion" is correct as far as it goes, but fails to point out that even a proof of completeness or consistency adds something to our knowledge. Cf. L. de Broglie, *Une Tentative d'Interpretation Causale et Non Lineaire de la Mécanique Ondulatoire* (Gauthier-Villars, Paris, 1956), p. 66; *Non-Linear Wave Mechanivs—A Causal Interpretation* (Elsevier, Amsterdam, 1960), p. 68. Cf. also L. de Broglie, *La Théorie de la Mesure en Mécanique Ondulatoire* (Gauthier-Villars, Paris, 1957), pp. 25–30. A view similar to ours has been expressed also by Paul K. Feyerabend in a lecture delivered in 1965 at Alpach entitled "Der Neumannsche Beweis" (mimeographed).

Do these considerations justify Grete Hermann's charge of circularity? To answer this question, let us recall that a proof is circular if the conclusion to be established is contained in the premises, and let us find out whether this can be said of von Neumann's proof.

To demonstrate the nonexistence of dispersion-free states von Neumann tried to show that for no ensemble α whatever the relation $\langle A^2\rangle_\alpha = \langle A\rangle_\alpha^2$ can be satisfied for all self-adjoint operators A on Hilbert space. To prove this he used the relation, postulated in P.IV, that for all α, $\langle A+B\rangle_\alpha = \langle A\rangle_\alpha + \langle B\rangle_\alpha$ for all self-adjoint operators A and B, whether commuting or not. Now this relation, although always valid for quantum mechanical states ($\alpha \equiv \psi$), cannot be satisfied, in general, by (nonquantum mechanical) dispersion-free states if A and B do not commute because, as just stated, eigenvalues do not combine linearly. In this case P.IV would automatically exclude dispersion-free ensembles and the proof would be circular. However, von Neumann did *not* stipulate that that relation or P.IV should be restricted to noncommuting operators or incompatible observables alone. Since for commuting operators the relation does not exclude dispersion-free ensembles from the outset, the conclusion to be established was not contained in the premises and the charge of circularity therefore is not justified. What should have been criticized, instead, is the fact that the proof severely restricts the class of conceivable ensembles by admitting only those for which P.IV is valid.

In France, Paulette Destouches-Février, shortly after having completed her thesis "Recherches sur la Structure des théories Physiques"[46] at the Sorbonne, published two papers[47] in 1945 in which she studied the logical status of von Neumann's contention from the viewpoint of theory construction, without, however, questioning the validity of the proof as such. Her work, important as it is for the philosophy of science, seems to have exerted only a limited influence on the development of hidden variable theories outside the Paris school of theorists headed by Louis de Broglie. It did exercise considerable influence in France, for example, on Mioara Mugur-Schächter[48] (née Iscovici), a graduate of the Sorbonne, who investigated primarily the measure-theoretical aspects implied by von Neumann's proof.

[46] Faculty of Science, University of Paris, 1945. (Mimeographed) Cf. also P. Destouches-Février, *La Structure des Théories Physiques* (Presses Universitaires, Paris, 1951).

[47] P. Destouches-Février, "Une nouvelle preuve du caractère essentiel de l'indéterminisme quantique," *Comptes Rendus* **222**, 553–555 (1945); "Sur l'impossibilité d'un retour au déterminisme en microphysique," *ibid.*, 587–589; cf. also *L'Interprétation Physique de la Mécanique Ondulatoire et des Théories Quantiques* (Gauthier-Villars, Paris, 1956).

[48] M. Mugur-Schächter, "Sur la possibilité de trancher experimentalement le problème du caractère 'complet' de la Mécanique quantique," *Comptes Rendus* **256**, 5514–5517 (1963);

The view that von Neumann's theorem on hidden variables proved the breakdown of causality at the microphysical level was rejected by Jean Ullmo, a distinguished philosopher at the École Polytechnique in Paris. In a series of essays[49] written between 1949 and 1963 Ullmo insisted on a sharp distinction between *similar systems* [*systèmes semblables*], that is, systems described by the same state function as a result of having undergone the same process of preparation, and *identical systems* [*systèmes identiques*], that is, systems which, in addition, have been observed at the same location [deux systèmes quantiques sont identiques lorsqu'ils ont même fonction d'onde et lorsqu'en outre ils sont observés dans la même localisation].[50] The state function alone gives only an incomplete knowledge about the systems and does not suffice to affirm their identity.

Following Enriques and Philipp Frank Ullmo thought it also necessary to differentiate between the principle of determinism and the principle of causality, defining the former as implying perfect predictability and the latter as the statement that "two identical systems evolve in the same way" [*deux systèmes identiques évoluent de même*].[51] Since, according to Ullmo, von Neumann's proof concerns only similar systems, it cannot be used as an argument against causality; it is merely a demonstration of the internal consistency of axiomatized quantum mechanics and without any relevance for theories of a different logical structure.

That von Neumann's proof presupposes the exclusive validity of the standard formalism of quantum mechanics and that within the framework of a different theory hidden variables, leading to deterministic predictions, may well be conceivable, provided the statistical relations among them are not expressed by state functions, had been emphasized also by Hans Reichenbach.[52] "Although...we cannot adduce logical reasons excluding such a further development of physics, and, although some eminent physicists believe in such a possibility, we cannot find much empirical evidence for such an assumption," he declared in 1944.

If Reichenbach regarded hidden variables as a logically possible, but physically inappropriate, assumption, Dimitry Ivanovich Blokhinzev of

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Étude du Caractère Complet de la Théorie Quantique (Gauthier-Villars, Paris, 1964).

[49] J. Ullmo, "La mécanique quantique et la causalité," *Revue Philosophique* **139**, 257–287, 441–473 (1949); "Le theorème de von Neumann et la causalité," *Revue de Métaphysique et de Morale* **56**, 143–170 (1951); *La Crise de la Physique Quantique* (Hermann, Paris, 1955); "La philosophie d'Heisenberg," *La Nouvelle Revue Francaise* **22**, 296–308 (1963).

[50] *Ibid.* (1949), p. 267.

[51] *Ibid.* (1949), p. 257.

[52] H. Reichenbach, *Philosophic Foundations of Quantum Mechanics* (University of California Press, Berkeley and Los Angeles, 1944, 1946, 1965), p. 13; *Philosophische Grundlagen der Quantenmechanik* (Verlag Birkhäuser, Basel, 1949), p. 24.

Moscow's Lomonosov State University considered it an open question to be decided by future research. In a paper published in Russian but soon translated into French and German,[53] in which he severely criticized the "subjective-idealistic conceptions" of the Copenhagen school, he also referred to von Neumann's proof, calling it "not satisfactory" since it is based on the formalism of quantum mechanics. In his view a consistent hidden variable theory may be established only if these variables are not accommodated within the usual formalism of quantum mechanics. As an argument for his contention that the statistical properties of these variables must be *sui generis* he referred to the very same example of consecutive spin-component measurements which von Neumann had adduced to refute hidden variables. Gradually, as we see, the cogency of von Neumann's "proof," once regarded as incontestable, began to be called into question and it seemed no longer unreasonable, in spite of von Neumann, to search for a determinsitic refinement of quantum mechanics.

Before we discuss the major breakthrough in this direction let us mention a strange coincidence in the history of modern physics: At the time when hidden variable theories were proposed to reinstate the determinism of classical physics in quantum mechanics claims were voiced that classical physics is indeterministic, perhaps even more so than quantum mechanics. One of the first to express this view was Karl Popper,[54] who argued "that most systems of physics, including classical physics... are indeterministic in perhaps an even more fundamental sense than the one usually ascribed to the indeterminism of quantum physics (in so far as the unpredictability of the events...is not mitigated by the predictability of their frequencies)." He was soon followed by Max Born,[55] who based his claim on the observation that the assumption of exact initial values for classical observables is an unjustified idealization. One year later Léon Brillouin[56] referred to the classical investigations of A. M. Liapunov and H. Poincaré on the stability of motion as supporting this thesis. Two

[53] D. I. Blokhinzev, "Kritika filosofikich vossrenij tak nasyvajemoj 'Kopengagenskoj shkoly' v fisike," *Filosofskie Voprosy Sovremennoj Fisike* (Moscow, 1952), pp. 358–395; "Critique de la conception idéaliste de Théorie Quantique," in *Questions Scientifiques* (Éditions de la Nouvelle Critique, Paris, 1952), pp. 95–193; "Kritik der philosophischen Anschauungen der sogenannten 'Kopenhagener Schule' in der Physik," *Sowjetwissenschaft* **6**, 545–574 (1953).

[54] K. Popper, "Indeterminism in quantum physics and in classical physics," *British Journal for the Philosophy of Science* **1**, 117–133, 173–195 (1950). For a criticism of this paper cf. G. F. Dear, "Determinism in classical physics," *ibid.*, **11**, 289–304 (1961).

[55] M. Born, "Continuity, determinism and reality," *Kongelige Danske Videnskabernes Selskabs (Math.-Fys. Medd.)* **30**, 1–26 (1955); "Ist die klassische Mechanik tatsächlich deterministisch?" *Physikalische Blätter* **11**, 49–54 (1955).

[56] L. Brillouin, *Science and Information Theory* (Academic Press, New York, 1956).

opposing tendencies developed independently of each other at the same time.[57]

7.5. THE REVIVAL OF HIDDEN VARIABLES BY BOHM

It was primarily due to Bohm's early work on hidden variables that these ideas began again to attract the attention of physicists. David Bohm, a native of Pennsylvania and graduate of the Pennsylvania State College (1939), had become interested in the foundations of quantum mechanics during his postgraduate studies at the University of California, Berkeley), where he attended J. Robert Oppenheimer's lectures on quantum mechanics and where he obtained his Ph.D. in 1943. In long discussions with Joseph Weinberg, another graduate student at Berkeley and enthusiastic adherent to Bohr's complementarity philosophy, he tried to reach full clarity on the basic notions of the quantum theory and their philosophical implications for the concept of motion and Zeno's famous paradoxes.[58]

Bohm's interest in these problems did not abate after he accepted, in 1946, the position of assistant professor at Princeton University. Following Benjamin Disraeli's advice that "the best way to become acquainted with a subject is to write a book about it," Bohm wrote a text to explain "the precise nature of the new quantum-theoretical concepts." The book,[59] which started with "the classical concept of a continuous and precisely defined trajectory [which is] fundamentally altered by the introduction of a description of motion in terms of a series of indivisible transitions," was published in February 1951; it was based on Oppenheimer's lectures and on Bohr's views which, as Bohm explicitly admitted,[60] were "of crucial importance in supplying the general philosophical basis needed for a rational understanding of quantum theory." In fact, at the end of the text[61] Bohm offered a "proof that quantum theory is inconsistent with hidden variables," which was based on the Einstein-Podolsky-Rosen argument; it showed that if the world could be analyzed into distinctly defined elements, an assumption underlying such hidden variable interpretation, noncommuting variables would have to correspond to simultaneously

[57]For details see M. Jammer, "Indeterminacy in physics," *Dictionary of the History of Ideas*, P. P. Wiener, ed. (Charles Scribner's Sons, New York, 1973), Vol. 2, pp. 586–594.

[58]Interview with David Bohm, April 24, 1972, in Jerusalem.

[59]D. Bohm, *Quantum Theory* (Prentice-Hall, Englewood Cliffs, N.J., 1951); *Kvantovaya Teoriya* (GITTL, Moscow, 1961).

[60]*Ibid.*, Preface, p. V.

[61]*Ibid.*, Chapter 22, section 19.

existing elements of reality and the indeterminacy principle would have to be regarded as merely expressing an operational limitation of obtaining complete precision. But since such an interpretation of the indeterminacy principle, as Bohm had shown in an earlier chapter of the book, would be untenable, he concluded that "no theory of mechanically determined hidden variables can lead to *all* of the results of the quantum theory."

Still, on closer inspection, the critical reader of Bohm's book could not have failed to note that some explanations—in particular in the treatment of the process of measurement—did not fully reflect the spirit of Bohr's philosophy. Bohm sent copies to Einstein, Pauli, and Bohr. "Einstein liked the book,"[62] Pauli likewise expressed his appreciation, but Bohr remained silent!

At that time, as a result of the crusade launched by Senator Joseph Raymond McCarthy, chairman of the United States Congressional Committee on Un-American Activities, Bohm was suspended from his position. Taking advantage of his involuntary vacation, before leaving for the University of São Paulo in Brazil, he "experimented with physical concepts," as he later liked to call it. Stimulated by his discussions with Einstein and influenced by an essay which, as he told the present author, was "written in English" and "probably by Blokhintsev or some other Russian theorist like Terletzkii,"[63] and which criticized Bohr's approach, Bohm began to study the possibility of introducing hidden variables. During the next few weeks he wrote a paper[64] on his suggested interpretation of quantum mechanics, preprints of which he sent to his colleagues as well as to Pauli. Pauli rejected the paper, saying that it is "old stuff, dealt with long ago." Prompted by these remarks Bohm wrote a sequel paper[65] in which he proposed a new theory of measurement in conformity with his hidden variable theory.

[62]Interview with Bohm, April 20, 1967, in Jerusalem.

[63]Bohm has forgotten the exact title and author of this paper. The present author asked Prof. Blokhintsev about this matter and received the following answer: "Unfortunately it is difficult for me to indicate which paper Prof. Bohm might have thought of. To all appearance, this paper is published in *Uspekhi Fiz. Nauk.* **45**, 195 (1951)." (Letter from D. I. Blokhintsev, dated May 21, 1970.) This paper, "Kritika idealističeskovo ponimaniia kvantovoj teorii," was a draft for the article mentioned in Ref. 53, but it was never published in English. Nor does any paper written by Terletzkii at that time suit this description. P. J. Vigier, in his essay "K voprosu o teorii povedeniya individual'nykh mikroobjektov" *Voprossy Filosofii* **1956**, 91–106, also claimed (pp. 93–94) that the revival of hidden variable interpretations in the early 1950s was inspired, at least in part, by writings of Blokhintsev and Terletzkii, but no documented evidence is given.

[64]D. Bohm, "A suggested interpretation of the quantum theory in terms of 'hidden variables,' Part I," *Physical Review* **85**, 166–179 (1952); received July 5, 1951.

[65]D. Bohm, "A suggested interpretation of the quantum theory in terms of 'hidden variables,'

In the introduction to his paper Bohm admitted that the usual interpretation of quantum mechanics is self-consistent but argued that it involves an assumption that cannot be experimentally verified: the wave function offers the most complete possible description of an individual system. This assumption could, however, be theoretically refuted if it were possible to construct a hidden variable interpretation which "permits us to conceive of each individual system as being in a precisely defined state, whose changes with time are determined by definite laws, analogous to (but not identical with) the classical equations of motion."

In Bohm's view the two basic and mutually consistent assumptions of the usual interpretation—(1) that the wave function with its probabilistic interpretation offers the most complete possible specification, and (2) that the transfer of a single quantum from the observed system to the measuring apparatus is inherently unpredictable, uncontrollable, and unanalyzable—though delimiting the possible forms of the mathematical formalism do not determine it uniquely. Should the theory prove to be inadequate, it would always be possible to slightly modify the formalism without fundamentally changing its physical interpretation. "The usual interpretation therefore presents us with a considerable danger of falling into a trap, consisting of a self-closed chain of circular hypotheses, which are in principle unverifiable if true."[66] To avoid the possibility of such a trap Bohm suggested studying the consequences of postulates which contradict (1) and (2) at the outset.

After these apologetic remarks Bohm described his hidden variable approach which may be summarized as follows. Substituting $\psi = R\exp(iS/h)$, where R and S are real functions, in the Schrödinger equation he obtained $\partial R/\partial t = -(1/2m)(R\Delta S + 2\nabla R\nabla S)$ and $\partial S/\partial t = -[(\nabla S)^2/2m + V - \hbar^2\Delta R/2mR]$, or in terms of the probability density $\rho(x) = R^2(x) = |\psi|^2$,

$$\frac{\partial\rho}{\partial t} + \nabla\cdot\left(\frac{\rho\nabla S}{m}\right) = 0$$

and

$$\frac{\partial S}{\partial t} + \frac{(\nabla S)^2}{2m} + V - \left(\frac{\hbar^2}{4m}\right)\left[\frac{\Delta\rho}{\rho} - \frac{1}{2}\frac{(\nabla\rho)^2}{\rho^2}\right] = 0.$$

Part II," *Physical Review* **85**, 180–193 (1952); received July 5, 1951.

[66]Ref. 64, p. 169. For further details on Bohm's philosophy of physics see D. Bohm, *Causality and Chance in Modern Physics* (Routledge and Kegan Paul, London; Van Nostrand, New York, 1957), and its informative review by P. K. Feyerabend in *The British Journal for the Philosophy of Science* **10**, 321–388 (1960).

In the classical limit ($\hbar \to 0$) the last equation reduces to the Hamilton-Jacobi equation so that according to a well-known theorem $\nabla S/m$ denotes the velocity $v(x)$ of a particle passing the point x on its trajectory normal to the surface $S = \text{const}$. The preceding equation, expressing the conservation of probability, then shows that ρv can consistently be interpreted as the mean current of particles in the ensemble characterized by ψ. Obviously

$$v = \frac{\nabla S}{m} = \frac{\hbar}{2im} \ln\left(\frac{\psi}{\psi^*}\right) = \frac{\hbar}{2im} \frac{\psi^* \nabla \psi - \psi \nabla \psi^*}{\psi \psi^*} \tag{3}$$

and ρv coincides with the well-known probability current density of quantum mechanics. So far Bohm's analysis resembles Madelung's hydrodynamical model, de Broglie's theory of pilot waves, and similar interpretations.

For the sake of historical accuracy it should be noted that the similarity between the Schrödinger equation and the Hamilton-Jacobi equation had also been regarded as indicative of the possibility of some sort of classical reinterpretation of quantum mechanics by Walter Glaser (1906–1960), professor of applied physics at the Technical University of Vienna, in an address delivered early in December 1950 at a meeting of the Austrian Physical Society in Graz.[67] The analogy had been further elaborated, also for the case of electromagnetic forces, by Károly F. Novobátzky[68] of the University of Budapest, who claimed that quantum mechanics differed from classical mechanics only because of the use of an "anticlassical operator statistics." These suggestions, instructive as they were, did not lead to important results or to novel insights.

The new idea introduced by Bohm was to ascribe to every particle of the ensemble ψ a position x (the hidden variables) and a momentum mv, that is, a continuous trajectory which would be well defined if only the initial (or final) position of the particle were known. Moreover, if the initial velocity is determined by $\nabla S/m$ for $x = \xi$ where ξ is the hidden variable the particle's velocity will satisfy the classical law of motion $md^2x/dt^2 = -\nabla(V + U)$ where, in addition to the classical potential $V(x)$, $U(x)$ is the "quantum mechanical potential." To find its explicit form Bohm calculated the total time derivative of the momentum $mv = \text{Re}\{(\hbar/i)\ln\psi\} = P$, that is, $(\partial/\partial t + v \cdot \nabla)P$, and obtained $-\nabla[V - (\hbar^2/2m)\Delta R/R]$, from which

[67] W. Glaser, "Zur Herleitung der Schrödigerschen Wellengleichung," (unpublished).

[68] K. F. Novobátzky, "Das klassische Modell der Quantentheorie," *Annalen der Physik* **9**, 406–412 (1951).

he concluded[69] that

$$U(x)=\frac{-(\hbar^2/2m)\Delta R}{R}. \tag{4}$$

The quantum mechanical potential which, as we see, is independent of the phase S/h but depends on the local value of the density and its first two derivatives and which is not affected by multiplying ψ by a numerical constant factor is the source of the nonclassical motion of the particle. For in the absence of U Newton's law of motion holds. The fact that U is a physically significant quantity, determined by ψ, shows that the wave function ψ likewise represents an objectively real field whose "field equation" is the Schrödinger equation just as the Maxwell equations are the field equations of the electromagnetic field; the ψ-field exerts a force $-\nabla U$ on the particle just as the electromagnetic field affects charges through the Lorentz force.

If the initial position of a particle were known, its trajectory could be determined in one of the two following ways:

1. For a given V the Schrödinger equation is solved and U is calculated in terms of $|\psi|^2$; the initial momentum mv is calculated from (3) and the law of motion is integrated.
2. Alternatively, the first-order differential equation $m\,dx/dt=\nabla S$ is solved for $S=(\hbar/2i)\ln(\psi/\psi^*)$.

Since the initial position cannot be measured by any methods known so far it is a hidden variable; future discoveries of as yet unknown kinds of experiments may well change its methodological status. Bohm's approach, as developed so far, will until then not lead to predictions more detailed

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$$\partial P/\partial t=\mathrm{Re}\left\{\nabla\left(\frac{\hbar}{i}\right)\frac{(\partial\psi/\partial t)}{\psi}\right\}=$$

$$\mathrm{Re}\left\{\nabla\frac{(\hbar^2/2m)\Delta\psi-V\psi}{\psi}\right\}=-\nabla V+\frac{\hbar^2}{2m}\mathrm{Re}\left\{\nabla\frac{\Delta^2\psi}{\psi}\right\}$$

$$=-\nabla V+\frac{\hbar^2}{2m}\nabla\frac{\Delta R}{R}-\nabla\frac{P^2}{2m}.$$

Hence

$$\left(\frac{\partial}{\partial t}+v\nabla\right)P=-\nabla\left(V-\frac{\hbar^2}{2m}\frac{\Delta R}{R}\right)-\frac{1}{m}\sum_{j=1}^{3}P_j\nabla P_j+\frac{1}{m}\sum_{j=1}^{3}P_j\nabla_j P.$$

Since $P_j=\nabla_j S$ the last two terms cancel each other.

than those given by standard quantum mechanics; and since it is based on the Schrödinger equation and the formalism of quantum mechanics its predictions will not be less detailed than those of ordinary quantum mechanics: though *conceptually* at variance with the usual interpretation, it agreed *empirically* with it in all respects.

Anticipating the argument that his approach, being experimentally unverifiable, should be rejected as a piece of "metaphysics," Bohm pointed out, as we have seen, that "the usual interpretation of the quantum theory...involves an assumption that cannot be tested experimentally, viz., that the most complete possible specification of an individual system is in terms of a wave function that determines only probable results of actual measurement processes." But, according to Bohm, his interpretation is not only not inferior but even superior to the usual one for it "provides a broader conceptual framework than the usual interpretation" and thus allows for certain modifications of the formalism which could not even be described in terms of the usual interpretation, but which may lead to the resolution of at present insoluble difficulties (in the domain of dimensions of the order of 10^{-13} cm or less) without impairing the existing agreement with experiment (above that domain).

Realizing that, in contrast to the situation in other field theories, the initial momentum depends on the field ψ, Bohm proposed, as a modification, that the equation $P = \nabla S$, which if it holds at a certain time will hold for all later times, becomes valid only after a relaxation time of the order of 10^{-23} sec; he thus introduced in the law of motion an additive term $f(P - \nabla S)$ which tends to make the difference $P - \nabla S$ decay sufficiently rapidly, say with a mean decay time of the order of $\tau = 10^{-13}/c$, where c is the velocity of light. The initial momentum can then be arbitrary even though it rapidly assumes the value ∇S.

Before proceeding to his theory of measurement Bohm examplified his ideas by a number of applications to special cases. The solution $\psi = \text{const.}\exp[-i(px + Et)/\hbar]$ of the Schrödinger equation for a free particle of energy E shows that $S = px + Et$ and $R = \text{const.}$ so that U vanishes identically and the modified Hamilton-Jacobi equation turns out to be the classical one. If solved in the usual way, it determines the trajectory of the particle in terms of the initial values of the momentum and the position of the particle $x = \pm(2E/m)^{1/2}(t - \beta)$, where β depends on the initial position. Were β known, the exact trajectory could be predicted; but not being measureable by methods known to date that do not destroy the monochromatic momentum character of the solution, β is a hidden variable. Its postulated existence in no way conflicts with the Schrödinger equation but only with its interpretation as embodying all available information about the state of the particle. In the case of a stationary state, where the calculation shows that $S = \text{const.} - Et$ so that the particle velocity is zero, Bohm explained this fact by the assumption that the applied force $-\nabla V(x)$ is balanced by the quantum mechanical force $-\nabla U$. The quantum mechanical potential U also let Bohm explain the notorious double-slit experiment: It affects the particle after its passage through one of the slits in such a way that the probability of entering a given region dx is equal to the usual expression $|\psi(x)|^2 dx$. In particular, where this probability vanishes R becomes zero and U approaches infinity through either positive or negative values, thus either causing an infinite force repelling the particle or affecting it so that "the particle will go through this point with infinite speed, and thus spend no time there."

In Bohm's theory of measurement, for the details of which the reader is referred to the original paper, dynamical variables appear with three distinct meanings:

1. As the true variables of the system in the classical sense, not subject to the indeterminacy relations, but with presently available experimental methods not measurable and hence hidden.
2. As the measured values of observables, subject to the indeterminacy relations and only statistically predictable.
3. As the quantum mechanical expectation values of the former.

Paradoxically, the true classical variables, because of the peculiar quantum mechanical potential U, satisfy nonclassical equations of motion, whereas the expectation values, as shown earlier by Ehrenfest, satisfy classical equations and the measured observables, finally, satisfy no equations of motion at all.

That these three meanings should not be confused was shown by Bohm in his treatment of a free particle contained in a one-dimensional box, that is, between two impenetrable and perfectly reflecting walls, separated by a distance L. In this case $\psi = \sin(2\pi nx/L)\exp(-iEt/\hbar)$, where $E = (1/2m)(nh/L)^2$ with integral n. Since $\nabla S = 0$ the particle is at rest in spite of $E \neq 0$. This apparent contradiction is resolved by Bohm on the grounds that the quantum mechanical potential U is here precisely equal to the total energy E so that the potential energy of the interaction with the objectively real field ψ absorbs the entire kinetic energy of the particle. The true momentum, in the sense (1), is consequently zero. However, when we measure the momentum observable, in the sense (2), for instance by removing the walls suddenly and dividing the distance covered by the time of transit, this removal, according to Bohm, will change the real field and through it the particle's momentum. Not knowing the initial position of the particle, we can only deduce from the probability density that the particle must be contained in one of the two wave packets that emerge in opposite directions upon removal of the walls. Its momentum turns out to be very close to $\pm nh/L$, the sign depending on which packet it is contained in. Using other methods of observing the particle velocity will likewise lead to the same result as that predicted by the usual interpretation. The measuring process affects the quantum mechanical potential in such a way that the formerly stored kinetic energy E will be released. The expectation value of the momentum is in this case equal to its true value.

Finally, in his discussion of the Einstein-Podolsky-Rosen thought-experiment Bohm pointed out that the fact that the particle not directly affected mechanically has either a sharp position or a sharp momentum, depending on what measurement has been performed on its partner, has nothing paradoxical in it, for classical particles always have sharp values of position and momentum. What has to be explained, however, is the fact that a position measurement on the first particle disturbs the momentum of the second, and so on. Bohm accounted for it by stating that as soon as such a measurement is performed uncontrollable fluctuations occur in the field and in the potential of the entire system, causing corresponding fluctuations in the momenta. It is U which transmits instantaneous disturbances from one particle to the other through the medium of the field. Since this disturbance does not increase information it is not a signal and hence its speed, although exceeding that of light, does not violate the principles of relativity.

Soon after completing the manuscript of the first part of his paper Bohm realized that some of his ideas had already been expressed by Louis de Broglie, in his early theories of the double solution and pilot wave, and also by Nathan Rosen[70] ina paper published in 1945. It is interesting to find out why Rosen, then at the University of North Carolina, did not work out a full-fledged theory like Bohm's. Essentially, Rosen's approach, up to the modified Hamilton-Jacobi equation, was identical with that of Bohm; he also introduced the quantum mechanical potential which he called "the quantum mechanical correction." But when he applied this formalism to the case of a free particle in a one-dimensional box and obtained the paradoxical result that the velocity vanishes, a conclusion which, in similar cases, contradicts "our concepts of angular momentum and polarization and their relation to the selection rules for the emission of radiation," Rosen abandoned the interpretation of the wave function in terms of particle trajectories in favor of Bohr's complementarity principle.

According to Bohm this problem of the free particle in the box also misled Einstein in his previously mentioned contribution[71] to the Born volume. There is no experimental evidence, Bohm argued, which could

[70]N. Rosen, "On waves and particles," *Journal of the Elisha Mitchell Scientific Society* **61**, 67–73 (1945).
[71]Ref. 6–111.

support Einstein's claim that the motion of macroscopic systems approach those predicted by classical mechanics. At best Einstein's claim can be regarded as a new principle or "enlarged correspondence principle," as Bohm called it, according to which "all microscopic theories must always become identical with previously accepted macroscopic theories, when one considers sufficiently large dimensions." Bohm showed by various examples, however, that such a principle does not generally hold in physics.[72]

Before concluding our outline of Bohm's early work on hidden variables we wish to emphasize one of its features which has been almost universally ignored. In Bohm's theory of measurement the hidden variables under discussion were assumed to depend not only on the state of the observed system but also on that of the measuring device. Since such a class of hidden variables had not been considered by von Neumann, Bohm could claim that von Neumann's proof of the inconsistency of hidden variables with the quantum theory did not apply. In fact, Bohm fully agreed with von Neumann's conclusion that "it would be inconsistent with the usual rules of calculating quantum-mechanical probabilities to assume that there were in the observed system a set of hidden parameters which simultaneously determined the results of measurement [of two noncommuting observables such as] position and momentum." In his hidden variable theory, Bohm pointed out, the observables, as he had shown, are not properties belonging to the observed system alone but rather potentialities whose precise development depends just as much on the observing apparatus as on the observed system. The statistical distribution of the variables will therefore vary "in accordance with the different mutually exclusive experimental arrangements of matter that must be used in making different kinds of measurements. "In this point," Bohm declared, "we are in agreement with Bohr, who repeatedly stresses the fundamental role of the measuring apparatus as an inseparable part of the observed system."[73]

These statements of Bohm's in Part II of his paper go far beyond those made in the introduction to his paper as quoted above. Whereas Part I, on the whole, appears to be written in the spirit of the early proponents of hidden variables who searched for a mechanistic and deterministic interpretation of quantum phenomena—as the reader may easily check by rereading those statements—Part II with its emphasis, probably under the impact of Pauli's criticisms, on the dependence of the hidden variable probability measures upon the kind of observables measured, that is, on

[72]D. Bohm, "A discussion of certain remarks by Einstein on Born's interpretation of the ψ-function," *Born volume* (Ref. 6–111), pp. 13–19.
[73]Ref. 65, pp. 187–188.

the integral relationship between the system and the measuring device, is obviously a definite *rapprochement*[74] with Bohr's "feature of wholeness." Bohm's theory of measurement, as we see, has made his hidden variable approach, even if we completely ignore its suggested nonlinear modifications, incompatible with the conditions of Definition II.

The early reactions to Bohm's paper, as could have been expected, were generally unfavorable. In fact, it was severely criticized even before it appeared in print, and by someone whom Bohm least expected to be critical. In the summer of 1951, more than six months prior to the publication, Bohm had sent a preprint to de Broglie, whose early theories, as we have mentioned, Bohm realized were somewhat akin to his own. It thus happened that at the meeting of the French Academy of Sciences on September 17, 1951, de Broglie referred to Bohm's work, which he regarded essentially as a revival of his own ideas, ideas which he had rejected long ago. For the same reasons, that is, primarily because the wave function, being a function in $3n$-dimensional configuration space and hence merely a fictitious one, cannot be assumed to regulate the motion of the particle, he called Bohm's work untenable, adding "...le travail de M. Bohm...me paraît toujours se heurter à des difficultés insurmontables, principalement en raison de l'impossibilité d'attribuer à l'onde ψ une realité physique."[75]

Not much later, however, de Broglie's attitude toward Bohm's work became more sympathetic, for Jean-Pierre Vigier, who was then working at the Institut Henri Poincaré on unified theories in general relativity (he later specialized on causal theories in quantum mechanics),[76] had drawn de Broglie's attention to the striking similarities that existed between the theory of double solution and certain studies in general relativity, especially those carried out by Georges Darmois and by Einstein and Grommer, in which the motion of a particle was also regarded, as in the theory of double solution, as that of a singularity of the field.[77] After that

[74]For the still remaining important differences between hidden variable theories, compatible with Bohr's "feature of wholeness," and Bohr's complementarity interpretation see J. Bub, "What is a hidden variable theory of quantum phenomena?," *International Journal of Theoretical Physics* **2**, 101–123 (1969).

[75]L. de Broglie, "Remarques sur la théorie de l'onde pilote," *Comptes Rendus* **233**, 641–644 (1951); reprinted in L. de Broglie, *La Physique Quantique restera-t-elle indéterministe?* (Gauthier-Villars, Paris, 1953), pp. 65–69.

[76]J.-P. Vigier's thesis "Structure des micro-objets dans l'interpretation causale de la théorie des quanta," (Paris, 1954) was published in 1956.

[77]Cf. J.-P. Vigier, "Introduction géometrique de l'onde pilote en théorie unitaire affine," *Comptes Rendus* **233**, 1010–1012 (1951) and L. de Broglie, "Remarques sur la note précedente de M. Vigier," *ibid.*, 1012–1013.

de Broglie attempted to resuscitate his earlier ideas which, as we remember, he had given up primarily because of their unfavorable acceptance at the 1927 Solvay Congress. Though not agreeing with Bohm in all details he now welcomed the similarity of Bohm's approach to his own.

The earliest criticism leveled against Bohm's paper after its publication was that by Otto Halpern,[78] who argued that Bohm did not prove the possibility of a mechanical interpretation because $S(x)$, as defined by Bohm, does not depend on f nonadditive integration constants as required in order to be a solution in the Hamilton-Jacobi theory and that it had not been shown that the treatment could be relativistically generalized to include spin. In his rebuttal[79] Bohm promised he would soon publish a paper in which the causal interpretation would be extended to the Dirac equation. The first positive reaction came from Saul Theodore Epstein, a graduate and Ph.D. of M.I.T. and at that time a lecturer at Boston University. Epstein[80] suggested a reformulation of Bohm's interpretation in terms of a momentum representation or of other representations intermediate between the coordinate and the momentum representation. Although Bohm[81] expressed his disagreement with Epstein's proposal, it was the first indication that Bohm's ideas were seriously considered as an alternative interpretation of quantum mechanics.

By a method similar to those used in classical statistical mechanics Bohm[82] had meanwhile succeeded in proving his conjecture that as a result of random collisions an arbitrary initial probability density will ultimately decay into one with a density of $|\psi|^2$, a relation which once established will be maintained for all time by virtue of the equations of motion of the particles. Although all quantum mechanical experiments carried out so far have been concerned with ensembles of systems that had been colliding with other systems for a time sufficiently long to establish this relation Bohm was able to suggest conditions in which the density still differs from $|\psi|^2$ to such an extent that it may be experimentally testable. It was the first proposal of an empirical verification of a hidden variable theory.

[78] O. Halpern, "A proposed re-interpretation of quantum mechanics," *Physical Review* **87**, 389 (1952).

[79] D. Bohm, "Reply to a criticism of a causal re-interpretation of the quantum theory," *Physical Review* **87**, 389–390 (1952).

[80] S. T. Epstein, "The causal interpretation of quantum mechanics," *Physical Review* **89**, 319 (1952).

[81] D. Bohm, "Comments on a letter concerning the causal interpretation of the quantum theory," *Physical Review* **89**, 319–320 (1952). But see also S. T. Epstein, "The causal interpretation of quantum mechanics," *Physical Review* **91**, 985 (1953).

[82] D. Bohm, "Proof that probability density approaches $|\psi|^2$ in causal interpretation of the quantum theory," *Physical Review* **89**, 458–466 (1953).

That Bohm's contention of the derivability of the equality between the density distribution and $|\psi|^2$ is of decisive importance for the question of whether his suggested interpretation is an ordinary statistical mechanics of a deterministic theory or not has been pointed out by Joseph Bishop Keller,[83] a professor of mathematics at the Courant Institute of Mathematical Sciences at the University of New York. His lucid mathematical analysis of Bohm's theory showed that if this relation between these two functions (which, though satisfying the same differential equation but with independent initial conditions, need therefore not be necessarily equal) had to be postulated separately, probability would enter the theory not only through incomplete knowledge of the initial data but in a much deeper way: the initial field in an ensemble of experiments would be determined by the nature of the ensemble itself; only if the relation could be derived from the postulates of the theory, as claimed by Bohm, would the status of probability be the same as in ordinary statistical mechanics.

Criticisms similar to those voiced by Halpern and Keller, in addition to criticisms of a more philosophical nature, were leveled at Bohm's theory in a 40-page long essay by Takehiko Takabayasi,[84] a graduate of Tokyo University and professor at Nagoya University who began his academic career in colloid chemistry and high polymers but soon became interested in the history and philosophy of physics and decided finally to specialize in theoretical physics. He rejected Bohm's theory on the grounds that it could not be extended into a relativistic field theory of the electron, that it could be developed only in the space-time representation of the wave function and therefore would not satisfy the invariance requirements under unitary transformations, that the peculiarities of the ψ-field would impair its adequacy as a satisfactory model for quantum phenomena, and that the proposed hidden variables would become physically significant only through a modification of the Schrödinger equation or law of motion, a modification which is "artificial and improbable."

In his reply Bohm[85] extended his theory, as earlier promised, to the Dirac equation and gave various arguments for the retention of causality as long as no positive proof is available of its inconsistency with experimental facts. Concerning the specific criticism that the interpretation conflicts with the transformation theory of quantum mechanics Bohm observed that this transformation theory is largely a mathematical su-

[83] J. B. Keller, "Bohm's interpretation of the quantum theory in terms of 'hidden' variables," *Physical Review* **89**, 1040–1041 (1953).

[84] T. Takabayasi, "On the formulation of quantum mechanics associated with classical pictures," *Progress of Theoretical Physics* **8**, 143–182 (1952).

[85] D. Bohm, "Comments on an article of Takabayasi concerning the formulation of quantum mechanics with classical pictures," *Progress of Theoretical Physics* **9**, 273–287 (1953).

perstructure of only limited physical significance; the physically meaningful observables—position, momentum, angular momentum, dipole and quadrupole moment, energy, and spin—are all included within the causal interpretation; moreover, it would be inconsistent to insist on the transformation theory and to reject hidden variables, for accepting the former means accepting the postulate that all Hermitian operators are observable and hoping that future experiments will bear this out, while rejecting the latter means rejecting a similar hope.

Continuing our summary of the acceptance of Bohm's ideas we should point out that Bohm's proposal of modifying the theory by the inclusion of a nonlinear term in the law of motion or the Schrödinger equation, that is, by modifying quantum theory into a nonlinear theory, was likely to be welcomed by those who for ideological reasons rejected Bohr's complementarity principle. Since the interference of probability waves, as in the double-slit experiment, is one of the basic arguments for complementarity and wave-particle duality, since this interference is a consequence of the superposition principle, and since such a principle can be established only within the framework of a linear theory, it will be understood that the proposal of nonlinearity has far-reaching philosophical implications. In fact, encouraged by the nonlinear field theories of Mie, Einstein, Born, and Infeld[86] as well as by the fact that the conventional quantum field theory, based on linear quantum mechanics, faces serious conceptual difficulties (divergences), Soviet physicists, and among them most prominently Yakov P. Terletskii,[87] had already searched for a nonlinear formulation of quantum mechanics before Bohm published his paper. It is therefore not surprising that the first favorable criticisms of Bohm's ideas came from those sources who for one reason or another sympathized with such ideological considerations.

The first laudatory criticism of Bohm's work was written by Evry Schatzman, an astrophysicist (Ph.D., Sorbonne, 1947) by profession but greatly interested in the philosophy of physics. In an article[88] published in

[86]Cf. M. Jammer, *Concepts of Mass in Classical and Modern Physics* (Harvard University Press, Cambridge, Mass., 1961; Harper and Row, New York, 1964; Wissenschaftliche Buchgesellschaft, Darmstadt, 1964; Progress, Moscow, 1967; Feltrinelli, Milano, 1974), Chapter 14.

[87]Y. P. Terletskii, "Problemi rascitija kvantovoj teorii," *Voprossy Filosofii* **1951** (5), 51–61; "Probleme der Entwicklung der Quantentheorie," *Sowjetwissenschaft (Naturwissenschaftliche Abteilung)* **5**, 597–608 (1952). Cf. also R. Wahsner, "J. P. Terletzkis Determinismusauffassung in der modernen Quantenphysik," *Deutsche Zeitschrift für Philosophie* **10**, 1019–1032 (1962).

[88]E. Schatzman, "Physique quantique et realité," *La Pensée* **42-43**, 107–122 (1952); German translation "Quantenphysik und Realität," *Deutsche Zeitschrift für Philosophie* **2**, 621–641 (1954).

La Pensée, a bimonthly edited by Paul Langevin and Georges Cogniot,[89] Schatzman emphasized the conceptual advantages of de Broglie's theory of pilot waves and of Bohm's suggestion of a nonlinear theory which he called "a decisive progress." It was soon followed by another acclamatory review in the 1953 spring issue of the quarterly *Science and Society*, an independent journal of Marxism, written by Hans Freistadt,[90] at that time an assistant professor at the Newark College of Engineering.

Bohm's interpretation, wrote Freistadt, is from the philosophical point of view less restrictive on the scope of physics. Freistadt, in full agreement with Bohm, declared that the "doctrinaire assumption" of the Copenhagen interpretation, according to which ψ is the most complete description ever attainable, "goes far beyond what is warranted by experiment, [and] is not likely to spur original thought and new, bold hypotheses." Three years later Freistadt[91] published a survey of causal theories known at that time which offers a profound analysis of the conceptual and mathematical foundations of these theories and may still be regarded today as one of the best expositions on this subject. Freistadt, in particular, praised Bohm's theory of the process of measurement as conceptually superior to that of the usual interpretation.

It should be mentioned in this context that any ideological motive for the establishment of a deterministic quantum theory, such as Bohm's, in reaction to the alleged positivism or idealism of the probabilistic interpretation was condemned by no one more severely than by the Russian physicist Vladimir Fock. "To force upon nature," he declared, "a deterministic form of laws, to renounce, discarding all evidence, the possibility of a more general probabilistic form of these laws—means to start up from some kind of dogma, but not from the properties of nature itself. Such a position is philosophically incorrect."[92] Conceiving causality not in the restricted sense of Laplacian determinism but rather as "a statement on the existence of laws of nature and in particular of those laws which are

[89]Langevin, apart from his scientific work at the Collège de France, where he was the successor of Pierre Curie, served also as president of the Ligue des Droits de l'Homme. Cogniot, professor agrégé (Lettres) at the Sorbonne, was a member of the Central Committee of the Communist Party in France.

[90]H. Freistadt, "The crisis in physics," *Science and Society* **17**, 211–237 (1953). On the "Freistadt Case" see *Bulletin of the Atomic Scientist* **5**, 169 (1949).

[91]H. Freistadt, "The causal formulation of quantum mechanics of particles," *Nuovo Cimento* **5**, Supplementary volume, 1–70 (1957).

[92]V. Fock, "Ob interpretatzii kvantovoj mekhaniki," *Uspekhi Fisičeskikh Nauk* **62**, 461–474 (1957); "On the interpretation of quantum mechanics," *Czechoslovak Journal of Physics* **7**, 643–656 (1957); "Über die Deutung der Quantenmechanik" in *Max Planck Festschrift* (Deutscher Verlag der Wissenschaften, Berlin, 1958), pp. 177–195.

connected with the general properties of space and time," Fock, like Ullmo, regarded the causality principle as fully consistent with quantum mechanics where its range of applications is merely extended to probability laws. Hence, he concluded, the probabilistic interpretation in no way comes into conflict even with the materialistic point of view.

An interesting side-effect of Bohm's work was the renewed interest in von Neumann's proof of the impossibility of hidden variables. Even if objectionable on other grounds, Bohm's work seemed to be logically consistent and hence to contradict this proof. Bohm's resolution of this contradiction was not always regarded as convincing. It seemed therefore worthwhile to look again into the logic of von Neumann's proof.

The idea that von Neumann's proof does not actually refer so much to quantum mechanics as such but rather to its underlying theory of probability or statistics, an idea which, as we shall see, was also independently brought forth in the early development of the stochastic interpretations of quantum mechanics and in particular by Imre Fényes, was given prominence especially by Paul K. Feyerabend,[93] who at that time taught philosophy at the University of Bristol. He first pointed out that the two theorems on which the proof is based—the nonexistence of dispersion-free ensembles and the existence of homogeneous ensembles—are not characteristic of quantum mechanics as such but rather of statistical theories in general. Following a suggestion made by Popper, Feyerabend cited the example of throwing dice. The first theorem, he argued, would simply mean that in no infinite sequence of throws (of one die) the same face would invariably appear; the second theorem would mean that no gambling system, however cleverly devised, would produce subensembles having a frequency distribution that differs from that of the original ensemble, that is 1/6 for each face.

Were von Neumann's thesis a logical conclusion of these two theorems it should be impossible to account for the throwing of dice by the addition of unobservable parameters in terms of a deterministic theory, contrary to what is commonly agreed upon. It thus follows, according to Feyerabend, that von Neumann's reasoning is not based on those two theorems alone but, in addition, on the surreptitiously introduced assumption that a deterministic description must always make it possible to construct dispersion-free ensembles. This assumption, however, is not justified since

[93] P. K. Feyerabend, "Eine Bemerkung zum Neumannschen Beweis," *Zeitschrift für Physik* **145**, 421–423 (1956). Twelve months later, in April 1957, Feyerabend himself renounced this paper as being based on a serious mistake. Cf. footnote 6 in his article "On the quantum theory of measurement" in *Observation and Interpretation in the Philosophy of Physics* (Ref. 102, below), pp. 121–130.

determinism asserts an unambiguous definition of events in their temporal sequence or their consecutive evolvement but not in their simultaneous distribution at a given time. Since the hypothesis of hidden variables is in no way confuted by the correctly proved nonexistence of dispersion-free ensembles (hidden variables may exist but also scatter) von Neumann, according to Feyerabend, committed the fallacy of a *non sequitur*.

Feyerabend's rejection of the foregoing assumption was questioned, in turn, by Pietro Bocchieri and Angelo Loinger,[94] two nuclear physicists at the University of Pavia and the National Institute of Nuclear Physics in Milan. As long as the throwing of dice is described only in terms of the theory of probability, they pointed out, that is, as long as it is assumed that the small impacts exerted on the die are uncontrollable, it is obviously impossible to construct a dispersion-free ensemble. In a mathematical deterministic description, however, the effects of such impacts have to be known in all detail, for otherwise such a description would be impossible; but then it must be possible, by an appropriate regulation of the throwing process, to produce a dispersion-free ensemble.

Analyzing von Neumann's proof from a different point of view Irving I. Zinnes[95] of the University of Oklahoma came to the conclusion "that the theorem is correct only if one requires that these hidden variables merely enlarge the set of observables employed at present in quantum mechanics. The theorem cannot be interpreted to mean that one cannot introduce such additional variables in order (1) to specify a state with classical completeness and (2) to define the usual observables as statistical parameters."

That von Neumann's reasoning contains a logical gap was charged by Gerhard Schulz,[96] a mathematician at the Institute of Optics and Spectroscopy at the German Academy of Sciences in Berlin (Adlershof), who later turned to philosophy. Calling subensembles for which von Neumann's statistical formula (as proved in the first part of the proof) has an experimental meaning, that is, for which the expectation value can be experimentally measured, "experimentally accessible subensembles," Schulz claimed that von Neumann considered only "experimentally accessible subensembles," but not all conceivable subensembles as he should

[94]P. Bocchieri and A. Loinger, "Einige Bemerkungen über die Frage der verborgenen Parameter," *Zeitschrift für Physik* **148**, 308–313 (1957).

[95]I. I. Zinnes, "Hidden variables in quantum mechanics," *American Journal of Physics* **26**, 1–4 (1958).

[96]G. Schulz, "Kritik des v. Neumannschen Beweises gegen die Kausalität in der Quantenmechanik," *Annalen der Physik* **3**, 94–104 (1959).

have done. In 1960 Jesus Tharrats[97] criticized the proof on two grounds. In the first place, he declared, the condition $(\varphi, U\varphi)=0$ or 1 for all φ has as solutions not only $U=0$ or $U=1$, as stated by von Neumann, but also $U=P_\psi=|\psi><\psi|$, where ψ is a vector in an orthonormal set containing φ, a solution that invalidates the whole proof. That this objection is mistaken has been explained by James Albertson[98] on the ground that for $U=P_\psi$ the condition would not be satisfied for *all* φ, "but only for those φ belonging to an orthonormal set containing ψ." But neither is Albertson's reasoning correct.

Both Albertson and Tharrats seem to have ignored one point: Due to the facts[99] that $(\varphi, U\varphi)$ is continuous in φ and that two normalized vectors φ_0 and φ_1 in Hilbert space can always be connected by a continuously varying normalized vector $\varphi(x)$ such that $\varphi(0)=\varphi_0$ and $\varphi(1)=\varphi_1$, the condition implies that $(\varphi, U\varphi)$ is a constant for given U, that is, it is either 0 for all φ or it is 1 for all φ, but not 0 for $\varphi \perp \psi$ and 1 for $\varphi=\psi$. Tharrats second objection that the property "to be dispersion-free" has a meaning only for observables and that therefore the equation $\mathrm{Tr}(UR^2)=[\mathrm{Tr}(UR)]^2$ does not admit the substitution P_φ of R since projection operators do not represent observables, seems in fact to be justified as far as von Neumann's terminology is concerned.[100] This inconsistency, however, can be eliminated[101] by defining, with Dirac, observables as Hermitian operators whose eigenvectors form complete sets. Since for an arbitrary ψ $\psi=P_\varphi\psi+(1-P_\varphi)\psi$ is an expansion of ψ in the eigenvectors $P_\varphi\psi$ (with eigenvalue 1) and $(1-P_\varphi)\psi$ (with eigenvalue 0) of P_φ, the projection operator P_φ has a complete set of eigenvectors and is consequently (in the sense of Dirac) an observable.

Another phase of the battle *pro* and *contra* the legitimacy of hidden variables was fought out at the Ninth Symposium of the Colston Research

[97]J. Tharrats, "Sur le théorème de von Neumann concernant l'indéterminisme essentiel de la Mécanique quantique," *Comptes Rendus* **250**, 3786–3788 (1960).

[98]J. Albertson, "Von Neumann's hidden-parameter proof," *American Journal of Physics* **29**, 478–484 (1961).

[99]For a proof put $\varphi_1=a\varphi_0+b\varphi'$ with $\varphi'\perp\varphi_0, a={}^{i\alpha}\cos\vartheta, b=e^{i\beta}\sin\vartheta$. Then $\varphi(x)=a_x\varphi_0+b_x\varphi'$ with $a_x=e^{ix\alpha}\cos x\vartheta, b_x=e^{ix\beta}\sin x\vartheta$ is the required "connecting" vector which satisfies $\varphi(0)=\varphi_0$ and $\varphi(1)=\varphi_1$.

[100]Von Neumann's terminology distinguishes between operators representing "physical quantities" and projections representing "properties" or "propositions" (yes-no experiments). Cf., e.g., "Apart from the physical quantities $\mathfrak{R}$, there exists another category of concepts that are important objects of physics—namely the properties of the states of the system S." Ref. 1-2. (1955, p. 249).

[101]See Ref. 98. We ignore, as usual, systems with superselection rules.

Society,[102] held at the University of Bristol in the beginning of April 1957. This Society, established as a memorial to the seventeenth-century educator and philanthropist Edward Colston, decided to devote the annual sysmposium to an exchange of views among physicists such as D. Bohm, F. Bopp, Sir Charles Darwin, M. Fierz, H. J. Groenewold, L. Rosenfeld, G. Süssmann, J.-P. Vigier and philosophers such as A. J. Ayer, R. B. Braithwaite, P. K. Feyerabend, W. B. Gallie, W. C. Kneale, S. Körner, M. Polanyi, K. R. Popper, G. Ryle, primarily on the interpretative problems of quantum mechanics.

One of the highlights of the meeting was the second session in which Bohm with his talk, "A Proposed Explanation of Quantum Theory in Terms of Hidden Variables at a Sub-Quantum-Mechanical Level," confronted Rosenfeld, who spoke on "Misunderstandings about the Foundations of Quantum Theory." Whereas Bohm insisted on the need of a new kind of theory, "more nearly determinate than the quantum theory," to which it approaches as a limiting case, but from which it differs essentially at a deeper level where it would even predict qualitatively new properties of matter, Rosenfeld regarded such an approach as "a short-lived decay-product of the mechanistic philosophy of the nineteenth century" and any suggestion for circumventing the nonclassical character of quantum mechanics by the introduction of hidden parameters as "empty talk." He argued that "such parameters, in order to be of any use, should be linked with observable quantities, i.e. with classical concepts: and this link, whatever it is, could not possibly violate the restrictions in the use of the latter concepts imposed by the existence of the quantum of action."

Bohm explained his ideas about how quantum mechanical fluctuations at a subquantum level may produce the observed Heisenberg indeterminacies, just as the complicated atomic motions produce the erratic indeterminacies of a Brownian particle, and how the assumption of such a subquantum level makes it no longer possible to use the Heisenberg principle as an argument against the possibility of a theory more determinate than the existing quantum theory. "The same," he continued, "can be said of von Neumann's arguments, which are certainly logically consistent, but which depend in an essential way on the assumption that the general quantum-mechanical framework of law is valid universally and without approximation. What the introduction of a sub-quantum level does is then to contradict the basic assumptions on which the arguments of Heisenberg and von Neumann are founded." Rosenfeld, in contrast,

[102] *Observation and Interpretation in the Philosophy of Physics—With special reference to Quantum Mechanics*, S. Körner, in collaboration with M. H. L. Pryce, eds. (Constable and Company, London, 1957; Dover Publications, New York, 1962).

pointed out that there is "no logical justification for requiring a 'deterministic substratum' to a given set of statistical laws. There may or may not be such a substratum: this is a question to be decided by experience, not by metaphysics." The peculiar wholeness of quantal processes, he went on, which implies the reciprocal lack of determination of canonically conjugate quantities, is an immediate consequence of the physical content of the law of quantification of action. "No logically consistent formal device can therefore produce a deterministic substratum without doing violence to the immense body of experience embodied in the quantum laws.... The true road to future progress in this domain is clearly marked: the new conceptions which we need will be obtained not by a return to a mode of description already found too narrow, but by a rational extension of quantum theory."[103]

And so the controversy continued—without reaching any agreement. The reason for this inconclusiveness was, according to Fritz Bopp, that "we say that Bohm's theory cannot be refuted, adding, however, that we don't believe in it." Summarizing the debate Bopp commented: "The task of physics has been defined by saying that it predicts the possible results of experiment. But what we have done today was predicting the possible development of physics—we were not doing physics but metaphysics."[104]

7.6. THE WORK OF GLEASON, JAUCH, AND OTHERS

While philosophical arguments for and against the possibility of hidden variables were debated in England (Bristol), in New England (Cambridge, Massachusetts) a mathematical result was obtained which turned out to be extremely important for the whole complex of these questions. The problem of how to construct in a unified way all measures (i.e., nonnegative completely additive real-valued functions) on the (closed) subspaces of a separable Hilbert space had already intrigued von Neumann in connection with his proof[105] of the impossibility of hidden variables, which was based on the assumption that all possible statistical states in quantum mechanics are either pure states or mixtures, that is, countable (finite or denumerable) convex combinations of pure states. When in 1957 George Whitelaw Mackey, a mathematician at Harvard University, tried to establish his quantum logical approach, which will be discussed in the next chapter in full detail, on mathematically rigorous foundations he realized how urgent

[103] *Ibid.*, p. 42.
[104] *Ibid.*, p. 51.
[105] Ref. 35.

it was to provide an unimpeachable proof of this assumption and proposed this problem to his collaborators and students.

Within a relatively short time Mackey's colleague Andrew Mattel Gleason, five years his junior, a California-born graduate of Yale University (B.S., 1942) and postgraduate at Harvard, where he obtained his professorship in 1957, solved the problem by proving[106] that in a separable real or complex Hilbert space $\mathcal{H}$ of more than two dimensions every measure $\mu(A)$ on the subspaces of $\mathcal{H}$ has the form $\mu(A)=\text{trace}(TP_A)$, where P_A denotes the orthogonal projection on the subspace A and T is a fixed operator of trace class. In other words, Gleason proved that every measure on the subspaces of $\mathcal{H}$ is a linear combination of $\|P_A\varphi\|^2$, where φ is a unit vector and P_A is the orthogonal projection on the subspace A. The proof, most cleverly contrived, was based on the notion of a "frame function of weight W," that is, a real-valued function f which, defined on the unit sphere of $\mathcal{H}$, satisfies $\Sigma f(\varphi_i)=W$, where $\{\varphi_i\}$ is an orthonormal basis of $\mathcal{H}$, and on the demonstration that every nonnegative frame function on $\mathcal{H}$ is regular, that is, that there exists a self-adjoint operator T on $\mathcal{H}$ such that $f(\varphi)=(\varphi, T\varphi)$ for all unit vectors φ.

Gleason's theorem simplified the axiomatics of quantum mechanics since it showed that quantum mechanical probabilities can always be calculated by means of the density matrix. It established von Neumann's statistical formula (1) on fewer assumptions for it showed that if in $\mathcal{H}$ with each projection P a nonnegative number $\langle P\rangle$ is associated such that $\langle I\rangle=1$ and $\langle\Sigma P_i\rangle=\Sigma\langle P_i\rangle$ for mutually orthogonal P_i, then there exists a unique semidefinite self-adjoint operator U such that, for every projection P, $\langle P\rangle=\text{trace}\,(UP)$.

Through Gleason's work[107] it became clear that von Neumann's result concerning the impossibility of hidden variables does not hinge on Postulate IV (P.IV, Section 7.4 above) that is, on the additivity assumption for the expectation values of any operators, even if they do not commute. It now became clear that in Hilbert spaces of at least three dimensions it suffices to postulate such an additivity for commuting operators *alone* in order to exclude the possibility of dispersion-free states.

To prove this contention (we shall give only an outline of the proof) it

[106] A. M. Gleason, "Measures on the closed subspaces of a Hilbert space," *Journal of Mathematics and Mechanics* **6**, 885–893 (1957), reprinted in *The Logico-Algebraic Approach to Quantum Mechanics*, C. A. Hooker, ed. (Reidel, Dordrecht-Holland, forthcoming), Vol. 1.

[107] Part of the proof of Gleason's theorem has been simplified by C. Piron, "Survey of general quantum physics" (mimeographed, University of Geneva, 1970) *Foundations of Physics* **2**, 287–314 (1972), reprinted in *The Logico-Algebraic Approach*, Ref. 106, by S. P. Gudder (unpublished) and by J. S. Bell (see Ref. 118), whom we mainly follow.

suffices to consider projection operators. If $\{\alpha_j\}_{j=1,2\ldots}$ is a complete set of orthogonal vectors, the projectors $P(\alpha_j)$, defined by $P(\alpha_j)\varphi = \|\alpha_j\|^{-2}(\alpha_j,\varphi)\alpha_j$, commute with each other and satisfy

$$\sum_j P(\alpha_j) = I \qquad \text{(identity operator)}.$$

According to our assumption on the additivity of commuting operators, which will be referred to as *the Restricted Postulate IV* (or P.IV′) for a given state ψ,

$$\left(\psi, \sum_j P(\alpha_j)\psi\right) = \left\langle \sum_j P(\alpha_j)\right\rangle = \sum_j \langle P(\alpha_j)\rangle.$$

Hence since

$$\left\langle \sum_j P(\alpha_j)\right\rangle = \langle I\rangle = 1,$$

we obtain

$$\sum_j \langle P(\alpha_j)\rangle = 1. \tag{5}$$

It is now easy to prove, with the help of (4), the following two lemmata.

LEMMA 1. If α_1 and α_2 are orthogonal vectors, $\langle P(\alpha_1)\rangle = 1$ implies $\langle P(\alpha_2)\rangle = 0$.

LEMMA 2. If α_1 and α_2 are orthogonal vectors and for a given state $\langle P(\alpha_1)\rangle = \langle P(\alpha_2)\rangle = 0$, then $\langle P(a\alpha_1 + b\alpha_2)\rangle = 0$ for all real a, b.

The proof, in both cases, is based on the nonnegativity of the expectation values and on the fact that two orthogonal vectors can always be regarded as members of a complete orthogonal set. It is more difficult to prove[108] Lemma 3.

LEMMA 3. If α and β are vectors in a Hilbert space of at least three dimensions and for a given state $\langle P(\alpha)\rangle = 1$ and $\langle P(\beta)\rangle = 0$, then $\|\alpha - \beta\| > 1/2$.

[108] Put $\beta = \alpha + s\alpha'$ where the normalized α' is normal on α and s is real. By assumption there exists a unit vector α'' normal on α and α' and hence on β. By Lemma 1 $\langle P(\alpha')\rangle = \langle P(\alpha'')\rangle = 0$, and hence by Lemma 2 for arbitrary s, t, $\langle P(ts\alpha'' - s\alpha')\rangle = 0$ and therefore also $\langle P(\beta + t^{-1}s\alpha'')\rangle = 0$. Since $\gamma_1 = \beta + t^{-1}s\alpha''$ and $\gamma_2 = ts\alpha'' - s\alpha'$ are orthogonal vectors Lemma 2 shows that $\langle P(\gamma_1 + \gamma_2)\rangle = \langle P(\alpha + s(t + t^{-1})\alpha''\rangle = 0$. Now, were $|s| \leqslant 1/2$ there would exist a real t such that $s(t + t^{-1}) = \pm 1$ and $\langle P(\alpha + \alpha'')\rangle = \langle P(\alpha - \alpha'')\rangle = 0$. Since $\gamma_1' = \alpha + \alpha''$ and $\gamma_2' = \alpha - \alpha''$ are orthogonal, Lemma 2 implies that $\langle P(\gamma_1' + \gamma_2')\rangle = \langle P(2\alpha)\rangle = \langle P(\alpha)\rangle = 0$, contrary to hypothesis. Hence $|s| > \frac{1}{2}$ and $\|\alpha - \beta\| > \frac{1}{2}$.

Finally, we can prove the theorem: The Restricted Postulate IV′ precludes the possibility of dispersion-free states (and hence of hidden variables). For a dispersion-free state the expectation value of a projector is 1 or 0. The set of all vectors in the Hilbert space can consequently be exhaustively divided into two classes, one for which the expectation value is 1 and the other for which the value is 0. By an argument similar to that outlined in footnote 99 above it can be shown that there exist two vectors, one of the first class and the other of the second, such that their difference is less than $\frac{1}{2}$, contrary to Lemma 3. Hence dispersion-free states cannot exist.

If, in particular, in a Hilbert space there are three Hermitian operators A, B, and C such that A commutes with B as well as with C, but B does not commute with C (e.g., the operators L^2, L_x, and L_y in the theory of angular momentum), then no single phase space function $f_A(\lambda)$ can exist, in the sense of Definition II, which offers a deterministic hidden variable description of the measurement of the observable represented by A. In other words, under these conditions hidden variables, if they exist, must be contextual.

The first to realize the importance of Gleason's result for the problem of hidden variables was probably Josef Maria Jauch, a graduate of the Swiss Federal Institute of Technology in Zurich and Ph.D. (1940) of the University of Minnesota. After an extended stay in the United States (Princeton and the University of Iowa) and in England (Office of Naval Research, London) he returned to his native Switzerland, where he became director of the Institute of Theoretical Physics at the University of Geneva in 1960 and established a school of foundational research in quantum mechanics. Being an ardent admirer of von Neumann but aware of the fact that some criticisms leveled against him were not completely unfounded, Jauch tried, as Saccheri once did with Euclid, to reinstate von Neumann *ab omni naevo vindicatus*. "I know," Jauch once wrote,[109] "that many of the founders of quantum mechanics have consistently underestimated the importance of von Neumann's work. The importance will, however, become more evident as the need for a more rigorous foundation as well as a generalization of the theory becomes more widely felt. Much of the work of myself and my collaborators during the past years has been concerned with the elaboration of this work of v. Neumann and I can say that today the situation for nonrelativistic quantum mechanics is very well settled and understood,"

Impressed by Gleason's result to which his attention was drawn by a reference in Mackey's *Lecture Notes*,[110] and admitting that von Neumann's assumptions for his hidden variable proof were too stringent and that, in

[109]Letter from Jauch to author, dated October 3, 1966.
[110]See Ref. 8-145 below.

particular, the additivity postulate P.IV was difficult to justify, Jauch, in collaboration with Constantin Piron, remodeled the proof to avoid all objections of circular reasoning and to make it independent of the additivity assumption. In addition, their revised proof was to show precisely what empirical facts, on which the acceptance of ordinary quantum mechanics is based, preclude its hidden variable extension.

The Jauch and Piron reformulation[111] of von Neumann's proof is based on their study of the order relations between quantum mechanical propositions and consequently uses the lattice-theoretic language of what is usually called "quantum logic."[112] Their point of departure is the statement that the quantum mechanical propositions ("yes-no experiments" or more precisely their equivalence classes) form a complete orthocomplemented lattice L, the partial ordering being defined by the statement that $a \leqslant b$ if, whenever a is true, b is also true. Two propositions a and b are said to be *compatible* (or symbolically $a \leftrightarrow b$) if they satisfy the symmetrical relation $(a \cap b') \cup b = (b \cap a') \cup a$. It is postulated (Postulate P) that $a \leqslant b$ implies $a \leftrightarrow b$. The state of a quantum mechanical system—the physical meaning of such a state is usually defined by the probability distribution of a sufficiently large set of observables—is expressed in terms of a function $w(a)$ on the set of all propositions a of L (briefly: on L). The function, it is further postulated, satisfies the following five conditions:

P.W1. $0 \leqslant w(a) \leqslant 1$ on L

P.W2. $w(\phi) = 0, w(I) = 1$

P.W3. $a \leftrightarrow b$ implies $w(a) + w(b) = w(a \cap b) + w(a \cup b)$ on L

P.W4.[113] $w(a) = w(b) = 1$ implies $w(a \cap b) = 1$ on L

P.W5. $a \neq \phi$ implies the existence of a state w such that $w(a) \neq 0$, where $\phi = \bigcap_{a_j \in L} a_j$ is the absurd proposition, $I = \bigcup_{a_j \in L} a_j$ is the trivial proposition.

For the sequel recall that the orthocomplement a' of a, the "negation" of a,

[111] J. M. Jauch and C. Piron, "Can hidden variables be excluded in quantum mechanics?," *Helvetica Physica Acta* **36**, 827–837 (1963).

[112] The reader not familiar with this terminology is advised to read the Appendix on Lattice Theory at the end of this book and the discussion on the paper by G. Birkhoff and J. von Neumann in the next chapter before proceeding.

[113] Jauch and Piron postulated the stronger postulate *P*.W4: $w(a_j) = 1$ for all j of a certain index set J implies $w(\bigcap_j a_j) = 1$. For our present purpose this stronger version of P.W4 is not needed.

has the following properties: $(a')'=a$, $a\cup a'=I$, $a\cap a'=\phi$ and $(a\cup b)'=a'\cap b'$.

A state is dispersion-free if $w(a)=w^2(a)$ on L, that is, every proposition is either true ($=1$) or false ($=0$). Two states w_1 and w_2 are different if there exists a proposition a such that $w_1(a)\neq w_2(a)$. If w_1 and w_2 are different states, $\lambda_1>0$, $\lambda_2>0$, and $\lambda_1+\lambda_2=1$, then $w(a)=\lambda_1 w_1(a)+\lambda_2 w_2(a)$ defines a new state that differs from w_1 and from w_2; it is a mixture. A state which cannot be represented thus in terms of (at least) two different states is homogeneous or pure. Since the mixture as defined satisfies for the proposition for which $w_1(a)\neq w_2(a)$ the relation $0<\lambda_1 w_1(a)+\lambda_2 w_2(a)<1$ for all $\lambda_1>0$, $\lambda_2>0$, and $\lambda_1+\lambda_2=1$, it is clear that every dispersion-free state is homogeneous.

With these definitions and conclusions in mind we are now in a position to understand the Jauch-Piron reformulation of von Neumann's proof. Their reformulation consists of two lemmata and one theorem.

LEMMA 1. If L admits hidden variables and if $w(a)=w(b)$ for all states, then $a=b$.

If $x=a\cap(a\cap b)'\neq\phi$, then by P.W5 there exists a state such that $w(x)\neq 0$. Since L admits hidden variables, every state is a mixture of dispersion-free states and there must even exist a state such that $w(x)=1$. From $a\cap(a\cap b)'\leqslant a$ it follows that $w(a)=1=w(b)$ and by P.W4 that $w(a\cap b)=1$ or $w((a\cap b)')=0$. Hence, since $a\cap(a\cap b)'\leqslant(a\cap b)', w(x)=0$ contrary to assumption. Thus $a\cap(a\cap b)'=\phi$. Since $a\cap b\leqslant a$, by Postulate P $a\cap b\leftrightarrow a$ or $((a\cap b)\cap a')\cup a=(a\cap(a\cap b)')\cup(a\cap b)$, which reduces to $a=a\cap b$ so that $a\leqslant b$. The premise being symmetric in a and b, it follows by the same token that $b\leqslant a$. Hence $a=b$.

LEMMA 2. If L admits hidden variables, then for any pair of propositions a, b of L and any state w, $w(a)+w(b)=w(a\cup b)+w(a\cap b)$.

It suffices to consider dispersion-free states $w(a)$ which are 0 or 1. The four possibilities—(1) $w(a)=w(b)=0$; (2) $w(a)=1, w(b)=0$; (3) $w(a)=0, w(b)=1$; (4) $w(a)=w(b)=1$—reduce, by replacing a by a' and b by b', to the first two. In case (1) $w(a\cap b)\leqslant w(a)=0$ implies $w(a\cap b)=0$; since $w(a')=1-w(a)=1$ and $w(b')=1$ P.W4 requires $w(a'\cap b')=1$ and therefore $w(a\cup b)=1-w((a\cup b)')=1-w(a'\cap b')=0$ and the contention is proved.In case (2) $w(a\cup b)\geqslant w(a)=1$ implies $w(a\cup b)=1$; since $w(a\cap b)\leqslant w(b)=0$ clearly $w(a\cap b)=0$ and the contention is proved.

THEOREM . If L admits hidden variables, then $a \in L$ and $b \in L$ implies $a \leftrightarrow b$.

For every state $w, w((a \cap b') \cup b) = w(a \cap b') + w(b) = w(a) + w(b') - w(a \cup b') + w(b) = w(a) + 1 - w(a \cup b') = w(a) + w(a' \cap b) = w(a \cup (a' \cap b))$, because $w((a \cap b') \cap b) = w(a \cap (a' \cap b)) = 0$. Hence by Lemma 1, $(a \cap b') \cup b = (b \cap a') \cup a$ or $a \leftrightarrow b$.

Jauch and Piron have thus proved, on the basis of their postulates, that a propositional system admits hidden variables only if all of its propositions are compatible. "With this result," they concluded, "the possible existence of hidden variables is decided in the negative." Since the propositional lattice L of quantum mechanics contains incompatible propositions, as experience shows, quantum mechanics cannot be interpreted in terms of hidden variables. Having thus rehabilitated von Neumann's proof on supposedly securer ground they added: "This result will perhaps suffice to devaluate the reproach that von Neumann's proof contains circular reasoning. On the contrary we hope that it will serve to understand the far-reaching implications of von Neumann's analysis at an early stage of the evolution of quantum mechanics. We feel this work is today even more relevant than at the time when it was written since the mounting pressure from high energy physics for a modification of our conceptual frame should not lead us to lose sight of the foundations which are secure and on which a future expansion will have to be constructed."[114]

7.7. BELL'S CONTRIBUTIONS

Were the foundations on which Jauch and Piron established their proof really secure? We shall presently see that their soundness was soon called into question by John Stewart Bell, who, as mentioned previously, was dissatisfied with the quantum mechanical partition of the world into systems and observers, with no clear demarcation of either. This prompted him to study the problem of hidden variables. As a graduate student he read the chapter on indeterministic physics in Max Born's *Natural Philosophy of Cause and Chance*[115] and was greatly impressed by Born's description of von Neumann's work on quantum mechanics. Unable to read German—the English translation of von Neumann's book appeared

[114]Ref. 111, p. 829.

[115]Oxford University Press, London, 1949.

only in 1955—Bell could not study von Neumann's proof at that time. Subsequently specializing in accelerator design at Harwell, Bell read there Bohm's 1952 papers, which revived his interest in the foundational problems of quantum mechanics. Assisted by his German-speaking colleague Franz Mandl, Bell now studied von Neumann's work thoroughly. While Mandl tried to convince Bell that Bohm must somehow be wrong, Bell recognized the peculiar role of the additivity postulate P.IV in von Neumann's reasoning, but did not yet feel certain enough to make any definite statements on this matter.

After obtaining a Ph.D. degree in 1955 for his thesis on invariance problems in field theory at the University of Birmingham, Bell joined the staff of CERN at the end of 1959. There he attended in 1963 a seminar given by Jauch, who had just completed the paper with Piron, and again Bell returned to his *premières amours*. It was on this occasion that Jauch directed his attention to the work of Gleason. Stimulated by the discussions with Jauch, Bell wrote a paper[116] on the problem of hidden variables in the summer of 1964 while at SLAC in Stanford, California, on leave from CERN, and submitted it to the *Reviews of Modern Physics*. The referee thought that too little has been said about the measuring process, so the paper was returned to Bell with the request that he enlarge on this subject. Bell did this[117] and sent the revised manuscript to the editor in January 1965. Unfortunately, the manuscript was misfiled at the office of the *Reviews* while Edward U. Condon, its editor since 1957, waited impatiently to receive Bell's paper for publication. In June 1965 Condon lost his patience and sent a reminder to Bell at his SLAC address. But Bell had meanwhile returned to CERN. Thus the letter was returned to Condon, unopened and marked "Return to Sender—Addressee not in Directory." Only when in January 1966 Bell finally decided to inquire about the fate of his manuscript did Condon at last realize what had happened, and he asked Bell for a new manuscript with any additions he might care to make. These facts explain how such an important paper,[118] written in 1964, was not published until 1966.

Bell's doubts about the relevance—not mathematical correctness!—of von Neumann's proof for the problem of hidden variables in quantum mechanics were raised by the very fact that he succeeded in constructing a

[116]J. S. Bell, "On the problem of hidden variables in quantum mechanics," Preprint, August 1964 (Mimeographed, SLAC-PUB-44).

[117]The added statements are at the end of section II of the published version; see Ref. 118.

[118]J. S. Bell, "On the problem of hidden variables in quantum mechanics," *Reviews of Modern Physics* **38**, 447–452 (1966).

consistent hidden variable theory for particles of spin-$\frac{1}{2}$ without translational motion. All observables, in this case, are representable by 2×2 Hermitian matrices of the form $A=\alpha+\boldsymbol{\beta}\cdot\boldsymbol{\sigma}$, where α is a real number (multiplied by the 2×2 unit matrix), $\boldsymbol{\beta}$ is a real vector and the components of $\boldsymbol{\sigma}$ are the Pauli matrices. A measurement of A yields either $a_1=\alpha+|\boldsymbol{\beta}|$ or $a_2=\alpha-|\boldsymbol{\beta}|$. Bell associated with every state vector ψ (spinor) of the two-dimensional Hilbert spin space $\mathcal{H}_2$ a set of dispersion-free states, specified (in addition to ψ) by a real number λ in the closed interval $[-\frac{1}{2}, +\frac{1}{2}]$. Choosing a coordinate system in which ψ has the form $\begin{pmatrix}1\\0\end{pmatrix}$ and denoting in this system the components of $\boldsymbol{\beta}$ by β_x, β_y, and β_z so that the angle θ between the direction of $\boldsymbol{\beta}$ and the z-axis is given by $\cos\theta=\beta_z/|\boldsymbol{\beta}|$, Bell pointed out that quantum mechanics predicts for a_1 the probability $p_1=\cos^2\theta/2$ and for a_2 the probability $p_2=\sin^2\theta/2$, and for the expectation value $\alpha+\beta_z$. Conversely, for any state vector the relative probabilities p_1 and p_2 can be inferred from the expectation value $p_1a_1+p_2a_2$ and $p_1+p_2=1$. Bell now stipulated that a measurement of A on the state specified by ψ and λ yields with certainty the result

$$a(\psi;\lambda)=\alpha+|\boldsymbol{\beta}|\ \mathrm{sign}\ (\lambda|\boldsymbol{\beta}|+\tfrac{1}{2}|\beta_z|)\ \mathrm{sign}\ X \tag{6}$$

where X is the first nonzero number among β_z, β_x, β_y (in this order). The quantum mechanical expectation value is obtained, whatever α and $\boldsymbol{\beta}$, by uniform averaging over λ:

$$\langle\alpha+\beta\cdot\sigma\rangle=\int_{-1/2}^{1/2}d\lambda[\alpha+|\boldsymbol{\beta}|\ \mathrm{sign}\ (\lambda|\boldsymbol{\beta}|+\tfrac{1}{2}|\beta_z|)\ \mathrm{sign}\ X]=\alpha+\beta_z.$$

Though not capable of attributing any physical significance to λ, Bell demonstrated "a possibility which von Neumann's reasoning was for a long time thought to exclude." That the hidden variables, thus defined, were not contextual was no violation of Gleason's theorem since the Hilbert space under discussion is only two dimensional.

How, then, is it possible, Bell asked, to construct such a logically consistent hidden variable interpretation if von Neumann's proof is valid? To resolve this apparent contradiction Bell reformulated the proof so far as it refers to the example under discussion. According to von Neumann's additivity postulate P.IV, $\langle A\rangle$ should be a linear combination of $\langle\alpha\rangle$ and $\langle\boldsymbol{\beta}\rangle$, since A itself is such a combination of the observables represented by α, $\beta_x\sigma_x$, $\beta_y\sigma_y$, and $\beta_z\sigma_z$, and this should apply to quantum mechanical states as well as to the hypothetical dispersion-free states. In the latter, however, expectation values are eigenvalues. But $\alpha\pm|\boldsymbol{\beta}|$ is not a linear function of $\boldsymbol{\beta}$. Hence dispersion-free states are impossible. Having thus shown that von Neumann's argumentation depended decisively on the validity of the additivity postulate P.IV, Bell clarified the situation by pointing out that it was not the "objective verified predictions of quantum mechanics," as von Neumann asserted, but rather von Neumann's arbitrary additivity assumption, postulated to be valid also for dispersion-free states, that precluded the possibility of hidden variables. Its validity for quantum states, Bell realized, is a peculiarity of quantum mechanics and not at all a priori obvious; for in the case of incompatible observables $\mathfrak{R}$ and $\mathfrak{S}$ the apparatus to measure $\mathfrak{R}+\mathfrak{S}$ is in general entirely different from the devices required to measure the individual observables and a priori no statistical relations between the corresponding results can be expected. It is valid for quantum states because, in the words of Frederik J. Belinfante,[119] "it so happens that the other axioms and postulates of quantum theory conspire to make $\langle\mathfrak{R}\rangle$ expressible at $\int\psi^*\mathrm{R}\psi\,dx$." If this expectation value additivity is waived for dispersion-free states the step leading to equation (1) (this chapter) would be unjustified and the proof would break down.

But what about the Jauch-Piron proof not depending on P.IV? To find its weak point Bell again reformulated the proof as follows. The representation of any proposition in terms of yes-no experiments corresponds to the representation of any observable in terms of projection operators which in $\mathcal{H}_2$, apart from the trivial null and identity operators, are all of the form $A(\mathbf{u})=\frac{1}{2}+\frac{1}{2}\mathbf{u}\boldsymbol{\sigma}$, where $\mathbf{u}$ is a unit vector. Its eigenvalue 1 corresponds to $w(a)=1$ and its eigenvalues 0 to $w(a)=0$. The commuting $A(\mathbf{u})$ and $A(-\mathbf{u})$ add up to the unit operator and hence, expectation value additivity being assumed for commuting projections, $\langle A(\mathbf{u})\rangle=1$ or $\langle A(-\mathbf{u})\rangle=1$. By the same token, for a unit vector $\mathbf{v}$, not collinear with $\mathbf{u}$, either $\langle B(\mathbf{v})\rangle=1$ or $\langle B(-\mathbf{v})\rangle=1$. Let us assume that $\langle A(\mathbf{u})\rangle=\langle B(\mathbf{v})\rangle=1$ so that $w(a)=w(b)=1$. According to the Jauch-Piron postulate P.W4 then we must also have $w(a\cap b)=1$, which means that the projector whose range is the intersection of the ranges of $A(\mathbf{u})$ and $B(\mathbf{v})$ has in this state the eigenvalues 1. But the intersection being empty (because of the non-collinearity of $\mathbf{u}$ and $\mathbf{v}$), $w(a\cap b)=0$. Hence dispersion-free states cannot exist.

Bell thus came to the conclusion that P.W4 has to be rejected for dispersion-free states in spite of its formal analogy to the general theorem in pure logic according to which the conjunction (product) of two true

[119]F. J. Belinfante, "A survey of hidden-variables theories" (mimeographed, revised edition, 1971). *A Survey of Hidden-Variables Theories* (Pergamon Press, Oxford, New York, 1973), p. 25.

propositions is itself a true proposition. Said Bell: "We are not dealing [here] with logical propositions, but with measurements involving, for example, differently oriented magnets. The axiom holds for quantum mechanical states. But it is quite a peculiar property of them, in no way a necessity of thought." According to Bell, von Neumann's error of an unjustified attribution of quantum state properties to the hypothetical hidden variable states has been committed again.

While working on his *Review* paper Bell became obsessed with the Einstein-Podolsky-Rosen argument and in September 1964, when he stayed for a short time at Brandeis University, he proved what is now usually called Bell's theorem:[120] a local hidden-variable theory cannot reproduce all statistical predictions of quantum mechanics. Bell referred to the Bohm version of this argument in terms of spin functions which describe the state of the combined composite system by the rotationally invariant singlet state function (6.60). As will be recalled, quantum mechanics predicts with certainty that, if a measurement of $\sigma_x(1)$, the x-component of $\boldsymbol{\sigma}(1)$, the spin of particle 1, yields $+1$, the measurement of $\sigma_x(2)$, the x-component of $\boldsymbol{\sigma}(2)$, the spin of particle 2, however far it may be from particle 1, will yield the value -1. Since in view of the rotational invariance this correlation holds for any direction in space, that is, for $\boldsymbol{\sigma}(1)\cdot\mathbf{b}$ and $\boldsymbol{\sigma}(2)\cdot\mathbf{b}$, where $\mathbf{b}$ is any unit vector, a hidden variable extension, in conformance with (3) of Definition II, requires the existence of a function $f_{\boldsymbol{\sigma}\cdot\mathbf{b}}(\lambda_2)$ or, in different notation, $B(\mathbf{b},\lambda_2)$, which specifies the value of $\boldsymbol{\sigma}(2)$ along $\mathbf{b}$ if the hidden state is characterized by λ_2.

Since similarly knowledge of the result of measuring $\boldsymbol{\sigma}(2)\cdot\mathbf{a}$, where $\mathbf{a}$ is a unit vector whatever, allows in quantum mechanics the prediction with certainty of the result of $\boldsymbol{\sigma}(1)\cdot\mathbf{a}$, a hidden variable extension requires also the existence of a function $A(\mathbf{a},\lambda_1)$, which specifies the value of $\boldsymbol{\sigma}(1)$ along $\mathbf{a}$ if the hidden state is characterized by λ_1. The assumption that $A(\mathbf{a},\lambda_1)$ does *not* depend on $\mathbf{b}$, nor $B(\mathbf{b},\lambda_2)$ on $\mathbf{a}$, or in other words that the result of one measurement does not depend on the setting of the instrument used for the other measurement, expresses the locality character of the hidden variables employed. The domain of these functions is -1, $+1$. If $\mathbf{a}$ and $\mathbf{b}$ are not in the same direction any one of the four combinations $(+1,+1)$, $(+1,-1)$, $(-1,+1)$, or $(-1,-1)$ may be the result of the two measurements and quantum mechanics predicts the expectation value for the product of the spin observable under discussion as follows:

$$P_{\text{q.m.}}(\mathbf{a},\mathbf{b})=\langle\boldsymbol{\sigma}(1)\cdot\mathbf{a}\,\boldsymbol{\sigma}(2)\cdot\mathbf{b}\rangle=-\mathbf{a}\cdot\mathbf{b}. \tag{7}$$

[120] J. S. Bell, "On the Einstein Podolsky Rosen paradox," *Physics* **1**, 195–200 (1964), received November 4, 1964.

The hidden variable expectation value for this product is given by the expression

$$P_{\text{h.v.}}(\mathbf{a},\mathbf{b})=\int \rho(\lambda_1,\lambda_2)A(\mathbf{a},\lambda_1)B(\mathbf{b},\lambda_2)\,d\lambda_1\,d\lambda_2$$

where $\rho(\lambda_1,\lambda_2)$ is the probability distribution for pairs of hidden states characterized by $\lambda_1\in\Gamma_1$ and $\lambda_2\in\Gamma_2$ and may be assumed to be independent of **a** and **b**. Without loss of generality $P_{\text{h.v.}}$ can be written

$$P_{\text{h.v.}}(\mathbf{a},\mathbf{b})=P_{ab}=\int \rho(\lambda)A(\mathbf{a},\lambda)B(\mathbf{b},\lambda)\,d\lambda \tag{8}$$

where $\lambda\in\Gamma_1\otimes\Gamma_2$ and $\int\rho(\lambda)\,d\lambda=1$. Since for $\mathbf{a}=\mathbf{b}$ the expectation values is -1, $P_{bb}=-1$ implies that $B(\mathbf{b},\lambda)=-A(\mathbf{b},\lambda)$ for all λ and

$$P_{ab}=-\int \rho(\lambda)A(\mathbf{a},\lambda)A(\mathbf{b},\lambda)\,d\lambda.$$

For a third unit vector **c** it then follows that

$$\begin{aligned}P_{ab}-P_{ac}&=-\int d\lambda\rho(\lambda)[A(\mathbf{a},\lambda)A(\mathbf{b},\lambda)-A(\mathbf{a},\lambda)A(\mathbf{c},\lambda)]\\&=\int d\lambda\rho(\lambda)A(\mathbf{a},\lambda)A(\mathbf{b},\lambda)[A(\mathbf{b},\lambda)A(\mathbf{c},\lambda)-1]\end{aligned}$$

and consequently

$$|P_{ab}-P_{ac}|\leqslant\int d\lambda\rho(\lambda)[1-A(\mathbf{b},\lambda)A(\mathbf{c},\lambda)]=1+P_{bc}.$$

The relation

$$|P(\mathbf{a},\mathbf{b})-P(\mathbf{a},\mathbf{c})|\leqslant 1+P(\mathbf{b},\mathbf{c}) \tag{9}$$

is called "Bell's inequality." From (9) Bell concluded that $P_{\text{h.v.}}$ is not always equal to $P_{\text{q.m.}}$ since for small $|\mathbf{b}-\mathbf{c}|$ the left-hand side of (9) is in general of the order $|\mathbf{b}-\mathbf{c}|$ and $P_{\text{h.v.}}(\mathbf{b},\mathbf{c})$—in contrast to $P_{\text{q.m.}}(\mathbf{b},\mathbf{c})$—can therefore not be stationary at the minimum value -1.

The last step which Bell has proved by comparing orders of magnitude can be given an alternative, mathematically simpler, proof[121] as follows. For $\mathbf{a}=\mathbf{b}-\mathbf{c}/|\mathbf{b}-\mathbf{c}|$ the assumption $P_{\text{h.v.}}=P_{\text{q.m.}}$ (i.e., $P_{\text{h.v.}}(\mathbf{x},\mathbf{y})=-\mathbf{x}\cdot\mathbf{y}$) implies as a consequence of (9) that $|-\mathbf{a}\cdot\mathbf{b}+\mathbf{a}\cdot\mathbf{c}|=|\mathbf{b}-\mathbf{c}|\leqslant 1-\mathbf{b}\cdot\mathbf{c}$. Since for the case $\mathbf{b}\perp\mathbf{c}$ this last inequality entails $\sqrt{2}\leqslant 1$ the assumption $P_{\text{h.v.}}=P_{\text{q.m.}}$ has been disproved by a *reductio ad absurdum*.

To sum up, Bell proved that no correlation function P_{ab} as given by (8) for any ρ, A or B can reproduce the quantum mechanical expectations for *all* **a** and **b**. The result can be extended to systems whose state spaces have dimensionalities greater than two and the proof can be generalized[122] to

[121] This alternative argument is not found in Bell's paper.

[122] Ref. 1.

include external hidden variables characterizing the states of the measuring instruments, provided their distribution at either of the two measuring devices does not depend on the setting of the other device. Bell concluded his 1964 paper with the statement: "In a theory in which parameters are added to quantum mechanics to determine the results of individual measurements, without changing the statistical predictions, there must be a mechanism whereby the setting of one measuring device can influence the reading of another instrument, however remote. Moreover, the signal involved must propagate instantaneously, so that such a theory could not be Lorentz invariant."

From the historical point of view it is interesting to note that the essential idea of Bell's result had been anticipated by T. D. Lee by about four years. Tsung-Dao Lee, a native of Shanghai, studied at the universitites of Kueichow and Kunming, China, obtained his Ph.D. in 1950, working under Edward Teller at the University of Chicago, and has been at Columbia University since 1951. On May 28, 1960, three years after receiving (together with Chen Ning Yang whose, acquaintance he had made in Kunming) the Nobel Prize for his well-known work on the breakdown of parity conservation, Lee gave a talk at Argonne National Laboratory on some striking effects of quantum mechanics in the large. In the course of his lecture he discussed certain correlations which exist, as he pointed out, between two simultaneously created neutral K-mesons (kaons) moving off in opposite directions.[123] Realizing that the situation under discussion is intimately related to the problem raised by Einstein, Podolsky, and Rosen, he soon convinced himself that classical ensembles (or, for that matter, systems with hidden variables) could never reproduce such correlations. But due to the complications caused by the finite lifetime of kaons—for infinite lifetime the situation would "degenerate" into that discussed by Bell—he did not derive any conclusion equivalent to Bell's inequality but assigned the further elaboration of these ideas to his assistant Jonas Schürtz, who, however, soon began to work on another project.[124]

Bell's thesis of the irreproducibility of all quantum mechanical predictions by any local hidden variable theory has subsequently been given

[123]The papers written by T. B. Day, "Demonstration of quantum mechanics in the large," *Physical Review* **121**, 1204–1206 (1961), and by D. R. Inglis, "Completeness of quantum mechanics and charge-conjugation correlations of theta particles," *Reviews of Modern Physics* **33**, 1–7 (1961), give only a fragmentary report of Lee's ideas presented in his talk.

[124]Interview with T. D. Lee, March 12, 1973. Professor Lee made it clear that all the credit should be given to Professor Bell.

different proofs by a number of authors. Richard Friedberg[125] of Columbia University showed in 1967, independently of Bell's calculations, that the locality assumption, if applied to spin measurements, leads to contradictions with the observed predictions of quantum mechanics. Wolfgang Büchel,[126] a theologian and philosopher of science, formerly of Berchmanskolleg at Pullach near Munich and more recently of the University of Bochum, Germany, who distinguished himself in introducing students of divinity to the problematics of modern science, in 1967 gave a reformulation of Bell's proof, somewhat similar to Friedberg's approach. The credit for having provided the simplest proof, though without Bell's inequality, probabily goes to Eugene P. Wigner.[127] The system considered by Wigner consists again of two spin-$\frac{1}{2}$ particles in a singlet state; the spin components are measured in three direction w_1, w_2, and w_3. Let $\sigma_k = \pm\frac{1}{2}h$, or briefly $\pm$, denote the result of measuring the spin of the first particle in the direction w_k and $\tau_k = \pm$ that of the second. If such results are to be interpreted in terms of hidden variables, it makes sense to define $(\sigma_1, \sigma_2, \sigma_3; \tau_1, \tau_2, \tau_3)$ as the relative number of systems (or probability) on which these measurements would lead to the results as indicated, for example, $(+--;-++)$ expresses the probability that $\sigma_1 = +, \sigma_2 = -$, etc. If $\theta_{jk} = \theta(w_j, w_k)$, between 0 and π, denotes the angle between w_j and w_k, the probability of obtaining $\sigma_j = \tau_k$ is according to quantum mechanics $\frac{1}{2}\sin^2(\theta_{jk}/2)$ [for the probability to obtain $\sigma_1 = +$ is, because of the spherical symmetry, $\frac{1}{2}$; but if $\sigma_1 = +$, then certainly $\tau_1 = -$ and the probability for $\tau_2 = +$ is $\sin^2(\theta_{12}/2)$; hence the combined probability for $\sigma_1 = +$ and $\tau_2 = +$ is $\frac{1}{2}\sin^2(\theta_{12}/2)$]. Clearly,

$$\sum_{\sigma_2, \sigma_3, \tau_1, \tau_2} (+, \sigma_2, \sigma_3; \tau_1, \tau_2, +) = (++-;--+) + (+--;-++)$$

$$= \tfrac{1}{2}\sin^2(\theta_{13}/2).$$

[125]Unpublished. Friedberg proved his thesis before he knew of Bell's paper. Private communication, December 11, 1968.

[126]W. Büchel, "Ein quantenphysikalisches 'Paradoxon,'" *Physikalische Blätter* **23**, 162–165 (1967). Büchel's interesting book *Philosophische Probleme der Physik* (Herder, Freiburg, Basel, Vienna, 1965) was completed just before Bell published his 1964 paper and consequently does not touch upon this issue, although it deals with the von Weizsäcker version of the Einstein-Podolsky-Rosen argument in great detail. Other (unpublished) proofs of the incompatibility of local hidden variables with quantum mechanical predictions were given in the late sixties by F. J. Belinfante and H. P. Stapp.

[127]E. P. Wigner, "On hidden variables and quantum mechanical probabilities," *American Journal of Physics* **38**, 1005–1009 (1970).

But $(++-;--+)$ is a term in the analogous expression for $\frac{1}{2}\sin^2(\theta_{23}/2)$ and hence not greater than this value; by the same token, $(+--;-++)$ is not greater than $\frac{1}{2}\sin^2(\theta_{12}/2)$. Consequently, agreement with quantum mechanics would imply that $\frac{1}{2}\sin^2(\theta_{13}/2) \leqslant \frac{1}{2}\sin^2(\theta_{12}/2) + \frac{1}{2}\sin^2(\theta_{23}/2)$, a relation which is not valid for arbitrary w_1, w_2, w_3. Hence a quantum mechanical measurement of this kind cannot be interpreted in terms of hidden variables if the variable determining the result for one particle is assumed to be independent of the direction in which the spin of the other particle is measured.

Bell's inequality (9) was derived on the assumption of perfect correlation, that is, on the assumption that for every **b** there exists a unit vector **b**′ (i.e., $\mathbf{b}' = -\mathbf{b}$) such that $P(\mathbf{b}',\mathbf{b}) = 1$. As experience later showed this assumption of perfect correlation was experimentally unrealistic. John F. Clauser, Michael A. Horne, Abner Shimony, and Richard A. Holt[128] therefore studied the case of imperfect correlation: for every **b** there exists

[128] J. F. Clauser, M. A. Horne, A. Shimony, and R. A. Holt, "Proposed experiment to test local hidden-variable theories," *Physical Review Letters* **23**, 880–884 (1969). They defined

$$\Gamma_{\pm} = \{\lambda | A(\mathbf{b}',\lambda) = \pm B(\mathbf{b},\lambda)\}.$$

From

$$\begin{aligned} 1-\delta = P(\mathbf{b}',\mathbf{b}) &= \int_{\Gamma_+} A(\mathbf{b}',\lambda)B(\mathbf{b},\lambda)\rho\, d\lambda + \int_{\Gamma_-} A(\mathbf{b}',\lambda)B(\mathbf{b},\lambda)\rho\, d\lambda \\ &= \int_{\Gamma_+} \rho\, d\lambda - \int_{\Gamma_-} \rho\, d\lambda \end{aligned}$$

and

$$1 = \int_{\Gamma_+} \rho\, d\lambda + \int_{\Gamma_-} \rho\, d\lambda$$

they obtained

$$\int_{\Gamma_-} \rho\, d\lambda = \tfrac{1}{2}\delta.$$

Since

$$\begin{aligned} |P(\mathbf{a},\mathbf{b}) - P(\mathbf{a},\mathbf{c})| &\leqslant A(\mathbf{a},\lambda)B(\mathbf{b},\lambda)\cdot[1 - B(\mathbf{b},\lambda)B(\mathbf{c},\lambda)]\rho\, d\lambda \\ &= 1 - \int_\Gamma B(\mathbf{b},\lambda)B(\mathbf{c},\lambda)\rho\, d\lambda = 1 - D, \end{aligned}$$

where

$$\begin{aligned} D &= \int_{\Gamma_+} B(\mathbf{b},\lambda)B(\mathbf{c},\lambda)\rho\, d\lambda + \int_{\Gamma_-} B(\mathbf{b},\lambda)B(\mathbf{c},\lambda)\rho\, d\lambda \\ &= \left\{\int_{\Gamma_+} - \int_{\Gamma_-}\right\} A(\mathbf{b}',\lambda)B(\mathbf{c},\lambda)\rho\, d\lambda \\ &= \left\{\int_{\Gamma} - 2\int_{\Gamma_-}\right\} A(\mathbf{b}',\lambda)B(\mathbf{c},\lambda)\rho\, d\lambda \geqslant P(\mathbf{b}',\mathbf{c}) - 2\int_{\Gamma_-} |A(\mathbf{b}',\lambda)B(\mathbf{c},\lambda)|\rho\, d\lambda \\ &= P(\mathbf{b}',\mathbf{c}) - 2\int_{\Gamma_-} \rho\, d\lambda = P(\mathbf{b}',\mathbf{c}) - \delta \end{aligned}$$

a $\mathbf{b}'$ such that $P(\mathbf{b}',\mathbf{b}) = 1-\delta$, where $0 \leqslant \delta \leqslant 1$; for good correlation δ is close to zero. Under these conditions they derived the generalized inequality

$$|P(\mathbf{a},\mathbf{b}) - P(\mathbf{a},\mathbf{c})| + P(\mathbf{b}',\mathbf{b}) + P(\mathbf{b}',\mathbf{c}) \leqslant 2. \tag{10}$$

Recently Franco Selleri[129] of the University of Bari, Italy, gave a simple proof of a stronger form of Bell's inequality, that for any four unit vectors **a**, **b**, **c**, **d** the following relation holds:

$$|P(\mathbf{a},\mathbf{b}) - P(\mathbf{a},\mathbf{c})| + |P(\mathbf{d},\mathbf{b}) + P(\mathbf{d},\mathbf{c})| \leqslant 2 \tag{11}$$

He also studied, in collaboration with Vincenzo Capasso and Donato Fortunato,[130] the following problem: For what state functions (of which the singlet states considered by Bell are particular cases) can the inequalities (9) and (11) be violated for suitable choices of vectors?

Bell's result that P_{ab} as defined by (8) does not fully reproduce the quantum mechanical predictions seemed to shatter all hope[131] of ever restoring classical determinism to quantum phenomena. True, a hidden variable theory in which P_{ab} is defined instead by $P_{ab} = \int d\lambda \rho(\lambda) A(\mathbf{a},\mathbf{b},\lambda) B(\mathbf{b},\mathbf{a},\lambda)$ would be in full agreement with quantum mechanics, but its nonlocality would endow it with features that seemed to belong to magic rather than to physics. Thus, for example, since in the two-spin experiment the setting of each instrument may be changed at a time when the two

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they concluded that

$$|P(\mathbf{a},\mathbf{b}) - P(\mathbf{a},\mathbf{c})| \leqslant 1 - P(\mathbf{b}',\mathbf{c}) + \delta = 2 - P(\mathbf{b}',\mathbf{b}) - P(\mathbf{b}',\mathbf{c})$$

or finally the inequality (10).

[129]F. Selleri, "A stronger form of Bell's inequality" (preprint, 1972). Selleri's derivation may be summarized as follows:

$$\begin{aligned}|P(\mathbf{x},\mathbf{b}) \pm P(\mathbf{x},\mathbf{c})| &= |\int d\lambda\rho[A(\mathbf{x},\lambda)B(\mathbf{b},\lambda) \pm A(\mathbf{x},\lambda)B(\mathbf{c},\lambda)] \\ &\leqslant \int d\lambda\rho|A(\mathbf{x},\lambda)||B(\mathbf{b},\lambda) \pm B(\mathbf{c},\lambda)| \\ &= \int d\lambda\rho|B(\mathbf{b},\lambda)||1 \pm B(\mathbf{b},\lambda)B(\mathbf{c},\lambda)| \\ &= \int d\lambda\rho[1 \pm B(\mathbf{b},\lambda)B(\mathbf{c},\lambda)] \\ &= 1 \pm \int d\lambda\rho B(\mathbf{b},\lambda)B(\mathbf{c},\lambda).\end{aligned}$$

Taking $\mathbf{x} = \mathbf{d}$ in one of these inequalities, $\mathbf{x} = a$ in the other, and adding yields (11).

[130]V. Capasso, D. Fortunato, and F. Selleri, "Sensitive observables of quantum mechanics," *International Journal of Theoretical Physics* **7**, 319–326 (1973).

[131]Paraphrasing Lucius Accius' dictum "*Saepe ignavavit fortem ex spe expectatio*" *(Aeneadae)* one may say that Bell's treatment of "expectations" shattered all expectations.

particles are well in flight, the two measurements would exhibit what looks more like a mysterious conspiracy than a physical correlation. Even to many nonconformists Bohr's complementarity interpretation seemed to be less bizarre.

But was the derivation of Bell's inequality (9), or of its stronger form (11), which led to the conflict with quantum mechanics, really based on assumptions general enough to warrant such far-reaching conclusions? Or was it perhaps based on a tacitly assumed postulate which, if omitted, would greatly restrict the validity of Bell's result? That this was in fact the case was recently claimed by Luis de la Peña, Ana M. Cetto, and T. A. Brody[132] of the National University of Mexico. In their analysis of the derivation of these equations they pointed out that the retention of the same distribution $\rho(\lambda)$ for different correlation functions such as P_{ab} and P_{ac} or $A(\mathbf{a},\lambda)$, $A(\mathbf{d},\lambda)$, $B(\mathbf{b},\lambda)$, and $B(\mathbf{c},\lambda)$, which were evaluated for the same λ's and referred to the same pair of particles, would be justified only on the assumption that the measuring process does not produce any change in $\rho(\lambda)$. Since, however, such a change cannot a priori be excluded, especially not for quantum mechanical systems, Bell's inequality has to be reformulated in terms of conditioned correlations, that is, correlations obtained on systems conditioned by previous measurements. But if thus modified, the inequality will no longer cause any conflict with quantum mechanical predictions. It would therefore be wrong to conclude that Bell's result excludes *all* local hidden variable theories; it excludes only those in which the measuring process is assumed not to affect the distribution of the hidden variables. Any experimental refutation of Bell's unmodified inequality should be regarded, from the point of view expressed by these authors, as a confirmation of the fact that the measuring process does affect the distribution of the hidden variables. Their objection raised against Bell's work is very similar to that raised 40 years earlier by Schrödinger against von Neumann's original version of his famous proof.

7.8. RECENT WORK ON HIDDEN VARIABLES

Bell's criticism of von Neumann's work prompted Bohm and Bub[133] to construct a hidden variable theory independent of von Neumann's postulate P.IV. Jeffrey Bub, a graduate (1962) of the University of Cape Town,

[132] L. de la Peña, A. M. Cetto, and T. A. Brody, "On hidden-variable theories and Bell's inequality" (Preprint, IFUNAM-72-6, 1972.) Cf. also D. S. Kershaw, "Is there an experimental reality to hidden variables?" Preprint, 1974.

[133] D. Bohm and J. Bub, "A proposed solution of the measurement problem in quantum mechanics by a hidden variable theory," *Reviews of Modern Physics* **38**, 453–469 (1966).

where the philosopher Michael Whitman roused his interest in the philosophy of physics, had won a Jan Smuts Memorial Scholarship to study abroad. This he used to participate in Popper's graduate seminar at the London School of Economics and to work under Bohm at Birkbeck College on his Ph.D. thesis (1966), "The Problem of Measurement in Quantum Mechanics," the second part of which formed his contribution to this paper.

Using certain ideas implicitly contained in the differential space theory of Wiener and Siegel, which will be discussed in Chapter 9, Bohm and Bub proposed a deterministic equation of motion to account for the reduction of the wave packet in a measuring process. They also formulated their theory in such a way that it became compatible with Bohr's "feature of wholeness" and thus with the conclusions drawn by Bell from his analysis of Gleason's work. Reviewing von Neumann's statistical formula (2) they wrote

$$\langle \mathfrak{R} \rangle = \sum_{mn} \bar{u}_{nm} R_{mn}$$

where

$$\bar{u}_{nm} = u_{nm}(\psi, \lambda)\rho(\lambda)\, d\lambda$$

and hence

$$\langle \mathfrak{R} \rangle = \sum_{mn} \int u_{nm}(\psi, \lambda) R_{mn} \rho(\lambda)\, d\lambda.$$

A particular value R' of R would then be given by the linear relation

$$R' = \sum_{mn} u_{nm}(\psi', \lambda') R_{mn}$$

between the value of R and its associated matrix R_{mn}. To avoid von Neumann's unnecessarily restrictive linearity requirement they replaced it by a nonlinear relation $R = F(\psi, \lambda, R_{mn})$ so that for an ensemble $\langle \mathfrak{R} \rangle = F(\psi, \lambda, R_{mn})\rho(\lambda)\, d\lambda$ would reproduce the statistics of quantum mechanics for an appropriate "normal" distribution $\rho(\lambda) = \rho_N(\lambda)$. The linear relation, as can be easily seen, would entail von Neumann's objectionable additivity postulate.

To show the conceptual possibility of constructing a hidden variable theory based on this nonlinear relation Bohm and Bub considered the case of a spin-$\frac{1}{2}$ particle without translational motion whose normalized wave function ψ is represented in a two-dimensional Hilbert space $\mathcal{H}_2$ by the vector $|\psi\rangle = \psi_1|S_1\rangle + \psi_2|S_2\rangle$, where $|S_1\rangle$ and $|S_2\rangle$ are the basis in which the spin operator S is diagonal. They now postulated a dual Hilbert space $\mathcal{H}_2'$ with vectors $\langle\xi| = \xi_1\langle S_1| + \xi_2\langle S_2|$ and regarded the components ξ_1 and ξ_2 of $\langle\xi|$ as the hidden variables of their theory, assumed to be randomly distributed on the unit hypersphere

$$\sum_j |\xi_j|^2 = 1.$$

The measurement process was now described by nonlinear differential equations relating the components of $|\psi\rangle$ to those of $\langle\xi|$:

$$\frac{d\psi_1}{dt} = \gamma(R_1 - R_2)\psi_1 J_2, \qquad \frac{d\psi_2}{dt} = \gamma(R_2 - R_1)\psi_2 J_1$$

where

$$R_1 = \frac{|\psi_1|^2}{|\xi_1|^2} = \frac{J_1}{|\xi_1|^2}, \qquad R_2 = \frac{|\psi_2|^2}{|\xi_1|^2} = \frac{J_2}{|\xi_2|^2},$$

and γ is a nearly constant positive quantity, negligible before and after the (impulsive) measurement process. It is easily verified that $|\psi\rangle$ remains normalized. If initially $R_1 > R_2$ and $J_2 \neq 0$, the J_1 increases until $J_1 = 1$ and $J_2 = 0$, producing the eigenstate $|S_1\rangle$. Similarly, $R_2 > R_1$ leads to the eigenstate $|S_2\rangle$. The mutually exclusive measurement results $+\hbar/2$ and $-\hbar/2$ are thus determined by the initial values of the components of $\langle\xi|$ (the hidden variables) and of $|\psi\rangle$. To demonstrate that the statistical predictions of quantum mechanics are recovered Bohm and Bub wrote $\xi_1 = \rho_1 \exp(i\theta_1) = a_1 + ib_1$, $\xi_2 = \rho_2 \exp(i\theta_2) = a_2 + ib_2$ and with $\rho_1 = \rho \cos\varphi$, $\rho_2 = \rho \sin\varphi$ obtained for the volume element $d\Omega$ of the hidden parameter space $d\Omega = \frac{1}{2} d(\sin^2\varphi) d\theta_1 d\theta_2$, where the dual vector also is assumed to be normalized ($\rho = 1$). With the normal distribution $\rho_N = \text{const.}$ on the hyperspherical shell of unit radius, the probability of obtaining $+\frac{1}{2}\hbar$ is given by the integration over all points in the dual space which satisfy the conditions

$$\frac{|\psi_1|^2}{|\xi_1|^2} > \frac{|\psi_2|^2}{|\xi_2|^2}, \qquad |\psi_1|^2 + |\psi_2|^2 = 1, \qquad |\xi_1|^2 + |\xi_2|^2 = 1.$$

Evaluating the integral they obtained for this probability the value $|\psi_1|^2$. Similarly, the integration over all points for which $R_2 > R_1$ yielded for the probability of obtaining $-\frac{1}{2}\hbar$ the value $|\psi_2|^2$. The proposed hidden variable theory is thus seen to reproduce the usual predictions of quantum mechanics and to describe the reduction of the wave packet as a deterministic physical process: If initially $R_1 > R_2$, the outcome is $+\frac{1}{2}\hbar$; if initially $R_2 > R_1$, it is $-\frac{1}{2}\hbar$ ($R_1 = R_2$ being negligible since it corresponds to a set of measure zero).

The fact that the deterministic equations, postulated in addition to the Schrödinger equation, depend on the representation in which the matrix of the operator representing the observable to be measured is diagonal means physically that the representation depends on the effect of the measuring device. Since, accordingly, an apparatus designed to measure, for example, the x-component of the spin produces a motion in $\mathcal{H}_2$ that differs from that produced by an apparatus oriented to measure the y-component, the observable measured is associated not only with the system alone but also with the particular process of interaction with the measuring device. In other words, if the experimental setup or macroscopic environment allows a particular set of vectors $\Sigma = \{\psi_1, \psi_2, \dots\}$ to become the possible eigenvectors through the act of measurement, the initial values of ψ and λ determine which particular member ψ_k of this set will have to be associated with the system as a result of the measurement. Σ itself is not defined by the initial values but rather by the measuring device or, more generally, by the specific environment in which the measurement is being performed.

Considering the measuring device as merely part of the system's large-scale environment and deeming it necessary to take into account all interactions between the system and its large-scale environment, Bohm and Bub suggested the following generalization for the equations of motion:

$$\dot{\psi}_k = \gamma\psi_k \sum_j J_j(R_k - R_j) - \frac{i}{\hbar}\sum_j H_{kj}\psi_j \qquad (k = 1, 2, \dots, n),$$

where the additional nonunitary term represents the interaction with the environment. In the continuous case the preceding equation reads

$$\dot{\psi}_x = \gamma\psi_x \int J_y(R_x - R_y)\,dy - \frac{i}{\hbar}H\psi_x$$

and shows that a change in the wave function at one point depends on the value of the wave function at every other point in space. Their proposed equation of motion is therefore not only nonlinear but also nonlocal and precludes, as the authors admitted, any generalization of the theory to relativistic phenomena. A second limitation, connected with this nonlocality, is the admission of only "complete" measurements, that is, measurement processes which in the case of a composite system reduce only the state function of the composite system and not those of its components, to one of its eigenfunctions. Since in this case a measurement on only one system (component) cannot be carried out the theory precludes even the very formulation of thought-experiments of the kind considered by

Einstein, Podolsky, and Rosen. A composite system must always be regarded, according to the Bohm-Bub hidden variable theory, as an indivisible totality. It is in this sense that this theory fully reflects Bohr's "feature of wholeness."[134]

Finally, it is claimed, the theory may be tested against the statistical predictions of standard quantum mechanics, which, as we have seen, agree with those of the proposed theory provided the dual vector $\langle\xi|$ has initially a random distribution over the unit hypersphere. Since, however, with the measurement of the spin in a given direction the dual vector loses its randomization (for then either $R_1 > 1$ or $R_1 < 1$), this initial condition is not satisfied for an immediately subsequent measurement of the spin in another direction and the statistical results may well differ from those of standard quantum mechanics. Speculating on the possibility of performing experiments with durations shorter than the randomization time, that is, the time needed to completely randomize the dual vector again, Bohm and Bub pointed out that their theory offers various suggestions for further experimental investigations which within the conceptual framework of the ordinary theory would be meaningless. Although they regarded their theory "merely as a step towards a more elaborate theory, which should include relativistic phenomena," they saw in it "a theoretical structure which makes it possible to discuss relationships which go beyond those of formal quantum mechanics," relationships they believed to be relevant for a full understanding of the measurement problem.

The proof advanced by Jauch and Piron against the possibility of hidden variables did not depend, as we have seen, on von Neumann's linearity assumption. The argument used by Bohm and Bub to make their theory immune to von Neumann's proof did not apply therefore against the challenge of Jauch and Piron. To defend their theory Bohm and Bub thus thought it necessary to refute the proof by Jauch and Piron. The tactic they applied resembles that used by Grete Hermann in her attempt, in 1935, to refute von Neumann's proof: to show that the argument to be refuted is circular. Jauch and Piron, according to Bohm and Bub,[135] "tacitly suppose that all experiments must be analyzed in terms of the usual terminology of quantum mechanics," a terminology which in the view of Bohm and Bub provides "an inadequate set of propositions for the problem."

[134]This aspect of the theory is discussed in more detail in J. Bub, "Hidden variables and the Copenhagen Interpretation—A reconciliation," *British Journal for the Philosophy of Science* **19**, 185–210 (1968).

[135]D. Bohm and J. Bub, "A refutation of the proof by Jauch and Piron that hidden variables can be excluded in quantum mechanics," *Reviews of Modern Physics* **38**, 470–475 (1966).

Bohm and Bub charged Jauch and Piron's proof with circularity on the ground that it is based on the assumption that the impossibility of propositions describing simultaneously the results of measurements of two incompatible observables is an empirical fact; but this assumption follows if and only if one first assumes what Jauch and Piron set out to prove, that is, "that the current linguistic structure of quantum mechanics is the only one that can be used correctly to describe the empirical facts underlying the theory."

To substantiate their claim Bohm and Bub referred to Jauch and Piron's postulate P.W4, against the validity of which Bell[136] had already raised serious objections. If a and b are incompatible, their corresponding projection operators being noncommutative and without common eigenvectors, $a \cap b$ is the absurd proposition ϕ and, according to P.W2, $w(a \cap b) = 0$ so that according to P.W4 the possibility $w(a) = w(b) = 1$ is excluded. In their own theory, however, Bohm and Bub argued, propositions are statements about ψ, the (in principle measurable) hidden variables represented in the dual space and the measuring device; each observable is associated with a specific process of interaction between the system and a suitable measuring device; two measurement processes may thus be incompatible in the sense that their actions interfere but there is no sense in which the two corresponding statements can be considered incompatible: a and b, represented by noncommuting projection operators, can both be true with certainty if they are verified as such by corresponding processes, whereas (because of interference) no process exists to verify the proposition $a \cap b$. In this case $w(a \cap b) = 0$ without excluding the possibility $w(a) = w(b) = 1$. In short, although $a \cap b$ may be the absurd proposition, it is, within the framework of the Bohm-Bub theory, physically unjustified to conclude that then a and b can never be true with certainty in the same state.

In their rather sharply worded rebuttal of this "refutation" Jauch and Piron[137] accused their opponents of having misinterpreted their ideas. The validity of quantum mechanics, they declared, has not been assumed. "Instead, one assumes only a lattice structure of yes-no experiments (called propositions) originating directly from experimental facts." Jauch and Piron also emphasized that microphysics can be axiomatized in different ways and that, for example, Misra,[138] in his study of the problem of hidden variables in a general algebraic setting, proved that within the C*-algebraic

[136]Ref. 118.

[137]J. M. Jauch and C. Piron, "Hidden variables revisited," *Reviews of Modern Physics* **40**, 228–229 (1968), Letter to the Editor.

[138]B. Misra, "When can hidden variables be excluded in quantum mechanics?," *Nuovo Cimento* **A47**, 841–859 (1967).

formulation which cannot be subsumed under the lattice theoretic approach (nor vice versa), for factors of type I hidden variables have to be excluded. Although they agreed with Misra that "the quest for hidden variables becomes a meaningful scientific pursuit only if states, even physically non-realizable states, are restricted by physical considerations," they objected to Bohm and Bub's modifications of the equation of motion which according to Jauch and Piron imply "that all systems evolve with a Schrödinger equation except those which constitute a measurement." "It is contrary to good scientific methodology," Jauch and Piron declared, "to modify a generally verified scientific theory for the sole purpose of accommodating hidden variables."

In their reply to this criticism Bohm and Bub[139] insisted that the statement made by Jauch and Piron, "one assumes only a lattice structure of yes-no experiments originating directly from experimental facts," is ambiguous since it is not clear whether it means that the lattice structure itself is a fact or whether the observed facts lead uniquely to this lattice structure; both interpretations, however, are wrong, for no experiments in physics reveal the lattice structure of propositions as a directly observed fact and, as shown by their hidden variable theory, the observed facts are well compatible with an alternative approach. To show the fallacy involved they compared the Jauch and Piron thesis with the statement that since the postulates of Euclid "originate directly from the experimental facts," a non-Euclidian geometry would be impossible. "If it is accepted," Bohm and Bub concluded, "that hidden variables are excluded by the facts, it becomes impossible to frame the question of how to decide experimentally whether or not there are hidden variables."

Meanwhile a compromise solution of this dispute was suggested by Stanley P. Gudder, a graduate of the University of Illinois (M.S., 1960) where in 1964 he obtained his Ph.D. for a thesis on "A Generalized Probability Model for Quantum Mechanics" under Robert G. Bartle, an expert on spectral theory and coauthor of N. Dunford and J. T. Schwartz' influential book, *Linear Operators.* He was also greatly influenced by the applied mathematician Herber S. Wilf and the theoretician Rudolf Haag, who were at that time affiliated with the University of Illinois. According to Gudder[140] the problem of hidden variables has been incorrectly phrased: "One should not ask whether a physical system admits hidden variables or not, but only if a particular model used to describe the system

[139] D. Bohm and J. Bub, "On hidden variables—A reply to comments by Jauch and Piron and by Gudder," *Reviews of Modern Physics* **40**, 235–236 (1968), Letter to the Editor.

[140] S. P. Gudder, "Hidden variables in quantum mechanics reconsidered," *Reviews of Modern Physics* **40**, 229–231 (1968), Letter to the Editor.

admits hidden variables." Since there are conceivably different mathematical models for the same physical systems, some admitting hidden variables and some not, the problem, according to Gudder, consists of finding which model describes most closely the physical situation. On the one hand, Gudder therefore agreed with Bohm and Bub that in an absolute sense hidden variables cannot be excluded; on the other hand, Gudder tried to show that the objections raised by Bohm and Bub against Jauch and Piron's specific model can be overcome. In fact, in his paper Gudder defined a large class of mathematical models that exclude hidden variables.

Differing slightly from Mackey, Jauch, and Piron, Gudder called a question an *experimental question* concerning a physical system S if it is possible to construct an experiment on S, the outcome of which is capable of giving both a *Yes* and a *No* answer to a, and he called the set of all such experimental questions Q_0. This Q_0, together with the idealized question ϕ, which has a No answer for every experiment that answers it, and the idealized question I, which has a Yes answer for every experiment that answers it, constitutes the *question system* Q. Defining like Jauch the relation $a \leqslant b$ as valid if, whenever a has a Yes answer, b also has a Yes answer, Gudder postulated that Q is partially ordered under this relation. Thus the following properties of Q are postulated:

Q.1. $a \leqslant a$ for all $a \in Q$.
Q.2. $a \leqslant b$ and $b \leqslant c$ imply $a \leqslant c$.
Q.3. $a \leqslant b$ and $b \leqslant a$ imply $a = b$.
Q.4, $\phi \leqslant a \leqslant I$ for all $a \in Q$.

The question $a \cap b$, asking "Is the answer to *a and b* Yes?," is an element of Q only if there exists an experiment capable of giving both a Yes answer and No answer to this question. The same applies to the question $a \cup b$, asking "Is the answer to *a or b* Yes?" Gudder further postulated:

Q.5. If $a \cap b \in Q$, then $a \cap b \leqslant a$ and $a \cap b \leqslant b$; and if $c \leqslant a$ and $c \leqslant b$, then $c \leqslant a \cap b$. The dual property holds for $a \cup b$. If for all $\alpha \in A$ (index set) $a_\alpha \in Q$, then $\bigcap_A a_\alpha$ and $\bigcup_A a_\alpha$, if they exist, are defined analogously.
Q.6. There exists a mapping $a \rightarrow a'$ from Q into Q such that $(a')' = a$ for all $a \in Q$.
Q.7. $a \leqslant b$ implies $b' \leqslant a'$.
Q.8. $a \cup a' = I$ for all $a \in Q$.

Now a' is interpreted as the experimental question which has a Yes answer whenever a has a No answer (a and a' are answered by the same experiment); a and b are *disjoint* if $a \leqslant b'$ (symbolically $a \perp b$), and ab are compatible (symbolically $a \leftrightarrow b$) if there exist mutually disjoint elements a_1, b_1, and c in Q such that $a_1 \cup c$ and $b_1 \cup c$ are in Q and $a = a_1 \cup c$ and $b = b_1 \cup c$.

Q.9. If for all $\alpha \in A$ (index set) a_α are mutually compatible elements in Q, then $\bigcup_A a_\alpha$ and $\bigcap_A a_\alpha$ exist in Q.

Q.10. If $a \leqslant b$, then there is a $c \in Q$ such that $a \perp c$ and $b = a \cup c$.

Since the *state* of S is determined if the probability of obtaining an affirmative answer is known for every element in Q, Gudder considered a set M of real functions on Q which satisfy the following postulates:

M.1. $0 \leqslant m(a) \leqslant 1$ for every $a \in Q$.

M.2. $m(I) = 1$.

M.3. If $a \perp b$, then $m(a \cup b) = m(a) + m(b)$.

M.4. If $a \neq \phi$, then there exists an $m \in M$ such that $m(a) = 1$.

M.5. If $m(a) = m(b) = 1$ for some $m \in M$ and if $a \cap b \in Q$, then $m(a \cap b) = 1$.

M.6. If for all $\alpha \in A$ (index set) the elements a_α are mutually compatible and $m(a_\alpha) = 1$ for all α and some $m \in M$, then $m\left(\bigcap_A a_\alpha\right) = 1$.

Functions on Q which satisfy M.1 to M.6 are called *states* and a state which has only the value 0 or 1 is called *dispersion-free*. If m_j is a sequence of states, λ_j a sequence of nonnegative real numbers and $\Sigma\lambda_j = 1$, then the state $m = \Sigma\lambda_j m_j$ defined by $m(a) = \Sigma\lambda_j m_j(a)$ for all a in Q is called a *mixture* of the states m_j. The pair (Q, M) is a *quantum system* if Q is a question system satisfying Q.1 to Q.10 and M is a set of real functions on Q, closed under mixtures and satisfying M.1 to M.6.

To discuss the two main theorems in Gudder's paper[141] we have to recall that the *center* of Q is the subset of Q whose elements are compatible with all elements of Q and that $a \in Q$ is an *atom* if $a \neq \phi$ and $b \leqslant a$ imply $b = a$

[141]For a more detailed mathematical elaboration see also S. P. Gudder, "Dispersion-free states and the exclusion of hidden variables," *Proceedings of the American Mathematical Society* **19**, 319–324 (1968), and "Axiomatic quantum mechanics and generalized probability theory," in *Probabilistic Methods in Applied Mathematics*, A. T. Bharucha-Reid, ed. (Academic Press, New York, London, 1970), pp. 53–129.

or $b=\phi$, and finally the Q is *atomic* if every nonzero element in Q is preceded by an atom (see Appendix). Q is said to admit hidden variables if there exists a set M of states which are mixtures of dispersion-free states such that (Q, M) is a quantum system.

Gudder's theorem (for the proofs see the original paper) can now be stated:

THEOREM 1. A quantum system (Q, M) has a dispersion-free state if and only if Q has an atom in its center.

THEOREM 2. A question system Q admits hidden variables if and only if Q is an atomic Boolean algebra.

Since any question system corresponding to a truly quantum mechanical situation has incompatible experimental questions Gudder concluded that "no physically interesting quantum mechanical question system admits hidden variables."

Reviewing Gudder's approach we cannot fail to realize that, apart from terminology, it differs only slightly from the axiomatics of Jauch and Piron. By weakening their axiomatics to the extent that the quantum proposition $a \cap b$ need not exist at all Gudder eliminated the main objection raised by Bohm and Bub against the formalism of Jauch and Piron. As we shall see in Chapter 8, Quantum Logic, similar restrictions on the formation of such conjunctions had been proposed as early as 1936 by Martin Strauss in his "complementarity logic." Still, by thus avoiding certain physically questionable or perhaps unreasonable assumptions Gudder's proof of the inadmissibility of hidden variables in quantum mechanics is certainly an improvement over the demonstration proposed by Jauch and Piron. In fact, Gudder's proof seems to have impressed Capasso, Fortunato, and Selleri to such an extent that in their review paper[142] on the hidden variable problem they did not hesitate to make this statement: "Only when someone will be able to criticize in a satisfactory way Gudder's hypotheses (just as has been done for those of von Neumann and Jauch and Piron), will we again be able to take into account the possibility of obtaining a theory more general than quantum mechanics in the sense of the hidden variables."

Recently, Wilhelm Ochs[143] at the University of Munich did indeed challenge of the correctness of Gudder's demonstration as well as the soundness of his hypotheses. Ochs succeeded in constructing a model

[142]V. Capasso, D. Fortunato, and F. Selleri, "Von Neumann's theorem and hidden-variable models," *Rivista del Nuovo Cimento* **2**, 149–199 (1970).

[143]W. Ochs, "On Gudder's hidden-variable theorems," *Nuovo Cimento* **10B**, 172–184 (1972).

which in spite of its satisfying all postulates Q.1 to Q.10 and M.1 to M.6 does admit hidden variables. Gudder's weaker version of his proof (published in the *Proceedings of the American Mathematical Society* see Ref. 141), which is based on a stronger assumption than postulate Q.5, although mathematically correct, rests according to Ochs upon an assumption which, as in the case of previous "impossibility proofs," is unreasonable from a physical point of view. In his defense[144] Gudder pointed out that his *Review* paper was written only as a mathematically simplified survey for interested physicists, and the looseness of its formulation led Ochs to believe that its intentions differ from those expressed in the *Proceedings* paper.

Meanwhile two mathematicians, Simon Bernard Kochen, a Belgian-born graduate of McGill University, Ph.D. (1958) from Princeton, and professor at Cornell University, who had spent the year 1962–1963 on a Guggenheim Fellowship at the Swiss Federal Institute of Technology (E.T.H.) in Zurich, and his Swiss colleague Ernst P. Specker of the E.T.H. and pupil of F. Gonseth, had published a proof[145] on the impossibility of hidden variables, which was based on a new argument. According to Kochen and Specker a set of observables A_j (the same letter denotes an observable and its associated operator) is called "commeasurable" if there exist an observable B and (Borel) functions f_j such that $A_j = f_j(B)$. In this case it is possible to measure simultaneously the A_j for it is only necessary to measure B and apply the functions f_j to the measured result to obtain the value of each A_j. According to a theorem[146], proved first by von Neumann and subsequently in a different way by Mark Aronovich Neumark, pairwise commuting observables are commeasurable. Kochen and Specker called a set of observables a "partial algebra" if it is closed under the formation of the "partial operations" of sums and products for commeasurable observables in accordance with the following rules: if $A_1 = f_1(B)$ and $A_2 = f_2(B)$, then $r_1A_1 + r_2A_2 = (r_1f_1 + r_2f_2)(B)$ and $A_1A_2 = (f_1f_2)(B)$ where r_1 and r_2 are real numbers.

[144]Letter from S. P. Gudder to the author, dated September 27, 1971. Cf. also S. Gudder, "Hidden-variable models for quantum mechanics," *Nuovo Cimento* **10B**, 518–522 (1972).

[145]S. Kochen and E. P. Specker, "The problem of hidden variables in quantum mechanics," *Journal of Mathematics and Mechanics* **17**, 59–87 (1967), reprinted in *The Logico-Algebraic Approach*, Ref. 106.

[146]J. von Neumann, "Über Funktionen von Funktionaloperatoren," *Annals of Mathematics* **32**, 191–226 (1931); *Collected Works* (Ref. 1-5), Vol. 2, pp. 177–212. M. A. Neumark, "Operatorenalgebren im Hilbertschen Raum," in *Sowjetische Arbeiten zur Funktionalanalysis* (Verlag Kultur und Fortschritt, Berlin, 1954), p. 227. For discrete spectra the proof is simple. Cf., e.g., J. L. Park and H. Margenau, "Simultaneous measurement in quantum theory," *International Journal of Theoretical Physics* **1**, 211–283 (1968), esp. pp. 231–232.

Since the algebraic structure of a partial algebra, they argued, is independent of the particular theory employed, a hidden variable theory must not only satisfy the conditions of Definition II (Section 7.3) but must also preserve this structure. "A necessary condition then for the existence of hidden variables is that this partial algebra be imbeddable in a commutative algebra (such as the algebra of all real-valued functions on a phase space)." Let us call this briefly "the Kochen and Specker condition" and let us denote by Σ the set R^{Γ} of all real-valued functions on Γ which is a commutative algebra. The Kochen and Specker condition imposes certain restrictions on the functions f of Definition II. If $f_A(\lambda)=a_n$, an eigenvalue of A, and $B=g(A)$, then B has in the state $|a_n\rangle$ the value $g(a_n)$ and consequently $f_B(\lambda)=g(a_n)=g(f_A(\lambda))$ or

$$f_{g(A)}=g(f_A). \tag{12}$$

It is easy[147] to show that the preceding restriction implies that the mapping $f:\Gamma\to R$ preserves the partial operations. A hidden variable extension is therefore possible only if there exists an embedding of a partial algebra P of Hermitian operators into Σ. Since h_λ, defined by the relation $h_\lambda(A)=f_A(\lambda)$, defines for each $\lambda\in\Gamma$ a homomorphism[148] h_λ : $P\to R$ of the partial algebra P into R, the existence of such a homomorphism is a necessary condition for the possibility of hidden variable theories. But as Kochen and Specker could show by a rather complicated mathematical proof, no such homomorphism exists.

Kochen and Specker also demonstrated this contention by a direct intuitive argument in which they referred to the angular momentum operator equation $J^2=J_x^{\,2}+J_y^{\,2}+J_z^{\,2}$ for the lowest orbital state $n=2$, $l=0$, $s=1$ of orthohelium in the $j=1$ state. Since for any three mutually orthogonal directions (x, y, z) the operators $J_x^{\,2}$, $J_y^{\,2}$, $J_z^{\,2}$ commute among themselves as well as with J^2 (which yields 2 with $\hbar=1$) and are therefore simultaneously measurable, always yielding one value which is 0 and two values each of which is 1, a hidden variable theory should provide a

147
$$\begin{aligned} f_{r_1A_1+r_2A_2}&=f_{(r_1g_1+r_2g_2)(B)}=(r_1g_1+r_2g_2)(f_B)=r_1g_1(f_B)+r_2g_2(f_B)\\ &=r_1f_{g_1(B)}+r_2f_{g_2(B)}=r_1f_{A_1}+r_2f_{A_2}\\ f_{A_1A_2}&=f_{(g_1g_2)(B)}=(g_1g_2)(f_B)=g_1(f_B)g_2(f_B)=f_{g_1(B)}f_{g_2(B)}\\ &=f_{A_1}f_{A_2}. \end{aligned}$$

148 For $h_\lambda(r_1A_1+r_2A_2)=r_1h_\lambda(A_1)+r_2h_\lambda(A_2)$, $h_\lambda(A_1A_2)=h_\lambda(A_1)h_\lambda(A_2)$ and $h_\lambda(I)=1$.

function which correctly assigns to each direction the result that will be obtained when measuring the square of such a component in that direction. By a geometrical argument they could show that no such function or assignment exists.

This geometrical argument was considerably simplified by Frederick J. Belinfante of Purdue University in his detailed study[149] of hidden variable theories. Another interesting simplification had been worked out in 1969 by Richard Friedberg of Columbia University but was never published. Following the latter, we shall present what is probably the simplest version of the argument.

Consider the spin matrices for a system of spin 1 (cf., e.g., A. Messiah, Ref. 5-58 (Chapter XIII, section 21)):

$$S_x = \begin{pmatrix} 0 & 0 & 0 \\ 0 & 0 & -i \\ 0 & i & 0 \end{pmatrix}, \quad S_y = \begin{pmatrix} 0 & 0 & i \\ 0 & 0 & 0 \\ -i & 0 & 0 \end{pmatrix},$$

$$S_z = \begin{pmatrix} 0 & -i & 0 \\ i & 0 & 0 \\ 0 & 0 & 0 \end{pmatrix}. \tag{13}$$

Obviously, the three matrices

$$S_x^2 = \begin{pmatrix} 0 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{pmatrix}, \quad S_y^2 = \begin{pmatrix} 1 & 0 & 0 \\ 0 & 0 & 0 \\ 0 & 0 & 1 \end{pmatrix},$$

$$S_z^2 = \begin{pmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 0 \end{pmatrix}, \tag{14}$$

each having only eigenvalues 0 and 1, commute with each other so that the observables, represented by them, can be measured simultaneously. Their sum S^2 equals 2 (multiplied by the unit matrix). A measurement of the observable represented by the Hermitian operator

$$aS_x^2 + bS_y^2 + cS_z^2 = \begin{pmatrix} b+c & 0 & 0 \\ 0 & a+c & 0 \\ 0 & 0 & a+b \end{pmatrix}, \tag{15}$$

[149]Ref. 119, pp. 35–43, 63–64.

where a, b, and c are three different numbers, yields according to quantum mechanics one of the eigenvalues $a+b$, $a+c$, $b+c$. Hence each single of these three measurement results reveals which one of S_x^2, S_y^2, S_z^2 has the value 0, while the other two have the value 1. By associating with each direction d (unit vector) in space a d-proposition the proposition "the component of S in direction d has the value 0," we establish a one-to-one correspondence between directions d and d-propositions so that we may even identify directions with such propositions. It thus makes sense to speak about "orthogonal propositions" or about "propositions making an angle α." Since (x, y, z) is an arbitrary triad of mutually orthogonal directions we conclude from our preceding considerations that the statement

one and only one of three mutually orthogonal d-propositions is true (16)

is experimentally verifiable. Now, the directions $x+y$, $x-y$, z—or in vector notation $(1\sqrt{2})\,(1, 1, 0)$, $(1/\sqrt{2})\,(1, -1, 0)$, $(0, 0, 1)$— are mutually orthogonal; so are $x+z$, $x-z$, y. Hence the four d-propositions $x+y$, $x-y$, $x+z$, $x-z$ cannot all be false; for if they were, both y and z would be true in contradiction to (16). Furthermore, since each of the four directions $x+y$, $x-y$, $x+z$, $x-z$ is orthogonal to one of the two directions $d_1=y+z+x$, $d_2=y+z-x$, the propositions d_1 and d_2 cannot both be true, for if they were, the propositions $x+y$, $x-y$, $x+z$, $x-z$ would all be false, in contradiction to our preceding conclusion. Clearly, the angle α between d_1 and d_2 is $\cos^{-1}(\frac{1}{3})$ (or approximately 70°30′). Hence, in view of the arbitrariness of the coordinate system, we conclude

Two d-propositions making an angle α cannot both be true. (17)

Now, let the direction d_3 lie in the xy-plane and make an angle α with y. If d_3 is true, it follows from (16) that z is false and it follows from (17) that y is false, and hence it follows, again from (16), that x is true. Since the angle β between d_3 and x is $\pi/2-\alpha$ (or approximately 19°30′) and the coordinate system is arbitrary we arrive at the conclusion

Two d-propositions making an angle β imply each other. (18)

But from each direction d in space any other direction d' can be reached by a finite number of rotations through the angle β about axes suitably chosen. In fact, since $\beta > 18°$, not more than five rotations will be required. It therefore follows from (18) that every d-proposition implies every other, in contradiction to (16).

If we ignore the possibility that the truth value of a d-proposition may depend on which other d-propositions are measured simultaneously with it, we must conclude that any simultaneous assignment A of truth values to all conceivable d-propositions contradicts (16). But since every theory of (noncontextual) hidden variables ascribes to every d-proposition a definite truth value and hence implies the possibility of such an assignment A, and since it has just been shown that such an assignment A is impossible, we are led to the Kochen and Specker conclusion that quantum mechanics excludes such hidden variables.

This conclusion, they pointed out, does not necessarily hold for restricted parts of quantum mechanics. In fact, by imbedding the partial algebra of Hermitian operators on a two-dimensional complex Hilbert space into the commutative algebra of real-valued functions on a suitable phase space Kochen and Specker themselves constructed a hidden variable theory of the electron spin which satisfies all the required conditions. Realizing the contradiction with von Neumann's proof they compared von Neumann's assumptions with those underlying their own proof. In their reformulation of von Neumann's proof Kochen and Specker pointed out that von Neumann considered as a necessary condition for the existence of hidden variables the existence of a function $E : H \to R$ where E denotes the expectation value $\langle\rangle$, H the set of self-adjoint operators which is thus mapped into the set R of reals. The function (or functional) E is supposed to satisfy the following postulates:

$\mathrm{P_I}$. $E(I)=1$.
$\mathrm{P_{II}}$. $E(aA)=aE(A)$ for all $a \in R$ and all $A \in H$.
$\mathrm{P_{III}}$. $E(A^2)=E^2(A)$ for all $A \in H$.
$\mathrm{P_{IV}}$. $E(A+B)=E(A)+E(B)$ for all $A, B \in H$.

(Von Neumann's P.III is not required.)

In a lemma it is shown that if a mapping E satisfies $\mathrm{P_I}$ to $\mathrm{P_{III}}$ and $\mathrm{P'_{IV}}$: $E(A+B)=E(A)+E(B)$ for all $A,B \in H$ that *commute*, then E is multiplicative for commuting operators, for

$$\begin{aligned} E^2(A)+2E(A)E(B)+E^2(B) &= [E(A)+E(B)]^2 \\ &= E^2(A+B) = E\left[(A+B)^2\right] \\ &= E(A^2+2AB+B^2) \\ &= E^2(A)+2E(AB)+E^2(B). \end{aligned}$$

Next, in a corollary it is proved that for a mapping E satisfying $\mathrm{P_I}$ to $\mathrm{P_{III}}$ and $\mathrm{P'_{IV}}$ for all $A \in H, E(A)$ lies in the spectrum of A. For otherwise

$A - E(A)$ would have an inverse B and $1 = E(I) = E\{[A - E(A)]B\} = E[A - E(A)]E(B) = [E(A) - E(A)]E(B) = 0$. Finally, Kochen and Specker considered the operators

$$A = \begin{pmatrix} 1 & 0 \\ 0 & 0 \end{pmatrix} \quad \text{and} \quad B = \frac{1}{2}\begin{pmatrix} 1 & 1 \\ 1 & 1 \end{pmatrix} \tag{19}$$

whose eigenvalues are 0 and 1. Since the eigenvalues of $A + B$ are $1 \pm 2^{-1/2}$ (which differ from 0, 1 and 2), $E(A+B) \neq E(A) + E(B)$ in contradiction to P_{IV}. Hence the incompatibility of P_I to P_{IV} has been demonstrated even if the domain of E is restricted to Hermitian operators on a two-dimensional Hilbert space. Without using von Neumann's P.IV whose soundness, as we have seen, has already been questioned by Grete Hermann, and replacing it by P'_{IV}, which refers only to commuting observables, Kochen and Specker have thus recovered in an almost trivial way Gleason's refinement of von Neumann's proof. Furthermore, the main conclusion of their paper—the nonexistence of the previously mentioned homomorphism—can be obtained, as Bell already pointed out, from the theorem of Gleason. Kochen and Specker have proved once again that noncontextual hidden variables do not exist in quantum mechanics. Hidden variables in quantum theory thus can never predict what *is* but, at best, only what *would be found*.[150]

These results were fully taken into consideration in Gudder's[151] redefinition of hidden variable theories, which will be referred to as *Definition III*. This definition no longer regards hidden variables as a conceptual instrument to predict, from a knowledge of ψ and λ, the outcome of *all* possible measurements, but rather restricts their role to predicting the outcome of *any* single, fully specified measurement without violating, of course, the statistics of the standard theory. Using the language of quantum logic, known to us in its essentials from our discussion of the Jauch and Piron impossibility proof, Gudder defined a *proposition system* as a partially ordered set $L = \{a, b, c, \ldots\}$ with first and last elements (zero and one, respectively), with an orthocomplementation, and in which with a sequence of disjoint elements a_j also $\cup a_j$ is in L. A *state* is defined as a map $m: L \rightarrow [0,1]$ with $m(1) = 1$ and $m(\cup a_j) = \Sigma m(a_j)$ for a sequence of disjoint a_j.

More precisely, the logical relation between Gleason's work and that of Kochen and Specker, so far as it concerns the former, may be explained as

[150]For a particularly lucid accentuation of this point see Belinfante, Ref. 149.

[151]S. P. Gudder, "On hidden-variable theories," *Journal of Mathematical Physics* **11**, 431–436 (1970).

follows. Gleason's work implied that for any Hilbert space H of at least three dimensions, no probability measure μ exists which, defined on the subspaces of H, assigns the value 1 to one and only one vector of every orthonormal basis B which spans H (it is of course assumed that μ also satisfies certain other conditions which in the present context can be ignored). If to every such B there belongs an observable (whose eigenvectors would then be the vectors of B), it would follow that not all quantum observables possess simultaneously precise values, a result fatal to noncontextual hidden variable theories.

It may be objected, however, that although to every observable there belongs a basis B as described, the converse statement (according to which to every such B there belongs an observable) is not necessarily true. And if indeed the set of all observables of a system whose states are represented by vectors in H is smaller than the set of all orthonormal bases in H, the bases for which μ does not satisfy the aforementioned condition may conceivably be just those with which no observables are associated. But then the preceding argument against hidden variable theories would break down. The situation could probably be remedied by a general theory—if one were available—that mathematically characterizes quantum observables (not every Hermitian operator represents a quantum mechanical observable!). The problem would then be: Does there always exist in H a μ which, besides satisfying other conditions, assigns the value 1 to one and only one vector in every orthonormal basis B which spans H *and* to which there belongs an observable?

But even in the absence of a general theory of that kind a negative decision on the problem can be reached. To this end it suffices to produce only one example of a quantum mechanical system Σ and a set A of observables on it for which it can be shown that no μ exists which assigns the value 1 to precisely one eigenvector of each member of the set A. This is precisely what Kochen and Specker have done. For Σ they chose orthohelium in its lowest orbital state and for A a set of 117 different coordinate decompositions of the spin angular momentum.

A set of M of states is said to be *full* if for any $a \neq \phi$ M contains an m such that $m(a)=1$ and for $a \neq b$ an m such that $m(a) \neq m(b)$. A pair (L,M) is called a *quantum proposition system*. A dispersion-free state s on L is a map $s: L \to \{0,1\}$ such that $s(I)=1$ and $s(\cup a_j)=\Sigma m(a_j)$ for a finite set of disjoint a_j. Single measurements—and this is now the important point—correspond to Boolean sub-σ-algebras[152] B of L and to say that the results

[152]A σ-algebra B of subsets of a nonempty set L is a nonempty class of sets which contains L and is closed under complementation and countable unions.

of such a measurement are completely determined means that one deals with a dispersion-free state s on B. Denoting the set of such states by M_B we can now define, following Gudder, a hidden variable theory as follows.

Definition III. (L, M) admits a hidden variable theory if there is a probability space (Ω, F, μ) with the property that, for any maximal[153] Boolean sub-σ-algebra $B \subseteq L$, there is a map h_B from $M \times \Omega$ onto M_B such that (1)$\omega \rightarrow h_B(m,\omega)(a)$ is measurable for every $m \in M$, $a \in B$, and (2) $\int_\Omega h_B(m,\omega)(a)\,d\mu(\omega) = m(a)$ for every $m \in M$, $a \in B$. Finally, if $\mathfrak{B}$ denotes the set of maximal Boolean sub-σ-algebras of L, $((\Omega, F, \mu), \{h_B | B \in \mathfrak{B}\})$ is called a hidden variable theory for (L, M).

On the basis of this abstract definition of a hidden variable theory T' Gudder showed that within the present framework of quantum mechanics T it is always possible to construct such an extension T' and even to show that such a T', if it is *minimal* [i.e., if $h_B(m,\omega_1) = h_B(m,\omega_2)$ for all $m \in M$ and $B \in \mathfrak{B}$ implies $\omega_1 = \omega_2$], is essentially uniquely defined (i.e., up to a one-to-one mapping).

7.9. THE APPEAL TO EXPERIMENT

Our account of experiments that have been designed or performed to find out whether nature is ruled by indeterministic quantum theory or by some deterministic hidden variable theory will be confined to a brief historical survey without technical details.[154]

It is clear that the realizability of such experiments depends on the existence of effects for which predictions based on hidden variables differ from those based on ordinary quantum mechanics; this disagreement, moreover, must be experimentally detectable. A hidden variable interpretation which precisely reproduces all predictions of the ordinary theory is of course empirically indistinguishable from quantum mechanics. But, as we have seen, there was hardly a hidden variable proposal of this kind. Even Bohm's theory of the early 1950s claimed to imply effects that contradict ordinary quantum mechanics.

[153] B is *maximal* if it cannot be augmented without violating the distributive law.

[154] For additional information see the original papers, Ph.D. theses, technical reports and F. J. Belinfante, "Experiments to disprove that nature would be deterministic" (mimeographed, Purdue University, 1970), to be published.

Most important in this respect was Bell's result according to which any local hidden variable theory, whatever the nature of its variables, conflicts with quantum mechanics for certain parameters. It was in the search for such discrepancies that during the past few years most elaborate and expensive experiments have been designed to enforce a decision on the previous question, a question whose importance according to some authors well transcends the purely scientific interests of physics. Frederik J. Belinfante, for example, stressed its implications for religion by pointing out that quantum mechanical indeterminism may be conceived as harmonizing with the belief in God, who continuously makes his own decisions about the happenings in this world, decisions unpredictable for us, while "if nature would be fully deterministic, one might reason that there would be no task for a 'God' in this world."[155]

The earliest reference to an experimental test of a possible conflict with the usual interpretation of quantum mechanics was not directly concerned with the problem of hidden variables; but as it dealt with the Einstein-Podolsky-Rosen argument it had a certain bearing on this issue, especially since it provided the pattern for the subsequent tests of hidden variables. The question under discussion was whether the available experimental facts are compatible with the hypothesis, discussed by Furry,[156] that in the case of spatially separated particles the prevailing quantum mechanical formulation of the many-body problem breaks down, or, more specifically, with the hypothesis that the state function (pure state) under discussion should be replaced by a mixture. Raising the question of whether the physical situation described in the Einstein-Podolsky-Rosen paper has ever experimentally been verified, Bohm and Aharonov[157] remarked that in the absence of such a verification one could feel free to adopt the just-mentioned hypothesis and thus avoid the "paradoxical" aspects without getting into conflict with available experimental results. Bohm and Aharonov now pointed out that if the two separated electrons which had interacted with each other were replaced by the two photons produced in a positron-electron annihilation and if the spin components of the former were replaced by the polarization components of the latter, such an experiment had indeed already been performed: the measurement of the polarization correlation of annihilation photons carried out in November 1949 at the Pupin Laboratories of Columbia University by Chien-Shiung

[155] *Op. cit.* (1970), p. 27.

[156] See Section 6.6.

[157] D. Bohm and Y. Aharonov, "Discussion of experimental proof for the paradox of Einstein, Rosen and Podolsky," *Physical Review* **108**, 1070–1076 (1957).

Wu and Irving Shaknov.[158] This experiment has an interesting history itself.

On October 1, 1945, John Archibald Wheeler, a member of the Princeton faculty since 1938 who at that time had just completed a research project for the du Pont de Nemours Company in Wilmington, Delaware, and Richland, Washington, submitted to the New York Academy of Sciences a paper[159] on annihilation radiation in competition for the A. Cressy Morrison Prize in Natural Science. Discussing the theory of electron-positron pairs and possible applications of the pair theory Wheeler suggested possibilities of testing some of the predictions of that theory and, in particular, the prediction that the two photons, emitted in opposite directions, in the annihilation of the singlet state are polarized at right angles. Thus if one photon is linearly polarized in one plane, its partner, which is emitted in the opposite direction with equal momentum, must be linearly polarized in the perpendicular plane. Recalling that the scattering of a photon by a free electron depends on its polarization Wheeler suggested testing the theory as follows. A radioactive source of slow positrons is covered with a foil sufficiently thick to guarantee annihilation of all the positrons and is placed at the center of a lead sphere through which a narrow hole is drilled along one of its diameters. At both ends of the channel carbon scatterers are placed. Photons scattered by one of these scatterers through approximately 90° are recorded by a counter at a given azimuth, while those scattered at the other end are recorded by a second counter in coincidence with the former. By measuring the coincidence counting rates with different relative azimuths the perpendicular correlation of polarizations can be tested. For the sake of historical accuracy it should be mentioned that a similar suggestion was made in 1947 independently by R. C. Hanna at the Cavendish Laboratory in Cambridge, England.

The ratio $\rho = N_{\perp} : N_{\parallel}$, where $N_{\perp}$ denotes the coincidence counts when the two detectors are at right angles to each other and $N_{\parallel}$ the coincidence counts when they are coplanar, was calculated for different scattering angles θ by Pryce and Ward[160] in 1947 and subsequently by Snyder,

[158] C. S. Wu and I. Shaknov, "The angular correlation of scattered annihilation radiation," *Physical Review* **77**, (1950); Letter to the Editor, dated November 21, 1949.

[159] J. A. Wheeler, "Polyelectrons," *Annals of the New York Academy of Sciences* **48**, 219–238 (1946). Awarded an A. Cressy Morrison Prize in 1945.

[160] M. H. L. Pryce and J. C. Ward, "Angular correlation effects with annihilation radiation," *Nature* **160**, 435 (1947).

Pasternack, and Hornbostel.[161] The first experimental verifications of these predictions were carried out simultaneously in 1948 by Bleuler and Bradt[162] at Purdue University and by Hanna[163] at Cambridge. Soon afterwards Vlasov and Dzeljepov[164] repeated these measurements in Russia. It was in view of the fact that the margin of error associated with the results obtained by Bleuler and Bradt was so large that a detailed comparison with the theory seemed rather difficult and in view of the fact that Hanna's results were consistently smaller than those predicted that Wu and Shaknov thought it desirable to reinvestigate the problem by using newly developed scintillation counters of considerably improved efficiency. Their result was in excellent agreement with the theoretically calculated value.[165]

The relation of these experiments to the physical situation of the Einstein-Podolsky-Rosen paper can be clarified as follows. If the two photons are assumed to be moving in the $-z$ and $+z$ directions, the conservation laws of parity and angular momentum imply that the state of the combined system may be written $\psi=2^{-1/2}(\psi_r{}^1\psi_r{}^2+\psi_l{}^1\psi_l{}^2)$, where ψ_r^k denotes that the kth photon ($k=1$ or 2) is right circularly polarized and $\psi_l{}^k$, that it is left circularly polarized, or may be written $\psi=2^{-1/2}(\psi_x{}^1\psi_y{}^2-\psi_y{}^1\psi_x 2)$, where $\psi_x{}^k$ denotes that the linear polarization of the kth photon is along the x-direction and similarly for $\psi_y{}^k$. Thus just as in the Bohm version of the Einstein-Podolsky-Rosen argument for electrons, if the circular polarization of photon 1 is measured, the result (say $\psi_r{}^1$) entails, according to the laws of quantum mechanics, that photon 2 is in state $\psi_r{}^2$. But if the linear polarization of photon 1 is measured, the result (say $\psi_x{}^1$) implies that photon 2 is in state $\psi_y{}^2$. One has therefore the option of predicting either the linear or the circular polarization of photon 2 by means of choosing for the directly measured observable $\mathcal{Q}_1$ (see Section 6.3) either the linear or the circular polarization of photon 1. But according to quantum mechanics photon 2 cannot have simultaneously definite

[161] H. S. Snyder, S. Pasternack, and J. Hornbostel, "Angular correlation of scattered annihilation radiation," *Physical Review* **73**, 440–448 (1948).

[162] E. Bleuler and H. L. Bradt, "Correlation between the states of polarization of the two quanta of annihilation radiation," *Physical Review* **73**, 1398 (1948).

[163] R. C. Hanna, "Polarization of annihilation radiation," *Nature* **162**, 332 (1948).

[164] N. A. Vlasov and B. S. Dzeljepov, "Poljarizatzija annigiljatzionnikh gamma-kvantov," *Doklady Akademii Nauk SSSR* **69**, 777–779 (1949).

[165] It should be noted that a less satisfactory result was later obtained by G. Bertolini, M. Bettoni, and E. Lazzarini, "Angular correlation of scattered annihilation radiation," *Il Nuovo Cimento* **2**, 661–662 (1955). Cf. also H. Langhoff, "Die Linearpolarisation der Vernichtungsstrahlung von Positronen," *Zeitschrift für Physik* **160**, 186–193 (1960).

values for both kinds of polarization. The situation is therefore completely analogous to that considered by Einstein, Podolsky, and Rosen.

Bohm and Aharonov now considered the assumption, mentioned by Furry, according to which, once the photons have sufficiently separated, the wave function is no longer a superposition with definite phase relations of its components, implying those peculiar correlations, but that instead each photon goes into some definite state of polarization, related to that of its partner, and that an ensemble of such pairs possesses rotational symmetry around the z-axis as well as reflection symmetry in the xy-plane, to recover the expected symmetries. They also showed that, contrary to the case of the position and momentum measurements considered by Einstein and his collaborators, in the present case this assumption can be tested. In fact, according to Bohm and Aharonov, the Wu and Shaknov experiment was precisely such a test and it disproved this assumption. As Richard Friedberg[166] recently pointed out, the issue under discussion may be presented in the mathematical form of Bell's treatment of hidden variables for it can be shown that the foregoing assumption implies that mixtures of the type considered would lead to a correlation function $P(a,b) = C\cos 2(a-b)$ where a and b are the directions of the analyzer axes and $|C| \leqslant \frac{1}{2}$, whereas according to quantum mechanics and in agreement with the experimental results obtained by Wu and Shaknov $C = -1$.

Bohm and Aharonov's claim that the Wu and Shaknov experiment can be considered as empirical evidence against the Furry hypothesis was seriously criticized by Asher Peres and Paul Singer,[167] who, like Bohm and Aharonov at that time, were affiliated with the Haifa Institute of Technology. Their main argument was that Bohm and Aharonov overlooked an important difference between polarization and spin: a photon's spin (circular polarization) is always oriented in the direction of propagation, hence not gauge-invariant and therefore without physical meaning. "One thus sees," they declared, "that the spin of photons, if taken *alone*, cannot be used for Bohm's formulation of the EPR paradox, because it has only one component." These and similar objections were rebutted by Bohm and Aharonov[168] and, as Professor Aharonov told the present author,[169] Peres

[166]Cf. L. Kasday, "Experimental test of quantum predictions for widely separated photons," in *Foundations of Quantum Mechanics* (Ref. 1, pp. 195–210).

[167]A. Peres and P. Singer, "On possible experimental tests for the paradox of Einstein, Podolsky and Rosen," *Il Nuovo Cimento* **15**, 907–915 (1960).

[168]D. Bohm and Y. Aharonov, "Further discussion of possible experimental tests for the paradox of Einstein, Podolsky and Rosen," *Il Nuovo Cimento* **17**, 964–976 (1960).

[169]Oral communication, April 9, 1968.

and Singer eventually admitted the untenability of their objections. Recently Michael Horne[170] adduced other weighty arguments to show that "an experimental test of deterministic local hidden-variable theories is not possible with annihilation gamma-rays and Compton polarimeters."

In any case, none of the alternative suggestions for experimental verifications of the Einstein-Podolsky-Rosen situation, such as the experiments based on pion decay, autoionization and proton-proton scattering proposed by Peres and Singer in their paper or the interesting, but highly problematic experiment proposed earlier by L. Jánossy and K. Nagy[171] of scattering electrons by a proton which is represented by a superposition of two wave functions, seem ever to have been carried out in the laboratory. All these experiments would have required scattering techniques and Compton polarimeters and, if feasible at all, would therefore have been open to the same criticisms as the Wu and Shaknov experiment, which, after all, did not analyze the polarization correlation but rather the polarization-dependent joint distribution for Compton scattering.

To eliminate the need of scattering techniques it was necessary to confine the study of the polarization correlation of photons to lower energies for which effective polarization filters were available. Such an experiment was performed in 1966 at Berkeley, California, by Carl Alvin Kocher and Eugene D. Commins.[172] They studied the correlation in linear polarization of two successive photons emitted in the cascade $6\,{}^1S_0 \to 4\,{}^1P_1 \to 4\,{}^1S_0$ in calcium ($\lambda_1 = 5513$ Å, $\lambda_2 = 4227$ Å). Since such cascade photons behave like a spin singlet they may be used to test the disagreement between the quantum mechanical predictions and those based on hidden variables and, being in the visible range of the spectrum, admit for this purpose the use of optical polarizers.

Although Kocher and Commins made no reference in their paper to Bell's inequality, it is most instructive to analyze their work from this point of view. In the cascade used by them ($J=0 \to J=1 \to J=0$) the initial and final angular momenta are zero. Conservation of angular momentum implies therefore that the two-photon state has zero angular momentum. The most general expression of such a state ψ is

$$a\psi_{r+}{}^1\psi_{l+}{}^2 + b\psi_{l+}{}^1\psi_{r+}{}^2 + c\psi_{r-}{}^1\psi_{l-}{}^2 + d\psi_{l-}{}^1\psi_{l-}{}^2 + e\psi_{r+}{}^1\psi_{r-}{}^2$$

$$+ f\psi_{l+}{}^1\psi_{l-}{}^2 + g\psi_{r-}{}^1\psi_{r+}{}^2 + h\psi_{l-}{}^1\psi_{l+}{}^2$$

[170] M. Horne, "Experimental consequences of local hidden variable theories," Ph.D. thesis, Boston University (mimeographed, 1970), pp. 82–85.

[171] L. Jánossy and K. Nagy, "Über eine Form des Einsteinschen Paradoxes der Quantentheorie," *Annalen der Physik* **17**, 115–121 (1956).

[172] C. A. Kocher and E. D. Commins, "Polarization correlation of photons emitted in an atomic cascade," *Physical Review Letters* **18**, 575–577 (1967).

where, for example, $\psi_{r+}{}^1$ denotes the eigenstate of photon 1 if right circularly polarized and moving along the $+z$-axis. Conservation of parity (implying $a=d$, $b=c$, $e=h$, $f=g$), rotational invariance (implying $a=b$ and $e=f$) and the fact that photon 1 moves along the $+z$-axis and photon 2 along the $-z$-axis reduce the state function to $2^{-1/2}(\psi_{r+}{}^1\psi_{r-}{}^2+\psi_{l+}{}^1\psi_{l-}{}^2)$ or, after transforming to a linear polarization basis with a separate right-handed coordinate system for each photon, to $2^{-1/2}(\psi_x{}^1\psi_x{}^2-\psi_y{}^1\psi_y{}^2)$ where redundant symbols have been omitted. Representing $\psi_x{}^1$ by $\begin{pmatrix}1\\0\end{pmatrix}_1$, $\psi_y{}^2$ by $\begin{pmatrix}0\\1\end{pmatrix}_2$, and so on, we obtain

$$\psi=2^{-1/2}\left[\begin{pmatrix}1\\0\end{pmatrix}_1\begin{pmatrix}1\\0\end{pmatrix}_2-\begin{pmatrix}0\\1\end{pmatrix}_1\begin{pmatrix}0\\1\end{pmatrix}_2\right].$$

If φ_k denotes the angle between the x-axis and the polarizer's axis (with respect to x, looking along the $+z$-axis) in the individual coordinate system of photon $k(k=1$ or $2)$, then the measurement of the linear polarization of photon k is represented by the Hermitian operator

$$Z_k(\varphi_k)=\begin{pmatrix}\cos 2\varphi_k & \sin 2\varphi_k\\ \sin 2\varphi_k & -\cos 2\varphi_k\end{pmatrix}.$$

Hence the expectation value for joint polarization measurement is given by

$$P_{\text{q.m.}}(\varphi_1,\varphi_2)=\langle\psi|Z_1(\varphi_1)Z_2(\varphi_2)|\psi\rangle=\cos 2(\varphi_1+\varphi_2)=\cos 2\varphi$$

where $\varphi=\varphi_1+\varphi_2$ is by definition the angle between the axes of the two polarizers. Now, using Bell's notation but noting that for polarization correlations $B(\mathbf{b},\lambda)=A(\mathbf{b},\lambda)$—and not as above—we obtain the inequality $|P(\mathbf{a},\mathbf{b})-P(\mathbf{a},\mathbf{c})|\leqslant 1-P(\mathbf{b},\mathbf{c})$ instead of (9) in Section 7.7. If we define α as the angle between the unit vectors $\mathbf{a}$ and $\mathbf{b}$, β as that between $\mathbf{b}$ and $\mathbf{c}$ so that $\alpha+\beta$ is the angle between $\mathbf{a}$ and $\mathbf{c}$, we finally obtain $|P(\alpha)-P(\alpha+\beta)|\leqslant 1-P(\beta)$. For $\alpha=\beta=30°$ (maximum violation) insertion of $P_{\text{q.m.}}(\varphi)=\cos 2\varphi$ yields $|\cos 60°-\cos 120°|\leqslant 1-\cos 60°$ or $1\leqslant 1/2$, which shows that for these orientations hidden variable predictions cannot agree with quantum mechanical predictions.

Kocher and Commins' measurements of the coincidence counting rates agreed with the quantum mechanical predictions. But their polarizers were not efficient enough to make their result reliable. More important still, they took data only for $\varphi=0°$ and $90°$ and not for intermediate values like $30°$ or $60°$ as required. Since it soon became clear that certain types of hidden variable theory can well reproduce the results for $\varphi=0°$ and $\varphi=90°$ the Kocher and Commins experiment (which at first seemed to disprove hidden variables) could not exclude, even if carried out with ideal polarizers, all possible types of hidden variable theories and thus turned out to be inconclusive.

Linear polarizers were used at that time also for a different experiment on hidden variables. Costas Papaliolios[173] of Harvard University tried to verify with their help the existence of the hidden variables ξ which had been introduced by Bohm and Bub in their specific hidden variable theory.[174] The distribution of these variables was assumed to randomize with a characteristic relaxation time τ of the order of 10^{-13} sec at room temperature. By sending photons through a stack of three linear polarizers and measuring the transmission variation, as the third polarizer was being rotated through an angle θ, (the flight time between the second and third polarizer being 7.5×10^{-14} sec, ξ would presumably not have sufficient time to relax) Papaliolios hoped to detect a discrepancy in the transmission (as a function of θ) with the usual quantum mechanical prediction. His results, however, agreed with the quantum theoretical predictions to within 1%. By using thinner polarizers and thus setting a lower upper bound for τ Papaliolios[175] planned to refine his measurements, which, had they yielded positive results, would have given substance to Schrödinger's objection to von Neumann's nonmathematical argument against hidden variables.[176]

Coming back to the Kocher and Commins experiment, it is interesting and will be important later to note that the cases of electron spin and photon polarization, although formally similar from the mathematical point of view, are fundamentally different from the physical point of view. As shown in an unpublished paper "On a Proposed Experimental Test of Hidden Variables" (1970) by M. R. Nelson and D. M. Warrington of Princeton University (the latter is now at the University of Ontago in Dunnedin, New Zealand), in the electron spin case the two assumptions that the measurements (1) are determined by a hidden variable theory and

[173]C. Papaliolios, "Experimental test of a hidden-variable quantum theory," *Physical Review Letters* **18**, 622–625 (1967).

[174]See Ref. 133.

[175]Letter to the author of December 19, 1967. For a different proposal to test the Bohm and Bub theory see P. M. Clark and J. E. Turner, "Experimental tests of quantum mechanics," *Physics Letters* **26** A, 447 (1968).

[176]See Section 7.4.

(2) agree with quantum mechanics when $\mathbf{a}_1 = \mathbf{a}_2$ (these unit vectors denote, like $\mathbf{a}$ and $\mathbf{b}$ in Bell's notation, the orientations of the measuring devices) suffice to disprove the existence of hidden variables. As we shall see, in the photon polarization case this argument breaks down.

To prove the assertion let us write down Bell's inequality with the abbreviation P_{ij} for $P(\mathbf{a}_i, \mathbf{a}_j)$, $|P_{ij} - P_{ik}| \leqslant 1 + P_{jk}$ and iterate it as follows:

$$|P_{12} - P_{13}| \leqslant 1 + P_{23},$$

$$|P_{12} - P_{14}| = |P_{12} - P_{13} + P_{13} - P_{14}| \leqslant |P_{12} - P_{13}| + |P_{13} - P_{14}|,$$

or

$$|P_{12} - P_{14}| \leqslant 1 + P_{23} + 1 + P_{34}.$$

After $n-2$ steps we obtain

$$P_{12} - P_{1n} \leqslant \sum_{k=3}^{n} (1 + P_{k-1,k}). \tag{20}$$

Now taking all vectors coplanar, $\mathbf{a}_1 = \mathbf{a}_2$, $\mathbf{a}_n = -\mathbf{a}_1$ and the remaining intermediate unit vectors evenly spaced at angles $\pi/(n-2)$, let $n \to \infty$ so that $a_{k-1} \to a_k$ and $P_{k-1,k} \to -1$ (in the spin case !); we obtain $|P_{12} - P_{1n}| \to |(-1) - (+1)| = 2$, whereas the sum on the right-hand side tends toward zero.[177] This contradiction refutes the assumption of local hidden variables. It is clear that this proof does not work for photon polarization since in this case $P_{k-1,k} \to +1$ and the sum diverges.

As the preceding considerations show, it was necessary not only to improve the precision of the Kocher and Commins measurements but also to extend their range to make them conclusive.

Inspired by Bell's 1964 paper[178] John F. Clauser,[179] a graduate of

[177]If $P_{k-1,k} \approx -\cos\pi/(n-2) \approx -\cos\pi/n$, then

$$\lim_{n=\infty} \sum_{3}^{n} \left(1 - \cos\frac{\pi}{n}\right) = \lim\left[n\left(1 - \cos\frac{\pi}{n}\right)\right] = 0$$

according to l'Hospital's rule for indeterminate forms.

[178]Ref. 120.

[179]J. F. Clauser, "Proposed experiment to test local hidden-variable theories," *Bulletin of the American Physical Society* **14**, 578 (1969). Clauser began his work at Columbia University (in 1968) where he attended a seminar given by the present author. Later he went to Berkeley to use the Kocher and Commins apparatus in collaboration with Stuart J. Freedman, a student of Commins.

Columbia University, studied precisely the possibilities of thus extending the Kocher and Commins experiment. Simultaneously Abner Shimony of Boston University and his student Michael Horne[180] hit upon the same idea. The theory of their experiment was based on the generalization of Bell's inequality.[181] An ensemble of correlated pairs of particles is considered, one particle of each pair entering apparatus I_a and the other apparatus II_b, where a and b are adjustable apparatus parameters (i.e., orientations of polarizers). In each apparatus every particle enters one of two channels marked $+1$ and -1 so that the result in I_a is a sequence $A(a)$ of the numbers $+1$ or -1, where the number is $+1$ (-1) if the particle entered channel $+1$ (-1), and $B(b)$ is likewise for II_b a sequence of the numbers $+1$ or -1, defined correspondingly. Interpreting $A(a)=\pm 1$ and $B(b)=\pm 1$ as emergence or nonemergence of the optical photon from the linear polarization filters (followed by detectors), oriented at a and b, respectively, the authors introduced the particular value ∞ of a (of b) to denote the removal of the polarizer at I_a (at II_b), so that $A(\infty)=B(\infty)=1$. Assuming that the probability of the joint detection of a pair of photons emerging from I_a and II_b as well as the flux into each measuring device are independent of a and b and are constant, the authors considered the experimentally measurable coincidence counting rate $R(a,b)$ which is proportional to $W[A(a)_+,B(b)_+]$, where $W[A(a)_\pm,B(b)_\pm]$ denotes the probability that $A(a)=\pm 1$ and $B(b)=\pm 1$. Defining $R_0=R(\infty,\infty)$, $R_1(a)=R(a,\infty)$, $R_2(b)=R(\infty,b)$ and using the formulae

$$P(a,b)=W[A(a)_+,B(b)_+]-W[A(a)_+,B(b)_-]-W[A(a)_-,B(b)_+]+W[A(a)_-,B(b)_-]$$

$$W[A(a)_+,B(\infty)_+]=W[A(a)_+,B(b)_+]+W[A(a)_+,B(b)_-],$$

and so on, they obtained

$$P(a,b)=\frac{4R(a,b)-2R_1(a)-2R_2(b)+R_0}{R_0} \tag{21}$$

so that with $R_1(a)=R_1(\text{constant})$, $R_2(b)=R_2(\text{constant})$ the experimentally testable relation

$$R(a,b)-R(a,c)+R(b',b)+R(b',c)-R_1-R_2\leqslant 0 \tag{22}$$

[180]J. F. Clauser, M. A. Horne, A. Shimony, and R. A. Holt, "Proposed experiment to test local hidden-variable theories," *Physical Review Letters* **23**, 880–884 (1969).
[181]Ref. 128.

followed from (10), a relation that is violated by the statistical predictions of ordinary quantum mechanics.

Since the experiment, although conceptually straightforward, is rather delicate from the instrumental point of view it was decided to check the outcome by several independent tests. The experiment was therefore carried out at Bell Laboratories, at Harvard University, and at Berkeley and at each place different atomic sources were used for the photon pair production. The results obtained seem to confirm the predictions of quantum mechanics and not those of local hidden variable theories. They thus provide strong evidence against the existence of local hidden variables.[182]

[182] S. J. Freedman and J. F. Clauser, "Experimental test of local hidden-variable theories," *Physical Review Letters* **28**, 938–941 (1972). It should be noted, however, that the results obtained at Harvard by Richard A. Holt (who used a mercury source) do not fully agree with the predictions of quantum mechanics. Whether this is due solely to experimental errors or has a deeper cause remains to be tested with improved instrumentation.

QUANTUM LOGIC Chapter Eight

8.1. THE HISTORICAL ROOTS OF QUANTUM LOGIC

The decomposition of a physical theory T, as outlined in Chapter 1, into a mathematical formalism F, a set of epistemic relations R, and a physical picture M implied that an interpretation of T should concentrate on one or more of these components. All the interpretations of quantum mechanics described so far were based on this assumption.

Certain developments in mathematics and philosophy, however, have led to the idea that the alternatives discussed so far were not exhaustive and that a fourth component, so to say, of a most general nature—which for this very reason had been altogether ignored—could also be an object of inquiry in the search for an interpretation: the formal structure of the deductive reasoning applied in formulating T. If a certain theory T leads to an impasse, it was claimed, it is not necessarily its mathematical formalism as such nor the meaning of its extralogical concepts that may have to be modified; it may equally well be the logic underlying the formulation of T which has to be revised. A search for an interpretation of quantum mechanics along these lines is usually called a quantum logical approach.

A historical precedent of such an approach was the intuitionistic revision of classical logic which L. E. J. Brouwer proposed in 1908 and A. Heyting further developed in the early 1930s. This was motivated by the desire to avoid the notorious antinomies in the theory of sets such as the "Russell paradox" or the "Burali-Forti paradox." At the same time it also became clear that pure mathematics can be based on different logical systems such as on that proposed by Russell and Whitehead in *Principia Mathematica* (1910–1913) and its revisions by Carnap, Church, and Tarski, or on the system of S. Leśniewski (1927–1931) or on W. V. Quine's *New Foundations* (1937). This fact strongly supported the thesis of the *relativity of logic*, which was put forward in the early 1930s by C. I. Lewis, H. Hahn, R. Carnap, and others.

The earliest consequence of applying the principle of the relativity of logic to quantum mechanics was the suggestion that one renounce for microphysics the validity of the traditional Aristotelian or, more precisely, Chrysippean[1] logic, which in its so-called law of bivalence recognizes only the two truth values "true" and "false." Since even theoretical physicists are rarely acquainted with these issues a short historical digression may serve a useful function.

Whether Aristotle admitted that there are propositions which are neither

[1]Chrysippus of Soli (c. 280–210 BC), one of the leaders of the Stoic school, was probably the first to state categorically that propositions are either true or false. Cf. J. Łukasiewicz, "Philosophische Bemerkungen zu mehrwertigen Systemen des Aussagenkalküls," *Comptes Rendus des Séances de la Société des Sciences et des Lettres de Varsovie* **23**, 51–77 (1930).

true nor false is a matter of some dispute.[2] His remarks about propositions dealing with future contingent events, such as "there will be a sea battle tomorrow"[3] (provided it is still undecided whether the battle will or will not occur), do not seem to commit him to either position. In the Middle Ages the truth status of future contingents [*futura contingentia*] was a much discussed problem in the Islamic and in the Latin worlds.[4] Some medieval thinkers classified such propositions as neither true nor false but indeterminate.[5] In the fifteenth century Peter de Rivo[6] defended most vigorously the thesis of indeterminate truth values at the University of Louvain. The earliest suggestion of a multivalued logic in modern times was possibly[7] made by Hugh MacColl.[8] As recently demonstrated,[9] Charles Santiago Sanders Peirce, the well-known American logician, psychologist, and cofounder of pragmatism, who—as is less well known—was also a devoted student of the philosophy of Duns Scotus, envisaged early in 1909 the possibility of a three-valued logic.

The first explicit formulation of a non-Chrysippean logic which was published was the system proposed by Nikolaj Aleksandrovic Vasil'ev,[10] a professor of philosophy at the University of Kazan. At the same place where 80 years earlier Lobachevski refuted the exclusive validity of Euclidean geometry Vasil'ev now challenged the truth of the law of bivalence, which, like Euclidean geometry, had virtually monopolized human thought

[2]W. Kneale and M. Kneale, *The Development of Logic* (Oxford University Press, Oxford 1962, 1968), pp. 45–54. Cf. also "On the history of the law of bivalence," *Polish Logic*, S. McCall, ed. (Oxford University Press, London 1967), pp. 63–65.

[3]Aristotle, *De Interpretatione*, 19a27–19b4.

[4]N. Rescher, *Studies in the History of Arabic Logic* (University of Pittsburgh Press, Pittsburgh, Pa., 1963), pp. 43–54.

[5]It has been claimed that explicit anticipations of a three-valued logic are found in the writings of Duns Scotus (1266–1308) and William of Ockham (c. 1295–1349). Cf. K. Michalski, "Le problème de la volonté à Oxford et à Paris au XIVe siècle," *Studia Philosophica* **2**, 233–365 (1937), and the review of Ph. Boehner's edition of Ockham's *Tractatus de Praedestinatione* by H. Scholz, *Deutsche Literaturzeitung* **69**, 47–50 (1948).

[6]L. Baudry, *La Querelle des Futurs Contingents* (Vrin, Paris, 1950).

[7]It may be objected that what MacColl regarded as "propositions" were actually propositional functions. Cf. B. Russell, "Symbolic logic and its applications," *Mind* **15**, 255–260 (1906).

[8]H. MacColl, *Symbolic Logic and its Applications* (Longmans, Green, London, 1906).

[9]M. Fisch and A. R. Turquette, "Peirce's triadic logic," *The Transactions of the Charles S. Peirce Society* **2**, 71–85 (1966).

[10]N. A. Vasil'ev, "O častnykh suždenijakh, o treugol'nike protivopoložnostej, o zakone isključennogo četvertogo" (On particular propositions, the triangle of opposition, and the law of the excluded fourth), *Učenie i Zapiski Kazanskogo Universiteta* **1910**, 47–61. *Voobražaemaya Logika* (Konspekt Lektsii) (*Imaginary Logic—Lecture Notes*), Kazan, 1911. "Voobražaemaya (nearistoteleva) logika," *Žurnal Ministerstva Narodnogo Prosveščeniya* **40**, 207–246 (1912).

for over 20 centuries. In his system of logic—which Vasil'ev, in analogy to Lobachevski's "imaginary" geometry, called "imaginary" [*voobražaemaya*] —propositions may be affirmative, negative, or "indifferent," each being false if one but not both of the other two kinds is true.[11] Thus rejecting the law of contradiction, Vasil'ev concluded that the "indifferent" proposition "S is A and S is not A" is neither true nor false.

The first propositional calculus based on three (or more) truth values was worked out by Jan Łukasiewicz, who had studied (Ph.D. 1902, University of Lvov) under Kazimierz Twardowski, a student of Brentano, and who became one of the founders of the famous Lvov-Warsaw school of philosophy. In 1915 he accepted a lectureship at the University of Warsaw where he subsequently became professor and rector until he left, in 1946, for Dublin to spend the last 10 years of his life as professor of mathematical logic at the Royal Irish Academy. To solve Aristotle's problem of future contingents Łukasiewicz introduced in a paper[12] presented in 1920 to the Polish Philosophical Society a third truth value $\frac{1}{2}$, different from 0, the truth value of a false proposition, and from 1, the truth value of a true proposition. He thus generalized the conventional truth table as follows: (1) if only 0 and 1 are involved, the old rules survive, (2) the truth-value of Np ("non p") is $\frac{1}{2}$ if that of p is $\frac{1}{2}$, (3) the implication pCq ("p implies q") is evaluated according to the rule that if the value of the antecedent p is less than or equal to the value of the consequent q, its value is 1 and otherwise $\frac{1}{2}$. The three-valued system of Łukasiewicz can thus be summarized by the following truth tables:

p	1	$\frac{1}{2}$	0
Np	0	$\frac{1}{2}$	1

p	1	1	1	$\frac{1}{2}$	$\frac{1}{2}$	$\frac{1}{2}$	0	0	0
q	1	$\frac{1}{2}$	0	1	$\frac{1}{2}$	0	1	$\frac{1}{2}$	0
pCq	1	$\frac{1}{2}$	0	1	1	$\frac{1}{2}$	1	1	1

[11]For details see V. A. Smirnov "Logičeskie vzglady N. A. Vasil'eva" (The logical views of N. A. Vasil'ev) in *Očerki po Istorii v Rossii* (Essays in the History of Logic in Russia), (Izdetel'stvo Moskovskogo Universiteta, Moscow, 1962), pp. 242–257. G. L. Kline, "N. A. Vasil'ev and the development of many-valued logics," in *Contributions to Logic and Methodology in honor of J. M. Bochenski*, A. T. Tymieniecka, ed. (North-Holland Publishing Company, Amsterdam, 1965), pp. 315–325.

[12]J. Łukasiewicz, "O logice trójwartościowej" (On three-valued logic), *Ruch Filozoficzny* **5**, 169–171 (1920); reprinted in J. Lukasiewicz, *Selected Works*, L. Borkowski, ed. (North-Holland Publishing Company, Amsterdam, 1970), pp. 87–88. An alternative many-valued propositional logic was developed at the same time by Emil Leon Post, "Introduction to a general theory of elementary propositions," *Journal of Mathematics* **43**, 163–185 (1921).

pOq, defined by $pCq.C.q$, corresponds to "p or q"; pAq, defined by $N(Np.0.Nq)$, corresponds to "p and q;" and pEq, defined by $pCq.A.qCp$, corresponds to "p is equivalent to q." In contrast to the ordinary two-valued logic in which $pV\text{-}p$ ("p or not-p") always has the truth value 1 whatever the truth value of p, $p.0.Np$ in Łukasiewicz' logic has for $p=\frac{1}{2}$ the value $\frac{1}{2}$ so that the law of the excluded middle (*tertium non datur*) is no longer valid.

Łukasiewicz also envisaged the possibility of generalizing his system to an infinitely-many-valued logic: If $[p]$ denotes the truth value of p, assumed to lie in the closed interval $[0, 1]$, and if the following definitions of the negation and implication are adopted:

$$[Np]=1-[p],$$

$$[pCq]= \begin{array}{lll} 1 & \text{if} & [p]\leqslant[q] \\ 1-[p]+[q] & \text{if} & [p]>[q] \end{array},$$

then it is clear that if only the limits of the interval, that is, 0 and 1, are admitted as possible truth values, ordinary two-valued logic is recovered; if in addition $\frac{1}{2}$ is a permissible value, Łukasiewicz' three-valued logic is retrieved.

The credit of priority in regarding quantum mechanics as a field for applying the system of three-valued logic which Łukasiewicz constructed for the purpose of mathematical logic alone goes to the Polish logician Zygmunt Zawirski, who had studied in Berlin and Paris (Ph.D. 1906) and, under the influence of Twardowski, soon joined the Lvov-Warsaw school. In the late 1920s Zawirski became professor of logic at the University of Poznan where he published in 1931 his suggestion that one apply three-valued logic to quantum mechanics. His paper,[13] printed in Polish, remained virtually unknown in the world of physics. His second paper,[14] published in French, also attracted little attention. Zawirski's point of departure was his claim that the wave-particle duality is a self-contradictory and yet valid statement. According to Heisenberg, whom he repeatedly quoted, "a thing cannot be a form of wave motion and composed of particles at the same time"[15] while, nevertheless, both these statements describe correctly the same physical situation; the equal legi-

[13]Z. Zawirski, "Logika trójwartościowa Jana Lukasiewicza. Próby stosowania logiki wielowartościowej do współczesnego przyrodoznawstwa" (Jan Lukasiewicz's three-valued logic. Attempts at application of many-valued logic to contemporary natural science), *Sprawozdania PoznańskiegoTowarzystwa Przyjaciol Nauk* **2**, nos. 2–4 (1931).

[14]Z. Zawirski, "Les logiques nouvelles et le champ de leur application," *Revue de Métaphysique et de Morale* **39**, 503–519 (1932).

[15]Ref. 3–19 (Chicago, 1930, p. 10).

timacy of both descriptions and the impossibility of eliminating either in favor of the other are inevitable consequences of Heisenberg's indeterminacy relations. Now, in two-valued logic, Zawirski pointed out, a proposition p which implies the equivalence between two contradictory propositions $q \equiv -q$ must be false. Hence within the framework of ordinary logic Heisenberg's indeterminacy relation p and the wave-particle parallelism $q \equiv -q$ are incompatible principles. Since in Łukasiewicz's three-valued logic, however, $[p \supset (q \equiv -q)] \supset -p$ does not hold, $q \equiv -q$ for $q = \frac{1}{2}$ having the truth value 1, and since no physicist, Zawirski argued, has any doubt about the Heisenberg principle or the wave-particle duality, the only way to solve this dilemma is to adopt the new logic: "*L'unique manière de se tirer d'affaire, c'est de se placer au point de vue de la nouvelle logique de Łukasiewicz.*"[16] Zawirski also discussed in this paper the possibility of applying Łukasiewicz's infinitely many-valued logic to the theory of probability.

One year later a similar proposal was made by Hans Reichenbach. Reichenbach began his career as an electronic engineer but soon realized that his major interest lay in the philosophy of science. Since 1926 professor of the philosophy of physics at the University of Berlin, Reichenbach, like Zawirski, constructed a logic of probability[17] with a continuous scale of truth values. Since Reichenbach's probability logic assigned to each proposition a determinate probability but not a truth value of indeterminacy, it conformed to classical physics rather than quantum mechanics.

A suggestion that one use nonclassical logic in microphysics was made in 1933 by the Bulgarian-born Swiss-American astrophysicist Fritz Zwicky, who after being awarded his doctorate (1922) at the Swiss Federal Institute of Technology in Zurich joined the California Institute of Technology (1925). Zwicky[18] proposed what he called a "principle of flexibility of scientific truth," according to which "no set of two-valued truths can be established with the expectation that this set ultimately will stand the test of experience." Formulations of scientific truth, he claimed, "must be *manyvalued.*" In an analysis of various scattering processes of electrons and annihilation processes as well as of the exclusion principle Zwicky suggested rejecting the law of the excluded middle to allow for a broader range of possibilities. "The conceptual difficulties in quantum mechanics

[16] Ref. 14 (p. 513). In a later paper "Über die Anwendung der mehrwertigen Logik in der empirischen Wissenschaft," *Erkenntnis* **6**, 430–435 (1936), Zawirski modified his view to some extent.

[17] H. Reichenbach, "Wahrscheinlichkeitslogik," *Berliner Berichte* **1932**, 476–488.

[18] F. Zwicky, "On a new type of reasoning and some of its possible consequences," *Physical Review* **43**, 1031–1033 (1933).

may be interpreted as due to the peculiar inconsistencies of this theory which in certain respects conforms with our principle of flexibility, whereas in other respects... quantum mechanics and the relativity theory are based on very antiquated notions. It should also be clear from our discussion that the recent controversies regarding the absolute truth of uncertainty principle versus causality are quite futile, as scientific truth intrinsically cannot be absolute." Zwicky's colleague, the historian of mathematics Eric Temple Bell, emphasized in a Postscript[19] the similarity of this "principle of flexibility" with the "relativity of logic" and the work of Łukasiewicz.

Zwicky's ideas were criticized by Henry Margenau,[20] who pointed out that the use of a many-valued logic does not lead to many-valued truths; true, physical laws, due to their empirical character, are in a state of flux and one replaces the other with the progress of research; this, however, does not mean that at a given time a law may be true in some cases and false in others—for then it ceases to be a law. The very application of a system of many-valued logic to the present body of physical knowledge, Margenau declared, cannot change the validity which has its source not in logic but in the status of physics as an empirical science.

The rejection of Zwicky's ideas by Margenau and others prompted Bell to include the following statement in his well-known book on the history of mathematics:[21] "... the reception of these [many-valued] logics by physicists was similar to that of all non-Euclidean geometries until Einstein discovered Riemann." Still, whereas about 100 years had to pass before the discovery of non-Euclidean geometry was followed by its first application in physics, the corresponding period for non-Aristotelian logic was only about 20 years. In fact, the first serious breakthrough of nonclassical logic in quantum mechanics was made in 1936. Not the law of bivalence, but rather the distributivity law of classical logic was the major target of this assault.

8.2. NONDISTRIBUTIVE LOGIC AND COMPLEMENTARITY LOGIC

The idea that the logic of quantum mechanics may differ in certain aspects

[19]E. T. Bell, "Remarks on the preceding note on many-valued truths," *ibid.*, 1033.

[20]H. Margenau, "On the application of many-valued systems of logic to physics," *Philosophy of Science* **1**, 118–121 (1934). In his later essay on Reichenbach's probability theory Margenau adopted a more positive attitude toward the applications of many-valued logics. He even declared that "many-valued logics may someday revolutionize science," but he also warned that "their potential value would be impaired if confusion as to their bearing on contemporary methods were allowed to creep into their very making." Cf. his "Probability, many-valued logics, and physics," *Philosophy of Science* **6**, 65–87 (1939).

[21]E. T. Bell, *The Development of Mathematics* (McGraw-Hill, New York, 1945), p. 574.

from that of classical mechanics had already been envisaged by John von Neumann when he wrote in his treatise on the foundations of quantum mechanics that "the relation between the properties of a physical system on the one hand, and the projections on the other, makes possible a sort of logical calculus with these. However, in contrast to the concepts of ordinary logic, this system is extended by the concept of 'simultaneous decidability' which is characteristic for quantum mechanics."[22] A few years later he discussed these ideas with Garrett Birkhoff, the son of the mathematician George David Birkhoff. Garrett Birkhoff, a graduate of Harvard (1932), had done important postgraduate work at Cambridge in England on abstract algebra and lattice theory and had just become a member of the Harvard faculty. This collaboration between the analyst and the algebraist[23] resulted in the publication of a paper[24] designed "to discover what logical structure one may hope to find in physical theories which, like quantum mechanics, do not conform to classical logic."

Birkhoff and von Neumann began their exposition with an analysis of the propositional calculus of classical dynamics. Their approach, somewhat simplified, was as follows. Propositions about the state of a classical mechanical system can be made best by reference to a suitable phase space Γ in which each state is represented by a point P. A proposition expressing the result of a measurement states in which subset S of Γ the representative point P is found with certainty. Each experimental proposition a thus corresponds to a subset S_a of Γ and is true if P, the point representing the state referred to in a, lies in S_a. The conjunction $a \cap b$ of the two propositions a and b is true if P lies in the intersection of S_a and S_b, the disjunction $a \cup b$ is true if P lies in the union of S_a and S_b, while the negation (or complementation) a' of a asserts that P does not lie in S_a. If whenever a is true b is also true, a relation expressed by saying that "a implies b" and denoted by $a \subseteq b$, then S_a is a subset of S_b. The implication $\subseteq$ is reflexive, transitive, and antisymmetric.[25] "a is equivalent to b" or $a = b$ if $a \subseteq b$ and $b \subseteq a$. A physical quality was now defined by Birkhoff and von Neumann as the set of all experimental propositions equivalent to a given experimental proposition. Since the partial order generated by the implication induces also a partial order among the equivalence classes Birkhoff and von Neumann could conclude that "the physical qualities

[22]Ref. 1–2 (1932, p. 134; 1955, p. 253).

[23]Garrett Birkhoff's *A Survey of Modern Algebra* (Macmillan, New York, 1941), written with S. MacLane, became a standard text on this subject. Reprinted in Ref. 7.106.

[24]G. Birkhoff and J. von Neumann, "The logic of quantum mechanics," *Annals of Mathematics* **37**, 823–843 (1936), reprinted in J. von Neumann, *Collected Works* (Ref. 1–5), Vol. 4, pp. 105–125.

[25]For the sequel see the Appendix on lattice theory.

attributable to any physical system form a partially ordered system."[26] Moreover, since the distributive identity expressing a characteristic property of set combinations is valid in classical mechanics they could easily show that the propositional calculus of classical mechanics forms a Boolean lattice.[27]

Turning now to quantum mechanics where states are defined by eigenvectors of Hermitian operators so that the *subsets* of Γ as used in classical mechanics have to be replaced by *subspaces* of a Hilbert space $\mathcal{H}$ and the truth value of a proposition a has to correspond to the eigenvalues (1 or 0) of the projection operator associated with the subspace referred to in a, Birkhoff and von Neumann suggested that the propositional calculus of quantum mechanics is a complemented[28] lattice where the complementation corresponds to the passage from a subspace to its orthogonal complement.

Thus far the formal features of the logical structures of classical and quantum mechanics are essentially identical. Their characteristic difference, according to Birkhoff and von Neumann, becomes apparent only if we consider the distributive identities. For these, Birkhoff and von Neumann discovered, hold in classical mechanics but not in quantum mechanics. Here they have to be replaced by the much weaker "modular identity":

$$\text{If} \quad a \subseteq c, \quad \text{then} \quad a \cup (b \cap c) = (a \cup b) \cap c, \tag{1}$$

[26]The concept of a partial order was introduced by C. S. Peirce in "On the algebra of logic," *American Journal of Mathematics* **3**, 15–57 (1880), **7**, 180–202 (1884).

[27]The concept of a lattice was introduced by Ernst Schröder (1841–1902) in his *Vorlesungen über die Logik der Algebra* (Teubner, Leipzig, 1890; Chelsea Publishing Co., New York, 1966). The concept of a "dual group" (which is equivalent to a lattice, see Theorem 6 of the Appendix) was introduced by Richard Dedekind (1831–1916) in his paper "Über die Zerlegungen von Zahlen durch ihre grössten gemeinsamen Teiler," *Festschrift der Technischen Hochschule zu Braunschweig* **1897**, 1–40; reprinted in R. Dedekind, *Gesammelte Mathematische Werke* (Vieweg, Braunschweig, 1931), Vol. 2, pp. 103–147. A complemented distributive lattice (see Definition 7 of the Appendix) is called Boolean after the English mathematician and logician George Boole (1815–1864), who in his *Mathematical Analysis of Logic* (Macmillan, Cambridge, 1847; Blackwell, Oxford, 1848) initiated the study of logical systems of this kind.

[28]What Birkhoff and von Neumann called in this paper "complementation" was later called "orthocomplementation," a term introduced by Garrett Birkhoff in the first edition of his momumental *Lattice Theory* (American Mathematical Society, Colloquium Publications, Vol. 25, Providence, R. I, 1940). In 1961 Birkhoff referred to the 1936 paper (Ref. 24) as a suggestion of "treating the propositional calculus of quantum mechanics as an *orthocomplemented, perhaps modular lattice*." G. Birkhoff, "Lattices in applied mathematics," in *Proceedings of the Symposia in Pure Mathematics*, Vol. 2 (*Lattice Theory*) (American Mathematical Society, Providence, R. I., 1961), pp. 155–184, quotation on p. 157.

a relation which according to the authors applies[29] to the intersections and "straight linear sums" of subspaces in $\mathcal{H}$ just as the distributive relation $(a\cup b)\cap c=(a\cap c)\cup(b\cap c)$ or $(a\cap b)\cup c=(a\cup c)\cap(b\cup c)$ applies to the set combinations in Γ.

That the distributive identities, though valid in classical mechanics, do not in fact hold in quantum mechanics was shown by Birkhoff and von Neumann by the following thought-experiment:[30] "if a denotes the experimental observation of a wave-packet ψ on one side of a plane in ordinary space, a' correspondingly the observation of ψ on the other side, and b the observation of ψ in a state symmetric about the plane, then (as one can readily check)":

$$b\cap(a\cup a')=b\cap 1=b\supset 0=b\cap a=b\cap a'=(b\cap a)\cup(b\cap a') \qquad (2)$$

where $\supset$ denotes of course $\supseteq$ with the exclusion of equality. Hence $b\cap(a\cup a')\neq(b\cap a)\cup(b\cap a')$.[31] The conclusion reached by Birkhoff and

[29]According to Birkhoff and von Neumann this can be seen as follows. The hypothesis $S_a\subseteq S_c$ (here $\subseteq$ denotes inclusion, $\cup$ union, and $\cap$ intersection) entails $S_a\subseteq(S_a\cup S_b)\cap S_c$. Since generally $S_b\cap S_c\subseteq(S_a\cup S_b)\cap S_c$, it follows that $S_a\cup(S_b\cap S_c)\subseteq(S_a\cup S_b)\cap S_c$. Any vector ξ of $(S_a\cup S_b)\cap S_c$ is in S_c and in $S_a\cup S_b$. But every vector in $S_a\cup S_b$, the authors contended, is the sum of a vector in S_a and of a vector in S_b [*Remark*: This is correct only if at least one of the two subspaces has only finite dimensionality!] Hence ξ can be written $\xi=\alpha+\beta$ where $\alpha\epsilon S_a$ and $\beta\epsilon S_b$. By hypothesis $S_a\subseteq S_c$ so that $\alpha\epsilon S_c$. With ξ also $\beta=\xi-\alpha$ is in S_c so that $\beta\epsilon S_b\cap S_c$. It follows that $\xi=\alpha+\beta$ is in $S_a\cup(S_b\cap S_c)$. Thus, since $(S_a\cup S_b)\cap S_c\subseteq S_a\cup(S_b\cap S_c)$ as well as $S_a\cup(S_b\cap S_c)\subseteq(S_a\cup S_b)\cap S_c$, $S_a\cup(S_b\cap S_c)=(S_a\cup S_b)\cap S_c$, which completes the proof of (1). The identity (1) can equivalently be expressed by demanding that any three elements a,b,c satisfy the identity $a\cap(b\cap(a\cap c))=(a\cap b)\cup(a\cap c)$; cf. P. Jordan "Zum Dedekindschen Axiom in der Theorie der Verbände," *Abhandlungen aus dem Mathematischen Seminar der Universität Hamburg* **16**, 71–73 (1949). Since Dedekind introduced the notion of modularity ("Modulgesetz," *op. cit.*, p. 115) modular lattices are also called "Dedekind lattices." Obviously every distributive lattice is modular but not every modular lattice is distributive.

[30]Ref. 24 (1936, p. 831).

[31]The example proposed by Birkhoff and von Neumann was not very fortunate, as we shall see below. The following example would have avoided any misunderstanding. Let b denote the proposition "$s_z=\hbar/2$," a the proposition "$s_x=\hbar/2$," and hence a' the proposition "$s_x=-\hbar/2$," where s_z and s_x represent spin components for a spin-$\frac{1}{2}$ particle. Clearly, $b\cap(a\cup a')=b$, whereas $(b\cap a)\cup(b\cap a')=0$. That the exemplification of nondistributivity was apparently never an easy matter is well documented by the following historical account. As G. Birkhoff had pointed out [*Lattice Theory* (1948), Ref. 28, p. 133], "it is curious that C. S. Peirce should have thought that every lattice was distributive." In fact, in Ref. 26 (p. 33) Peirce declared that the distributivity formulae "are easily proved...but the proof is too tedious to give." Shortly afterward, however, E. Schröder (Ref. 27, Vol. 1, p. 283) demonstrated the underivability of $a\cap(b\cup c)<(a\cap b)\cup(a\cap c)$ (see Theorem 7 of the Appendix) from the postulates for general lattices by constructing a rather complicated realization of a nondistributive lattice. Probably the earliest *simple* example of a nondistributive lattice

von Neumann was therefore this: Whereas the logical structure of the propositional calculus of classical mechanics is that of a Boolean lattice, the logical structure of the propositional calculus of quantum mechanics is that of an orthocomplemented modular lattice.

The authors regarded this conclusion, which in their words was "based on admittedly heuristic arguments," as only a first step toward a deeper clarification of the relationship between the logics of classical and quantum physics. At the end of their essay they raised the following two questions: (1) What experimental meaning can be attached to the meet and join of two given experimental propositions? (2) What is the physical significance of the modularity identity? What apparently motivated these questions was the fact that, contrary to classical physics, quantum mechanics assigned a clear-cut meaning to the meet and join of two propositions only if they refer to compatible measurements.

That such questions engaged von Neumann's attention can be seen from an unfinished manuscript[32] written about 1937, in which he tried to generalize "strict logics," as discussed in his paper with Birkhoff, to a "probability logic." In the latter, account is taken of a "probability function" $P(a,b)$ which assigns a probability θ $(0 \leqslant \theta \leqslant 1)$ to a measurement showing that b is true if an immediately preceding measurement has shown a to be true. Although $P(a,b)=1$ and $P(a,b)=0$ can be reduced to $a \subseteq b$ and $a \subseteq b'$, respectively, in general such a reduction to strict logics seems impossible. In the words of von Neumann, "probability logics cannot be reduced to strict logics, but constitute an essentially wider system than the latter, and statements of the form $P(a,b)=\theta(0<\theta<1)$ are perfectly new and *sui generis* aspects of physical reality."

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was given by A. Korselt in a short paper, "Bemerkungen zur Algebra der Logik," *Mathematische Annalen* **44**, 156–157 (1894). Korselt considered the set L composed of the null space $(=0)$, points, straight lines, planes, and the whole (Euclidean) space $(=1)$, and defined $a \leqslant b$ by "a lies in b" (geometrical inclusion), and hence $a \cap b$ as the highest dimensional element of L which lies in a and in b, and $a \cup b$ as the lowest dimensional element of L in which a and b lie; after showing that L is a lattice he considered three points p_1, p_2, and p_3 which lie in a line g; since $p_1 \cap p_2 = p_1 \cap p_3 = 0$, $p_1 \cap g = p_1$, and $p_2 \cup p_3 = g$, clearly $p_1 = p_1 \cap (p_2 \cup p_3) \neq (p_1 \cap p_2) \cup (p_1 \cap p_3) = 0$. After having read Schröder's book Peirce admitted his error, writing (Ref. 26, p. 190) "My friend, Professor Schröder, detected the mistake and showed that the distributive formulae...could not be deduced from syllogistic principles." Later, however, in a letter to Edward V. Huntington, dated December 24, 1903, and quoted in Huntington's paper "Sets of independent postulates for the algebra of logic," *Transactions of the American Mathematical Society* **5**, 288–309 (1904), Peirce supplied the "proof," referred to as "tedious," but apparently without realizing that one of the postulates on which the proof depends (postulate 9 in Huntington's enumeration, *op. cit.*, p. 297) is equivalent to the distributivity requirement and hence an assumption additional to the postulates for general lattices.

[32] "Quantum logics (strict—and probability—logics)," in J. von Neumann, *Collected Works* (Ref. 1–5), Vol. 4, pp. 195–197.

Von Neumann also realized that once it is assumed that the propositional calculus for quantum mechanical systems is a modular orthocomplemented lattice, the mathematical formalism to describe the states of such systems must have the essentially unique structure of a matrix algebra. Von Neumann's proof of this theorem has never been published, nor is it even certain whether von Neumann has ever worked out such a proof in full detail.[33] Hans Julius Zassenhaus seems to have been the first to do so when as an assistant to Emil Artin in Hamburg he gave a lecture on the axiomatics of projective geometry. In spite of its novel features the lattice-theoretical approach of Birkhoff and von Neumann was generally ignored by quantum theorists. One of the few exceptions was Pascual Jordan, who retained an enthusiastic interest in the purely algebraic aspects of quantum mechanics ever since his historic collaboration with Max Born on the matrix formulation of the commutation relations[34] and was indefatigable in searching for algebraic generalizations of the quantum mechanical formalism.[35]

It was only in the early 1960s that interest in the lattice-theoretical approach was greatly revived primarily through the investigations of Josef Maria Jauch and his students at the University of Geneva, G. Emch, M. Guenin, J. P. Marchand, B. Misra, and C. Piron. It is therefore from the chronological point of view not particularly surprising that more than 30 years after its publication the Birkhoff-von Neumann paper was submitted to a scrutinizing examination by Karl R. Popper.[36] It must have been, however, quite a shock even for Popper himself to find out, as he claimed to have found, that the Birkhoff-von Neumann paper "culminates in a proposal which clashes with each of a number of assumptions made by the authors." To substantiate this severe criticism of the logical inconsistency of this paper Popper referred to the following theorems which were discovered after 1936: Any uniquely complemented lattice L is Boolean if it satisfies at least one of the following four conditions: (1) L is modular (or even only weakly modular),[37] (2) L is complete and atomic (or finite),[38]

[33]The proof can be reconstructed from remarks in von Neumann's paper "Continuous geometry," *Proceedings of the National Academy of Science* **22**, 92–100, 101–108 (1936), where it is stated that a modular orthocomplemented lattice is equivalent to a projective geometry : its elements are linear subspaces of that geometry.

[34]See Ref. 1-1 (pp. 209–215).

[35]P. Jordan, "Zur Quantum-Logik," *Archiv der Mathematik* **2**, 166–171 (1949); "Algebraische Betrachtungen zur Theorie des Wirkungsquantums und der Elementarlänge," *Abhandlungen aus dem Mathematischen Seminar der Universität Hamburg* **18**, 99–119 (1952).

[36]K. R. Popper, "Birkhoff and von Neumann's interpretation of quantum mechanics," *Nature* **219**, 682–685 (1968).

[37]The earliest proof of this theorem was given by von Neumann in his *Lectures on Continuous Geometries* (Princeton University Press, Princeton, N. J., 1936–1937). Note that a uniquely

(3) L is orthocomplemented,[39] (4) L is measurable (i.e., a bounded and additive real-valued function is defined which satisfies certain measuretheoretic conditions). Since the lattice proposed by Birkhoff and von Neumann satisfies each of the four conditions, said Popper, it is Boolean contrary to their intention and declaration. It would not help even to assume that the lattice, though uniquely orthocomplemented, is not uniquely complemented; for it can be shown, as Popper explained, that every complemented measurable lattice is uniquely complemented.

Another charge raised by Popper against Birkhoff and von Neumann concerns their thought-experiment designed to refute the admissibility of the distributivity identity for the propositional calculus of quantum mechanics. The error committed by the authors which, in Popper's words, "is no more than a simple slip—one of those slips which, once in a lifetime, may happen even to the greatest mathematician," and which in Popper's view has nothing specifically to do with quantum mechanics—an "elephant" in ordinary space, he wrote, may be substituted for the "wave packet"—was according to Popper simply this: The complement a' of the proposition a (denoting the experimental observation of a wave packet ψ on one side of a plane in ordinary space) is not "the observation of ψ on the other side" but rather "its observation not on the one side"; a' is therefore perfectly compatible with the proposition b of the argument ("the observation of ψ in a state symmetric about the plane"); hence $b = b \cap a'$, and the inequality (2) breaks down.[40] Popper also insisted that their assumption that b differs from a and a' implies the rejection of the law of the "excluded third," a proposal made by the intuitionists but rejected by Birkhoff and von Neumann.

Popper's devastating criticism, although leveled only against Bikhoff and von Neumann, was immediately regarded as an attack against the entire lattice-theoretical approach, contrary to Popper's intentions.[41] Theorists like Jauch, Arlan Ramsay, and James C. T. Pool[42] soon took up the

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orthocomplemented, and hence weakly modular, lattice is not necessarily Boolean.

[38] For a proof of this theorem see G. Birkhoff and M. Ward, "A characterization of Boolean algebra," *Annals of Mathematics* **40**, 609–610 (1939).

[39] Cf. G. Birkhoff, *Lattice Theory* (Ref. 28), 2nd ed., 1948, reprinted 1960, p. 171: "If every a in a lattice L has a unique complement a', and if $a \to a'$ is a dual automorphism, then L is a Boolean algebra."

[40] Compare "left $\cup$ right = 1" on p. 209 in D. Finkelstein, "Matter, space and logic," *Boston Studies in the Philosophy of Science* (Reidel, Dordrecht, Holland, 1969), Vol. 5, pp. 199–215.

[41] "My article on Birkhoff and von Neumann was a *historical* article, pointing out that these authors had made certain mistakes. I never asserted that others... made the same mistakes." Letter from Popper to author, dated June 7, 1971.

[42] A. Ramsay was a member of the Department of Mathematics at the University of Colorado (Boulder) and J. C. T. Pool of the Department of Mathematics and Statistics at the University

cudgels to defend their position against what they regarded as a "serious error," "apt to produce confusion among philosophers and scientists." In their correspondence with Popper they admitted that his comments exposed a number of ambiguities in the Birkhoff-von Neumann paper but charged him, as far as the main issue is concerned, with having refuted only his own misinterpretation of that paper. On October 16, 1968, Ramsay and Pool submitted to *Nature* a reply to Popper's paper which, in turn, was commented on by Popper in February 1969. Again in the fall of 1969 Ramsay and Pool wrote a reply to Popper's reply on their rebuttal of his criticism. Due to accidental but never fully clarified circumstances none of these papers, although obviously written for publication, has ever appeared in print. In fact, Popper's challenge has so far never been publicly defied.[43]

Popper's criticism of the thought-experiment, whose refutation was for Popper the central issue of his paper,[44] was justified insofar as it showed that the jargon used by physicists does not always meet the philological test of unambiguity. However, what Birkhoff and von Neumann presumably had in mind was the following situation: Let the projection operators P_l, P_r, and P_s be defined by the equations

$$\text{If } x \leqslant 0,\ P_l\psi(x)=\psi(x); \qquad \text{If } x>0,\ P_l\psi(x)=0;$$

$$\text{If } x \leqslant 0,\ P_r\psi(x)=0; \qquad \text{If } x>0,\ P_r\psi(x)=\psi(x),$$

$$P_s\psi(x)=\tfrac{1}{2}[\psi(x)+\psi(-x)]$$

and let a be the proposition $P_l\psi(x)=\psi(x)$, a' the proposition $P_r\psi(x)=\psi(x)$, and b the proposition $P_s\psi(x)=\psi(x)$. Clearly, the ranges of P_l and P_r are subspaces each of which is the orthogonal complement of the other. Hence a' is the orthocomplement of a. But a' is not compatible with b, for otherwise $P_sP_r=P_rP_s$, which does not generally hold.

Birkhoff and von Neumann seem to have been fully aware of the difficulty concerning the interpretation of the conjunction or disjunction of

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of Massachusetts (Amherst).

[43]The only exception known to the present author is a footnote, on p. 171 in the paper by J. M. Jauch and C. Piron, "What is 'quantum-logic?,'" in *Quanta—Essays in Theoretical Physics dedicated to Gregor Wentzel*, P. G. O. Freund, C. G. Goebel, and Y. Nambu, eds. (University of Chicago Press, Chicago, 1970), pp. 166–181: "Popper made...an additional assumption, viz. that for all states $p(a)+p(b)=p(a\cup b)+p(a\cap b)$ [the functional $p(a)$ on the lattice, with values between 0 and 1, represents the state of the system] which is not true in quantum mechanics."

[44]Letter from Popper to Jauch, dated November 27, 1968.

two incompatible propositions. "It is worth remarking," they declared, "that in classical mechanics, one can easily define the meet or join of any two experimental propositions as an *experimental proposition*—simply by having independent observers read off the measurement which either proposition involves, and combining the results logically. This is true in quantum mechanics only exceptionally—only when all the measurements involved commute (are compatible); in general, one can only express the join or meet of two given experimental propositions as a class of logically equivalent experimental propositions—i.e., as a *physical quality*."[45]

The earliest attempt to resolve this difficulty was probably made by Kodi Husimi in his proposal, presented at a meeting in 1937 of the Physico-Mathematical Society in Japan, to derive the laws of the non-Boolean logic of quantum mechanics directly from empirical facts and not, as Birkhoff and von Neumann have done, from the structure of the Hilbert space used in the formalism of quantum mechanics. Defining the implication by the statement that the numerical probability of the antecedent is smaller or equal to that of the consequent Husimi showed that the existence of the meet requires the sum of two quantities whose existence he derived from a correspondence principle according to which "every linear relation between the mean values in the classical theory is conserved in the process of quantization."[46] Later theorists, including Jauch[47] and Watanabe, suggested defining the proposition $a \cap b$ as true if the system passes an infinite sequence of alternating pairs of filters for a and b, respectively, and as false otherwise.

Jauch's solution was criticized by Patrick Heelan[48] as a violation of the empiricist tenet of quantum logicians since an *infinite* experimental filter can never *de facto* be constructed and hence hardly be regarded as *experimental*. Jauch obviously anticipated this objection when he stated that "although infinite processes are, of course, not possible in actual physical measurements, the construction can be used as a base for an approximate determination of the proposition to any needed degree of accuracy."[49]

But such a limit consideration, even if the practical impossibilities of infinite filter sequences are ignored, still involves the conceptual problem whether the alternating sequence $P_a, P_bP_a, P_aP_bP_a, \ldots$, of the projection

[45]Ref. 24 (pp. 829–830).
[46]K. Husimi, "Studies in the foundations of quantum mechanics. I," *Proceedings of the Physico-Mathematical Society of Japan* **19**, 766–789 (1937).
[47]Ref. 1–3 (p. 75). S. Watanabe, *Knowing and Guessing* (Wiley-Interscience, New York, 1969), p. 495.
[48]P. Heelan, "Quantum and classical logic: Their respective roles," *Synthese* **21**, 1–33 (1970).
[49]Ref. 47. (Jauch, 1968, p. 75)

operators P_a and P_b associated with the respective propositions a and b, approaches the same limit as the sequence $P_b, P_a P_b, P_b P_a P_b, \ldots$, and whether the limit itself is a projection operator. For such a convergence holds only in the strong topology and consequently not uniformly over all quantum mechanical states. Recently Abner Shimony[50] tried to overcome this difficulty by imposing certain restrictions on the structure of the class of filters to be used for this purpose.

The following suggestions were also proposed to solve this problem:

1. $a \cap b$ is defined as true if each of the following two experimental procedures always produces the truth value 1, a measurement of P_a is followed by that of P_b, a measurement of P_b is followed by that of P_a.
2. On the assumption of the reproducibility of state preparation one may measure P_a on one system in the state under discussion, then measure P_b on a second system in the same state, then P_a on a third system in the same state, and so on, and define $a \cap b$ as true if all measurements (now without mutual interference) yield the value 1, a suggestion made by Birkhoff himself.[51]
3. One may admit conjunctions only of propositions that belong to a sublattice which is Boolean.
4. One may assign to the conjunction of incompatible propositions a logically exceptional status by modifying the object language with the help of a many-valued logic so that no metalinguistic criteria are necessary to exclude such conjunctions from being used as meaningful propositions.

As we shall see later, some of these alternatives played an important role in the construction of different interpretations of quantum mechanics.

In fact, possibility 3 above served as a basis of a modified logic for quantum mechanics which was proposed by Martin Strauss and presented by Max von Laue to the Prussian Academy of Science in Berlin in October 1936, that is, at the same time that the Birkhoff and von Neumann paper was published. Convinced that progress in theoretical physics requires the construction of a technical language which by its very syntax accounts for the more general features of physical experience Strauss tried to show in his paper[52] that the usual formalism of quantum mechanics is but a

[50] A. Shimony, "Filters with infinitely many components," *Foundations of Physics* **1**, 325–328 (1971).

[51] Cf. M. D. MacLaren, "Notes on axioms for quantum mechanics," *AEC Research and Development Report ANL-7065* (1965), p. 11.

[52] M. Strauss, "Zur Begründung der statistischen Transformationstheorie der Quantenphysik," *Berliner Berichte* **1936**, 382–398, written in the winter 1935–1936 during a visit to Bohr's Institute in Copenhagen. English translation "The logic of complementarity and the founda-

mathematical representation of what he called "complementarity logic." Strauss accepted the von Neumann version of this formalism as mathematically correct but methodologically unsatisfactory on the grounds that, contrary to the formalism of thermodynamics[53] or of the theory of relativity, von Neumann's formalism lacks an explicit postulate which relates it with experimental experience and that the use it makes of classical probability theory is unwarranted since its applicability for quantum mechanics is severely restricted by the complementarity principle. Classical probability, Strauss argued, is based on the usual propositional calculus which is isomorphic to the set-theoretic system of subsets of a given set, has for its probability functions a domain which is likewise isomorphic to this system, and assumes therefore the simultaneous decidability of any two propositions. The propositional calculus of quantum mechanics, however, because of the complementarity principle, excludes the simultaneous decidability of incompatible propositions. Hence, Strauss concluded, a different probability theory has to be used in quantum mechanics. In his search for such a theory Strauss reasoned as follows.

The classical probability concept appears in statements of the type $w(a; b)=p$, that is, the probability for b being true if a is true is p, and this probability is interpreted by the mathematical identity $w(a;b)=f(a\cap b)/f(a)$ where $f(a)$ denotes the number of cases a is found to be true.[54] In quantum mechanics the conjunction $a\cap b$ has a meaning only if a is compatible with b. A syntactic rule has to exclude the conjunctions (disjunctions) of complementary propositions. In the propositional calculus thus modified which Strauss called "complementarity logic" [Komplementaritätslogik] classical probability ceases to be universally valid but may be applied in what he called certain "islands" (had Strauss used the language of lattice theory he would have said "Boolean sublattices") of the propositional space.

In quantum mechanics, according to Strauss, a physical property or its corresponding proposition a is isomorphically associated with a projection

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tion of quantum theory," in M. Strauss, *Modern Physics and its Philosophy* (Reidel, Dordrecht, Holland, 1972), pp. 186–199. Reprinted in Ref. 7-106.

[53]Strauss obviously considered only the formalism of thermodynamics which is based on Carnot's theorem or its equivalent and ignored Constantin Carathéodory's axiomatization [*Mathematische Annalen* **61**, (1909)] which, though conceptually superior to the former, would be open to the same charge. In insisting on the superiority of formulating the axioms of a physical theory in observationally meaningful terms Strauss seems to have followed his teacher Hans Reichenbach at the University of Berlin (1926–1932); see in this context H. Reichenbach, *Axiomatik der relativistischen Raum-Zeit-Lehre* (Vieweg, Braunschweig, 1924), pp. 2–3.

[54]In Strauss' paper, which was written prior to the introduction of lattice theory, the symbols $\cap$, etc., represent the ordinary logical connectives "and," etc.

operator P_a such that $\sim a$ (non a) corresponds to $I-P$ (I is the identity operator), $a \cap b$ corresponds to $P_a P_b$, and $a \cup b$ to $P_a + P_b - P_a P_b$, these being projection operators if and only if $P_a P_b = P_b P_a$. The probability function $w(a,b)$, which in general denotes the transition probability from a to b, coincides in exceptional cases with the above-mentioned expression of classical physics and must therefore satisfy a relation of the form $w(a,b) = g(P_a P_b)/g(P_a)$ where g is an additive real-valued function $g(P_a + P_b) = g(P_a) + g(P_b)$. Since the only function satisfying this condition is the trace of P Strauss concluded that

$$w(a,b) = \frac{\mathrm{Tr}(P_a P_b)}{\mathrm{Tr}(P_a)}, \tag{6}$$

which is, of course, von Neumann's statistical formula if P_a is the statistical operator. To preclude real values for nonexisting probabilities the metric of the representation space of the complementarity logic, that is, the space in which the projection operators are defined, must be unitary rather than real Euclidean. This, according to Strauss, is the reason why complex-valued functions (Hilbert vectors) are an unavoidable necessity in quantum mechanics and not merely an eliminable mathematical device as in classical mechanics: The unitarity of the metric is a necessary condition to ensure that "meaningless questions have no meaningful answers."[55] Just as von Neumann and Zassenhaus deduced the formalism of quantum mechanics from the structure of the lattice of quantum mechanical propositions, Strauss derived it—independently and even prior to them—from the structure of complementarity logic.

In his Ph.D. dissertation[56] "Mathematische und logische Beiträge zur quantenmechanischen Komplementaritätstheorie," written 1938 in Prague under Philipp Frank, Reinhold Fürth, and Karl Löwner, Strauss examined the Birkhoff and von Neumann paper and claimed to have proved that their approach admits of propositions which are "metaphysical" in the sense that no physical situation can exist which either conforms with them or contradicts them. Moreover, to renounce the distributive law for propositional connectives, he contended, means to abandon (semantical) two-valuedness, for "in any two-valued logic the two sides of the distributive

[55]M. Strauss, "Grundlagen der modernen Physik," in *Mikrokosmos-Makrokosmos: Philosophisch-theoretische Probleme der Naturwissenschaft, Technik und Medizin*, H. Ley and R. Löther, eds. (Akademie-Verlag, Berlin, 1967), pp. 55–92. English translation (in part) "Foundations of quantum mechanics," in Ref. 52 (1972, pp. 226–238). Reprinted in Ref. 7-106.

[56]Unpublished. The two copies which were in Strauss' possession were lost during the war. Letter from Strauss to author, dated May 26, 1970.

law have the same truth-value," an implication which the advocates of the Birkhoff and von Neumann "quantum logic," as he later remarked, seem never to have noticed. Finally, he argued, since this "quantum logic," in contrast to his own complementarity logic, does not lead to the unitary metric when combined with the probability calculus, it is not characteristic of quantum mechanics.

Concerning the last-mentioned argument it should be noted that due to more recent investigations[57] it is known that, to some extent at least, quantum mechanics can be formulated also in a Hilbert space over the field of reals or over the field of quaternions, in addition to the field of complex numbers, these being the only fields which according to a celebrated theorem by Frobenius (1878) contain the reals as a subfield.[58]

Let us add in this context that, as Ernst Carl Gerlach Stueckelberg and his collaborators[59] have shown, the existence of an uncertainty principle in a quantum theory over a real Hilbert space requires the introduction of an operator J (with $J^2 = -1$) that commutes with all observables, and hence a superselection rule by virtue of which at least for simple systems the realization of the propositional systems in a real Hilbert space and in a complex Hilbert space are essentially equivalent. The relation between quaternionic quantum mechanics and the usual complex quantum mechanics has been studied by Gérard Emch,[60] who has shown that at least for simple-particle systems relativistic considerations lead to an equivalence between the two formulations.

Thus, even though the differences in the physical implications of these various formulations of quantum mechanics are perhaps not yet fully clarified, the results obtained so far seem to speak against Strauss' last-

[57]D. Finkelstein, J. M. Jauch, S. Schiminovich, and D. Speiser, "Foundations of quaternion quantum mechanics," *Journal of Mathematical Physics* **3**, 207–220 (1962); cf. also *ibid.*, **4**, 136–140, 788–796 (1963).

[58]More precisely, that the only division algebras (i.e., linear associative algebras with unit elements and inverses of nonzero elements) over the field of reals are the fields of the reals, the complex numbers and the quaternions, the last one being the only noncommutative division algebra over the reals, has been proved by F. G. Frobenius, "Über lineare Substitutionen und bilineare Formen," *Journal für reine und angewandte Mathematik* **84**, 1–63 (1878), reprinted in F. G. Frobenius, *Gesammelte Abhandlungen* (Springer, Berlin, Heidelberg, New York, 1968), Vol. 1, pp. 343–405.

[59]E. C. G. Stueckelberg, "Quantum theory in real Hilbert space," *Helvetica Physica Acta* **33**, 727–752 (1960). E. C. G. Stueckelberg and M. Guenin, "Quantum theory in real Hilbert space II," *ibid.*, **34**, 621–628 (1961). Cf. also *ibid.*, **34**, 675–698 (1961), **35**, 673–695 (1962). That finite fields have generally to be excluded has been shown by J. P. Eckmann and Ph. Ch. Zabey, "Impossibility of quantum mechanics in a Hilbert space over a finite field," *ibid.*, **42**, 420–424 (1969).

[60]G. Emch, "Mécanique quantique quaternionienne et relativité restreinte," *Helvetica Physica Acta* **36**, 739–769, 770–788 (1963).

mentioned argument for the superiority of his complementarity logic over the Birkhoff and von Neumann lattice-theoretic approach.

The inadmissibility of the conjunction and disjunction of incompatible propositions which Strauss regarded in the construction of his complementarity logic as a syntactic rule has recently been revived and modified, though independently of Strauss, by Franz Kamber[61] of the University of Zurich and, along slightly different lines, by Patrick Suppes, the well-known statistician and logician of Stanford University, by adopting it not as a syntactic rule imposed on the propositional calculus, as Strauss suggested, but rather as a mathematical restriction pertaining to the foundations of the probability theory operating on this calculus. To fully understand this subtle difference let us briefly outline Suppes' approach to quantum logic.

At a colloquium held in Paris in May 1964, in honor of the memory of E. W. Beth, Suppes[62] presented what he called "the single most powerful argument for the use of a non-classical logic in quantum mechanics." Taking for granted that probability theory is a basic ingredient of quantum mechanics he insisted that in physical contexts involving the application of probability the "functional or working logic" of importance is the logic of events or propositions to which probability is assigned. He further assumed as premises that "the algebra of events should satisfy the requirement that a probability is assigned to every event or element of the algebra." From the absence of joint probability distributions for conjugate variables such as position and momentum he derived the conclusion that the working logic of quantum mechanics is not classical. Suppes' argument for the use of a nonclassical logic is ultimately a mathematization of Strauss' syntactic rule. As Suppes explicitly declared, a probability should be assigned to every element of the algebra of events; in the case of quantum mechanics, probabilities may be assigned to events but not, without restriction, to the conjunction of two events. To substantiate this contention he referred to his 1961 paper in which he had shown that the joint distribution for position and momentum at a given instant, as derived by Wigner and

[61]F. Kamber, "Die Struktur des Aussagenkalküls in einer physikalischen Theorie," *Göttinger Nachrichten* **1964**, 103–124; "Zweiwertige Wahrscheinlichkeitsfunktionen auf orthokomplementären Verbänden," *Mathematische Annalen* **158**, 158–196 (1965).

[62]P. Suppes, "The probabilistic argument for a non-classical logic of quantum mechanics," *Philosophy of Science* **33**, 14–21 (1966); "L'argument probabiliste pur une logique nonclassique de la mécanique quantique," *Synthese* **16**, 74–85 (1966); cf. also his "Probability concepts in quantum mechanics," *Philosophy of Science* **28**, 378–389 (1961). For a criticism of Suppes' approach and the contention that quantum theory can be established on a classical (Boolean) probabilistic setting which, moreover, is faithful to the ways in which the theory is commonly used see Arthur I. Fine, "Logic, probability, and quantum theory," *Philosophy of Science* **35**, 101–111 (1968).

Moyal (which we shall discuss in Chapter 9), is not at all a genuine probability distribution and that, consequently, no underlying sample space exists which may be used to represent the simultaneous measurements (whether exact or inexact) of such variables. The conclusion he drew was stronger than Heisenberg's indeterminacy principle: "Not only are position and momentum not precisely measurable, they are not simultaneously measurable at all."

The difference between Strauss and Suppes can now be stated as follows. To obtain consistency between probability theory and the propositional calculus Suppes did not impose, as Strauss did, syntactic restrictive rules on the propositional calculus but modified that part of the probability theory which refers to the algebra of events by allowing that this algebra need no longer be closed under the conjunction of events. More precisely, if $(X, \mathfrak{F}, P)$ denotes the probability space (that is, X is a nonempty set, $\mathfrak{F}$ an algebra of sets on X, and P a normalized nonnegative function on $\mathfrak{F}$) Suppes suggested to modify $\mathfrak{F}$ in the following way: Instead of postulating that $\mathfrak{F}$ is a classical algebra (i.e., $\mathfrak{F}$ is closed under complementation and countable unions), $\mathfrak{F}$ is assumed to be a "quantum-mechanical algebra" on X, that is, for any subset its complement also is an element of $\mathfrak{F}$, and for two *disjoint* subsets their union also is an element of $\mathfrak{F}$, and, finally, $\mathfrak{F}$ is closed under countable unions of pairwise disjoint sets. To obtain the propositional calculus Suppes defined the notion of validity in the usual way by associating implication with inclusion and negation with the complementation operations. A propositional formula is said to be valid if it is satisfied in all quantum mechanical algebras and the set of all such valid formulas, Suppes pointed out, characterizes the propositional logic of quantum mechanics.

This short outline of Suppes' approach will suffice, it is hoped, to substantiate our contention that his reasoning, which in the opinion of Wolfgang Stegmüller[63] is "the only convincing argument" for quantum logic, is ultimately but a mathematization of Strauss' syntactic considerations. This analysis, of course, in no way detracts from the importance of Suppes' contributions, particularly in view of the fact that Suppes seems to have been completely unaware of Strauss' work.

Some of the basic ideas of Strauss' complementarity logic reappeared, as we see, independently in various later interpretations and are still recogniz-

[63]W. Stegmüller, *Probleme und Resultate der Wissenschaftstheorie und Analytischen Philosophie*, Vol. 2), *Theorie und Erfahrung* (Springer, Berlin, Heidelberg, New York, 1970), pp. 438–462. Stegmüller's creditation of Suppes with having been the first to recognize the unwarrantedness of the usual interpretation of the Heisenberg relations is certainly wrong (p. 442). Nor does Stegmüller's argumentation for the rejection of this usual interpretation seem to us unassailable.

able, say, in Gudder's[64] work on hidden variables or in Gunson's[65] analysis of the algebraic structure of quantum mechanics. Even the motivation was often the same. Thus the same fact, namely that von Neumann's formalism "was obtained only at the expense of introducing axioms whose physical significance was far from apparent," prompted Gunson, as it had prompted Strauss more than 30 years earlier, to search for a reformulation of the axiomatic basis of the theory. Still, due primarily to the political situation of that time, Strauss' work itself remained virtually unknown and outside any of the main currents in the development of the interpretations of quantum mechanics.

8.3. MANY-VALUED LOGIC

The most important development in this field during the years immediately before and after World War II was based on the attempt to apply systematically a many-valued logic to quantum mechanics as implied by possibility 4 above.[66] This approach, as mentioned in the beginning of the present chapter, had its roots in the "relativity of logic." In particular, the epistemological elaborations of these ideas that had been proposed in the early 1930s in Geneva and Paris seem to have been decisively influential on this development, which indeed started in France. In contrast to the doctrine of the purely formal character of logic, both in the sense of the Platonist interpretation and in the sense of its modern modifications, and in contrast to the idea, voiced already by Boltzmann,[67] that the laws of thought are the product of evolution, these philosophers insisted that logic is a theory of reality. For Paul Hertz,[68] Gaston Bachelard,[69] Ferdinand Gonseth,[70] or Louis Rougier[71] logic was empirical: "*la logique est d'abord une science naturelle*."[72] The basic laws of logic such as the principle of

[64]Ref. 7–151.
[65]J. Gunson, "On the algebraic structure of quantum mechanics," *Communications in Mathematical Physics* **6**, 262–285 (1967).
[66]See Section 8.2.
[67]Cf. E. Broda, *Ludwig Boltzmann* (Deuticke, Vienna, 1955), pp. 104–111.
[68]P. Hertz, "Über das Wesen der Logik und der logischen Urteilsformen," lecture delivered at the International Congress for Mathematical Logic in Geneva, June 1934; reprinted in *Abhandlungen der Friesschen Schule*, (Öffentliches Leben, Berlin, 1935), Vol. 6, pp. 227–272.
[69]G. Bachelard, *Le Nouvel Esprit Scientifique* (Presses Universitaires de France, Paris, 1934).
[70]F. Gonseth, "La logique en tant que physique de l'objet quelconque," lecture delivered at the Congrès International de Philosophie Scientifique, Sorbonne, Paris, 1935; reprinted in *Actualités Scientifiques No. 393* (Hermann, Paris, 1936); *Les Mathématique et la Réalité* (Alcan, Paris, 1936); *Qu'est-ce que la Logique?* (Hermann, Paris, 1937).
[71]L. Rougier, "La relativité de la logique," *The Journal of Unified Science* **8**, 193–217 (1939).
[72]Ref. 70 (Alcan, 1936, p. 155).

identity or of contradiction are, in their view, abstractions from our experience with physical objects and of the same cognitive status as the laws of the geometry of physical space which, whether it is Euclidean or not, can be decided only by experiment and not by a priori thought. Their philosophy of science made it possible to claim that for a given science the system of logic used in the formalization of the theory depends on the progress of experimental techniques in the sense that the system of logic may be revised if newly discovered experiments make such a revision advisable or even imperative.

It was precisely under the influence of these ideas that Paulette Février, long before completing her Ph.D. thesis,[73] made what was probably the earliest proposal for a systematic application of a many-valued logic to the formalization of quantum mechanics. In her paper[74] which Louis de Broglie presented to the Paris Academy early in 1937 Février pointed out that, contrary to the view which regards Heisenberg's indeterminacy relations as a consequence of the mathematical formalism of quantum mechanics within the framework of classical logic, these relations should be considered as laws basic to the construction of a logic appropriate to the properties of micro-objects, in accordance with Gonseth's conception of logic as "*une physique de l'objet quelconque*." That this logic has to have three truth values Février explained by the following consideration. Let a denote the proposition "the energy E has the value E_0," it being assumed that the energy spectrum is discrete; if E_0 lies in the spectrum (i.e., E_0 can be obtained as a value of E), then a is "true" ("V" for "*vraie, nécessairement ou de façon contingente*") provided a measurement of E yields E_0, and is "false" ("F" for "*fausse de manière contingente*") provided it does not yield E_0; the proposition a is "absolutely (necessarily) false" ("A" for "*fausse nécessairement*") if E_0 does not belong to the spectrum (i.e., E_0 cannot be a value of E). Applying this trichotomy to the conjunction of what she called "non-conjugate" propositions, that is, propositions associated with simultaneously performable (compatible) measurements, Février arrived at the following truth table:[75]

&	*V*	*F*	*A*
V	*V*	*F*	*A*
F	*F*	*F*	*A*
A	*A*	*A*	*A*

[73]Ref. 7–46.

[74]P. Février, "Les relations d'incertitude de Heisenberg et la logique," *Comptes Rendus* **204**, 481–483 (1937).

[75]It is easy to see that this table agrees with the truth table for *pAq* in Łukasiewicz's three-valued logic.

Applying the same considerations to the conjunction of "conjugate" propositions and taking into account the Heisenberg relations Février obtained the following truth table [conjugate (complementary) propositions]:

&	*V*	*F*	*A*
V	*A*	*A*	*A*
F	*A*	*A*	*A*
A	*A*	*A*	*A*

In a sequel paper[76] Paulette Février formalized the distinctive features of the proposed three-valued logic by introducing a classification of logics according to "valency" (number of truth values) and "genus" (number of distinct matrice~~d~~s needed to characterize logical conjunction). Fevrier's proposed logic has thus the valency 3 and the genus 2.

Summarizing we may say that Paulette Février, instead of resorting to a metalinguistic argumentation to decide whether a conjunction forms an admissible proposition or not, preferred to extend the domain of the applicability of the conjunction. As she later explained,[77] such an approach seems to have certain technical advantages.

Février's proposal soon proved unsatisfactory, for a project of this kind could not confine itself to a modification of the axioms and rules of the propositional calculus but would have to deal also with the problems of quantification (e.g., existential quantifiers associated with the different versions of conjunction or disjunction) and of semantical rules by which truth values are to be assigned to well-formed formulae. In fact, some of the more technical arguments Février adduced in support of her thesis[78] have subsequently been refuted on these grounds.[79]

Another three-valued system of quantum logic which attracted much attention among philosophers of science was proposed in the early 1940s by Hans Reichenbach, whose conception of the indeterminacy relations has already been discussed.[80] After his emigration in 1933 from Berlin

[76] P. Février, "Sur une forme générale de la définition d'une logique," *Comptes Rendus* **204**, 958–959 (1937).

[77] *Or pour des raisons de technique mathématique il est beaucoup commode d'avoir des opérations qui s'appliquent à tous les éléments de la classe considérée...* P. Destouches-Février, *La Structure des Théories Physiques* (Presses Universitaires de France, Paris, 1951), p. 33.

[78] P. Destouches-Février, "Logique et théories physiques," *Congrès International de Philosophie des Sciences, Paris,* **1949**, Vol. 2 (Hermann, Paris, 1951), pp. 45–54.

[79] Cf. J. C. C. McKinsey's review in *The Journal of Symbolic Logic* **19**, 55 (1954).

[80] See Ref. 6–4.

where he had worked on his well-known contributions to the philosophy of space and time, Reichenbach assumed a professorship in Istanbul. There he concentrated on the philosophical problems of probability and developed a "probability theory of meaning," which he published[81] in 1938 soon after his arrival in the United States to teach philosophy at the University of California in Los Angeles. According to this "probability theory of meaning" "a proposition has meaning if it is possible to determine a weight, i.e. a degree of probability, for the proposition" and "two propositions have the same meaning if they obtain the same weight, or degree of probability, by every possible observation." Reichenbach, it seems, did not realize that had he applied these ideas to quantum mechanics, he would have obtained a logical framework for a two-valued quantum logic in which the state of a physical system is defined as a probability function on a set of propositions.[82] Instead, when working in Los Angeles on quantum mechanics, Reichenbach developed a system of three-valued logic which he communicated for the first time in a lecture on September 5, 1941, at the Unity of Science Meeting in the University of Chicago. Encouraged by the interest with which his ideas were discussed he published his suggestions in a book entitled *Philosophic Foundations of Quantum Mechanics*.[83]

In the first part of the book Reichenbach discussed some general ideas on which quantum mechanics is based and in the second part he presented an outline of the mathematical methods of the theory. In the third part use is made of the philosophical ideas of the first part and of the mathematical formulations of the second part in the proposal to interpret quantum mechanics in the language of a system of a three-valued logic. Our comments will be confined to this proposal without implying thereby that the numerous other issues raised by Reichenbach in this book do not deserve a detailed discussion.

Basic to Reichenbach's approach is his distinction between phenomena and interphenomena in microphysics. Phenomena are defined as "all those occurrences, such as coincidences between electrons, or electrons and

[81] H. Reichenbach, *Experience and Prediction* (University of Chicago Press, Chicago, 1938), pp. 54 ff. For additional biographical details on Reichenbach see M. Strauss, "Hans Reichenbach and the Berlin School," in Ref. 52 (1972, pp. 273–285). For a bibliography of Reichenbach's publications see his posthumous *Modern Philosophy of Science, Selected Essays by Hans Reichenbach*, Maria Reichenbach, trans. and ed. (Humanities Press, New York; Routledge and Kegan Paul, London, 1959), pp. 199–210.

[82] Cf., e.g., J. M. Jauch, *Foundations of Quantum Mechanics* (Addison-Wesley, Reading, Mass., 1968), p. 94.

[83] H. Reichenbach, *Philosophic Foundations of Quantum Mechanics* (University of California Press, Berkeley and Los Angeles, 1944, 1965); *Philosophische Grundlagen der Quantenmechanik* (Verlag Birkhäuser, Basel, 1949); *I Fondamenti Filosofici della Meccanica Quantitista* (Einaudi, Torino, 1954). Sections 29–37 reprinted in Ref. 7-106.

protons..." which are connected with macrocosmic occurrences by rather short causal chains" and are verifiable by such devices as the Geiger counter, a photographic film, a Wilson cloud chamber, etc." On the other hand, "occurrences which happen between the coincidences, such as the movement of an electron," are called interphenomena and are introduced by inferential chains of a much more complicated sort in the form of an interpolation within the world of phenomena. This distinction led Reichenbach to a division of all possible interpretations of quantum mechanics into two classes: *exhaustive interpretations*, which provide a description of phenomena as well as interphenomena, and *restrictive interpretations*, which "restrict the assertion of quantum mechanics to statements about phenomena." Calling *normal* (in the wider sense) a description which conforms to the principle that "the laws of nature are the same whether or not the objects are observed" and which implies therefore that the laws of phenomena are identical with those of interphenomena, Reichenbach referred to experiments such as the double-slit experiment to show that in quantum physics, contrary to classical physics, each exhaustive interpretation such as an interpretation in terms of a wave language or in terms of a corpuscle language leads to causal anomalies such as actions at a distance between events at separate slits. Even if there exists a normal description for *every* interphenomenon, a normal description for *all* interphenomena does not exist.

Since causal anomalies appear only in connection with interphenomena they can be avoided in restrictive interpretations such as the interpretation proposed by Bohr and Heisenberg according to which statements about interphenomena are discarded as meaningless. In particular, in such an interpretation the physical law which declares that of two noncommutative quantities both cannot at the same time have determinate values is expressed by the rule that of the two corresponding (complementary) statements which assert the assignments of such values to the noncommuting quantities one (at least) must be meaningless. A basic law in the object language of quantum theory, Reichenbach argued, is thus transformed into a semantical rule in the metalanguage of that theory: a physical law is expressed as a rule for the meaning of statements. "This is unsatisfactory," Reichenbach declared, "since usually physical laws are expressed in the object language, not in the metalanguage."[84]

In view of these and similar deficiencies Reichenbach raised the question whether one could not construct an interpretation which avoids these shortcomings without paying the price of introducing causal anomalies. If we wish to avoid the undesirable feature of including meaningless state-

[84]*Ibid.* (1965, p. 143; 1949, p. 157).

ments in the language of physics which originated in our regarding statements about values of unobserved entities as meaningless (Bohr), "we must use an interpretation which excludes such statements, not from the domain of *meaning*, but from the domain of *assertability*. We are thus led to a three-valued logic, which has a special category of this kind of statements."[85]

In addition to "true" (T) and "false" (F) Reichenbach thus introduced a third truth-value "indeterminate" (I), which characterizes statements that were regarded as "meaningless" in the Bohr-Heisenberg interpretation. That "indeterminate" should not be confused with "unknown" as used in macroscopic situations is explained by the following example. If John says (proposition a), "If I cast the die in the next throw, I shall get six," and Peter says (proposition b), "If I cast the die, instead, I shall get five," and John throws the die and gets four, then a is false. Whether b is true or false obviously no additional throw could ever decide; but since the throw is a macroscopic affair there are other means of testing the truth of b, for example, measuring exactly the position of the die or the status of Peter's muscles. Hence the truth value of b, though *unknown* (for us, not for Laplace's superman), is not *indeterminate*.

The number of logical operations or logical connectives definable by truth tables is of course much greater in a three-valued logic than in a two-valued logic. Among the numerous operations that can be considered as generalizations of the operations in two-valued logic the more important were defined by Reichenbach as follows. If the proposition a has the values T, I, F, the "cyclical negation" of a, $\sim a$, is defined as having the values I, F, T, the "diametrical negation" $-a$ as having the values F, I, T, and the "complete negation" $\bar{a}$ as having the values I, T, T, respectively. Among the binary operations the disjunction $\vee$, conjunction $\wedge$, alternative implication $\rightarrow$, and standard equivalence $\equiv$ were defined by the following table [nonconjugate (noncomplementary) propositions]:

a	T	T	T	I	I	I	F	F	F
b	T	I	F	T	I	F	T	I	F
$a \vee b$	T	T	T	T	I	I	T	I	F
$a \wedge b$	T	I	F	I	I	F	F	F	F
$a \rightarrow b$	T	F	F	T	T	T	T	T	T
$a \equiv b$	T	I	F	I	T	I	F	I	T

[85] *Ibid.* (1965, p. 145; 1949, p. 159).

According to Reichenbach the truth values "are so defined that only a statement having the truth value T can be asserted," although it is possible to state that a statement has a truth value other than T. Thus, for example, the assertion $\sim\sim a$ states that a is indeterminate. Similarly, $\sim a$ (or for that matter $-a$) states that a is false. "This use of the negations enables us to eliminate statements in the metalanguage about truth values" and we "can carry through the principle that *what we wish to say is said in a true statement of the object language*."[86]

In particular, the quantum mechanical rule of complementarity can be formulated in the object language as follows. Let U denote the statement "The physical quantity X has the value u" and let V denote the statement "The physical quantity Y (complementary to X) has the value v," X and Y being noncommutative quantities, then the rule of complementarity reads

$$U \vee \sim U \rightarrow \sim\sim V, \tag{4}$$

which has the value "true" (T) if and only if at least one of the two statements U, V has the value "indeterminate" (I). It is not difficult to prove that the condition of complementarity is symmetrical: if U is complementary to V, then V is complementary to U.[87]

At the end of his exposition Reichenbach explained how those statements which ordinarily lead to causal anomalies lose in the interpretation of quantum mechanics through a three-valued logic their pernicious character by becoming "indeterminate statements." For such statements, although as part of the object language of physics combinable with other statements by logical operations, can no longer be used as premises for the derivation of those undesired consequences. As an illustration of this suppression of causal anomalies let us discuss, following Reichenbach, the standard double-slit experiment in the particle language. A (binary) disjunction will be called *closed* if in case one term is false, the other must be true, *exclusive* if in case one term is true, the other must be false, and *complete* if one of its terms is true. Let a_i $(i=1,2)$ be the following statement: "The particle passes through slit S_i." Once the particle has been observed on the screen we know that the disjunction $a_1 \vee a_2$ is closed and exclusive, for we know that if the particle did not pass through one of the two slits, it went through the other, and that if it went through one of the two slits, it did not go through the other. The point now is that in Reichenbach's three-valued logic, contrary to ordinary two-valued logic, a closed and exclusive disjunction need not be true but may be indeterminate. In fact, it is indeterminate if a_1 (and hence necessarily also a_2) is

[86] *Ibid.* (1965, p. 153; 1949, p. 168).
[87] *Ibid.* (1965, p. 157; 1949, p. 172).

indeterminate, that is, if no observation has been made of which slit the particle passed through. The inassertability then prevents us from applying the usual probability considerations which ordinarily would have led to the causal anomalies. If, however, a localization had been made, say, at S_1, then a_1 would be either true or false and the disjunction $a_1 \vee a_2$ would be complete and true in which case, as we know, no interference effects would appear.

Comparing his interpretation with those suggested by Strauss and by Février, Reichenbach pointed out that Strauss' principle of nonconnectability and Février's assignment of the truth value A express the physical law of complementarity as a semantical rule and not as a statement in the object language as in his own interpretation. Referring to Février's approach and its later elaborations in conformance with the extremist view of the relativity of logic we may add the comment that this approach regards a logical theory as adequate or true for one part of the world and inadequate or false for another, whereas Reichenbach's philosophy of logic acknowledges only one logical system, which in the case of macroscopic systems reduces to ordinary logic as a special case.[88]

Reichenbach's work, whatever the final judgment about it may be, was an original, comprehensive, and cleverly conceived attempt toward a clarification of the basic epistemological problems of quantum mechanics. It was also the first book dealing exclusively with the philosophy of quantum mechanics written by a philosopher of science.

All reviewers of the book, even those who rejected its major thesis, concurred in the judgment that it was a significant, stimulating, and suggestive contribution. John Oulton Wisdom, at that time still professor of philosophy at Farouk I University in Alexandria prior to his appointment to the London School of Economics, declared in a review of Reichenbach's book: "There is almost nothing in the book that I can find to criticise—not because I like the conclusion, for I dislike it, and I think the author does so too."[89]

Carl Gustav Hempel, the noted logician and philosopher of science who had left Germany in 1937 and became professor at Yale University in 1948 (since 1955 at Princeton), was more critical. Admitting that "the book as a whole furthers greatly the understanding of the logical structure and the meaning of quantum theory," he voiced serious reservations about the advisability of the use of many-valued logic for the construction of

[88]For a technical study of the formal differences between these systems see H. Törnebohm, "On two logical systems proposed in the philosophy of quantum-mechanics," *Theoria* **23**, 84–101 (1957). Cf. also Max Bense's review of the German edition of Reichenbach's book in *Deutsche Literaturzeitung* **71**, 6–10 (1950).
[89]*Mind* **56**, 77–81 (1947).

linguistic systems in empirical science:[90] (1) Reichenbach did not explain the exact meaning of the truth values *T* and *F* which in his trivalent logic cannot be synonymous with the customary logical concepts of truth and falsity; (2) the same applies to Reichenbach's notion of assertability and his remark that *T* is a necessary condition thereof; (3) it is wrong to assume, as Reichenbach did, that the fact that two propositions in his system of logic have the truth value *T* in the same cases is a sufficient condition that they make the same assertion; (4) a rigorous formulation of his thesis would require the use of quantifiers and thus the logical apparatus of the lower and higher functional calculus and not only that of the propositional calculus as worked out by Reichenbach. "As long as the way toward such an extension has not been outlined, it does not seem clear precisely in what sense it can be said that quantum mechanics can be formulated in a language governed by a three-valued logic."

Similar exceptions were taken by Atwell R. Turquette of Cornell University, the well-known co-author of the standard treatise on many-valued logic.[91] Though "interesting and suggestive," Turquette declared,[92] Reichenbach's book "embraces some curious features," among them the following: whereas "true" and "false" in Reichenbach's three-valued logic are determined by the usual objective criteria, the criterion for "indeterminacy" is described merely in terms of such vague epistemic concepts as "unknowable in principle" or "unknowable to Laplace's superman."

The most penetrating criticism of Reichenbach's book was probably written by Ernest Nagel, the renowned philosopher of Columbia University who had just published his much-discussed essay "Logic without Ontology"[93] in which he stated that logical principles are "regulative principles," "prescriptive for the use of language," and that the choice between alternative logical systems should be grounded "on the relatively greater adequacy of one of them as an instrument for achieving a certain systematization of knowledge"—a position apparently most susceptible to the espousal of Reichenbach's ideas. And yet Nagel's critique[94] was so severe that it provoked Reichenbach to publish a reply[95] to

[90] *The Journal of Symbolic Logic* **10**, 97–100 (1945).

[91] J. B. Rosser and A. R. Turquette, *Many-Valued Logic* (North-Holland Publishing Company, Amsterdam, 1952).

[92] *The Philosophical Review* **54**, 513–516 (1945).

[93] E. Nagel, "Logic without ontology," in *Naturalism and the Human Spirit*, Y. H. Krikorian, ed. (Columbia University Press, New York, 1944), pp. 210–241; reprinted in *Readings in Philosophical Analysis*, H. Feigl and W. S. Sellars, eds. (Appleton, New York, 1949), pp. 191–210.

[94] *The Journal of Philosophy* **42**, 437–444 (1945).

[95] H. Reichenbach, "Reply to Ernest Nagel's criticism of my view on quantum mechanics,"

which Nagel, in turn, wrote a rejoinder.[96] Space does not allow us to analyze in detail this interesting debate between these two eminent philosophers.

Concerning the points discussed above, Nagel, too, censured Reichenbach for not having indicated just what has to be understood by the three possible truth values "true," "indeterminate," and "false"; for since in Reichenbach's system "true" has two exclusive alternatives and not just one, as in two-valued logic, "true" cannot have its usual meaning. Moreover, Nagel continued, according to Reichenbach certain statements are inherently indeterminate so that their truth or falsity cannot be verified, a situation which contradicts Reichenbach's declared endorsement of the "verifiability theory of meaning." In answer to this criticism Reichenbach declared that in his system "a statement is meaningful if it is verifiable as true, or false, or indeterminate," whereupon Nagel retorted that, if Reichenbach's reasoning were tenable, a statement, if verified to be meaningless, would be verifiable and hence not meaningless!

What did physicists think about the use of a three-valued logic in quantum mechanics? The 1948 fall issue of *Dialectica*, which, as mentioned previously,[97] was dedicated to discussions about complementarity, offered them an appropriate occasion to voice their opinions on this matter. In fact, the issue itself contained a contribution by Reichenbach[98] on his principle of anomaly, according to which "the principle of action by contact is violated whenever definite values are assigned to the unobserved quantities, i.e., when an exhaustive interpretation of quantum mechanics is used," and on its logical consequences which call for the use of his three-valued logic in order to eliminate such anomalies. The issue also contained articles by Jean-Louis Destouches[99] and by Paulette Destouches-Février,[100] who likewise insisted that complementarity, "one of the fundamental characteristics of contemporary scientific thought," calls for the application of non-Aristotelian logic to quantum mechanics. If we recall that Ferdinand Gonseth, Gaston Bachelard, and Paul Bernays invited Wolfgang Pauli, who shortly after his return from Princeton to Zurich had become a member of the consulting board of *Dialectica*, to edit this special issue, it seems not unlikely that the program was deliberately

The Journal of Philosophy **43**, 239–247 (1946).

[96] *Ibid.*, pp. 247–250.

[97] Ref. 4–6.

[98] H. Reichenbach, "The principle of anomaly in quantum mechanics," *Dialectica* **2**, 337–350 (1948).

[99] J.-L. Destouches, "Quelques aspects théoretiques de la notion de complementarité," *ibid.*, 351–382.

[100] P. Destouches, "Manifestations et sens de la notion de complementarité," *ibid.*, 383–412.

planned to include an exchange of views among physicists and philosophers on the problem of many-valued logic in quantum mechanics.

The plan worked. Previewing these papers in an editorial survey[101] Pauli remarked that "in spite of the many investigations in this field which have been already made—among them the very illustrative paper by von Neumann and Birkhoff—the physicists (among them myself) have a great resistence against the acceptance of new axioms of logic.... Indeed the physicist finds the common language not only in the description of the records of observation (spots in photographic plates etc.), but also in the purely mathematical model of vectors in the Hilbert-space and their projections in suitably chosen subspaces." The use of this model, according to Pauli, makes any new definition of logical operations unnecessary, and any statement on simultaneous values of observables to which no Hilbert space vector exists may be called meaningless. "This definition of 'meaning,'" Pauli declared, "supposes a knowledge a priori of the quantum mechanical model, but it does not suppose any actual empirical verification. Hence the objections of Reichenbach against such a 'restriction of meaning' seem to me not conclusive."

In the same issue of *Dialectica* Niels Bohr,[102] too, expressed his view on this matter. Complementarity, he pointed out, does not involve any renunciation of the customary demands of explanation but "aims at an appropriate dialectic expression for the actual conditions of analysis and synthesis in atomic physics.... Incidentally, it would seem," Bohr continued, "that the recourse to three-valued logic, sometimes proposed as means for dealing with the paradoxical features of quantum theory, is not suited to give a clearer account of the situation, since all well-defined experimental evidence, even if it cannot be analysed in terms of classical physics, must be expressed in ordinary language making use of common language." Bohr expressed himself in a similar vein in the article[103] which, as mentioned above,[104] he regarded as a particularly clear presentation of his views. All departures from ordinary logic, he stated there, are entirely avoidable if the word "phenomenon" is reserved solely for reference to unambiguously communicable information.

In his Waynflete Lectures, delivered in 1948 in Oxford, Max Born[105] called Reichenbach's approach "a game with symbols...which is certainly entertaining, but I doubt that natural philosophy will gain much by

[101]W. Pauli, Editorial, *ibid.*, 307–311.

[102]N. Bohr, "On the notions of causality and complementarity," *ibid.*, 312–319; reprinted in *Science* **111**, 51–54 (1950).

[103]Ref. 4–52.

[104]Ref. 6–63.

[105]Ref. 6–11 (1949, 1964, pp. 107-108).

playing it." The problem, in Born's view, "is not one of logic or logistic but of common sense. For the mathematical theory, which is perfectly capable of accounting for the actual observations, makes use only of ordinary two-valued logics." Even Reichenbach himself, Born argued, when explaining three-valued logic could do so only by the use of ordinary two-valued logic.

Against the objections raised by such leading quantum physicists Reichenbach tried to defend his viewpoint in an essay[106] published in honor of the noted nuclear and space physicist Erich Regener (1881–1955), with whom Reichenbach had friendly relations from the time of his studies at the Technische Hochschule in Stuttgart. In this paper Reichenbach repeated his argument that an exhaustive interpretation in microphysics cannot avoid causal anomalies, such as an action at a distance in the double-slit experiment or a reversal in the direction of time in Feynman's interpretation[107] of the positron as an electron "travelling backwards in time"—unless one admits an extension of logic. His opponents, Reichenbach charged, misunderstood this idea. As far as ordinary observations or "phenomena" are concerned, his system of logic provides the usual two-valued description, for a three-valued logic contains such a two-valued system as a particular case, just as the one-valued logic of tautologies is a particular case of ordinary two-valued logic. Three-valued logic, he re-emphasized, applies only to "interphenomena." Furthermore, the use of a two-valued metalanguage to explain three-valued logic, rather than involving any logical inconsistency, is merely sanctioned through a concession to our habit to employing two-valued logic. All systems of logic, Reichenbach continued, are intertranslatable and hence none of them, if taken without additional postulates, can express the structure of the real world. However, as soon as one postulates, for example, that causal anomalies should be excluded from any account of interphenomena then microphysics, in contrast to macrophysics, requires a three-valued logic as an adequate language for the description of physical reality.

Neither this article nor his frequent lectures on this subject, such as the

[106]H. Reichenbach, "Über die erkenntnistheoretische Problemlage und den Gebrauch einer dreiwertigen Logik in der Quantemechanik," *Zeitschrift für Naturforschung* **6a**, 569–575 (1951).

[107]When writing this paper, two years before his death, Reichenbach was preoccupied with the problem of temporal asymmetry. The then much discussed idea of time reversal in elementary particle physics, due to E. C. G. Stückelberg [*Helvetica Physica Acta* **14**, 588–594 (1941), **15**, 23–37 (1942)] and elaborated by R. P. Feynman in his famous paper "The theory of positrons" [*Physical Review* **76**, 749–759 (1949)], was regarded by Reichenbach as an elimination of one causal anomaly by another. C.f. his posthumous *The Direction of Time* (University of California Press, Berkeley and Los Angeles, 1956), p. 265.

series of lectures[108] which he delivered at the Institut Henri Poincaré in Paris, 10 months before his death converted any leading quantum physicist to his ideas. Among those who were influenced by him was Henning Müller,[109] an engineer and physicist and since 1949 lecturer at the University of Mainz, who fully endorsed Reichenbach's proposal, and Gotthard Günther of Richmond, Virginia (originally from Capetown, South Africa), who, when spending a year as visiting professor at the University of Hamburg, addressed a meeting of philosophers of science in Zurich on this subject in 1954 and tried to prove that only a three-valued logic can adequately present the non-classical features of quantum mechanics.[110]

Among Reichenbach's disciples, Hilary Whitehall Putnam, who after being awarded his doctorate (Ph.D., University of California, Los Angeles, 1951) taught at North Western University and at Princeton University from 1953 to 1961, eventually joining M.I.T. and Harvard, became an eloquent advocate of many-valued logic for quantum mechanics. From conversations with Garrett Birkhoff he gathered that von Neumann had already envisaged such ideas but abstained from publishing them for the same reason which had led Gauss, more than a 100 years earlier, to keep secret his discovery of non-Euclidean geometry in fear of "the clamor of the Boeothians." In 1957 Putnam[111] published a paper on three-valued logic in which he tried to vindicate the use of the third truth-value "middle" (or M) in addition to T ("true") and F ("false"). "To use three-valued logic," he claimed, "makes sense in the following way: to use a three-valued logic means to adopt a different way of using logical words. More exactly, it corresponds to the *ordinary* way in the case of molecular sentences in which the truth-value of all the components is known; but a man reveals that he is using three-valued logic and not the ordinary two-valued logic (or partially reveals this) by the way he handles sentences which contain components whose truth-value is not known." In a world in which not every empirically meaningful statement is at least potentially verifiable or falsifiable the introduction of the third truth value, which like the other two is tenseless, is a "move in the direction of simplifying a whole

[108] H. Reichenbach, "Les fondements logiques de la mécanique des quanta," *Annales de l'Institut Henri Poincaré* **13**, 109–158 (1952-1953).

[109] H. Müller, "Mehrwertige Logik und Quantenphysik," *Physikalische Blätter* **10**, 151–157 (1954).

[110] G. Günther, "Dreiwertige Logik und die Heisenbergsche Unbestimmtheitsrelation," *Proceedings of the Second International Congress of the International Union for the Philosophy of Science* (Griffon, Neuchatel, 1955), pp. 53–59. C.f. also G. Günther, "Über Anschauung und Abstraktion," in *Dialog des Abendlandes-Physik und Philosophie*, E. Heimendahl, ed. (List, Munich, 1966), pp. 199–207.

[111] H. Putnam, "Three-valued logic," *Philosophical Studies* **8**, 73–80 (1957). Reprinted in Ref. 7-106.

system of laws." Such a world, Putnam contended, is precisely the microcosmos where, as explained by Reichenbach, the use of a three-valued logic "permits one to preserve both the laws of quantum mechanics and the principle that no causal signal travels with infinite speed—'no action at a distance.'"

Putnam and Reichenbach were severely criticized by Paul K. Feyerabend of the University of Bristol and the University of California (Berkeley) whose views on complementarity have been mentioned previously.[112] The most general among the numerous charges[113] leveled against them was his claim that their "sly" procedure constitutes a violation of one "of the most fundamental principles of scientific methodology," namely "the principle to take refutations seriously." For if, as stated by Reichenbach and Putnam, the laws of quantum mechanics are logically incompatible with the principle of contact action within the framework of ordinary two-valued logic, the adoption of a three-valued logic as advocated by them explicitly for this purpose would remove the need to modify either quantum mechanics or the principle of contact action; but then it would merely keep alive an incorrect theory in the face of refuting evidence. Putnam's insistence upon non-Euclidean geometry as an example of the advantages of changing the formal structure of a theory, Feyerabend contended, is altogether misleading, for whereas the application of non-Euclidean geometry produced fruitful new theories (e.g., Einstein's explanation of the precession of Mercury's perihelion) no results whatever derived from the Reichenbach-Putnam procedure which, *tout au contraire*, would only lead to the arrest of scientific progress.

In his second argument, directed primarily against Reichenbach, Feyerabend pointed out that two definitions had been given of the notion of an exhaustive interpretation, once[114] as an interpretation which "includes a complete description of interphenomena" and once[115] as an interpretation which "attributes definite values to the unobservables" for certain properties of given categories; but an exhaustive interpretation in the first sense, that is, a description of the nature of a quantum mechanical system, need not be an interpretation in the second sense, that is, "an attempt to represent quantum-mechanical systems as things which always possess some property out of each classical category relevant to them."

Third, Reichenbach's statement that all exhaustive interpretations (in the

[112]Ref. 6–78.

[113]P. Feyerabend, "Reichenbach's interpretation of quantum-mechanics," *Philosophical Studies* 9, 49–59 (1958). Reprinted in Ref. 7-106.

[114]Ref. 83 (1965, p. 33; 1949, p. 46).

[115]*Ibid.* (1965, p. 139; 1949, p. 153).

second sense) lead to causal anomalies is merely a restatement of the fact that quantum mechanics cannot be interpreted as a classical theory. However, the fact that a theory, if classically interpreted, leads to inconsistencies is no reason to renounce classical logic. Furthermore, Putnam's thesis of the incompatibility of the principle of contact action with quantum mechanics is unwarranted, Feyerabend continued, for neither the collapse of a wave in the wave language nor the particular features of the particle language can be used for the transmission of signals, but only to fields in which such transmissions occur can the principle of contact action be applied. Feyerabend then showed that every quantum mechanical statement which contains noncommuting operators can possess in Reichenbach's formalism only the truth value "indeterminate," which implies that even the basic commutation rules will be "indeterminate," a result that contradicts Reichenbach's own criterion of adequacy of the interpretation according to which every law of quantum mechanics must have either the truth value "true or the truth value false," but never the truth values "indeterminate."[116] In the remainder of his paper Feyerabend challenged all those arguments which Reichenbach and Putnam presented in favor of their thesis and against the Copenhagen interpretation.

Interestingly, in spite of his obviously censorious attitude Feyerabend was asked to review Reichenbach's book when it was republished in 1965 in a paperback edition. This gave Feyerabend an opportunity[117] to restate some of his major criticisms and to elaborate on some of them. In a lecture delivered at a conference on analytic statements in science held in Salzburg at about the same time Feyerabend[118] brought forth the following argument. If the principle of contact action were really a well-confirmed principle and if its confirmations were really, as Putnam claimed, refutations of the quantum theory, then by eliminating these conflicts on the basis of a three-valued logic Reichenbach and Putnam would merely decrease the empirical contents of the theory which is rated by the possibilities of refutation. Feyerabend's objections, it seems, have never been answered by any of the proponents of many-valued logic. Recently Michael R. Gardner of Harvard University adduced additional arguments,[119] some involving what he called "the modified EPR paradox," to refute the tenability of Reichenbach's theory.

[116] *Ibid.* (1965, p. 160; 1949, p. 174).

[117] *British Journal for the Philosophy of Science* **17**, 326–328 (1966).

[118] P. Feyerabend, "Bemerkungen zur Verwendung nicht-klassischer Logiken in der Quantentheorie," in *Deskription, Analyzität und Existenz*, P. Weingartner, ed. (A. Pustet, Salzburg, Munich, 1966), pp. 351–359.

[119] M. R. Gardner, "Two deviant logics for quantum theory: Bohr and Reichenbach," *British*

An even more audacious attempt than Reichenbach's to introduce many-valued logic into quantum mechanics is associated with the name of Carl Friedrich Freiherr von Weizsäcker, whom we have already mentioned earlier. In 1953, while still a member of the Max-Planck-Institute in Göttingen, von Weizsäcker visited, in an administrative capacity, Brazil where he met with David Bohm in São Paulo and discussed with him the problem of hidden variables. After his return to Göttingen von Weizsäcker, anxious to work out some ideas raised in his discussion with Bohm, decided to conduct a seminar, together with Georg Süssmann, with the objective of studying alternative formulations of quantum mechanics. It was in the course of this seminar, which was also attended by Heisenberg, that von Weizsäcker worked out his "complementarity logic."[120]

This work, as von Weizsäcker himself admitted, was also influenced by Georg Picht's philosophic studies on the relation between logic and science. Picht, a philologist and educator, conducted together with the physicist Clemens Münster over the Bavarian Broadcasting Service a discussion on the aims of modern education which touched upon the fundamentals of contemporary culture and civilization. Having come to the conclusion that the ultimate aim of education in its most general sense is the discovery and assimilation of truth, Münster and Picht were faced with the problem of how far scientific knowledge represents objective truth. This problem, in turn, led Picht to analyze the relation between ontology and logic and to conclude that the laws of logic reflect the structure of being,[121] a result similar to that obtained by the foregoing Swiss and French philosophers.[122] In this context Picht described the epistemological crisis of modern physics as the result of having discovered a domain whose ontological structure is no longer amenable to the application of conventional logic.

Greatly impressed by these ideas, von Weizsäcker wrote a review-essay on Picht's studies. Picht's thesis, which, in the words of this review, stated that "logic is a design which hypothetically implies a certain ontology,"[123]

Journal for the Philosophy of Science **23**, 89–109 (1972).

[120]Interview with C. F. von Weizsäcker, Starnberg, July 21, 1971.

[121]*"Die Gesetze der Logik sind erkannte Seinsgesetze... Die bisherigen Überlegungen haben uns dazu geführt, zu erkennen, dass die Gesetze der Logik...primär überhaupt nicht Gesetze der Aussagen, sondern Gesetze des in den Aussagen bezeichneten Seienden selbst sind und für die Aussagen nur insofern gelten, als die Aussagen dieses Seiende präsentieren."* G. Picht, "Bildung and Naturwissenschaft," in Cl. Münster and G. Picht, *Naturwissenschaft und Bildung* (Werkbund-Verlag, Würzburg, 1953), pp. 33–126, quotation on pp. 62, 63.

[122] Refs. 68, 69, 70.

[123] *"Für Picht ist Logik ein Entwurf, der eine bestimmte Ontologie hypothetisch impliziert,"*

served von Weizsäcker as a guideline for the construction of a unique, but little accepted, form of quantum logic which he published in the article[124] mentioned above in connection with his distinction between "parallel" and "circular" complementarity.

Von Weizsäcker's "complementarity logic" was constructed as a modification of the logic of contingent propositions and, in particular, of "simple alternatives" [*einfache Alternative*] such as the statement made when discussing the double-slit experiment that the particle, before reaching the screen, has passed either through slit 1 or through slit 2. In conformance with the general ideas mentioned earlier von Weizsäcker proposed to derive the rules of logic from the quantum mechanical situation itself. The state function ψ, behind the diaphragm with the slits, is uniquely determined by the two complex numbers u and v defined by the equation $\psi = u\varphi_1 + v\varphi_2$ where $uu^* + vv^* = 1$, φ_1 and φ_2 are normalized to unity, and $w_1 = |u|^2$, $w_2 = |v|^2$ are the probabilities that the particle has passed through slit 1 or slit 2, respectively. If $u = 1$, then $v = 0$ and the proposition a_1, "the particle passed through slit 1," is true; if $v = 1$, then $u = 0$ and a_1 is false. Thus if 1 denotes truth and 0 falsity and if we call "elementary" those propositions which describe pure cases, the idea of the proposed complementarity logic can be summed up, according to von Weizsäcker, as follows: Every elementary proposition can have, apart from 1 and 0, a complex number as its truth value. The square of the absolute value, just as $|u|^2$ above, gives the probability that the proposition (with the truth value u), if experimentally tested, will prove true. Von Weizsäcker's system may therefore be regarded as an infinite-valued logic. To understand the meaning of the phase of the complex truth value, the complementary alternative has to be considered. Associating the two-component vector (u,v) with the original alternative (i.e., the question of whether a_1 or a_2 is true) so that $(1,0)$ corresponds to the truth of a_1 and $(0,1)$ to the truth of a_2, von Weizsäcker contended that for every vector (u,v), if normalized as before, there exists a proposition which is true if the propositions of the original alternative have the truth values u and v. Every proposition, characterized by (u,v), which differs from a_1 and from a_2, is called complementary to a_1 and a_2. Complementarity has thus been introduced as a purely logical conception. If one of two complementary propositions is true or false, the other is neither true nor false.

To illustrate these ideas von Weizsäcker referred to the quantum theory of a spin-$\frac{1}{2}$ particle which, incidentally, seems to have been the paradigm

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Göttingische Gelehrte Anzeigen **208**, 117–136 (1954).
[124]Refs. 4-11, 4-48.

for his mathematical elaborations. The vector (u,v) in this example is simply the Pauli two-component spinor. If a given magnetic field is directed in the positive z-direction, the original alternative consists of the proposition a_1: "The spin is in the positive z-direction" and of the proposition a_2: "The spin is in the negative z-direction," and to every spinor (u,v) corresponds a direction of spin orientation which, if a_1 has the truth value u and a_2 the truth value v, is given in terms of the polar angle θ and the azimuthal angle φ by the equations

$$u=\cos\frac{\theta}{2}\exp\left(-\frac{i\varphi}{2}\right), \qquad v=\sin\frac{\theta}{2}\exp\left(\frac{i\varphi}{2}\right).$$

If θ is neither zero nor π, the proposition "the spin has the direction $\{\theta,\varphi\}$" is complementary to a_1 and a_2.

Through the assignment of truth values u and v, von Weizsäcker pointed out, a higher logical level is introduced, for the original alternative referred to a property of the physical object, but now we inquire about the truth of the alternative or rather of the answers to it, and this is a "metaquestion": What are the truth-values of the possible answers? It is answered in the complementary logic by the following infinite alternative: The possible answers to the metaquestion are the normalized vectors (u,v). This is an alternative in the sense of classical logic, any answer being either true or false. The complementarity logic has thus been introduced into the object language by means of a metalanguage which applies ordinary two-valued logic. In classical logic a_1 and "a_1 is true" are equivalent, but this is not so in the complementarity logic. Although from the truth (or falsity) of a_1 follows the truth (or falsity) of the proposition "a_1 is true," the converse does not hold: If the proposition "a_1 is true" is false, a_1 need not necessarily be true or false, even though "a_1 is true" is definitely false.

In the rest of the paper and its sequels[125] which appeared three years later the relation between the object language and the metalanguage is analyzed in greater detail, von Neumann's mathematical formalism of quantum mechanics is reinterpreted as an application of complementarity logic, and it is shown how higher quantizations leading to quantum field theory can be regarded as iterated applications of the proposed quantum logical quantization. Finally, antiparticles are introduced by generalizing the quantization procedure, that is, by assigning not one complex number but rather a pair of complex numbers as truth values.

[125] C. F. von Weizsäcker, "Die Quantentheorie der einfachen Alternative," *Zeitschrift für Naturforschung* **13a**, 245–253 (1958). C. F. von Weizsäcker, E. Scheibe, and G. Süssmann, "Komplementarität und Logik," *ibid.*, 705–721.

Von Weizsäcker's complementarity logic found little acceptance among physicists or philosophers with the exception, of course, of those who, like Georg Süssmann and Erhard Scheibe, were actively involved in this project. Niels Bohr, to whom, as mentioned previously,[126] von Weizsäcker had dedicated Part One of these papers, rejected these ideas just as he rejected von Weizsäcker's differentiation between "parallel" and "circular" complementarity. In a letter[127] to von Weizsäcker Bohr explained why he "fears that the introduction of an explicit 'complementary logic,' however consistently it may be set up, would obscure the clarification of the situation, which had been obtained on a simpler logical foundation" rather than contribute to its elucidation. The complementary mode of description, he wrote, is ultimately based on the communication of experience "which has to use the language adapted to our usual orientation in daily life," while all details such as the state description of an atomic system in terms of a wave function or the reduction of the wave packet have to be regarded as purely mathematical abstractions not describable by classical pictures. For further details on how, in spite of the fact "that all knowledge presents itself within a conceptual framework adapted to account for previous experience" which "frame may prove too narrow to comprehend new experience," objective description of experience must always be formulated in "plain language which serves the needs of practical life and social intercourse," he referred von Weizsäcker to a talk[128] he had given at Columbia University. Von Weizsäcker, not surprisingly, was not satisfied with Bohr's answer. In a letter[129] to Pauli he even expressed his doubts as to whether Bohr had ever given serious thought to the problem of the nature of logic or to the question of why logic can be applied at all to the study of nature.

8.4. THE ALGEBRAIC APPROACH

After von Weizsäcker's work no serious attempt seems ever to have been made to elaborate further a many-valued logical approach to quantum mechanics. In any case, interest in this branch of quantum logic soon

[126]Ref. 4–11.
[127]Dated December 20, 1955.
[128]N. Bohr, "Unity of knowledge," address delivered at a conference in October 27–30, 1954, at Arden House in Harriman, New York, in connection with the Bicentennial of Columbia University. Reprinted in *The Unity of Knowledge*, L. Leary, ed. (Doubleday, Garden City, N.Y., 1955), pp. 47–62, and in Ref. 4–5 (Wiley, pp. 67–82; Vieweg, pp. 68–83; 1957, pp. 83–100: 1961, pp. 67–80).
[129]Dated August 27, 1956.

abated. It is also strange that if, as claimed by Putnam, von Neumann had really favored such an approach, he never contributed to it or even expressed the slightest approval of it after the publication of Reichenbach's work. Apparently his major interest lay in another direction of development which had been initiated, in part at least, by Pascual Jordan.

Prompted primarily by the investigations of Landau and Peierls[130] into the difficulties of extending the quantum mechanical methods to relativistic phenomena and in particular by their conclusion that a relativistic quantum theory could not be established in terms of physical observables and their reproducible measurements in the sense of the nonrelativistic theory, Jordan attributed these difficulties to the quantum mechanical formalism itself. With his attempt to modify the algebra of operators by renouncing the associative law of multiplication he laid the foundations of what became known as the "algebraic approach" to quantum mechanics. Asking himself[131] what algebraic operations on observables are physically meaningful, he considered the set of all bounded self-adjoint operators; this contains sums of operators, the multiplication of an operator by reals and integral powers of operators, but not products of such operators which generally are not self-adjoint, although the (symmetric) "quasi-multiplication," $\frac{1}{2}(AB+BA)$, being expressible in terms of only sums and squares by $\frac{1}{2}[(A+B)^2-A^2-B^2]$, had to be admitted. A nonassociative algebra satisfying the identity $(A^2B)A=(AB)A^2$, over any field (only the field of reals seems to be of physical interest) was later called a "Jordan algebra."

Jordan's thesis that the statistics of measurements can be expressed rather simply in terms of a certain hypercomplex algebra was, in fact, elaborated by von Neumann in collaboration with Jordan and Wigner. In their study[132] of finite dimensional, real, and nonassociative algebras, that is, Jordan algebras of finite linear basis which satisfy the condition that $A^2+B^2+\cdots=0$ only if $A=B=\cdots=0$, they showed that every algebra of this kind is the direct sum of irreducible algebras. The most important result they obtained was the proof that all these irreducible algebras are precisely the algebras of (finite) Hermitian matrices over real, complex, or quaternionic numbers; the only exception was an algebra with 27 units,

[130] Ref. 5–50.

[131] P. Jordan, "Über eine Klasse nichtassoziativer hyperkomplexer Algebren," *Göttinger Nachrichten* **1932**, 569–575, "Über Verallgemeinerungsmöglichkeiten des Formalismus der Quantenmechanik," *ibid.*, **1933**, 209–217.

[132] P. Jordan, J. von Neumann, and E. Wigner, "On an algebraic generalization of the quantum mechanical formalism," *Annals of Mathematics* **35**, 29–64 (1934).

called $\mathfrak{M}_3^8$, of 3×3 matrices whose elements are Cayley numbers.[133] Abraham Adrian Albert[134] soon showed that this algebra cannot be faithfully represented by any algebra obtained by "quasi-multiplication" of real matrices but constitutes a new algebra on which a novel formalism of quantum mechanics may be constructed.

Since such algebras of finite dimensionality could not satisfy the Heisenberg commutation relations von Neumann[135] modified their postulational basis by replacing the finite dimensional restrictions by weaker topological conditions but retained the distributive postulate $(A+B)C = AC+BC$, which, in spite of its lack of physical interpretation, he thought he was forced to admit since otherwise "an algebraic discussion will be scarcely possible." The set of self-adjoint operators on a weakly closed self-adjoint algebra of operators on a real or complex Hilbert space turned out to be a model for von Neumann's modified axiomatization and its ensuing spectral theory.

Inspired by von Neumann's work, Irving Ezra Segal, a graduate of Princeton (A.B., 1937) and Yale (Ph.D., 1940), who as an assistant to Oswald Veblen at the Princeton Institute for Advanced Study in 1946 had personal contact with von Neumann, became greatly interested[136] in operator algebras. In the course of his study he was led to a conceptually simple axiomatization[137] of quantum mechanics which became the basis of the quantum field theories of Haag and Wightman.

Following Jordan and von Neumann, Segal took observables as the primitive (undefined) elements of his axiomatization. A set $\mathcal{Q}$ of objects is called a *system* of observables if it satisfies the following postulates:

1. $\mathcal{Q}$ is a linear space of the reals.

[133] On these numbers, introduced as generalizations of quaternions by Arthur Cayley in his paper "On Jacobi's elliptic function and on quaternions," *Philosophical Magazine* **26**, 208–211 (1845), reprinted in A. Cayley, *The Collected Mathematical Papers* (Cambridge University Press, Cambridge 1889), Vol. 1, pp. 127–130, cf., e.g., L. E. Dickson, *Linear Algebras* (Cambridge University Press, Cambridge, 1930), pp. 14–16.

[134] A. A. Albert, "On a certain algebra of quantum mechanics," *Annals of Mathematics* **35**, 65–73 (1934).

[135] J. von Neumann, "On an algebraic generalization of the quantum mechanical formalism," *Matematiceskij Sbornik—Recueil Mathématique* **1**, 415–484 (1936), reprinted in *Collected Works* (Ref. 1-5), Vol. 3, pp. 492–559. Part 2 of this paper was never published nor was it found in von Neumann's files.

[136] I. E. Segal, "Irreducible representations of operator algebras," *Bulletin of the American Mathematical Society* **53**, 73–88 (1947).

[137] I. E. Segal, "Postulates for general quantum mechanics," *Annals of Mathematics* **48**, 930–948 (1947).

2. There exists in $\mathcal{Q}$ an identity element I, and for every $U \in \mathcal{Q}$ and every nonnegative integer n there exists in $\mathcal{Q}$ an element U^n such that the usual rules for operating with polynomials in a single variable are valid.
3. For each observable U a nonnegative real number $\|U\|$ is defined such that $\mathcal{Q}$ is a real Banach space (i.e., a complete normed linear space) with $\|U\|$ as norm.
4. $\|U^2 - V^2\| \leqslant \max\{\|U^2\|, \|V^2\|\}$.
5. $\|U^2\| = \|U\|^2$.
6. If S and R are finite subsets of $\mathcal{Q}$ and $R \subset S$, then

$$\left\| \sum_{U \in R} U^2 \right\| \leqslant \left\| \sum_{U \in S} U^2 \right\|.$$

7. U^2 is a continuous function of U.

A *state* is defined as a real-valued linear function w on $\mathcal{Q}$ such that $w(U^2) \geqslant 0$ for all $U \in \mathcal{Q}$ and $w(I) = 1$; it is a pure state if it is not a linear combination, with positive coefficients, of two other states. $w(U)$ is called the expectation value of U in the state w. A set of states is called *full* if it contains for every two observables a state in which the observables have different expectation values. The *formal product* $U \cdot V$ of any two observables U and V is defined by $\frac{1}{4}[(U+V)^2 - (U-V)^2]$, a system is called *commutative* if this formal product is associative, distributive (relative to addition), and homogeneous (relative to scalar multiplication), and a set of observables is called *commutative* if the (closed) subsystem generated by the observables is commutative.

On the basis of these two algebraic and five metric postulates Segal could prove that a commutative system is algebraically and metrically isomorphic with the system of all real-valued continuous functions on a compact Hausdorff space (i.e., a topological space in which for every $x \neq y$ there exist open sets S_x and S_y such that $x \in S_x$, $y \in S_y$, and $S_x \cap S_y = \emptyset$, and in which the Borel-Lebesgue compactness condition is satisfied, namely that every open covering of a subset includes a finite subfamily which covers this subsets). In view of this result Segal could define the spectral values of an observable as the values of the function associated with this observable by this isomorphism. He proved that any system S of observables has a full set of states, that $\|U\| = \sup\{|w(U)|\}$, that the joint probability distribution of a commutative system of observables for a given state may be so defined that the expectation value of an observable in the state is the average of the spectral values of the observables relative to the probability distribution, a condition which, as he showed, uniquely determines the distribution.

Segal's proofs that for any two observables there exists a pure state with different expectation values, that each observable has a spectral resolution, that a state induces, in a natural way, a probability distribution on the range of the spectral values of each observable, and that observables are simultaneously observable (measurable) if and only if they commute, in brief, all major features of the quantum theory of stationary states, have been obtained, it will be noticed, without any reference to Hilbert space. Segal regarded this approach as superior to the Hilbert space formalism for, as he pointed out, "Hilbert space appears to be somewhat inadequate as a space even for the [system of all bounded self-adjoint operators], in that there exist pure states of the system which cannot be represented in the usual way by rays of the Hilbert space."[138] Thus without resorting to Hilbert space Segal developed a spectral theory which accounted for the main features of the quantum theory of stationary states. It is remarkable that Segal's axiomatization, simple as it is from the conceptual, though perhaps not quite so from the mathematical point of view, implies the existence of a general indeterminacy principle. Segal commented on this feature as follows: "Inasmuch as the postulates are of a relatively simple character, this serves to confute the view that the indeterminacy principle is a reflection of an unduly complex formulation of quantum mechanics, and strengthens the view that the principle is quite intrinsic in physics, or in any empirical science based on quantitative measurements."[139]

Yet in spite of such spectacular results Segal's formalization still proved to have a number of weak points. That Segal's postulate 6 is redundant was demonstrated in 1956 by Seymour Sherman[140] of the University of Pennsylvania (Moore School) and since 1964 of Indiana University, Bloomington. Sherman and, independently, D. B. Lowdenslager[141] of the University of California (Berkeley), showed by constructing appropriate models that the distributivity of the formal product, which in spite of the lack of any known physical reason for its necessity is a requirement in the formalism of quantum mechanics as actually used, does not follow from Segal's postulates. Sherman and Lowdenslager studied the relation between Segal's systems of observables and the self-adjoint elements of a C*-algebra. Lowdenslager, in particular, obtained necessary and sufficient conditions for a system of observables to be a C*-algebra.

[138]Ref. 137, p. 930.

[139]Ibid., p. 931.

[140]S. Sherman, "On Segal's postulates for general quantum mechanics," *Annals of Mathematics* **64**, 593–601 (1956).

[141]D. B. Lowdenslager, "On postulates for general quantum mechanics," *Proceedings of the American Mathematical Society* **8**, 88–91 (1957).

8.5. THE AXIOMATIC APPROACH

While Segal's work, as we have mentioned, was an important contribution to the development of the modern C*-algebra theory of quantum mechanics, Mackey's axiomatic approach was no less important a contribution to the more recent development of quantum logic. When George Whitelaw Mackey, a graduate of Rice Institute (B.A., 1938) and Harvard (A.M., 1939, Ph.D., 1942) and since 1943 a member of the Harvard faculty, spent the summer of 1955 as visiting professor at the University of Chicago, he met Segal, who was at that time professor of mathematics at Chicago. Mackey became intimately acquainted with Segal's work and in particular with his lecture notes on "A Mathematical Approach to Elementary Particles and Their Fields,"[142] which were based on the 1948 paper. These notes, in turn, served as a point of departure for Segal's contribution[143] to the Summer Seminar on Applied Mathematics which the American Mathematical Society arranged at the end of July 1960 at the University of Colorado with the express purpose of strengthening the contact between mathematicians and physicists. Mark Kac, the mathematician of Cornell University, opened this Boulder seminar with an address "A Mathematician's Look at Physics: What Sets Us Apart and What May Bring Us Together." In addition, George Mackey presented to this seminar a paper in which he discussed the relevance of the theory of group representations for quantum mechanics.[144] Mackey's work, which will now be discussed, became an outstanding example for the contribution of a modern mathematician to the development of current theoretical physics in the spirit commended by Kac.

Influenced by Segal, Mackey decided to study in detail the axiomatic and quantum logical approaches to the quantum mechanics. In the spring semester of 1960 he had given a course at Harvard on the mathematical foundations of quantum mechanics, which was published in a mimeographed version, edited by his pupils including Arlan Ramsay. During the following years Mackey corrected and improved these notes and eventually published them as a book.[145]

[142]University of Chicago, 1955.

[143]I. E. Segal, *Mathematical Problems of Relativistic Physics* (American Mathematical Society, Providence, R. I., 1963).

[144]G. W. Mackey, "Group representations in Hilbert space," Appendix, pp. 113–130, to *Mathematical Problems of Relativistic Physics* (Ref. 143).

[145]G. Mackey, *Lecture Notes on the Mathematical Foundations of Quantum Mechanics* (Harvard University, Cambridge, Mass., 1960, mimeographed); *Mathematical Foundations of Quantum Mechanics* (Benjamin, New York, 1963); *Lektsii po Mathematičeskim Osnovam Kvantovoj Mekhaniki* (Mir, Moscow, 1965). Cf. also G. Mackey, "Quantum mechanics and Hilbert space," *American Mathematical Monthly* **64**, 45–57 (1957).

Mackey, in contrast to Segal, based his axiomatization of quantum mechanics on *two* primitive (undefined) notions: *observables* and *states*. His approach may be summarized as follows.

To each physical system belongs a set $\mathfrak{O}$ of observables and a set $\mathfrak{S}$ of states. Let $\mathfrak{B}(R)$ be the set of all Borel subsets of the real line R. $p(A, \alpha, E)$, interpreted as the probability that a measurement of $A \in \mathfrak{O}$ for a system in state $\alpha \in \mathfrak{S}$ yields a value in the Borel set $E \in \mathfrak{B}$, is formally a purely mathematical concept: it is a map from the Cartesian product $\mathfrak{O} \times \mathfrak{S} \times \mathfrak{B}$ into the closed interval [0, 1]. Mackey's first three postulates state that p has the properties of a probability measure on $\mathfrak{B}$:

1. $p(A,\alpha,\phi)=0$. $\quad p(A,\alpha,R)=1, \quad p(A,\alpha,\cup E_j)=\sum_j p(A,\alpha,E_j)$, for all $A \in \mathfrak{O}$, all $\alpha \in \mathfrak{S}$, and all pairwise disjoint Borel sets E_j.
2. If $p(A,\alpha,E)=p(A',\alpha,E)$ for all α and E, then $A=A'$. If $p(A,\alpha,E)=p(A,\alpha',E)$ for all A and E, then $\alpha=\alpha'$.
3. If f is a real-valued Borel function on R and $A \in \mathfrak{O}$, then there exists B in $\mathfrak{O}$ such that $p(B,\alpha,E)=p(A,\alpha,f^{-1}(E))$ for all $\alpha \in \mathfrak{S}$ and all $E \in \mathfrak{B}$.

The observable B in 3 above, which by virtue of 2 is uniquely determined by A, will be denoted by $f(A)$. The next postulate assures the existence of mixtures:

4. If $\alpha_j \in \mathfrak{S}$ and $\sum_{j=1} t_j = 1$ where $0 < t_j < 1$, then there exists $\alpha \in \mathfrak{S}$ such that $p(A,\alpha,E)=\sum_j t_j p(A, \alpha_j, E)$ for all $E \in \mathfrak{B}$ and all $A \in \mathfrak{O}$.

The uniquely determined α will be denoted by $\sum t_j \alpha_j$.

Before proceeding to the remaining postulates we have to define what Mackey called "questions" (the forerunner of Jauch's "yes-no experiments" or "yes-no propositions"): A "question" Q is an observable whose probability measure is concentrated in the points 0 and 1 of R, that is, $p(Q,\alpha,\{0,1\})=\alpha_Q(\{0,1\})\doteq 1$ for all $\alpha \in \mathfrak{S}$. Let L be the set of all questions. L is not empty. For if χ_E is the characteristic function of $E \in \mathfrak{B}$ then by 3 above $\chi_E(A) \in L$ for all $A \in \mathfrak{O}$. This question will be denoted by Q_E^A. The functions $m_\alpha(Q)$, defined on L by $p(Q,\alpha,\{1\})$, induce a partial order in L:

$$Q_1 \leqslant Q_2 \qquad \text{if an only if} \quad m_\alpha(Q_1) \leqslant m_\alpha(Q_2) \qquad \text{for all } \alpha \in \mathfrak{S}.$$

The greatest lower bound $Q_1 \cap Q_2$ and the least upper bound $Q_1 \cup Q_2$, if they exist, can then be defined in the usual way. With Q also $1-Q$ (which by virtue of 3 above is an observable) is a question, namely that question which yields 0 (or "no") if Q yields 1 (or "yes") and vice versa. Q_1 and Q_2 are *disjoint*, $Q_1 \perp Q_2$, if $Q_1 \leqslant 1-Q_2$ or, equivalently, if $m_\alpha(Q_1)+m_\alpha(Q_2) \leqslant 1$ for all $\alpha \in \mathfrak{S}$; in this case $Q_1 \cup Q_2$ is written $Q_1 + Q_2$. To assure that a given sequence of questions is a question the following postulate was introduced by Mackey:

5. If $Q_1, Q_2, \ldots,$ is a sequence of pairwise disjoint questions, then $Q_1 + Q_2 + \ldots$ exists.

That this postulated question, the uniqueness of which follows from postulate 2 and which yields "yes" if and only if at least one of the questions Q_j yields "yes," is the least upper bound of all the Q_j has been proved by Richard V. Kadison.

Following Mackey we call a map $q: E \to q_E$ from $\mathfrak{B}$ to L a *question-valued measure* if it satisfies the following conditions:

a. $E \cap F = \emptyset$ implies $q_E \perp q_F$.
b. $E_j \cap E_k = \emptyset$ for $j = k$ implies $q_{E_1 \cup E_2 \cup \ldots} = q_{E_1} + q_{E_2} + \ldots.$
c. $q_\emptyset = 0$ and $q_R = 1$.

Clearly, for any $A \in \mathfrak{O}$, $Q_E{}^A$ is a question-valued measure which uniquely determines the observable A. Observables thus correspond one-to-one to certain question-valued measures. Mackey's sixth postulate extends this relation to all question-valued measures:

6. If q is a question-valued measure, then there exists an observable A such that $Q_E{}^A = q_E$ for all $E \in \mathfrak{B}$.

Realizing that $Q \to 1 - Q$ is an orthocomplementation $Q \to Q'$ in L we see that Mackey's first six postulates assure that the notion of a system $\{\mathfrak{O}, \mathfrak{S}, p\}$ is equivalent to the notion of an orthocomplemented partially ordered set $\mathfrak{L}$ which Mackey, following Birkhoff and von Neumann, called the *logic* of the system. Without further assumptions about the structure of $\mathfrak{L}$ a spectral theory can be developed. Following Mackey we define a Borel set B of the real line as of *A measure zero* if $Q_E{}^A = 0$ or equivalently $\alpha_A(E) = 0$ for all $\alpha \in \mathfrak{S}$. The union 0_A of all open intervals of A measure zero, itself an open set, also has A measure zero. The closed set S_A which consists of all real numbers not in 0_A is the *spectrum* of A, and the set of all points x for which $Q^A_{\{x\}} \neq 0$ is the *point spectrum* P_A of A. A has a *pure point spectrum* if P_A is a Borel set whose complement has A measure zero. The quantization rules of quantum theory are a consequence of the fact that certain observables have nonempty point spectra.

Summarizing the contents of Mackey's first six postulates we can say that they imply the following conclusion: with every physical system a partially ordered orthocomplemented set $\mathfrak{L}$ can be associated which allows to identify observables with $\mathfrak{L}$-valued measures on the Borel sets of the real line and states with probability measures on $\mathfrak{L}$ and which allows to deduce some theorems of quantum mechanics without resorting to the notion of a Hilbert space.

Since the preceding six postulates apply both to classical and to quantum mechanics it is necessary to introduce a postulate which expresses the distinguishing feature of quantum mechanics. For this purpose let us consider, following Mackey, the problem of whether any two questions are simultaneously answerable. Q_1 and Q_2 are if there exist three mutually disjoint questions R_1, R_2, and Q_3 such that $Q_1 = R_1 + Q_3$ and $Q_2 = R_2 + Q_3$. It now can be shown that if any two questions are simultaneously answerable, then $\mathfrak{L}$ is a Boolean lattice and, conversely, that if $\mathfrak{L}$ is a Boolean lattice, then any two questions are simultaneously answerable. Now, since according to Mackey the fundamental difference between classical and quantum mechanics lies in the fact that in the latter not all two questions are simultaneously answerable, it follows that $\mathfrak{L}$ cannot have the structure of a Boolean lattice. To state not only what $\mathfrak{L}$ is not but what it is Mackey made the following assumption (later referred to as Mackey's "Hilbert space axiom"):

7. $\mathfrak{L}$ is isomorphic to the partially ordered set of all (closed) subspaces of a separable, infinite-dimensional Hilbert space $\mathcal{H}$.

This isomorphism, as Mackey and Shizuo Kakutani have shown, can always be chosen so that if Q corresponds to a subspace X of $\mathcal{H}$, then the question $1 - Q$ corresponds to the orthogonal complement $X^{\perp}$ of X in $\mathcal{H}$. Since furthermore the projections P in $\mathcal{H}$ stand in a one-to-one correspondence with the subspaces of $\mathcal{H}$, questions and projections do so as well.

To decide which measures on the questions are to be regarded as states Mackey considered, for each question Q, the expression $m_\varphi(Q) = (\varphi, P\varphi)$, where φ is a unit vector in $\mathcal{H}$ and P the projection corresponding to Q. Clearly, m_φ is a probability measure on $\mathfrak{L}$. Also any convex linear combination $\gamma_1 m_{\varphi_1} + \gamma_2 m_{\varphi_2} + \ldots$, where $\gamma_j \geqslant 0$, $\Sigma\gamma_j = 1$, and φ_j are unit vectors, is a probability measure on $\mathfrak{L}$; and according to Gleason's theorem every probability measure is such a combination. Although there is no a priori reason to assume that all these measures represent states, it can be proved, as Mackey has shown with the help of Gleason's theorem, that they all do if the following postulate is introduced:

8. If Q is any question different from 0 then there exists a state such that $m(Q) = 1$.

Since, as can be easily seen, the states that define measures on the questions of the form m_φ, where φ is a unit vector in $\mathcal{H}$, are all pure states and since no other states are pure it follows that $\varphi \rightarrow m_\varphi$ is a one-to-one map of the unit vectors of $\mathcal{H}$ onto the pure states, provided φ is identified with $c\varphi$ whenever $|c| = 1$ (since then $m_\varphi = m_{c\varphi}$). Questions and projections in $\mathcal{H}$, as we have seen, stand in a one-to-one correspondence and hence can be identified. Since, furthermore, each observable defines, and is defined by, a question-valued measure and all question-valued measures occur, it follows that observables correspond one-to-one to the projection-valued measures in $\mathcal{H}$ which, in turn, according to the spectral theorem, correspond one-to-one to the self-adjoint operators. Combining these correspondences we conclude that observables correspond one-to-one to self-adjoint operators.

Having thus established the one-to-one correspondence of observables with self-adjoint operators in a separable Hilbert space and of pure states with its one-dimensional subspaces Mackey concluded that the probability distribution of the observable defined by the operator A in the pure state defined by the one-dimensional subspace S is given by $E \rightarrow (\varphi, P_E{}^A \varphi)$, where φ is a unit vector in S, P^A the projection-valued measure associated with A by the spectral theorem, and hence $P_E{}^A$ the projection associated with the question "Does the value of the observable lie in the Borel set E?"

Mackey's last postulate 9, which refers to the change of states with the passage of time, is formulated in terms of a one-parameter semigroup of transformations of $\mathfrak{S}$ into $\mathfrak{S}$ which preserves convex combinations and leads in a natural way to the time-dependent Schrödinger equation. Since Mackey's work became an important framework for subsequent research in axiomatic quantum mechanics and quantum logic primarily in view of his approach to quantum statics, that is, the relationship between states and observables at a particular instant of time, we shall forgo any detailed discussion of his quantum dynamical axiom 9 and conclude our exposition of Mackey's work.

It was of course soon realized that Mackey's first six postulates, leading to the above-mentioned set $\mathfrak{L}$, are natural and physically plausible whereas his seventh postulate, "the Hilbert space axiom," was ad hoc and without any physical justification. In fact, prior to the publication of Mackey's book Neal Zierler,[146] a student of Mackey's, had already discussed this issue in his Harvard Ph.D. thesis. Although Mackey's first six axioms could be somewhat weakened and even reduced in number,[147] the Hilbert space axiom seemed to defy any attempt to replace it by a physically completely plausible assumption. The most advanced achievement in this direction is probably the result obtained recently by Maczyński. Having studied chemistry and mathematics in Warsaw (Ph.D., 1966) and done some research at Stanford University in California, Maciej J. Maczyński—like most of his

[146] N. Zierler, "Axioms for non-relativistic quantum mechanics," *Pacific Journal of Mathematics* **11**, 1151–1169 (1961). Reprinted in Ref. 7-106.

[147] Cf. M. J. Maczyński, "A remark on Mackey's axiom system for quantum mechanics," *Bulletin de l'Académie Polonaise des Sciences (Série des sciesces mathématiques, astronomiques et physiques)* **15**, 583–587 (1967), and S. Gudder and S. Boyce, "A comparison of the Mackey and Segal models for quantum mechanics," *International Journal of Theoretical Physics* **3**, 7–21 (1970).

Polish colleagues who work in his field—became interested in quantum logic when attending a course on mathematical methods in physics which Krzysztof Maurin gave at the University of Warsaw in 1966 in the spirit of Mackey's approach. After having visited the University of Illinois in 1968 Maczyński became particularly interested in physical interpretations of lattice theoretic theorems.

To understand his treatment of Mackey's Hilbert space axiom it must first be mentioned that in 1964 M. Donald MacLaren[148] and, independently, Constantin Piron[149] established the following theorem: If L is an irreducible complete orthocomplemented atomic lattice which satisfies the covering condition (i.e., if a is an atom and b is an element of L satisfying $a \cap b = 0$ then $b \prec b \cup a$) and which is of length $\geqslant 4$ (i.e., there exist in L elements $a_1, a_2, \ldots, a_n$ such that $0 < a_1 < \ldots < a_n = 1$ with $n \geqslant 4$), then there exists a division ring K with an involutive antiautomorphism and there exists a vector space $\mathcal{H}'$ over K with a definite Hermitian form f such that L is isomorphic to the lattice of the subspaces of $\mathcal{H}'$. If Mackey's logic $\mathfrak{L}$ could be shown to satisfy the conditions imposed on L the Hilbert space axiom could presumably be replaced by a simpler postulate as a consequence of which the vector space $\mathcal{H}'$ could be identified as a Hilbert space $\mathcal{H}$.

That such a program can in fact be carried out was shown by Maczyński. With the help of a theorem by Ichiro Amemiya and Huzihiro Araki[150] according to which the lattice of the subspaces of an inner product space (pre-Hilbert space) is orthomodular if and only if this space is a Hilbert space he could show [151] that instead of postulating the Hilbert space axiom it suffices to introduce what he called "the complex field postulate," namely that the division ring K determined by L is the field of complex numbers. Since, in carrying out this program, Maczyński slightly reformulated Mackey's first six axioms he had to make sure that this reformulation does not impair their physical plausibility. To this end a statement was defined as *physically basic* if it can be expressed solely in terms of the probability function $p(A, \alpha, E)$ with the help of algebraic signs, logical connectives, and quantifiers referring to the elements of $\mathfrak{O}$, $\mathfrak{S}$, and $\mathfrak{B}(R)$ or to their subsets, and it was shown that all of Maczyński's axioms are indeed expressed in the form of physically basic statements—

[148] M. D. MacLaren, "Atomic orthocomplemented lattices," *Pacific Journal of Mathematics* **14**, 597–612 (1964).

[149] C. Piron, "Axiomatique quantique," *Helvetica Physica Acta* **37**, 439–468 (1964).

[150] I. Amemiya and H. Araki, "A remark on Piron's papers," *Publications of the Research Institute for Mathematical Sciences, Kyoto University Series* **A2**, 423–427 (1966).

[151] M. J. Maczyński, "Hilbert space formalism of quantum mechanics without the Hilbert space axiom," *Reports on Mathematical Physics* **3**, 209–219 (1972).

with the sole exception of the complex field postulate "whose validity has to be confirmed by the fact that the theory developed from it gives predictions which are in agreement with reality." The ad hoc character of Mackey's seventh postulate has been reduced but not fully obliterated.

It was primarily due to Mackey that, since 1960 or so, the foundations of quantum mechanics and the quantum-logical approach in particular have become the subject of intensive research on a cooperative basis among mathematicians, physicists, and logicians alike. Josef M. Jauch, the dean of the Geneva school of quantum logicians, was greatly influenced by his discussions and correspondence with Mackey.[152] V. S. Varadarajan, a graduate of Calcutta University (Ph.D., 1959) and research associate at Princeton University (before he joined in 1965 the mathematics faculty of the University of California at Los Angeles) who eventually wrote a most comprehensive and profound treatise on the lattice-theoretical approach to quantum mechanics,[153] received the inspiration to work in this field while attending Mackey's lectures at the University of Washington in Seattle during the summer of 1961. True, Mackey's book, based as it was on abstract algebra, point set theory, topology, measure theory, functional analysis, and lattice theory, was not designed as a text for students. As the Boston physicist Paul Roman[154] in his review of this book in a mathematical journal rightly remarked, "There exists only a vanishing small section of the student population who would be able to follow this text and really benefit from it."

Mathematically and philosophically inclined physicists soon realized that the use of rigorous mathematics simplifies rather than complicates any logical analysis for it allows one to draw a clear distinction between the syntactical and the semantical problems of the interpretation; it may, furthermore, clarify precisely what the assumptions have to be in order to derive certain conclusions that are relevant for the interpretation. Thus, to give one example, a theorem of great importance for problems concerning joint probability distributions was proved by von Neumann[155] in 1931 for the case of discrete spectra: If self-adjoint operators commute, there exists a self-adjoint operator of which they are functions; this turned out to be derivable from the "logic" alone, merely on the basis of the orthocomplemented partially ordered set of questions (or "yes-no experiments") without any resort to Hilbert space.

Furthermore, in his book[156] Mackey suggested that on the basis of the

[152]Ref. 1-3 (1968, preface, p. VII).

[153]V. S. Varadarajan, *Geometry of Quantum Theory* (Van Nostrand, Princeton, N.J., 1968).

[154]*Mathematical Reviews* **27**, 1044–1045 (1964).

[155]Ref. 7-146.

[156]Ref. 145 (1963, pp. 70–71).

first six postulates alone it may be possible to prove that if A and B are two commensurable observables there exist a third observable C and Borel functions f and g from R to R such that $A=f(C)$ and $B=g(C)$. That indeed the less restrictive context of the "logic" suffices to prove this theorem had been shown by Varadarajan[157] even before Mackey's book appeared in print. Varadarajan's proof of the generalization for a countable set of commensurable observables, however, was erroneous, as James C. T. Pool[158] in his dissertation and, independently, Arlan Ramsay[159] have shown by counterexamples. If it is assumed, however, that the "logic" is a lattice, then the theorem is valid also for the case of denumerably many observables, as Varadarajan[160] has shown.

An example of the influence Mackey's approach exerted on philosophical considerations is the work of Howard Stein, a philosopher of science at Case Western Reserve University. Basing his conclusions explicitly on Mackey's approach, Stein contended in his writings[161] that, although there exist unsolved problems concerning the meaning of quantum mechanics in the sense of problems about the physical world, there are no difficulties which are peculiar to quantum mechanics and concern the epistemological interpretation of this theory; the conceptual structure of quantum mechanics, far from being loose or muddled, shows according to Stein an internal coherence which is "truly marvelous."

In spite of the at first seemingly promising results that could be derived merely from the "logic" of the propositional calculus (or questions) it soon became apparent that a real development of the theory of quantum logics requires the knowledge that this "logic" is a lattice. Since the "logic" is orthocomplemented this would be assured if one postulated that every pair of questions has a greatest lower bound. In fact, S. Gudder[162] and N. Zierler[163] as well as C. Piron[164] adopted this requirement as a postulate in

[157]V. S. Varadarajan, "Probability in physics and a theorem on simultaneous observability," *Communications on Pure and Applied Mathematics* **15**, 189–217 (1962), especially p. 206 (Theorem 3.3).

[158]J. C. T. Pool. "Simultaneous observability and the logic of quantum mechanics," Ph.D. thesis, State University of Iowa (1963).

[159]A. Ramsay, "A theorem on two commuting observables," *Journal of Mathematics and Mechanics* **15**, 227–234 (1966).

[160]Ref. 153.

[161]H. Stein, "Is there a problem of interpreting quantum mechanics?" *Nous* **4**, 93–104 (1970); "On the conceptual structure of quantum mechanics," in *Paradigms and Paradoxes* Ref. 6-152 (pp. 367–438).

[162]S. Gudder, "A generalized probability model for quantum mechanics," Ph.D. thesis, University of Illinois (1964).

[163]N. Zierler, "On the lattice of closed subspaces of Hilbert space," Technical Memorandum TM-04172, Mitre Corporation, Bedford, Mass. (1965).

[164]C. Piron, "Axiomatique quantique," thesis, Lausanne University (1963).

spite of the absence of a sufficient physical justification.[165] M. D. MacLaren,[166] on the other hand, postulated instead that the sum of any two bounded observables is a unique observable, connecting thereby quantum logic with the algebraic approach, which has meanwhile been significantly elaborated on the basis of Gelfand's representation theory of commutative Banach algebras, in an important but little known paper by Heinz Dieter Dombrowski and Klaus Horneffer.[167] From the historical point of view most influential among all these axiomatizations was undoubtedly Piron's work.

Constantin Piron, who in 1956 had received the diploma of *ingénieur physicien* at the Ecole Polytechnique de l'Universite de Lausanne and had served there as an assistant, working on descriptive geometry, in February 1959 attended a course on quaternionic quantum mechanics which Jauch, David Finkelstein, and David Speiser gave at CERN. It was on this occasion that Piron became acquainted for the first time with the Birkhoff and von Neumann paper on quantum logic and tried to express in its terms the notion of complementarity. Soon he was invited by Jauch to join the physics department of the University of Geneva. In Piron's axiomatics,[168] as in that of Birkhoff and von Neumann, the set of experimental propositions is characterized as a complete orthocomplemented lattice, but in contrast to theirs it is not modular in general. To prove this point, Piron made the following observation. In an infinite-dimensional Hilbert space the (closed) subspace $S_{a\cup b}$ is not[169] the linear sum of the (nonempty) subspaces S_a and S_b but also contains limit vectors not contained in either. If φ is such a limit vector, if S_a and S_b are disjoint, and if S_c is the subspace generated by the vectors of S_a and φ, then $S_{b\cap c}$ contains only the null vector. Hence $a\cup(b\cap c)=a$ and $(a\cup b)\cap c=c$, which shows that the lattice of subspaces is not modular.[170]

[165] Cf. the analogous problem for the conjunction in Refs. 46–51.

[166] Ref. 51.

[167] H. D. Dombrowski and K. Horneffer, "Der Begriff des physikalischen Systems in mathematischer Sicht," *Göttinger Nachrichten* (*Mathematisch-Physikalische Klasse*), **1964**, no. 8, 67–100.

[168] Piron's thesis "Axiomatique quantique" was published in *Helvetica Physica Acta* **37**, 439–468 (1964) and appeared also in a privately circulated English translation by Michael Cole, G.P.O. Engineering Department, Research Station, London, under the title "Quantum axiomatics."

[169] S_a denotes the subspace associated with the proposition a. For the mathematics involved see also P. R. Halmos, *A Hilbert Space Problem Book* (Van Nostrand, Princeton, N.J., 1967), p. 8 and p. 175 (problem 8).

[170] The question of the exact nature of this lattice was raised but not answered by G. Birkhoff in his paper "Lattices in applied mathematics," Ref. 28 (1961). Cf. p. 162 (line 25): "It would be very interesting to know...."

To counter the objection that since not every projection is necessarily a proposition, the lattice of propositions may still be modular Piron considered the example of spectral projectors corresponding to bounded intervals of the spectrum of the observables p (momentum) or q (position), which are certainly admissible propositions, and proved that the complete lattice generated by intersection and orthocomplementation of the projections which correspond to bounded disjoint intervals and cover the spectra of p and q are never modular. On the basis of additional arguments concerning the compatibility of propositions and their atomicity Piron arrived at the conclusion that the structure of the set of observables corresponding to "yes-no experiments" is that of a weakly modular (orthomodular) orthocomplemented atomic lattice. Following Mackey he defined the state of a physical system as an injection $w(a)$ of this lattice into the interval $[0, 1]$ such that

$$(1)\quad w(\emptyset)=0, \qquad (2)\quad w(I)=1, \qquad (3)\quad w(a)=w(b)=0$$

implies $w(a \cup b)=0$, and

$$(4)\quad w(\cup a_j)=\Sigma w(a_j) \qquad \text{for pairwise disjoint } a_j.$$

Since this definition of state involves probabilities and thus applies only to statistical ensembles and not to individual systems, Jauch and Piron[171] in 1969 revised this definition as follows: A state of a quantum mechanical system is the set of all true propositions where a proposition is defined as the class of equivalent "yes-no experiments" and is true if and only if any (and hence every) "yes-no experiment" of the corresponding equivalence class gives with certainty the measurement result "yes." By proving that for every state S the proposition $\bigcap_{a \in S} a$ is also contained in S and is an atom so that every proposition (with the exception of the absurd) contains at least one atom Jauch and Piron justified their final contention according to which, as Jauch explained in detail in his book,[172] the propositional system of all quantum mechanical "yes-no experiments" is a complete, orthocomplemented, weakly modular atomic lattice, which, moreover, is irreducible and satisfies the covering law.

Whereas the work of the Harvard and Geneva schools of quantum logic was essentially an elaboration of the ideas conceived by Birkhoff and von

[171] J. M. Jauch and C. Piron, "On the structure of quantal proposition systems," *Helvetica Physica Acta* **42**, 842–848 (1969).
[172] Ref. 1–3.

Neumann, in Germany a different approach was developed, primarily by Peter Mittelstaedt, which originated in the so-called operative logic of Paul Lorenzen. After his studies in Jena Mittelstaedt attended Lorenzen's seminar in Bonn before he wrote his Ph.D. thesis in 1956 under Heisenberg in Göttingen on a problem in theoretical nuclear physics. In Göttingen Mittelstaedt also participated in von Weizsäcker's seminar on the foundations of quantum mechanics (1954–1956) in the course of which he became interested in quantum logic. After some research at CERN and the Max-Planck-Institute in Munich he joined the faculty of the University of Munich where he published his major studies on quantum logic.

Mittelstaedt's[173] philosophy of quantum mechanics started with an ontological differentiation between the concepts of substance in classical physics and in quantum mechanics. Reviewing the development of the notion of substance from Aristotle through Descartes to Kant Mittelstaedt pointed out that the Kantian conception of substance of "thing" as expressed in his statement that "every thing, as regards its possibility, is likewise subject to the principle of *complete* determination, according to which if *all the possible* predicates of *things* be taken together with their contradictory opposites, then one of each pair of contradictory opposites belong to it,"[174] though valid for classical physics, loses its unrestricted applicability in quantum mechanics. Since only properties whose values do not depend on the order in which they are measured can meaningfully be assigned to the object on which they are measured and since only such properties can be regarded as "objective" (inherent in the object or substance), substance may be regarded, with Kant, as an a priori category of inherence-and-subsistence in classical physics where all properties satisfy these conditions.

As long as only compatible observables are considered, the application of the category of substance, that is, the objectification of such properties, can also be carried through in quantum mechanics, and this even more efficiently than in classical physics since one single quantity, the state vector, characterizes the object. If, however, *all* measurable properties (observables) of a quantum system are taken into consideration objectifiability becomes impossible within the framework of classical logic. For if a system is known with certainty to have a property A and if a property B,

[173] P. Mittelstaedt, "Untersuchungen zur Quantenlogik," *Sitzungsberichte der Bayerischen Akademie der Wissenschaften* **1959**, 321–386; "Quantenlogik," *Fortschritte der Physik* **9**, 106–147 (1961); *Philosophische Probleme der modernen Physik* (Bibliographisches Institut, Mannheim, 1963, 1965, 1968), Chapters 4, 5, 6.

[174] I. Kant, *Critique of Pure Reason*, translated by N. K. Smith (Macmillan, London, 1929), p. 488.

incompatible with A, is measured and it is found that the system has property B, then the probability that it has A is now less than 1 (certainty). The knowledge, originally possessed by the observer about the system, has thus been lost through the acquisition of additional information, a conclusion that contradicts the principle of "unrestricted availability" [unbeschränkte Verfügbarkeit] of classical logic if objectifiability is assumed. However, since this principle is logically independent of the other postulates of logic, Mittelstaedt argues, one may try to save objectifiability of all measurable properties by using a logic in which this principle is not presupposed.

Mittelstaedt was thus led to study the validity of logic in nature[175] and, in particular, in that part of spatiotemporal reality which can be described by the statements of quantum theory. To avoid the danger of circularity which arises from the fact that the formulation of quantum theory itself is already based on the use of logic, and to avoid the objection that the logic discussed is a contingent theory, verifiable only by experience, as could be suggested if the logic referred to were the formal logic as axiomatized by Hilbert and Ackermann, for example, Mittelstaedt resorted to the operative interpretation of logic in accordance with Paul Lorenzen.[176]

According to Lorenzen[177] the laws of logic are not arbitrary formalized assertions adapted to specific domains of facts but rules whose evidence follows from an examination of the possibilities to prove the assertions. If a, b, and c are elementary propositions for which there exist procedures to prove that they are true, they can be combined by means of the implication $\rightarrow$. If somebody asserts $a \rightarrow b$, he is committed to prove b if a can be, or has been, proved; similarly, if a "proponent" asserts $(a \rightarrow b) \rightarrow (c \rightarrow d)$, he is committed to prove $c \rightarrow d$ in case his "opponent" can prove $a \rightarrow b$. The proof thus assumes the form of a dialogue between the proponent and the opponent. If the proponent has a strategy which, independently of the factual contents of the elementary propositions, assures him in all cases the victory over his opponent, then his assertion is a logical statement or, more precisely, an "effective-logical" statement. For example, $a \rightarrow (b \rightarrow a)$ is a logical statement as the following dialogue illustrates:

[175] P. Mittelstaedt, "Über die Gültigkeit der Logik in der Natur," *Die Naturwissenschaften* **47**, 385–391 (1960).

[176] P. Lorenzen, *Einfünrung in die operative Logik und Mathematik* (Springer, Berlin and Heidelberg, 1955); *Metamathematik* (Bibliographisches Institut, Mannheim, 1962).

[177] On the historical origin and sybsequent development (during the years 1956–1960) of operative logic see P. Lorenzen, "Operative Logik," in *Contemporary Philosophy*, Vol. 1, R. Klibansky, ed. (La Nuova Italia Editrice, Firenze, 1968), pp. 135–140.

PROPONENT		OPPONENT	
1.	$a \rightarrow (b \rightarrow a)$		
		2.	a.
3.	Why a?		
		4.	Proof of a.
5.	$b \rightarrow a$.		
		6.	b.
7.	Why b?		
		8.	Proof of b.
9.	a.		
		10.	Why a?
11.	See 4.		

Since the opponent has been defeated, so to say, with his own weapon (the proof of a) and the proponent has won the battle whatever the particular contents of the propositions a and b, 1 above is a logical statement. Clearly, the principle of unrestricted availability has been employed, for an assertion that has been proved at one stage of the dialogue (as in 4) is assumed to remain valid evidence also at any later stage in the dialogue (as in 11). If a proponent asserts the conjunction $a \wedge b$, he commits himself to prove both a and b, and if he asserts the disjunction $a \vee b$, he commits himself to prove at least one of the two elementary propositions.

The following 10 statements L_1 to L_{10}, which can always be successfully defended by a proponent, constitute the so-called *affirmative logical calculus* (whereas $\rightarrow$ is part of the proposition, $\Rightarrow$ belongs to the metalanguage, i.e., $X \Rightarrow Y$ denotes that if the proposition X is derivable then also the proposition Y is derivable):

L_1. $a \rightarrow a$

L_2. $a \rightarrow b,\ b \rightarrow c \Rightarrow a \rightarrow c$

L_3. $a \wedge b \rightarrow a$

L_4. $a \wedge b \rightarrow b$

L_5. $c \rightarrow a,\ c \rightarrow b \Rightarrow c \rightarrow a \wedge b$

L_6. $a \rightarrow a \vee b$

L_7. $b \rightarrow a \vee b$.

L_8. $a \rightarrow c,\ b \rightarrow c \Rightarrow a \vee b \rightarrow c$

L_9. $(a \wedge (a \rightarrow b)) \rightarrow b$

L_{10}. $a \wedge c \rightarrow b \Rightarrow c \rightarrow (a \rightarrow b)$

To extend the affirmative logic, the (trivial) assertion V and the (absurd) assertion Λ are introduced; the former, by definition, can never be questioned and the latter, if asserted, makes one lose the dialogue. Finally, $\neg a$ (non-a) is defined, in accordance with the intuitionistic approach, by $a \rightarrow \Lambda$. With two additional assertions

L_{11}. $a \wedge \neg a \rightarrow \Lambda$

L_{12}. $a \wedge b \rightarrow \Lambda \Rightarrow c \rightarrow \neg a$?

the *calculus of effective logic* is completed. It will have been noted that the assertion

L_{13}. $V \rightarrow a \vee \neg a$ (tertium non datur)

is not dialogically demonstrable. Since it can be shown, however, that the statements of classical physics and those of the quantum physics of compatible observables satisfy L_{13}, it may be combined with L_1 to L_{12} to form the *classical logic* L_c. Its structure is that of a Boolean lattice with $\neg a$ as the complement of a.

Mittelstaedt now raised the question of whether L_c is also valid in quantum mechanics when incompatible observables are involved. His answer was this: If it is known in advance whether the statements under discussion are compatible, classical logic remains valid but some of its laws lose their applicability. If no such classification of statements is made in advance and all measurable quantities are treated alike as if they were objectifiable properties of ("improper," "*uneigentliche*") objects or substances, then some of the laws of L_c cease to be valid. Those that remain valid constitute what he called the quantum logic L_q.

Mittelstaedt argued that some of the laws of L_c lose their validity because the principle of unrestricted availability does not hold for incompatible propositions. Recalling, for example, the dialogical demonstration of $a \rightarrow (b \rightarrow a)$ but without assuming that a and b are compatible, we see that the proponent can in (11) no longer refer the opponent to (4) since with the proof of b in (8) the state of the system, as obtained in (4), may have been destroyed. A quantum mechanical proposition a, in contrast to a proposition in classical physics, has only restricted availability and may be "quoted" in dialogical demonstrations only if between the proof of a and its subsequent quotation all propositions proved were compatible with a. Mittelstaedt called this rule which significantly restricts the possibilities of dialogical demonstrations the "commensurability rule," and dialogically demonstrable implications, such as L_1 to L_9, which satisfy this rule, he called quantum-dialogically demonstrable. If L_{10}, which is not quantum-dialogically demonstrable, is replaced by

Q_{10}. $a\wedge c\to b\Rightarrow(a\to c)\to(a\to b)$

affirmative quantum logic is obtained. This becomes *effective* if, in addition, L_{12} is supplanted by

Q_{12}. $a\wedge a\to\Lambda\Rightarrow(a\to c)\to\neg a$.

Finally, since according to Mittelstaedt the properties of quantum mechanical propositions warrant the validity of the *tertium non datur*, its combination with the effective quantum logic completes the construction of the quantum logic, the structure of which is that of an orthocomplemented modular lattice.

Applying his quantum logic to the analysis of the double-slit experiment as an example, and denoting by a the proposition "the particle arrives somewhere on the screen" and by b the proposition "the particle passes through the upper slit," Mittelstaedt pointed out that the statement $a\to(a\wedge b)\vee(a\wedge\neg b)$, though valid in classical logic, is not valid in quantum logic. It would be a mistake, however, to interpret this result, Mittelstaedt added, as a renunciation of the law of the excluded third (*tertium non datur*); for this law, which asserts $V\to b\vee\neg b$ and remains valid also in quantum logic, does not refer to the fact that the system has been found to be in a definite state, as expressed by the proposition a in the just-mentioned similar, but quantum-logically invalid statement, which Mittelstaedt proposed to call "*tertium non datur* relative to a." The erroneous identification of the "relative" with the "absolute" *tertium non datur* was, in Mittelstaedt's view, the reason for the faulty abandonment of the two-valuedness of logic as proposed by Destouches-Février or Reichenbach.

Mittelstaedt's denial of the quantum-logical validity of $a\to(b\to a)$ was challenged by Kurt Hübner, a philosopher of the University of Kiel but at that time chairman of the philosophy department at the Technical University in Berlin. According to Hübner[178] this proposition asserts: "If a has been proved, then if b is proved, a is also proved." Hence if a is not proved, Hübner argued, the proposition is valid since it asserts something only in the case in which a has been proved; and if the demonstration of b

[178]K. Hübner, "Über den Begriff der Quantenlogik," *Sprache im technischen Zeitalter* **12**, 925–934 (1964). For a more general critique, claiming that by resorting to intuitionistic logic Mittelstaedt confused foundational problems of mathematics with those of physics and that he treated the *physical* hypothesis of complementarity as if it were a logical rule, cf. W. Stegmüller, Ref. 63, p. 459.

destroys the validity of a, then, again, the premise does not hold and the proposition is valid; whether it is applicable in a given case does not affect its validity. Moreover, Hübner asked, how can it be possible that a part of logic which, as Mittelstaedt explicitly admitted, has an a priori character can become false or inapplicable depending on whether one has an empirical knowledge, namely that of quantum mechanics, or not?

The Viennese philosopher of science Bela Juhos[179] also criticized Mittelstaedt's reasoning on the ground that a logical proposition, even if it refers to a certain time t, preserves its truth value (with reference to t) at any other time t' since truth values of logical propositions are time-independent. Mittelstaedt's contention that a proposition, once declared valid, can lose its "availability" is in Juhos' view a fallacy of the kind *quaternio terminorum*. Finally, Hans Lenk[180] of the University of Karlsruhe charged that Mittelstaedt's quantum-logical definition of the implication $a \rightarrow b$("if a has been verified by a measurement than b will always be found verified by an appropriate measurement") cannot be applied at all to the first implication in $a \rightarrow (b \rightarrow a)$ since $a \rightarrow b$ is itself no elementary proposition testable by measurement. Since, moreover, every physical measurement refers to a definite time t, the proposition under discussion, Lenk contended, should read $a(t_1) \rightarrow [b(t_2) \rightarrow a(t_1)]$, which shows that Mittelstaedt confused the temporal order of measurement processes with the logical order of the different stages in the dialogue. Mittelstaedt's alleged misconception was for Lenk just another argument for his general contention that all attempts, carried out so far, to show on *philosophical* grounds that classical logic has to be supplanted by some form of quantum logic in order to become adequate for modern physics have failed.

8.6. QUANTUM LOGIC AND LOGIC

To analyze the more recent views on the relation between quantum logic and logic in general let us resume the problem, partially discussed earlier, whether quantum logic could ever possibly supplant ordinary logic. Although certain expressions in the Birkhoff and von Neumann pioneering paper seem to indicate, as mentioned previously, that its authors believed they had laid the foundations of a new logic, the majority of subsequent

[179]B. Juhos, *Die erkenntnislogischen Grundlagen der modernen Physik* (Duncker and Humblot, Berlin, 1967), pp. 234–237.

[180]H. Lenk, "Philosophische Kritik an Begründungen von Quantenlogiken," *Philosophia Naturalis* **11**, 413–425 (1969), based on a lecture delivered at the 14th International Congress of Philosophy in Vienna, September 4, 1968. *Kritik der logischen Konstanten* (Walter de Gruyter, Berlin, 1968) pp. 611–618.

quantum logicians regarded their "logic" as a calculus of limited scope rather than as a full-fledged logic of exclusive validity. It would be inconsistent, it was argued, to claim for quantum mechanics as a theory the validity of some kind of *nonstandard* logic and, at the same time, to apply to quantum mechanical calculations ordinary mathematics which, as is well known, presupposes *standard* logic. To avoid such inconsistency it would have been necessary to construct a completely formalized theory, within the framework of the suggested nonstandard logic, comprising not only the logical and physical but also the mathematical principles to be employed, a project "somewhat analogous to the writing of *Principia Mathematica* (Russel and Whitehead, 1910–1913) though vastly more onerous," as McKinsey and Suppes once declared in a similar context.[181]

It was for such reasons that in 1959 Pascual Jordan restricted explicitly the scope of quantum logic to the laws of possible connections of statements about the state of a physical system, adding that "logic," thus defined, is an empirical science since only experience can tell which combinations of possible statements belong to a physical system.[182] In 1964 Constantine Piron, insisting that the lattice-theoretic approach as axiomatized by him does not constitute a new logic but merely formalizes rules of calculations within the framework of ordinary logic, adduced a new and more technical argument in support of his contention. "Certain authors have wanted to see in the preceding axioms the rules of a new logic," he wrote,[183] "but the axioms are merely the rules of calculation and the usual logic can be applied without the need of any modification." To prove his point Piron pointed out that the order relation $a \leqslant b$, the lattice-theoretic analog of the conditional "if a, then b" of classical logic, is not, as in classical logic, itself a new proposition.

To appreciate fully the importance of this point for the question whether quantum "logic" can serve as logic in the sense usually understood one has to recall that logic has always been defined as an investigation not only of the structure of propositions but also of deductive reasoning or demonstration. In fact, the opening statement in Aristotle's *Prior Analytics*, which

[181] J. C. C. McKinsey and P. Suppes, Review of P. Destouches-Février's *La Structure des Théories Physiques*, in *Journal of Symbolic Logic* **19**, 52–55 (1954).

[182] P. Jordan, "Quantenlogik und das kommutative Gesetz," in *The Axiomatic Method*, L. Henkin, P. Suppes, and A. Tarski, eds. (North-Holland Publishing Company, Amsterdam, 1959), pp. 365–375. Jordan's conception of quantum logic has recently been endorsed by B. C. van Fraassen for whom "each attempt at quantum logic has been an attempt to elucidate and exhibit semantic relations among the elementary statements." B. C. van Fraassen, "The labyrinth of quantum logic," *Boston Studies in the Philosophy of Science*, **13**, 224–254, (1974). Reprinted in Ref. 7-106.

[183] Ref. 168 (p. 441).

contains his most mature thought about logic, namely the statement that "the subject of our inquiry... is demonstration,"[184] shows that he valued logic chiefly for the insight it provides into the structure of demonstration. It is easy to show that this view was shared by the vast majority of logicians at all later times. To formalize logical inference it is necessary to have a rule of deduction which is generally phrased as follows: If p is true and if p implies q, then q is true. This, in turn, makes it necessary to consider the conditional "if p, then q" as a proposition, for otherwise one could not speak of the conjunction of a conditional statement with its antecedent entailing the consequent. This, it seems, was fully understood already by the Megaric philosopher Philo (fl.c.300 BC), a pupil-fellow of Diodorus Cronus. Philo defined the conditional truth functionally by stating that it is "sound unless it begins with a truth and ends with a falsehood."[185]

This conditional, the so-called Philonian conditional or material implication (Whitehead and Russel), denoted by $\supset$ or $\rightarrow$, must itself be a proposition in any propositional calculus which admits elementary inferential schemes such as the *modus ponendo ponens* $[(p\rightarrow q)\&p]\rightarrow q$ or the *modus tollendo tollens* $[(p\rightarrow q)\&\sim q]\rightarrow\sim p$. Applying this result to the calculus L_q of quantum mechanical propositions one is forced to conclude that L_q can qualify as a logic only if it associates with two propositions a and b a third proposition which represents the implication $a\rightarrow b$. But in L_q, it will be recalled, $a \leqslant b$ expresses an implication since it means that whenever a is true, then b is also true. Unfortunately, however, as already pointed out by Piron in his thesis, "although $a \leqslant b$ is the analogue of the logical implication $a\rightarrow b$, $a \leqslant b$ cannot be considered a proposition, since it is not a yes-no experiment (ce n'est pas une mesure du type oui-non); hence one cannot give any meaning to the expression $a \leqslant (b \leqslant c)$ which would be the counterpart of the well-defined relation $a\rightarrow(b\rightarrow c)$ in logic." This was one of the reasons why, according to Piron, the lattice L_q of quantum mechanical propositions could not qualify as a logic.

Additional arguments were adduced by Jauch and Piron.[186] Referring to the simple lattice of propositions on photon polarization states they showed that, contrary to the material implication $a\rightarrow b$ which can be defined by the proposition "not a or b," the relation $a \leqslant b$ cannot

[184]Aristotle, *Analytica Priora*, 24a10.

[185]Sextus Empiricus, *Pyrrhoneiae Hypotyposes* 2, 110; *Outlines of Pyrrhonism* (Loeb edition) (W. Heinemann, Harvard University Press, Cambridge, 1959), Vol. 1, pp. 220–221. On the history of the early debates on the nature of conditionals see W. Kneale and M. Kneale, *The Development of Logic* (Clarendon Press, Oxford, 1962, 1968), pp. 128–138.

[186]Ref. 43.

consistently be defined by the proposition $a' \cup b$. They also drew attention to a theorem by Fáy[187] according to which any uniquely orthocomplemented lattice L in which $a' \cup b = 1$ and $b' \cup c = 1$ imply $a' \cup c = 1$ (for all a, b, c of L) is distributive and hence Boolean.

Jauch and Piron also showed that the resort to a many-valued quantum "logic" would be of no avail. More precisely, they showed that for the lattice of subspaces in a Hilbert space the conditional $p \rightarrow q$ whose truth value in an infinite-valued logic is given by $[p \rightarrow q] = \min\{1, 1 - [p] + [q]\}$ where $[p]$ and $[q]$ are the truth values of p and q, respectively, cannot be defined as a yes-no experiment. That, in analogy to the case of two-valued logic, this implication can also be used for a law of deduction follows from the fact that $[p] = 1$ and $[p \rightarrow q] = 1$ implies $[q] = 1$.

The result obtained by Jauch and Piron was significantly generalized to orthomodular partially ordered sets L by R. J. Greechie and S. P. Gudder.[188] Considering an order determining set S of states m on L (which with m_1 and m_2 contains also $\frac{1}{2}m_1 + \frac{1}{2}m_2$) and calling a, b of L a "conditional" if there exists in L an element c such that $m(c) = \min\{1, m(a') + m(b)\}$ holds for all m of S, they defined L as "conditional" if every pair of its elements is "conditional." Greechie and Gudder then proved that L is "conditional" if and only if it is $\{0, 1\}$.

However, the conclusion, intimated by Jauch and Piron, that because $a' \cup b$ cannot be interpreted as an implication in non-Boolean lattices and because such lattices cannot be interpreted as logics unless they admit an implication was not fully warranted. For it is conceivable that an appropriate generalization of $a' \cup b$, that is, an extension which reduces to $a' \cup b$ in Boolean lattices, does for non-Boolean lattices precisely what $a' \cup b$ does for Boolean lattices. In fact, such a generalization was postulated by Walter R. Fuchs,[189] a pupil of Paul Lorenzen, and was explicitly given for the case of modular lattices by H. Kunsemüller,[190] a pupil of von Weizsäcker. That the same generalization, namely $a' \cup (a \cap b)$, can be used to this end also for orthomodular lattices was shown in 1970 by Peter Mittelstaedt[191] when he defined the binary operation $q(a, b)$, or in Mittelstaedt's original denotation $b \urcorner a$, by $a' \cup (a \cap b)$ and proved that $q(a,$

[187] G. Fáy, "Transitivity of implication in orthomodular lattices," *Acta Universitatis Szegediensis (Acta Scientiarum Mathematicarum)* **28**, 267–270 (1967).

[188] R. J. Greechie and S. P. Gudder, "Is a quantum logic a logic?," *Helvetica Physica Acta* **44**, 238–240 (1971).

[189] W. R. Fuchs, "Ansätze zu einer Quantenlogik," *Theoria* **30**, 137–140 (1964).

[190] H. Kunsemüller, "Zur Axiomatik der Quantenlogik," *Philosophia Naturalis* **8**, 363–376 (1964).

[191] P. Mittelstaedt, "Quantenlogische Interpretation orthokomplementärer quasimodularer Verbände," *Zeitschrift für Naturforschung* **25a**, 1773–1778 (1970).

$b)=1$ if and only if $a \leqslant b$. Indeed, if $a \leqslant b$ or $a \cap b = a$, then $a' \cup (a \cap b) = a' \cup a = 1$ or $q(a,b)=1$; conversely, if $1 = q(a,b) = a' \cup (a \cap b)$, then by the quasi-modularity condition, applied for $a \cap b \leqslant a \leqslant (a')'$ $a \cap b = a \cap ((a \cap b) \cup a') = a \cap 1 = a$ or $a \leqslant b$; but $a \cap q(a,b) \leqslant b$ is precisely the lattice-theoretic *modus ponens*. Mittelstaedt also showed on similar grounds that the commensurability relation can be interpreted as an operation by virtue of which propositions about commensurability become elements of the lattice. For both proofs the assumption of orthomodularity was a necessity. These results obviously disagreed with the conclusions reached by Piron and Jauch which, incidentally, were unknown to Mittelstaedt until the spring of 1972.

After the present author had drawn his attention to this disagreement Mittelstaedt decided to study the problem in greater depth. Having spent, to this end, the summer of 1972 at the Max-Planck-Institute in Munich Mittelstaedt arrived at the following conclusion.[192]

In classical logic it is postulated that with every two propositions a,b an implication $c(a,b)$ is associated which satisfies the conditions (1) $a \cap c(a,b) \leqslant b$ and (2) if $a \cap x \leqslant b$, then $x \leqslant c(a,b)$; it can then be shown that (3) $c(a,b)$ is unique, (4) $c(a,a)=1$, (5) $a \leqslant b$ if and only if $c(a,b)=1$, and (6) the logic is distributive. In the lattice L_q of the subspaces of Hilbert space or of quantum mechanical propositions no such $c(a,b)$ can exist since L_q is not distributive. However, for every a,b of L_q there exists in L_q an element $q(a,b)$, the "quasi-implication," which satisfies the conditions (1′) $a \cap q(a,b) \leqslant b$, (2′) if $a \cap x \leqslant b$, then $a' \cup (a \cap x) \leqslant q(a,b)$; it can then be shown that (3′) $q(a,b)$ is unique, in fact, it is $a' \cup (a \cap b)$, (4′) $q(a,a)=1$, (5′) $a \leqslant b$ if and only if $q(a,b)=1$ and (6′) the lattice is orthomodular (or quasi-modular, in Mittelstaedt's terminology). Since it can be shown that $q(a,b)$ is transitive in the sense that $q(a,b)=1$ and $q(b,c)=1$ imply $q(a,c)=1$ and since in the case of a Boolean lattice $q(a,b)$ reduces to $c(a,b)$ Mittelstaedt regarded $q(a,b)$ in view of (3′), (4′), and (5′) as a convenient generalization of the classical implication, even if, for example, $a \leqslant q(b,a)$, in contrast to $a \leqslant c(b,a)$, is not generally valid. As Mittelstaedt, moreover, pointed out, (6′) shows that (1′) and (2′) are the strongest possible conditions that can be imposed on $q(a,b)$, for they are necessary and sufficient conditions for orthomodularity.

According to the Geneva school, led by J. M. Jauch, quantum "logic" differs fundamentally in meaning and function from ordinary logic, the

[192] P. Mittelstaedt, "On the interpretation of the lattice of subspaces of the Hilbert space as a propositional calculus," *Zeitschrift für Naturforschung* **27a**, 1358–1362 (1972); "Zur aussagenlogischen Interpretation des Verbandes der Teilräume des Hilbertraumes," lecture delivered at the Munich Symposium "Grundlagen der Quantenmechanik," July 17–18, 1972.

former being the formalization of empirical facts obtained by induction and the latter an analysis of the meaning of propositional structures which is "true under all circumstances and even tautologically so."[193] The features these two logics have in common and which, in the view of these authors, caused considerable confusion should not be interpreted as manifestations of an essential identity or interchangeability in function. Similarly, P. Mittelstaedt[194] repeatedly emphasized the distinction between formal logic and quantum "logic"; the fact that for certain propositions in quantum mechanics, due to their protological property of only restricted availability [*beschränkter Verfügbarkeit*], formal logic (effective logic) ceases to be applicable does not affect the a priori character of logic. Even his lattice-theoretic formalization of the Philonian conditional as a quantum-logical operation should be regarded as a technical strategem within the confines of quantum "logic" and as such has no relevance for the relation between the two logics.

In sharp contrast to these views a number of theorists proposed regarding quantum "logic" as a full-fledged new logic which by dictate of experience is due to supersede classical logic. They justified their view on grounds similar to those advanced by the early proponents of the relativity of logic. According to one of their most eloquent proponents, the theoretical physicist David Finkelstein (Ph.D. M.I.T., 1950), who taught at Stevens Institute of Technology and since 1960 at Belfer Graduate School of Science and New York University, there is no such thing as an a priori universally valid logic; logic, like geometry, undergoes a process of evolution whose first major revolutionary change or "fracture," as he called it,[195] became apparent through the abandonment of distributivity—just as the first fracture in traditional Euclidean geometry appeared with the abandonment of Euclid's fifth postulate.

The analogy with the overthrow of Euclidean geometry by the general theory of relativity seems to have played an important role also in Hilary Putnam's shifting from his former espousal of the Reichenbachian interpretation to these new ideas. What in the past had been regarded as a "necessary truth" of logic, he declared,[196] may turn out later to be false on empirical grounds. "I regard the analogy between the epistemological

[193]Ref. 1-3 (Jauch, 1968, p. 77).

[194]Ref. 173 (1968, Chapter 6, pp. 162–201).

[195]D. Finkelstein, "Matter, space and logic," *Boston Studies in the Philosophy of Science* **5**, 199–215 (1969).

[196]H. Putnam, "Is logic empirical?" *Boston Studies in the Philosophy of Science* **5**, 216–241 (1969). Cf. R. Butnick's criticism of this paper, "Putnam's revolution," *Philosophy of Science* **38**, 290–292 (1971). Contrary to Butnick, Jeffrey Bub recently expressed his unreserved endorsement of Putnam's paper, which, in Bub's words, aroused him from his dogmatic slumbers. It prompted Bub to reformulate the completeness problem for statistical theories as

situation in logic and the epistemological situation in geometry as a perfect one," he proclaimed in his Boston Colloquium talk.[197] To meet the objection that in contrast to geometrical notions (e.g., "straight line") which have an operational meaning (geodesic or path of light ray) logical notions such as the connectives "and" or "or" have no such meaning, Putnam referred to the work of Finkelstein[198] in which the partial order of experimental propositions through the relation of implication[199] and with it the logical connectives are defined on the basis of operational "tests" (filters). Just as the renunciation of Euclidean geometry enabled Einstein to get rid of macroscopic anomalies such as universal forces, Putnam contended, so microphysical anomalies can be dissolved as soon as the distributive law of classical logic is given up. This he illustrated by an analysis of "complementarity cases" such as the double-slit experiment and the potential barrier paradox.

Moreover, what is usually referred to as "quantum mechanical indeterminism" also has in Putnam's view a simple explanation in the abandonment of distributivity. If, for example, $q_1,q_2,\ldots,q_n$ are the possible positions of a particle and $p_1,p_2,\ldots,p_n$ its possible momenta, then, although $q_1 \cup q_2 \cup \ldots \cup q_n$ and $p_1 \cup p_2 \cup \ldots \cup p_n$ are valid statements in quantum logic, no conjunction $q_i \cap p_j$ is valid. If I know the value of q at t_0, Putnam argued, I can deduce the position q at $t(>t_0)$ for there exists a unitary transformation $q(t_0) \rightarrow q(t) = Uq(t_0)$. But why can I not predict the out-

a problem of demonstrating a certain isomorphism between two algebraic structures inherent in the theory. He distinguished between a *statistical* interpretation of the role of the Hilbert space, which takes this space as the space of statistical states, and a *logical* interpretation, which regards the partial Boolean algebra of the subspaces of the Hilbert space as the algebraic structure of the idempotent magnitudes (to the ranges of possible values of which the probabilities are assigned). Bub argued that the statistical interpretation, which in the case of quantum mechanics does not lead to the required isomorphism and implies therefore that quantum mechanics is incomplete, has no other motive than the prejudice that the Boolean character of logic is a priori; however, if this a priori status of logic is denied, "a *realist* interpretation of quantum mechanics as a complete theory demands the *logical* interpretation of Hilbert space." Bub claimed to have shown that the completeness of quantum mechanics follows, once the assumption is rejected that logic is a priori and Boolean. See J. Bub, "On the completeness of quantum mechanics," in *Contemporary Research in the Foundations and Philosophy of Quantum Theory* (Proceedings of a conference held at the University of Western Ontario, London, Canada), C. A. Hooker, ed. (Reidel, Dordrecht, Holland, 1973), pp. 1–65.

[197]*Ibid.* (p. 234).

[198]D. Finkelstein, "The logic of quantum physics," *Transactions of the New York Academy of Sciences* **25**, 621–637 (1962-1963); "The physics of logic," International Centre for Theoretical Physics, Trieste, IC/68/35 (1968).

[199]It is assumed that to every physical property P_i there corresponds a "test" (filter) T such that if a system has P_i it "passes" T_i. The operational definition of the implication $P_i \leqslant P_j$ then amounts to showing that every system of a large sample taken from a population supposed to have P_i "passes" test T_j.

come of a measurement of p at t? The answer is that $q(t_0)$ is incompatible with $p_j(t_0)$ for all j although $q(t_0)$ is *not* incompatible with $(p_1(t_0) \cup p_2(t_0) \cup \ldots \cup p_n(t_0))$. In short, although at any time a particle, even of known position, *has* a momentum, the inability to predict its value at t is due to the ignorance of its value at t_0. "Quantum mechanics," Putnam concluded, "is more deterministic than indeterministic in that all inability to predict is due to ignorance."[200] Indeterminacy, in his view, arises not because the laws are indeterministic but because the states themselves, although logically strongest factual statements, do not contain the answers to all physically meaningful questions.

From the viewpoint of quantum logic as understood by Finkelstein and Putnam "probability" and "disturbance by measurement" have nothing to do with quantum mechanics but exist there on the same grounds and to the same extent as in classical physics. The interpretation, for instance, which claims that a momentum measurement "brings into being" the resulting value of p is based, they claimed, on the erroneous assumption that, if for a particle the position, say, q_i is known, $q_i \cap (p_1 \cup p_2 \cup \ldots \cup p_n)$ is false since all $q_i \cap p_j$ are false; in other words, if a particle has a definite position, it has no momentum and hence the measuring process must have "produced" it. The inference from the falsity of $(q_i \cap p_1) \cup (q_i \cap p_2) \cup \ldots \cup (q_i \cap p_n)$ to the falsity of $q_i \cap (p_1 \cup p_2 \cup \ldots \cup p_n)$, and hence the idea that the measurement "produces" some p_k has its origin in the illegitimate assumption of distributivity.

When in October 1968 at the Pittburgh biennial meeting of the Philosophy of Science Association Putnam gave a lecture on his ideas he challenged his audience to find out whether his reasoning and argumentation in support of the principles of quantum logic had been consistently presented. Patrick Heelan, then of Fordham University, who was in the audience, accepted the challenge and tried to refute Putnam by Putnam.[201] Putnam built his argument, just as Reichenbach had done before, along the following lines: (1) either quantum logic is correct or classical logic is correct; (2) classical logic is false (if hidden variables, the "Copenhagen double-think," and similar conceptions are rejected); (3) hence quantum logic is correct. The scheme of inference used by Putnam was therefore the following:

$$\begin{array}{c} p \cup q \\ \underline{\sim q} \\ p \end{array} \tag{5}$$

[200] Ref. 196 (p. 230).

[201] Ref. 48.

This scheme or mode of inference, as Heelan pointed out, is, however, precisely one of those that are invalid in quantum logic. To make his point clear Heelan interpreted p as the proposition "the electron has spin up" and q as the proposition "the electron has spin horizontally to the left"; then the superposition $p \cup q$ spans the entire spin space and hence is certainly true; but $\sim q$ implies that the spin is horizontally to the right and not up as would have followed from classical logic. Since this kind of *modus tollens* thus breaks down in quantum logic, Heelan claimed, Putnam has based his argument for quantum logic on an inference which his very quantum logic denies is valid.

Heelan also charged that the approach proposed by Finkelstein and Putnam suffers from ambiguities since it is based not on categorical propositions but rather on subjunctive conditionals of the form "if a certain test were to be made, then the system would pass it." The "logic of nature" which quantum logic seeks to reveal must be based on empirical propositions of a categorical kind and not on counterfactual conditionals, Heelan declared. Heelan's own view of the problem can be summarized by his statement: "I do not deny, however, that a special quantum logic has a place within quantum physics but I put it on the level of a meta-context-language about the conditions under which particular quantum event-languages are applicable, and not, as the writers on quantum logic are accustomed to put it, on the level of quantum event-language itself."[202]

Whereas Finkelstein and Putnam derived the new logic essentially from quantum mechanics,[203] Watanabe, whose ultimate point of departure was information theory rather than physics or philosophy, derived quantum mechanics from the new logic the nature of which he claimed to establish on nonquantal considerations. Michael Satosi Watanabe, a graduate of Tokyo University (Ph.D., 1933) and Docteur es Sciences Physiques of the Sorbonne (D.Sc., 1935), was from 1937 to 1939 a postdoctoral fellow at the University of Leipzig, joined Wayne State University in 1940, subsequently joined the IBM Research Laboratories at Yorktown Heights, New York, and after teaching at Yale and Columbia universities became in 1966 a member of the faculty at tthe University of Hawaii. Influenced by Husimi, a very close friend of his and classmate from the first year of high school to the last year of university, who, as mentioned, had tried to establish quantum logic on the basis of empirical facts in atomic physics without appealing to the elaborate formalism of quantum mechanics, Watanabe searched for arguments as general as possible to vindicate nondistributive logic. As early as 1948, in an article on non-Boolean logic published in a

[202]*Ibid.* (p. 9).

[203]Cf. Ref. 196 (1969, Chapter 5 "The quantum mechanical view of the world").

Japanese handbook of philosophy,[204] he maintained that this logic has a domain of application beyond atomic physics. In a series of lectures on quantum logic delivered in 1956 at the IBM Laboratories and in the fall semester of 1959–1960 at Yale Graduate School of Physics he elaborated[205] these ideas and introduced the function $f(A/a)$, which, as we shall presently see, plays a primary role in his theory. In 1959[206] and in 1960[207] he even proposed applying the new logic to the mind-body problem.

In his Yale lectures on physical information theory Watanabe pointed out that the isomorphism between conventional logic and Boolean lattices is a consequence of the fact that the laws of the usual logic can be derived from the characteristic function $f(A/a)$, which has the value 1 if the object a belongs to the class A—this class being understood as the extension of a predicate p_A, that is, $A = \{x|x$ has predicate $p_A\}$—and the value 0 if the object a does not belong to A. If it is assumed that each predicate corresponds one-to-one, at each time point, to a well-defined (fixed) set of objects that satisfy the predicate, an assumption which Watanabe called "the postulate of definite (or fixed) truth set" and more recently[208] "the Frege Principle"[209] then the values 1 and 0 of f are unambiguously determined; if so, the propositional calculus can be reduced to the axioms of set theory and thus becomes automatically Boolean.

Whereas Finkelstein and Putnam dated the breach or "fracture" of classical logic as having been initiated by Bohr's introduction of complementarity and fully exposed by the discovery, by Birkhoff and von Neumann, of the nondistributivity of the quantum mechanical proposi-

[204] *Handbook of Philosophy* (Kawade Shobo, Tokyo, 1950) (In Japanese).

[205] The contents of the Yale lectures, which first appeared in mimeographed form, was published in S. Watanabe, "Algebra of observation," *Supplement of the Progress of Theoretical Physics, Nos. 37 & 38*, 350–367 (1966), and in amplified version in S. Watanabe, *Knowing and Guessing* (Wiley, New York, London, 1969), Chapters 7 and 9.

[206] S. Watanabe, "Comments on key issues," in *Dimensions of Mind*, S. Hook, ed. (New York University Press, New York, 1960), pp. 143–147.

[207] S. Watanabe, "A model of mind-body relation in terms of modular logic," *Synthese* **13**, 261–301 (1961).

[208] S. Watanabe, "Logic, probability and complementarity," presented at the Special Seminar in commemoration of the Fiftieth Anniversary of the Niels Bohr Institute.

[209] Watanabe named this assumption after Frege because the German mathematician and philosopher Gottlob Frege was the first to discuss (and to question) this assumption: "*Oder sollen wir annehmen, es gebe Fälle, wo einem unanfechtbaren Begriffe keine Klasse entspreche, die sein Umfang wäre*?" [Or must we suppose there are cases where an unexceptionable concept has no class answering to it as its extension?] G. Frege, *Grundgesetze der Arithmetik* (H. Pohle, Jena, 1893-1903; G. Olms, Hildesheim, 1962) Vol. 2, p. 254; *Translations from the Philosophical Writings of Gottlob Frege*, translated by P. Geach and M. Black (Blackwell, Oxford, 1952), p. 235.

tional calculus, according to Watanabe the Frege Principle, and with it Boolean logic, already suffered a breakdown at the time of Galileo when primary qualities such as bulk, figure, or motion, assumed to be independent of the observer, were confronted with secondary qualities such as color, taste, or smell which, in the words of John Locke, are "nothing in the objects themselves, but powers to produce various sensations in us by their primary qualities."[210] In this case, Watanabe contended, the characteristic function depends not only on a and A but also on a third argument x, the observer. This challenge, however, was not very serious since by specifying precise conditions about x or by distinguishing classes with different values of x as different classes the Frege Principle was easily salvaged. The Frege Principle was later also challenged by Bertrand Russell's well-known paradox concerning the set S of all sets that do not include themselves as members. No well-defined set of objects seemed to correspond to the predicate "not self-including"; for, to be well-defined, such a set of objects has either to include S or not, but either alternative leads to a contradiction. However, again, Russell's own "theory of types," developed by F. P. Ramsey and W. V. Quine, or other solutions (E. Zermelo, A. Fraenkel, J. von Neumann) rescued the Principle.

When science was faced subsequently with situations in which object and observer mutually interact to such an extent that the act of observation leads to an uncontrollable disturbance on the object, as in many psychological tests or in quantum mechanics, the Frege Principle could no longer be maintained. In fact, the most devastating blow to the Principle, Watanabe admitted, was brought about by quantum mechanics. In this case the reconstruction of logic and probability theory can be based on the so-called Peirce Principle,[211] according to which implication is the most fundamental operation of human reasoning. As Watanabe explained in full detail, from the notion of implication ($\rightarrow$) conjunction and disjunction as well as all the laws of a nondistributive lattice can be derived. Thus $a \cap b$, for example, was defined by Watanabe as the element c which satisfies (1) $c \rightarrow a$ and $c \rightarrow b$ and (2) if $x \rightarrow a$ and $x \rightarrow b$, then $x \rightarrow c$. But our inferences and decisions in ordinary life, Watanabe continued, are usually probabilistic; for one very seldom knows with absolute certainty that if a, then b. Watanabe was thus led to conclude that probability (more precisely, conditional probability) precedes logic. In fact, if $f(A/a)$ is allowed to vary continuously over the closed interval $[0,1]$, implication may be defined by

[210] J. Locke, *Essay concerning Human Understanding* (London, 1690).

[211] So called because C. S. Peirce once wrote "I have maintained since 1867 that there is one primary and fundamental logical relation, that is illation...". Cf. C. S. Peirce, *Collected Papers*, Vol. 3 (Harvard University Press, Cambridge, Mass., 1933, 1960), p. 279.

the following considerations. The *product predicate* $C = BA$ is the predicate which is true [i.e., $f(C/a)=1$] if and only if A and B turn out to be true when B is tested immediately after A has been tested; A is *simple* if $f(AA/a)=f(A/a)$ for all a; and two simple predicates A and B are *compatible* if $f(AB/a)=f(BA/a)$ for all a. Now A *implies* $B(A \to B)$ if A and B are simple and $f(AB/a)=f(BA/a)=f(A/a)$ for all a. The implication thus defined is transitive and reflexive.

In accordance with the preceding derivation of the conjunction from implication Watanabe concluded that $A \cap B$ is equivalent to the infinite product $\ldots ABAB\ldots$; hence, after introducing the predicate 1 which is implied by all possible predicates and the predicate 0 which implies all possible predicates, Watanabe was in the position to define disjunction via the De Morgan rule. The lattice L thus derived from the function $f(A/a)$ turned out in general to be nondistributive. But if in a particular case all predicates are compatible, then $A \cap B$ reduces to $AB = BA$(A and B are simple) and the lattice L is Boolean. Having thus shown that the new logic, if restricted to certain domains, reduces to the usual Boolean logic, Watanabe, admitting that there exists a domain of inference where the usual logic remains valid, could consistently assign to the usual logic the role of a "metalogic" in terms of which the new logic may be explained and "handled,"[212] thereby avoiding the trap by which Heelan ensnared Putnam.

Under the compatibility assumption the equation $f(A \cap B/a)+f(A \cup B/a)=f(A/a)+f(B/a)$ together with $f(0/a)=1-f(1/a)=0$, valid for all a, shows, as Watanabe pointed out, that f satisfied the conditions of a probability function (a normalized, nonnegative measure on a σ-algebra), and any values of f between 0 and 1 can be explained as referring to a mixture of objects whose f-values are either 0 or 1. In the absence of the compatibility assumption these conclusions are no longer valid. Postulating that in this case there exists at least one object g such that $f(A \cap B/g) + f(A \cup B/g)=f(A/g)+f(B/g)$ and that if $A \to B$ but not $B \to A$, $f(A/g) < f(B/g)$, Watanabe proved with the help of a theorem due to Dedekind that the lattice is modular; finally, by replacing predicates A by projection operators P_A in a (finite-dimensional) Hilbert space whose subspaces, as is well known, form a modular lattice, and by replacing objects a by density matrices $Z(a)=\sum w_i P_i$ (states)[213] Watanabe derived the equation $f(A/a) = \mathrm{Tr}[P_A \cdot Z(a)]$, which is von Neumann's statistical formula.

[212]Ref. 205 (1969, p. 450).

[213]For details see Ref. 205 (1969) and S. Watanabe, "Modified concepts of logic, probability, and information based on generalized continuous characteristic function," *Information and Control* **15**, 1–21 (1969).

In concluding our outline of Watanabe's construction of quantum mechanics, which was based, as we have seen, on the renunciation of the universal validity of the Frege Principle, it should be remarked that this point of departure, leading to a continuous generalization of the characteristic function, had been adopted also by Lotfi A. Zadeh[214] in his development of what he called a theory of "fuzzy states." Concerning Watanabe's conception of logic, which to some writers seemed not clearly defined[215], it should be emphasized that he viewed logic as being essentially empirical,[216] provided "empirical" is understood to include "what the living beings have learned during the long years of evolution."[217]

8.7. GENERALIZATIONS

Whereas the earlier quantum logicians concentrated, generally speaking, on the analysis of the logical structures of classical physics and quantum mechanics, their similarities and differences, the more recent investigators tried to construct, within a unified conceptual framework, a general theory of physics which comprises classical as well as quantum mechanics, explains why certain phenomena are subject to only one of these "subtheories," and provides plausible reasons for the emergence of one from the other. Investigations toward this end have been, and are being, carried out by different schools of quantum logicians. Constantin Piron[218] proposed such a formalism in a series of lectures given in the fall of 1970 at the University of Denver.

A philosophically more ambitious attempt at deriving a unified physics from the very preconditions of experience has been undertaken by C. F. von Weizsäcker.[219] Admitting with Plato and Hume that experience alone can never establish strict laws von Weizsäcker contended that the only justification of laws by experience lies in the Kantian approach of regard-

[214]L. A. Zadeh, "Fuzzy sets," *Information and Control* **8**, 338–353 (1965).

[215]See, e.g., Ref. 48 (reference 2, p. 32).

[216]Cf. S. Watanabe, "Logic of the empirical world," lecture delivered at the International Conference on Philosophical Problems in Psychology, Honolulu, Hawaii, 1968 (unpublished).

[217]Letter from Watanabe to author, dated February 6, 1973. In this letter Watanabe espoused Quine's "naturalistic" viewpoint as expressed in W. V. Quine, *Ontological Relativity and Other Essays* (Columbia University Press, New York, London, 1969), pp. 114–138.

[218]Ref. 7-107.

[219]C. F. von Weizsäcker, "The unity of physics," in *Quantum Theory and Beyond*, T. Bastin, ed. (Cambridge University Press, London, New York, 1971), pp. 229–262; "Die Einheit der Physik" (esp. section 5: Die Quantentheorie) in C. F. von Weizsäcker, *Die Einheit der Natur* (Carl Hanser Verlag, Munich, 1971), pp. 129–275.

ing general laws as formulations of the conditions under which experience becomes possible. Von Weizsäcker tried therefore to show that the very preconditions of experience lead cogently to the logical foundations of a unified physics. Time, in his view the most general presupposition of experience—for the essence of experience is "to learn from the past for the future"—leads to the logic of temporal propositions (tense-logic) and the analysis of the latter provides the conceptual framework of quantum logic and the theory of objective probability which are merely special formulations of the tense-logic, namely of temporal decidable alternatives. Quantum mechanics, which provides the laws of motion of all possible objects, is thus according to von Weizsäcker the general basis of all physics and classical physics results from it via the introduction of irreversibility, which is a precondition of measurement and hence of the semantics of quantum mechanics. For classical physics "simply describes the approximation to quantum theory appropriate to objects as far as they really can be fully observed." Bohr's insistence that quantum mechanics becomes semantically meaningful only in terms of classical physics thus turns out to be a truism.

As von Weizsäcker emphasized, the full elaboration of this program has not yet been achieved. In particular, basic concepts of physics such as "state" or "change," indispensable for a full reconstruction of quantum mechanics, have not yet been derived from an analysis of the preconditions of experience. As a temporary makeshift to supplant the missing, one may take recourse in the axiomatic method and set up a system of axioms which suffice for the derivation of ordinary quantum mechanics and, at the same time, admit a simple interpretation in terms of the concepts obtained from an analysis of the preconditions of experience. With certain restrictions (finistic quantum theory, formulated for a finite-dimensional Hilbert space) such a construction of quantum mechanics from axioms "that specify nothing further than the preconditions for the possibility of any physical science" has been carried out by von Weizsäcker's pupil Michael Drieschner.[220]

A no less comprehensive project of reconstructing quantum mechanics on the basis of a general methodology of physical theory has been launched by Günther Ludwig and his collaborators at the University of Marburg (G. Dähn, K. E. Hellwig, R. Kanthack, H. Neumann, P. Stolz,

[220]M. Drieschner, "Quantum mechanics as a general theory of objective prediction," thesis, University of Hamburg and Max-Planck-Institute, Munich, mimeographed preprint, 1969. "The structure of quantum mechanics: Suggestions for a Unified Physics," in *Foundations of Quantum Mechanics and Ordered Linear Spaces*, A. Hartkämper and H. Neumann, eds. (Lecture Notes in Physics 29) (Springer-Verlag, Berlin, Heidelberg, New York, 1974), pp. 250–259.

and others). Ludwig, a Ph. D. (1943) of the University of Berlin, became professor of theoretical physics at Berlin's Freie Universität in 1949 and joined the faculty of Marburg in 1963. Starting with what seemed a rather modest task—revising his well-known text[221] on the foundations of quantum mechanics—Ludwig soon found himself confronted by a host of serious methodological problems.[222] He realized that a clear comprehension of quantum mechanics is possible only if it has been decided beforehand "what should properly be considered a physical theory and what a well-established construction of such a theory should look like." Among his own predecisions—which, as he repeatedly emphasized, nobody is obliged to accept—he listed his insistence on the exclusive use of classical logic, his restriction to objective data, and his refusal to admit statements involving "perceptions," "knowledge," or "content of consciousness." Even the concept of "prediction" which plays such an important role in von Weizsäcker's approach, is here regarded as inadmissible on the grounds that, "drastically speaking, one should consider predicting the concern of prophets or engineers, because engineers are supposed to construct devices that will function in a predictable way. To make predictions is not the matter of physics and, insisting on the strict sense of the word, they do not even occur in physics."[223]

Ludwig's approach is macrophysicalistic insofar as all knowledge about quantum mechanical objects is obtained from the behavior of macrophysical devices, for only objective data can, in his view, serve as a basis and point of departure for the theory. This implies, Ludwig continued, that quantum mechanics itself is not the most fundamental or most comprehensive theory in physics—a welcome conclusion, he added, for it is hard to believe that the quantum theory of a system of more than 10^{20} atoms could be the actual theory of, say, a table. The problem he now has to solve is to show that in spite of the reduction of quantum mechanics to the behavior of macroscopic systems, the latter can consistently be regarded as

[221]G. Ludwig, *Die Grundlagen der Quantenmechanik* (Springer, Berlin, Göttingen, Heidelberg, 1954).

[222]G. Ludwig, *Deutung des Begriffs "physikalische Theorie" und axiomatische Grundlegung der Hilbertraumstruktur der Quantenmechanik durch Hauptsätze des Messens* (Lecture Notes in Physics 4, Springer, Berlin, Heidelberg, New York, 1970), reviewed by K. Baumann in *Acta Physica Austriaca* **35**, 163–164 (1972); *The Measuring Process and an Axiomatic Foundations of Quantum Mechanics* (Notes in Mathematical Physics 3, mimeographed, Marburg, 1971); *Makroskopische Systeme und Quantenmechanik (ibid.*, 4, 1942); *Mess- und Präparierprozesse (ibid.*, 6, 1972); *Das Problem der Wahrscheinlichkeit und das Problem der Anerkennung einer physikalischen Theorie (ibid.*, 7, to appear); *Das Problem der Wirklichkeit der Mikroobjekte (ibid.*, **8** to appear.)

[223]Ref. 222 (1971, pp. 11–12).

being composed of atoms in precisely the sense in which the so far only vague conceptions have expressed this relationship. In his axiomatic exposition he classified macroscopic objects into preparative [*Präparierteile*] and effective [*Effektteile*] parts, such as a piece of uranium (preparative) and a cloud chamber (effective); both parts participate in the possible interactions which are described by the "theorems of measurement" [*Hauptsätze des Messens*] and take place by means of physically real "action carriers" [*Wirkungsträger*], alpha-particles in the example considered; what "physically real" means is defined by the syntactic rules of the theory itself.[224] The action carriers are divided into classes each of which, characterized by a different value of a superselection rule, is described by a Hilbert space.

A further analysis of the interaction modes between the macroscopic parts led Ludwig to the distinction between composite systems and elementary systems and to the conclusion that to the latter systems position and momentum observables can be uniquely ascribed which are subject to the Heisenberg indeterminacy relations. Thus, by means of a cleverly constructed intertheory relation Ludwig claimed to have solved the foregoing problem. In view of the fact that Ludwig's work, like that of von Weizsäcker, is still far from completed the preceding remarks are not intended to give even only a rough outline of these developments but rather to draw the reader's attention to what, it is hoped, will turn out to be important achievements in the philosophy of modern physics.

In the present chapter two kinds of quantum logic have been discussed, the many-valued quantum logic such as that proposed by Reichenbach, and the bivalent quantum logic such as that developed by Birkhoff and von Neumann, by Mackey, or by Watanabe. What, precisely, is the relation between these two approaches? To answer this question let us consider again the basic element of either approach, the quantum mechanical proposition p: "the value of an observable $\mathfrak{A}$ on a given physical system in the state φ is a." If $A\varphi = a\varphi$, where A denotes the self-adjoint operator associated with $\mathfrak{A}$, then the proposition p is *true*. On this point both approaches agree. If $A\varphi = a\varphi$ does not hold or, equivalently, if the proposition p is not true, two possibilities exist:

1. There exists a value a', other than a, such that $A\varphi = a'\varphi$.
2. No value x exists such that $A\varphi = x\varphi$.

It is possibility 2 by which quantum physics differs from classical physics since in classical physics every observable always has a definite value.

[224]Ref. 222 (1970, Chapter 2, section 10).

Now if we define p as being *false* if and only if the negation of p is true, we have to decide what we mean by negating (or denying) p: (α) is the denial of p equivalent to the assertion of (1), or (β) is the denial of p equivalent to the assertion of either (1) or (2). If we adopt (α) and construe the denial within the context of a definite set of alternatives, we conceive the negation of one of these alternatives as being tantamount to the assertion that one of the remaining alternatives or possibilities is realized; in our case, to deny $A\varphi = a\varphi$ means to assert that $A\varphi = x\varphi$ holds for an x that differs from a. Following the terminology of the Dutch philosopher of mathematics Gerrit Mannoury, we call this the *choice negation* [*keuzenegatie*, *négation de choix*].[225] If, however, we adopt (β), conceiving the denial of p as being true whenever p fails to be true, we embrace what Mannoury called the *exclusion negation* [*uitsluitingsnegatie*, *négation exclusive*]. It is only on the basis of choice negation that "not true" differs from "false" and thus leaves room for additional truth values. We thus realize that the bivalent systems of quantum logic were rooted in the exclusion negation, whereas the many-valued systems of quantum logic were based on the choice negation. If we recall that already in ancient Greek, the ultimate source of all philosophy, both kinds of negation (*οὐ* for choice negation, *μή* for exclusion negation) had been commonly used and that the distinction between them played an important role in medieval logic (Pierre Abelard, Muslim logicians) and, more recently, in the development of Intuitionism, their reappearance at the foundations of modern quantum logic should not take us by surprise.

Just as there are two schools among quantum logicians which differ on the issue of whether to formulate quantum logic bivalently or multivalently, so there are, roughly speaking, two opposing views concerning the role which quantum logic has to play methodologically. According to the more radical school the non-Boolean logic, qua empirical logic, plays the role of an explanatory principle in physics; the ultimate significance of the conceptual revolution brought about by quantum mechanics lies in the "emancipation" of logic from the status of an a priori and purely formal discipline to that of an empirically significant explicans. It is this idea that Finkelstein and Putnam had in mind when they pointed to the analogous development of geometry which in classical physics had an a priori status and became in general relativity an empirically manifested explanatory

[225]G. Mannoury, *Woord en Gedachte* (P. Noordhoff, Groningen, 1931), p. 55. *Les Fondements Psycho-Linguistiques des Mathématiques* (Editions du Griffon, Neuchatel, 1947), pp. 45–54. For a more detailed classification, based on the alternative between these two kinds of negation and on the alternative between two different characterizations of sentential connectives, of some major approaches to quantum logic see B. C. van Fraassen, "The labyrinth of quantum logic," Ref. 182.

principle of large-scale space-time phenomena.

The validity of this analogy is disputed by the opposing school of quantum logicians for the following reason: We can formulate a system of geometry without the use of geometrical principles, but we cannot formulate a system of logic without the use of logical principles; and since the prescientific, or metascientific, use of logical principles is always based on ordinary logic, the fundamental role of logic never changes. The proponents of this more conservative conception, such as Jauch or Piron, could point out that C. F. von Weizsäcker when developing his complex-valued quantum logic admitted that "the many-valued logic is no longer a real logic,"[226] but rather "a mathematical calculus for the interpretation of which we presuppose the two-valued logic," and that even Finkelstein, in constructing his operational definitions of the quantum logical connectives, acknowledged this difficulty when he declared, "We are in a delicate position, using logic to study the need for changing logic."[227]

In the view of the conservative school of quantum logicians nonstandard logic plays the role of an analytical formalism to describe the structure of the empirically given quantum mechanical propositional calculus; what it explains are not physical facts but the relation of their mathematical formulation to experimental propositions. In its attempt to clarify the logical relation between classical physics and quantum mechanics and to explain, in particular, the need of formulating the latter in terms of an operator calculus in Hilbert space, quantum logic, according to this school of thought, has produced insights that have substantially added to our understanding of the foundations of this theory.

[226]"*Die mehrwertige Logik ist nicht mehr eigentliche Logik.*" Letter from von Weizsäcker to Arnold Gehlen, dated August 28, 1942, published in *Die Tatwelt* **18**, 107 (1942).

[227]Ref. 198 (p. 622). More severe criticisms, directed also against the conservative quantum logicians, have recently been voiced by E. B. Davies, "Example related to the foundations of quantum theory," *Journal of Mathematical Physics* **13**, 39–41 (1972), where it is claimed "that the structure of quantum mechanics cannot be deduced from purely philosophical discussions of its statistical nature, and also that present arguments are a long way from reducing quantum theory to any collection of physically meaningful principles." To prove his point Davies constructed a rather simple example of a stastistical system which satisfies the axioms used by various quantum logicians but which cannot be described in terms of any Hilbert space or Jordan algebra.

STOCHASTIC Interpretations

Chapter Nine

9.1. FORMAL ANALOGIES

The main objective of the stochastic interpretations of quantum mechanics has been to show that quantum theory is fundamentally a classical theory of probabilistic or stochastic processes and as such conceptually of the same structure as, say, the Einstein-von Smoluchowski theory of Brownian motion, involving Markov processes in coordinate space, or its later refinements such as the Ornstein-Uhlenbeck theory involving Markov processes in phase space, and that consequently the radical departure from the conceptual framework of classical physics, such as Bohr's complementarity doctrine, was unnecessary and misleading. In support of this thesis it has been pointed out that, for example, the classical theory of density fluctuations explained successfully a vast multitude of physical and physicochemical phenomena in areas so diverse as colloid chemistry and stellar dynamics—and this without deviating at all from the ontology of classical physics. The immediate incentive for the interest in stochastic interpretations was, however, the conspicuous similarity between the Schrödinger equation and equations in the theory of diffusion processes or Brownian motion.

One of the first to draw attention to such similarities was Schrödinger himself. In a paper presented on March 12, 1931, to the Berlin Academy[1] he compared his wave equation with the diffusion equation $D\partial^2 w/\partial x^2 = \partial w/\partial t$ where $w(x,t)$ is the concentration or probability density of particles and D is the diffusion constant. Studying the problem of finding the distribution probability at a time t, with $t_1 \leqslant t \leqslant t_2$, if $w(x,t_1)$ and $w(x,t_2)$ are known, he showed that the solution is the product of two factors, in striking analogy to the quantum mechanical expression $\psi^*\psi$ for the probability density. In a lecture[2] delivered in May 1931 at the Institut Henri Poincaré in Paris Schrödinger discussed in further detail this "analogie superficielle qui existe entre cette théorie de probabilité classique et la mécanique ondulatoire," which, as he added, "n'a probablement échappé à aucun physicien qui les connaît toutes les deux."

Schrödinger was certainly one of the few who were equally familiar with both theories. He had studied stochastic problems in classical physics when he was still a student of Franz Exner and published a paper[3] on Brownian

[1]E. Schrödinger, "Über die Umkehrung der Naturgesetze," *Berliner Sitzungsberichte* **1931**, 144–153.

[2]E. Schrödinger, "Sur la théorie relativiste de l'électron et l'interpretation de la mécanique quantique," *Annales de l'Institut Henri Poincaré* **2**, 269–310 (1932).

[3]E. Schrödinger, "Zur Theorie der Fall- und Steigversuche an Teilchen mit Brownscher Bewegung," *Physikalische Zeitschrift* **16**, 289–295 (1915).

motion when he got involved in the issue concerning the existence of subelectronic charges which was raised by Felix Ehrenhaft, the discoverer of Brownian motion in gases. And yet, the analogies if compared with the disparities (reality of w versus complexity of ψ; in classical physics the probability density itself, in quantum theory only the probability amplitude, are subject to a differential equation, etc.) were, in Schrödinger's view, not convincing enough to persuade him to an espousal of a stochastic interpretation of quantum mechanics. Nor did his short-time collaborator in Paris, Jacques Metadier,[4] draw any such conclusions.

That not only the Schrödinger equation, which for a free particle can be written (we "dot" time derivatives and "dash" space derivatives) in the one-dimensional case

$$\dot{\psi} = \varepsilon\psi'' \qquad \left(\varepsilon = \frac{ih}{4\pi m}\right) \tag{1}$$

has a stochastic analog, namely the diffusion equation (or Fokker-Planck equation with vanishing convection current)

$$\dot{w} = Dw'' \tag{2}$$

(where the diffusion coefficient D is real), but that an analog also exists for the Heisenberg relations between position and momentum, which, as we have seen, was often regarded as the characteristic feature of quantum mechanics, was convincingly demonstrated in 1933 by Reinhold Fürth. As a student under Anton Lampa in Prague (where he later, in 1927, became professor of physics) Fürth became deeply impressed by the work of Maryan von Smoluchowski, who, in his view, was "the first who recognized the importance of statistical methods in physics as forming the link between our macro-world and the microcosmos of molecules and atoms."[5] Fürth later edited von Smoluchowski's works (1923), wrote the article on the principles of statistics for the *Handbuch* (1929), and became an acknowledged authority on fluctuations and diffusion processes.[6]

In his demonstration[7] of the stochastic analog of the Heisenberg relation

[4] J. Métadier, "Sur l'équation générale du mouvement brownien," *Comptes Rendus* **193**, 1173–1176 (1931).

[5] R. Fürth, *Schwankungserscheinungen in der Physik* (dedicated to the memory of M. von Smoluchowski) (Vieweg, Braunschweig, 1920), Preface.

[6] His investigations, too numerous to be listed here, were published between 1917 and 1930 in *Physikalische Zeitung, Zeitschrift für Physik,* and *Zeitschrift für physikalische Chemie.*

[7] R. Fürth, "Über einige Beziehungen zwischen klassischer Statistik und Quantenmechanik," *Zeitschrift für Physik* **81**, 143–162 (1933).

Fürth first derived the relation statistically as follows. If x_0 is the initial position of a particle in an ensemble and v its initial velocity, then its position at time t is

$$x = x_0 + vt. \tag{3}$$

The mean square value $\overline{x^2} = \alpha$ is therefore

$$\alpha = \overline{x_0{}^2} + 2\,\overline{x_0 vt} + \overline{v^2}\,t^2. \tag{4}$$

From

$$\alpha = \int x^2 \psi\psi^* dx, \tag{5}$$

by virtue of (1) and its complex conjugate, repeated partial integration yields

$$\frac{d^2\alpha}{dt^2} = -8\varepsilon^2 \int \psi' \psi^{*\prime} dx \qquad \text{and} \qquad \frac{d^3\alpha}{dt^3} = 0, \tag{6}$$

which shows that α is a quadratic function of t, the coefficient $\overline{v^2}$ of t^2 being

$$\overline{v^2} = \frac{1}{2}\,\frac{d^2\alpha}{dt^2} = -4\varepsilon^2 \int |\psi'|^2 dx. \tag{7}$$

From the obvious inequality

$$\left|\frac{x}{2\alpha}\psi + \psi'\right|^2 \geqslant 0 \tag{8}$$

and $\int |\psi|^2 dx = 1$ it follows that

$$\int |\psi'|^2 dx \geqslant \frac{1}{4\alpha} \tag{9}$$

and hence, by (7), that

$$\overline{x^2} \cdot \overline{v^2} \geqslant -\varepsilon^2. \tag{10}$$

Thus, with $\Delta x = \sqrt{\alpha}$ and $\Delta p = m\sqrt{\overline{v^2}}$, Fürth obtained the Heisenberg relation

$$\Delta x\, \Delta p \geqslant \frac{h}{4\pi} \tag{11}$$

In analogy to the preceding reasoning Fürth defined the indeterminacy of position for the diffusion process in terms of

$$\beta = \overline{x^2} = \int x^2 w \, dx \tag{12}$$

where w, normalized by $\int w \, dx = 1$, is a solution of the diffusion equation (2). Consequently,

$$\frac{d\beta}{dt} = 2D \tag{13}$$

and

$$\beta = x_0{}^2 + 2Dt. \tag{14}$$

Thus, contrary to (4), the indeterminacy increases linearly with time because the motion of each particle, instead of being endowed as before with an initial velocity, arises now because of erratic impacts of other particles. The diffusion current Q, that is, the quantity diffusing through a fixed unit area in unit time, given by

$$Q = -D \operatorname{grad} u, \tag{15}$$

was used by Fürth to define "velocity" by the equation

$$v = \frac{1}{u} Q = -\frac{D}{u} \frac{\partial u}{\partial x}. \tag{16}$$

Clearly,

$$\overline{v^2} = \int v^2 u \, dx = D^2 \int \frac{u'^2}{u} \, dx. \tag{17}$$

From the obvious inequality

$$\left(\frac{u'}{u} + \frac{x}{\beta} \right)^2 \geqslant 0 \tag{18}$$

and $\int u \, dx = 1$ Fürth obtained the relation

$$\int \frac{u'^2}{u} \, dx \geqslant \frac{1}{\beta} \tag{19}$$

and hence, by (17),

$$\overline{x^2} \, \overline{v^2} \geqslant D^2. \tag{20}$$

Now with Δx and Δv defined as before, Fürth obtained the diffusion-theoretic relation

$$\Delta x \Delta v \geqslant D \tag{21}$$

in analogy to (11). As he pointed out, however, in contrast to (11), where the lower bound is a universal constant originating in the disturbance produced by the measurement process itself, the lower bound in (21), associated with the randon agitation of the surrounding medium on the system under observation, can be made arbitrarily small, for example, by lowering the absolute temperature T which is a factor[8] in D.

The analogies discovered by Fürth encouraged further studies of possible relations between quantum mechanics and the theory of probability, which precisely at that time underwent the most important development in its history. In 1931 Richard von Mises published his influential treatise *Wahrscheinlichkeitslehre* with its emphasis on the frequency interpretation and, still more important, in 1933 there appeared Andrey Nikolajevich Kolmogorov's *Grundbegriffe der Wahrscheinlichkeitsrechnung* (Basic Concepts of Probability Theory), which, by presenting the theory as a measure theory over a Boolean σ-algebra, gave it a firm and rigorous foundation. Now it became possible to analyze with modern means collections of random variables or stochastic processes (in the narrow sense of the word) and, as was done by Kolmogorov, Doob, Khintchin, and others, to study, in particular, Markov processes, named for the Russian mathematician Andray Andrajevich Markov who was the first to realize their theoretical importance.

The search for an interpretation of quantum mechanics as a probabilistic theory in the classical sense found support and encouragement in the 1930s and 1940s in a mathematical result obtained by Eugene Wigner[9] which seemed to carry more weight than mere analogy considerations. In fact, it suggested the possibility of formulating quantum mechanics in terms of phase space ensembles.

To calculate the quantum correction to the second virial coefficient of a gas Wigner made use of a mathematical expression which, in collaboration

[8]As shown, e.g., by the well-known Einstein relation $D = BkT$. Cf. A. Einstein, "Die von der molekularkinetischen Theorie der Wärme geforderte Bewegung von in ruhenden Flüssigkeiten suspendierten Teilchen," *Annalen der Physik* **17**, 549–560 (1905); reprinted in A. Einstein, *Untersuchungen über die Theorie der Brownschen Bewegung*, R. Fürth, ed. (Oswalds Klassiker No. 199) (Akademische Verlagsgesellschaft, Leipzig, 1922); *Investigations on the Theory of the Brownian Movement* (Methuen, London, 1926; Dover, New York, 1956).

[9]E. Wigner, "On the quantum correction for thermodynamic equilibrium," *Physical Review* **40**, 749–759 (1932).

with Leo Szilard, he had found while still in Berlin working on an entirely different problem: it was a function of position and momentum variables, $f_w(q,p)$, which, when integrated over the momentum, yields the quantum mechanical probability distribution of position, $|\psi(q)|^2$, and when integrated over the position yields the corresponding probability distribution of momentum, $|\varphi(p)|^2$, and which, moreover, yields classically in many cases the correct expectation value of quantum mechanical observables, that is, if $a(q,p)$ is the classical function from which the quantum mechanical operator A is obtained, then $\int\int a(q,p)\, f_w(q,p)\,dq\,dp = \langle A \rangle$. This function is the so-called Wigner distribution

$$f_w(q,p) = (2\pi)^{-1} \int \psi^*\left(q - \frac{\hbar}{2}\alpha\right) \exp(-i\alpha p)\psi\left(q + \frac{\hbar}{2}\alpha\right) d\alpha \tag{22}$$

and has the marginal distributions

$$f_w\,dp = |\psi(q)|^2 \qquad \text{and} \qquad \int f_w\,dq = |\varphi(p)|^2 \tag{23}$$

where

$$\varphi(p) = (2\pi\hbar)^{-1/2} \int \exp\left(-\frac{iqp}{\hbar}\right)\psi(q)\,dq. \tag{24}$$

Wigner realized, of course, that f_w is not everywhere nonnegative and hence "cannot be really interpreted as the simultaneous probability for coordinates and momenta," though it may be used "in calculations as an auxiliary function which obeys many relations we would expect from such a probability." In fact, if used to compute expectation values of functions of either position only, or momentum only, or the sum of the two, it leads to the results obtained by the usual quantum mechanical methods.

It was natural to investigate whether these restrictions could be relaxed. This question and the more general problem of whether quantum processes could be described in terms of statistical averages over uniquely determined processes as in classical statistical mechanics were given special attention by Hilbrand Johannes Groenewold[10] of the University of Groningen in a comprehensive analysis of the principles of quantum mechanics. The possibility of reformulating quantum mechanics in purely statistical terms so that observables would be represented by random variables and the operators and wave functions of quantum mechanics, instead of having an intrinsic meaning, would merely serve as aids to the calculation of averages and distributions was studied by Jose E. Moyal[11] of

[10] H. J. Groenewold, "On the principles of quantum mechanics," *Physica* **12**, 405–460 (1946).

[11] J. E. Moyal, "Quantum mechanics as a statistical theory," *Proceedings of the Cambridge Philosophical Society* **45**, 99–124 (1949).

Queen's University in Belfast. His point of departure was the observation that the expansion of a given state ψ in terms of eigenfunctions ψ_i of a complete set S of commuting observables, $\psi = \Sigma a_i \psi_i$, provides directly the joint probability distribution of the eigenvalues of S since the corresponding probabilities are given by $|a_i|^2$. However, S alone does not specify the system completely; another complementary set, say R, is needed which, in general, does not commute with S. The argument that, because of the impossibility of measuring noncommuting observables simultaneously, joint distributions (analogous to phase-space distributions in classical statistical mechanics) do not exist, Moyal declared, is not conclusive for two reasons: (1) the impossibility of such measurements "does not preclude us from considering the proposition that there exists a well-defined probability for the two variables to take specified values or sets of values," and (2) it is possible in principle to form operators G corresponding to functions $G(r,s)$ of noncommuting observables, the expectation value of G in state ψ being given by $(\psi, G\psi)$; but, as was well known, the joint distribution of r and s can be reconstructed from such expectation values, for example, from the values of all joint moments $\overline{r^k s^n}$. Moyal thus concluded that "the formalism of quantum theory allows us therefore to derive the phase-space distributions indirectly *if a theory of functions of non-commuting observables is specified and conversely*."

To define these distributions unambiguously poses, however, serious difficulties whose nature Moyal illustrated by the example of the harmonic oscillator. Its position and momentum eigenfunctions (Hermite functions) are continuous functions of q and p, respectively, so that a joint distribution for q and p in a state consistent with the individual distributions $|\psi(q)|^2$ and $|\varphi(p)|^2$ (as marginal distributions) must extend continuously over the whole q,p-plane. Since, however, the energy eigenvalues $E_n = (n + \frac{1}{2})h\nu$ form a discrete set the joint distribution for the energy and phase angle will be concentrated on the set of ellipses $\frac{1}{2}(p^2/m + m\omega q^2) = E_n$. "We are thus forced," continued Moyal, "to the conclusion that phase-space distributions are not unique for a given state, but depend on the variables one is going to measure," a foreboding of the result which Bell later obtained, as we have seen, from his analysis of Gleason's work. In the sequel of his paper Moyal derived from the equations of motion the laws governing the transformation with time of these phase-space distributions and showed that they can be used as an alternative to the Schrödinger equation for the solution of problems such as the calculation of transition probabilities.

Moyal's method of obtaining the phase space distribution $f(r,s)$ if applied to the canonically conjugate variables q and p produced precisely the Wigner distribution $f_w(q,p)$, which, as noted, is not nonnegative definite. Moyal claimed that the Wigner distribution or possible alternatives could nevertheless be used for most practical purposes of solving quantum mechanical problems in a form almost identical with that of the usual probability theory. From his discussions of this problem with Maurice Stevenson Bartlett, at that time professor of mathematical statistics at the University of Manchester, there originated the latter's paper[12] on negative probabilities in which the attempt was made to justify the use of such probabilities as long as they comply with the rules of the probability calculus (except the requirement of nonnegativity) and are combined at the end to give nonnegative probabilities. That the most general distribution function[13] $f(q,p)$ which yields the correct marginal distributions and reproduces the usual quantum mechanical expectation values cannot be nonnegative definite has been shown more recently by Leon Cohen,[14] a student of Henry Margenau.

9.2. EARLY STOCHASTIC INTERPRETATIONS

The first serious attempt to interpret quantum mechanics as a theory of Markov processes in configuration space was made in 1952 by Imre Fényes[15] of the University of Debrecen in Hungary. Considering the position coordinates as a set of random variables Fényes defined a probability density and a transition probability as functions of these coordinates and of time; after appropriately normalizing the densities he interrelated them through an integral equation defining thereby a Markov process. From integral relations holding between the transition probability densities at three distinct instants of time he then derived two differential equations, one of which, representing a continuity or conservation equation, turned out to be the Fokker equation. Fényes subsequently defined a total stochastic velocity and derived an indeterminacy equation which

[12]M. S. Bartlett, "Negative probability," *Proceedings of the Cambridge Philosophical Society* **41**, 31–33 (1945).

[13]The Wigner distribution is a special case of $f(q,p)$, so is the Margenau and Hill distribution; cf. H. Margenau and R. W. Hill, "Correlation between measurements in quantum theory," *Progress of Theoretical Physics* **26**, 722–738 (1961).

[14]L. Cohen, "Can quantum mechanics be formulated as a classical probability theory?," *Philosophy of Science* **33**, 317–322 (1966); "Generalized phase distribution functions," Part II of Ph.D. thesis, Yale University, 1966 (unpublished); "Generalized phase-space distribution functions," *Journal of Mathematical Physics* **7**, 781–786 (1966).

[15]I. Fényes, "Eine wahrscheinlichkeitstheoretische Begründung und Interpretation der Quantenmechanik," *Zeitschrift für Physik* **132**, 81–106 (1952).

corresponds to the Heisenberg relation. In contrast to Schrödinger and Fürth he thus concluded that quantum mechanics is not merely an analogy to a stochastic diffusion theory but is itself inherently stochastic. The Heisenberg relations, he declared, arise not from the disturbances due to measurements but from the stochastic nature of the processes under discussion.[16] He supported his contention by presenting a stochastic derivation of the Schrödinger equation from a statistical Lagrange principle.

This derivation was disputed by Walter Weizel,[17] since 1936 professor at the University of Bonn, on the grounds of an analysis of a particular equilibrium problem in which forces balance the diffusion velocity and produce a stationary distribution in contradiction to Fényes' result. Claiming, therefore, that quantum mechanics cannot be interpreted as a stochastic theory, Weizel, influenced by Bohm's 1952 publications, proposed a causally determined model: the particles are assumed to be subject, apart from the external field, to a constant number of intermittent impulses which are independent of the motion of the particles and do not change their average momentum. These impulses are delivered by hypothetical particles moving with the speed of light which Weizel called "zerons" [*Zeronen*] since he assumed that their rest mass is zero. By averaging over the interactions for which Planck's h plays the role of a coupling constant Weizel retrieved the usual probability distributions in agreement with ordinary quantum mechanics. He extended his theory for systems of arbitrarily many particles in constant electric or magnetic fields in a sequel paper.[18]

Fényes' work was severely criticized also by A. F. Nicholson,[19] who at that time had just finished his Ph.D. thesis in statistical mechanics under Fürth at Birkbeck College in London. Nicholson charged that the Lagrangian function used by Fényes was ad hoc, that the resulting equation and its admissible solutions were not proved to be equivalent to the Schrödinger equation and its admissible solutions, and that, moreover, Fényes' integral equations did not lead to quantum mechanics unless additional restrictions were imposed on the class of solutions of the

[16]Cf. also G. Herdan, "Heisenberg's uncertainty relation as a case of stochastic dependence," *Die Naturwissenschaften* **39**, 350 (1952), and I. Fényes, "Stochastischer Abhängigkeitscharakter der Heisenbergschen Ungenauigkeitsrelation," *ibid.*, 568.

[17]W. Weizel, "Ableitung der Quantentheorie aus einem klassischen, kausal determinierten Modell," *Zeitschrift für Physik* **134**, 264–285 (1953).

[18]W. Weizel, "Ableitung der Quantentheorie aus einem klassischem Modell, II," *Zeitschrift für Physik* **135**, 270–273 (1953).

[19]A. F. Nicholson, "On a theory due to I. Fényes," *Australian Journal of Physics* **7**, 14–21 (1954).

Markov equations involved. Nicholson argued further that the Schrödinger equation deduced by Fényes applied only to particles whose interactions with external fields can be expressed completely in terms of scalar potential functions and hence not even to a particle moving in an external electromagnetic field. Finally he charged that Fényes' identification of his stochastically derived indeterminacy relation with the quantum mechanical Heisenberg relation, based as it is on the identification of a certain function of the configuration coordinates and the time with a linear momentum component, was logically inconsistent since this last-mentioned identification itself amounts to a violation of the Heisenberg relation. In short, "Fényes' theory is not a possible representation of quantum mechanics."[20]

During the 1950s Friedrich Arnold Bopp (Ph.D. Göttingen, 1937), since 1947 professor of theoretical physics at the University of Munich, developed an interpretation which was based on stochastic analogies and led to far-reaching philosophical conclusions. Having worked together with Arnold Sommerfeld, Bopp became greatly interested in the relation between quantum theory and classical statistical mechanics.[21] In a nontechnical exposition of the quantum theory,[22] written together with Oswald Riedel, Bopp described the wave-particle duality as "two isomorphic representations of reality," adding that no criterion exists to prefer the one in favor of the other. But, as we know from occasional autobiographical remarks,[23] Bopp by 1937 already ascribed greater fundamentality to Born's probabilistic interpretation in the corpuscular sense. In his early statistical investigations[24] Bopp came upon a correlation probability that could be split

[20] *Ibid.*, p. 18.

[21] F. Bopp, "Quantenmechanische Statistik und Korrelationsrechnung," *Zeitschrift für Naturforschung* **2a**, 202–216 (1947).

[22] F. Bopp and O. Riedel, *Die physikalische Entwicklung der Quantentheorie* (C. E. Schwab, Stuttgart, 1950).

[23] F. Bopp, "Die anschaulichen Grundlagen der Quantenmechanik," Lecture delivered at the Freiburg-Colloquium Foundations of Physics," Oberwolfach, June 30–July 7, 1966.

[24] F. Bopp, "Ein für die Quantenmechanik bemerkenswerter Satz der Korrelationsrechnung," *Zeitschrift für Naturforschung* **7a**, 82–87 (1952); "Statistische Untersuchung des Grundprozesses der Quantentheorie der Elementarteilchen," *ibid.*, **8a**, 6–13 (1953); "Ein statistisches Modell für den Grundprozess in der Quantentheorie der Teilchen," *ibid.*, **8a**, 228–233 (1953); "Wellen und Teilchen," *Optik* **11**, 255–269 (1954); "Über die Natur der Wellen," *Zeitschrift für angewandte Physik* **6**, 235–238 (1954); "Korpuskularstatistische Begründung der Quantenmechanik," *Zeitschrift für Naturforschung* **9a**, 579–600 (1954); "Das Korrespondenz-prinzip bei korpuskularstatistischer Auffassung der Quantenmechanik," *Münchener Sitzungsberichte* **1955**, 9–22; "Würfel-Brettspiele, deren Steine sich näherungsweise quantenmechanisch bewegen," *Zeitschrift für Naturforschung* **10a**, 783–789 (1955); "Quantenmechanische und stochastische Prozesse," *ibid.*, **10a**, 789–793 (1955); "Einfaches Beispiel aus der stochastischen

uniquely into two factors, satisfying specific conditions, and obtained a representation of the average values in the usual correlation statistics which turned out to agree with the average values of Gibbs ensembles in quantum mechanical systems. Bopp was thus led to the conclusion that "there exist stochastical equations which are experimentally indistinguishable from quantum mechanical equations."[25] This he illustrated in terms of a game with checkers and pawns whose movements depend stochastically on the outcome of throwing dice.

A contradiction between particle and wave conceptions exists according to Bopp only if waves are conceived, as in the theory of the electromagnetic field after the rejection of ether hypotheses, as independent existents; if, however, in contrast to such a substantial conception (in Weyl's sense) waves are regarded as merely collective properties of particle ensembles, for example, in the case of sound waves, no such contradiction exists; waves then describe only the motion of particles. Bopp suggested regarding quantum mechanical wave functions in the latter sense and associating Born's interpretation with the probability of the appearance in a given volume element not of a persisting and always identical particle but rather of a particle out of a virtual ensemble of identical particles, subject of course to the usual normalization conditions for probability functions. For in a statistical theory of a virtual ensemble of mutually independent particles, there can be associated with each volume element a certain probability of finding the particle therein. These probabilities, in particular, can oscillate, as Bopp demonstrated,[26] provided the statistics satisfies certain conditions connected with the Heisenberg indeterminacy relations.

Since in accordance with these relations the state of the system, defined by the position and momentum coordinates, is at no time determinable, a new statistics has to be set up which yields only expectation values but never the exact position of the system in phase space. Bopp's point of departure was therefore the following theorem,[27] which he proved in 1956: Every quantum mechanical system can be mapped into a statistical ensemble of particles in a certain phase space so that every quantum

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Quantenmechanik," *Zeitschrift für Physik* **143**, 233–238 (1955); "La mécanique quantique est-elle une mécanique statistique classique particulière?," *Annales de l'Institut Henri Poincaré* **15**, 81–112 (1956); "Statistische Mechanik bei Störung des Zustandes eines physikalischen Systems durch die Beobachtung," in *Werner Heisenberg und die Physik unserer Zeit*, F. Bopp, ed. (Vieweg, Braunschweig, 1961), pp. 128–149; "Zur Quantenmechanik relativistischer Teilchen bei gegebenem Hilbert-Raum," *Zeitschrift für Physik* **171**, 90–115 (1963). "Grundvorstellung der Quantenphysik" in *Quanten und Felder* (Ref. 7-17), pp. 111–124.

[25] Ref. 24 (1955, p. 233, p. 783).

[26] Ref. 24 (1954, p. 579).

[27] Ref. 24 (1956, p. 81).

mechanical process corresponds to a movement of this ensemble. By means of a number of rather general principles Bopp now attempted to reconstruct the formalism of quantum mechanics. His first principle[28] defined the notion of ensembles of particles moving in phase space, his second principle affirmed the possibility of statistical descriptions. From these and other postulates he derived von Neumann's equation $i\hbar\dot{\rho} = H\rho - \rho H$, where ρ is the statistical matrix and H the Hamiltonian of the system, and hence Schrödinger's equation for pure states. Only now did the notion of waves make its appearance, and in complete conformity with the physical situation: the relative frequencies of particle manifestations at given points have the same mutual relations as the intensities in interferences; these frequencies could consequently be described by wave equations without the need of assigning any ontologically independent status to these waves.

In his more recent publications[29] Bopp has phrased his conclusions in terms of what may be called a stroboscopic world-picture or, as he expressed it, in terms of the Heraclitean philosophy of continual change, creation, and annihilation in contrast to the conventional view (ascribed by him to Thales and Newton) according to which changes are continuous redispositions of material and permanent entities. Since quantum field theoretic creation or annihilation processes such as the production of neutral pions, $p + p \rightarrow p + p + \pi^0$, from high-energy protons, are elementary processes not reducible to ordinary or hidden motions, whereas ordinary motions in space are conceivable as chains of consecutive sudden creations and annihilations in contiguous positions, Bopp tried to construct a statistics, compatible with the Heisenberg relations, on ensembles of those sudden appearances of particles in space. The stochastic functions or probability waves were obtained by him by a method similar to that of assigning occupations numbers in field theory. The statistical equations expressing the order of events turned out to have the structure of local and mutually coupled oscillator equations and made it possible to introduce the notion of waves. Ultimately, of course, these ideas reformulated the new extension of statistical mechanics in terms of which Bopp derived the laws of quantum mechanics. According to his interpretation particles are the causes of possible occurrences [Ursachen möglicher Geschehensakte] in space and time while waves merely express the order in which these events occur [die Ordnung, nach der sich alles Geschehen vollzieht]. The

[28]F. Bopp, "The principles of the statistical equations of motion in quantum theory," in Ref. 7-102, pp. 189–196; also Ref. 24 (1961, p. 128).

[29]F. Bopp, "Elementarvorgänge der Quantenmechanik in stochastischer Sicht," *Annalen der Physik* **17**, 407–414 (1966).

laws of this order are only of a statistical nature since the fundamental events in nature are discrete creation and annihilation processes.[30]

Bopp's theory of which only a very brief outline could be presented seems to have found, in spite of its logical and philosophical consistency, only a very limited amount of interest among physicists or philosophers. One notable exception was a paper written by W. M. Machado and W. Schützer[31] of the University of São Paulo in which it is shown how Bopp's formalism, by using descriptions in terms of statistical ensembles which are never complete descriptions of individual systems, avoids the difficulties of the Einstein-Podolski-Rosen "paradox."

Another attempt to interpret the formalism of quantum mechanics by reconstructing it in terms of stochastic processes and newly hypothesized physical entities, surpassing in its boldness even Bopp's theory, was proposed in the late 1950s by Assène Borissoff Datzeff, a graduate of the University of Sofia (1933) and Docteur ès Sciences (1938) of the Sorbonne, and since 1950 head of the department of theoretical physics in Sofia as well as sometime scientific adviser to the Bulgarian Embassy in Moscow. Influenced primarily by the ideas of Bohm and Vigier[32] about random subquantum fluctuations, Datzeff[33] based his theory on the assumption of a material support of physical fields which he called "subvac" (substance du vacuum) to distinguish it from the ether of classical physics, from which it differs in its properties. Arguing that such an assumption does not violate the theory of relativity on grounds similar to those advanced by L. Jánossy, Datzeff ascribed to the subvac a discrete structure by postulating that it is composed of what he called AS corpuscles (*atomes du subvac*).

Whereas according to Datzeff ordinary microphysical particles such as the electron are built up of AS corpuscles and hence possess finite dimensions and an internal dynamics, the AS corpuscles themselves give rise to chaotic electric and magnetic forces so that the fields produced by

[30]F. Bopp, "Die anschaulichen Grundlagen der Quantenmechanik" (unpublished, 1966).

[31]W. M. Machado and W. Schützer, "Bopp's formulation of quantum mechanics and the Einstein-Podolski-Rosen paradox," *Anais da Academia Brasileira de Ciencias* **35**, 27–35 (1963).

[32]Refs. 2-48, 2-49.

[33]A. B. Datzeff, "Sur l'interprétation de la mécanique quantique," *Comptes Rendus* **246**, 1502–1505 (1958); "Sur la probabilité de présence en mécanique quantique," *ibid.*, 1670–1672; "Sur le formalisme mathématique de la mécanique quantique," *ibid.*, 1812–1815; "Sur les conditions de Sommerfeld et la mécanique ondulatoire," *ibid.*, **247**, 1565–1568 (1958); "Sur l'interprétation de la mécanique quantique I," *Journal de Physique et le Radium* **20**, 949–955 (1959); "Sur l'interprétation de la mécanique quantique II," *ibid.*, **21**, 201–211; III, ibid., **22**, 35–40; IV, *ibid.*, 101–111; *Mécanique Quantique et Réalité Physique* (Editions de l'Académie Bulgare des Sciences, Sofia, 1969). Cf. also p. 243 of the last-mentioned book for further bibliography on Datzeff's publications.

the ordinary particles are variable and oscillating. To a first approximation these fields can be regarded as composed of a diverging and a converging wave which together form a stationary wave. Under certain conditions the AS corpuscles create stable formations which interact with the microphysical particles and produce motions of the latter which in view of the fluctuations of the former can be described only stochastically. These qualitative considerations led Datzeff to the definition of a probability density $w(x,y,z)$ for the presence of a particle and to the definition of a function $f(x,y,z)$, satisfying $|f|^2 = w$, which, as he showed, satisfies under appropriate initial conditions a differential equation of the Sturm-Liouville type and results in a probability equation which is identical with the Schrödinger equation.

In his 1961 papers Datzeff derived step by step the whole formalism of nonrelativistic quantum mechanics from his basic assumptions. The indeterminacy principle, in particular, turned out to be an expression of a statistical dependence between canonically conjugate quantities rather than an interdiction of assigning simultaneous values of position and momentum to a particle.

Due to their highly conjectural nature Datzeff's ideas have hardly ever been commented upon. Even Bohm and Vigier, in whose theories Datzeff thought he found support for his conceptions, remained silent.

9.3. LATER DEVELOPMENTS

With the development of statistical electrodynamics in the 1950s and 1960s additional similarities between classical stochastic systems and quantum mechanical systems were discovered. It was found, for example, that in a random electromagnetic field certain charged systems such as the harmonic oscillator behave essentially like their quantum mechanical counterparts. Important contributions to this effect were made by Bourrett, Braffort, Marshall, Sardin, Taroni, and Tzara.[34] In 1964 David Kershaw,[35] while a graduate student at Harvard, supplied a rigorous proof of the fact that in the case of a single particle moving in a given potential or of a

[34]P. Braffort and C. Tzara, "Énergie de l'oscillateur harmonique dans le vide," *Comptes Rendus* **239**, 1779–1780 (1954). R. C. Bourrett, "Quantized fields as random classical fields," *Physics Letters* **12**, 323–325 (1964); "Ficton theory of dynamical systems with noisy parameters," *Canadian Journal of Physics* **43**, 619–639 (1965). P. Braffort, M. Surdin, and A. Taroni, "L'énergie moyenne d'un oscillateur harmonique non-relativiste en électromagnetique aléatoire," *Comptes Rendus* **261**, 4339–4341 (1965). T. W. Marshall, "Statistical electromagnetics," *Proceedings of the Cambridge Philosophical Society* **61**, 537–546 (1965).

[35]D. Kershaw, "Theory of hidden variables," *Physical Review* **136B**, 1850–1856 (1964).

system of two particles interacting through a potential which is a function of their distance from each other the stationary solutions of the Schrödinger equation are precisely the stationary probability distributions of the motions of the systems considered as Markov chains.

At the same time G. G. Comisar,[36] a research physicist at Aerospace Corporation in California, developed an interesting linear Brownian motion model which preserves the usual statistical interpretation of the wave function for sufficiently long time intervals. Comisar, like many other physicists working at that time on stochastic quantum mechanics, was greatly influenced by Feynman's path integral approach,[37] according to which the wave function could be regarded as a sum of path integrals over Brownian motion trajectories.

The connection among Feynmann integrals, Markov processes, and the Schrödinger equation became the subject of intensive research all over the world. In the United States Edward Nelson, a mathematician at Princeton University, derived by adopting the kinematics of the Einstein-Smoluchowski theory of Brownian motion (describing the motion as a Markov process in coordinate space with a diffusion coefficient inversely proportional to the mass) and Newtonian dynamics as in the Ornstein-Uhlenbeck theory, by means of classical ideas of motion in space-time, a nonlinear equation of motion which by an appropriate change of the dependent variable could be expressed in terms of a wave function satisfying the time-independent Schrödinger equation.[38] In Italy L. F. Favella[39] of the University of Torino independently obtained Nelson's result by proving that a transformation of the quantum mechanical Green's function into the transition probability of a Markov process leads to an identification of the Schrödinger equation with the Kolmogorov-Fokker-Planck equation with a diffusion coefficient equal to $h/4\pi m$.

In Poland Włodzimierz Garczyński[40] of the University of Wrocław tried to interpret the whole of quantum mechanics with the inclusion of S-

[36] G. G. Comisar, "Brownian-motion model of nonrelativistic quantum mechanics," *Physical Review* **138B**, 1332–1337 (1965).

[37] It became widely known with the publication of R. P. Feynman and A. R. Hibbs, *Quantum Mechanics and Path Integrals* (McGraw-Hill, New York, 1965), *Kvantovaya Mekhanika i Integrali po Trayektoriyam* (Mir, Moscow, 1968).

[38] E. Nelson, "Derivation of the Schrödinger equation from Newtonian mechanics," *Physical Review* **150**, 1079–1085 (1966); *Dynamical Theories of Brownian Motion* (Princeton University Press, Princeton, N. J. 1967).

[39] L. F. Favella, "Brownian motions and quantum mechanics," *Annales de l'Institut Henri Poincaré* **7**, 77–94 (1967).

[40] W. Garczyński, "Stochastic approach to quantum mechanics and to quantum field theory," *Acta Physica Austriaca, Suppl.* **6**, 501–517 (1969).

matrix theory and quantum field theory in terms of Markov processes. Garczyński assumed that to every quantum system there corresponds a Markov process given by a set of amplitudes $a_{ik}(s,t)$ such that $|a_{ik}(s,t)|^2$ is the probability of finding the system in state k at time t if it is known that at time s it was in state i. Garczyński derived the Schrödinger equation by subjecting these probability amplitudes to the following conditions:

1. $a_{ik}(s,t)=a_{ki}^*(t,s)$, the motion reversibility condition.
2. $\lim_{t\to s} a_{ik}(s,t)=\delta_{ik}$, the time continuity condition.
3. $\sum_j a_{ij}(s,t)a_{jk}(t,s)=\delta_{ik}$, the unitarity condition.
4. $\sum_j a_{ij}(s,r)a_{jk}(r,t)=a_{ik}(s,t)$ for $s\leqslant r\leqslant t$, the quantum causality condition, analogous to the Smoluchowski-Chapman-Kolmogorov equation.

His method was similar to the well-known derivation of the Kolmogorov equation in the classical theory of Markov processes.

The stochastic interpretation of quantum mechanics found an eloquent proponent in Luis de la Peña-Auerbach of the National University of Mexico. Having studied at the Instituto Politécnico Nacional in Mexico and under A. A. Sokolov at Moscow State University (Ph.D., 1964), de la Peña-Auerbach became greatly impressed by the work of Fényes, Weizel, and Nelson and decided to specialize in this field. Reviewing one of his earliest papers[41] gives us the welcome opportunity to illustrate the spirit of the stochastic approach by an example which can be understood with only a rudimentary knowledge of this branch of probability theory.

Let $x(t)$ be the set of random variables defining the stochastic process in the statistical description of the motion of a point in configuration space. The probability denisty $\rho(x,t)$ at $x(t)$, being a positive definite quantity, can be written

$$\rho=\exp(2R) \tag{25}$$

where $R=R(x,t)$ is real. Conservation of total probability implies a continuity equation

$$\frac{\partial\rho}{\partial t}+\operatorname{div} j=0 \tag{26}$$

as a consequence of which the components of the current, if conceived as a

[41] L. de la Peña-Auerbach, "A simple derivation of the Schroedinger equation from the theory of Markoff processes," *Physics Letters* **24A**, 603–604 (1967); "A new formulation of stochastic theory and quantum mechanics," *ibid.*, **27A**, 594–595 (1968).

Markov process (see the Appendix at the end of this chapter), can be written

$$j_i = a_i\rho + \sum_k \partial_k(b_{ik}\rho). \tag{27}$$

Substituting (25) into (27) we obtain

$$j = v\rho \tag{28}$$

where

$$v_i = a_i + 2\sum_k b_{ik}\,\partial_k R + \sum_k \partial_k b_{ik} \tag{29}$$

In view of (25) and (28), (26) can be written as

$$\frac{\partial R}{\partial t} = -\frac{1}{2}(\nabla\cdot v) - (v\cdot\nabla)R. \tag{30}$$

Setting

$$v = \alpha\nabla S \tag{31}$$

where $S = S(x,t)$ and α are real, and defining

$$\psi = \exp(R + iS) \tag{32}$$

so that for the probability amplitude $\psi, \rho = |\psi|^2$, we see that (30) connects $\nabla S = \alpha^{-1}v$ and R by a differential equation which is obtained if (30) is multiplied by ψ and the values of $\psi\partial R/\partial t$ and $\psi\nabla^2 S$, computed from (32), are substituted:

$$\frac{\partial\psi}{\partial t} - i\frac{\partial S}{\partial t} = \frac{1}{2}i\alpha\nabla^2\psi - \frac{1}{2}i\alpha\left[\nabla^2 R + (\nabla R)^2 - (\nabla S)^2\right]\psi. \tag{33}$$

Finally, introducing the function V defined by

$$V = -\frac{\partial S}{\partial t} + \frac{1}{2}\alpha\left[\nabla^2 R + (\nabla R)^2 - (\nabla S)^2\right] \tag{34}$$

we obtain

$$i\frac{\partial\psi}{\partial t} = -\frac{1}{2}\alpha\nabla^2\psi + V\psi, \tag{35}$$

which is the Schrödinger equation if $\alpha = h/2\pi m$. Thus on the basis of only two assumptions that are usually made to obtain differential equations in the theory of Markov processes the Schrödinger equation has been derived.

That the constant in this equation has to be determined by experiment also holds in ordinary quantum theory.

After having shown[42] in 1968 how the problem of Brownian motion can be treated, formally at least, by solving a Schrödinger-like equation for the probability amplitude, the treatment being restricted to time intervals very large compared with the relaxation time, de la Peña-Auerbach in collaboration with Leopoldo S. Garcia-Colin began to work in the opposite direction, that is, to account for the motion of a quantum mechanical particle in terms of classical trajectories and stochastic forces. The results were published in a series of papers[43] with increasing generality of treatment. In 1969, together with Ana M. Cetto, his wife, de la Peña-Auerbach[44] showed, starting from a generalized D'Alembert principle, that one can express the basic equations of the previously developed stochastic theory in Lagrangian form and thus derive the Schrödinger equation for a particle in an electromagnetic field of restricted generality. In 1970 the treatment was further generalized to a relativistic formulation of the stochastic theory for spinless particles[45] and to particles with integral or half-integral spin.[46]

To further explore the potentialities of the stochastic interpretation de la Peña-Auerbach and Cetto[47] investigated whether the stochastic formulation admits the introduction of the radiation damping terms of classical electrodynamics, a problem of particular interest since, as is well known, the classical treatment of the self-interaction of the electron is marred by divergences. A first-order perturbation calculation applied to the hydrogenlike atom yielded the nonrelativistic spinless part of the self-interaction

[42] L. de la Peña-Auerbach, E. Braun, and L. S. Garcia-Colin, "Quantum-mechanical description of a Brownian particle," *Journal of Mathematical Physics* **9**, 668–674 (1968).

[43] L. de la Peña-Auerbach and L. S. Garcia-Colin, "Possible interpretation of quantum mechanics," *Journal of Mathematical Physics* **9**, 916–921 (1968); "Simple generalization of Schrödinger's equation," *ibid.*, 922–927; "On the generalized Schroedinger equation," *Revista Mexicana de Fisica* **16**, 221–232 (1967); "A new formulation of stochastic theory and quantum mechanics," *ibid.*, **17**, 327–335 (1968); L. de la Peña-Auerbach, "New formulation of stochastic theory and quantum mechanics," *Journal of Mathematical Physics* **10**, 1620–1630 (1969).

[44] L. de la Peña-Auerbach and A. M. Cetto "Lagrangian form of stochastic equations and quantum theory," *Physics Letters* **29A**, 562–563 (1969); "A new formulation of stochastic theory and quantum mechanics III," *Revista Mexicana de Fisica* **18**, 253–264 (1969).

[45] L. de la Peña-Auerbach, "New formulation of stochastic theory and quantum mechanics," *Revista Mexicana de Fisica* **19**, 133–145 (1970).

[46] L. de la Peña-Auerbach, "Stochastic quantum mechanics for particles with spin," *Physics Letters* **31A**, 403–404 (1970).

[47] L. de la Peña-Auerbach and A. M. Cetto, "Self-interaction corrections in a nonrelativistic stochastic theory of quantum mechanics," *Physical Review* **D3**, 795–800 (1971).

(Lamb shift of the $2s$ level) without divergence or renormalization difficulties, an encouraging result.

In view of the highly technical nature of the subject it would lead us too far into mathematical considerations if we discussed in any detail the numerous other recent contributions to the stochastic interpretation of quantum mechanics such as the attempt made by Rylov to interpret nonrelativistic quantum mechanics as "a variety of the relativistic theory of Brownian motion."[48] An important critical assessment of the most prominent attempts in this direction was made recently by James George Gilson, a graduate of London University and student of M. S. Bartlett. Having read in the late 1950s Feynman's famous paper (1948) on the functional integral in quantum mechanics and, at about the same time, Norbert Wiener's much earlier papers on the functional integral in Brownian motion, Gilson became interested in the problem of how these two formally similar, but mathematically different, concepts were related to each other and to classical physics, considerations which led him to question the statistical basis of orthodox quantum mechanics.

In his critical review[49] Gilson showed that an analysis of the structure of quantum mechanics in terms of Feynman's integrals leads to the conclusion that, contrary to what should be expected in a real stochastic situation, the quantum transition probability is dependent on the initial state and is more like a delta function than a Gaussian; moreover, the Fokker-Planck equation and the Schrödinger equation are consistent only provided the coefficient of diffusion is identically zero. In view of these conclusions Gilson declared that "quantum mechanics has little if anything to do with stochastic theory.... However, it would seem possible from this work that Schrödinger quantum theory could be regarded as the continuous limit of a discrete time stochastic theory, but as we have seen, as going to this continuous limit the stochastic picture evaporates entirely." Gilson's analysis was based exclusively on the use of Feynman's integral and his conclusion is therefore contingent on how far Feynman's integral solution to the Schrödinger equation agrees with the solution obtained by conventional methods.

But even granted quantum mechanics could consistently be regarded as a stochastic theory and microphysical particles could be described as performing some kind of Brownian motion, such an account would immediately raise the question of an interaction of the particles with the ether

[48]Y. A. Rylov, "Quantum mechanics as a theory of relativistic Brownian motion," *Annalen der Physik* **27**, 1–11 (1971).

[49]J. G. Gilson, "On stochastic theories of quantum mechanics," *Proceedings of the Cambridge Philosophical Society* **64**, 1061–1070 (1968).

and thus the problem of the existence of hypothetical entities. As long as no empirical facts can be adduced to corroborate the existence of such conjectural entities any stochastic interpretation of quantum mechanics, even if mathematically defensible, would philosophically remain unsatisfactory.

APPENDIX

THE DERIVATION OF EQUATION 27 FROM EQUATION 26

In Ref. 9-41 the reader is referred to S. Chandrasekhar's review article "Stochastic Problems in Physics and Astronomy" [*Reviews of Modern Physics* **15**, 1–89 (1943)]. For the nonspecialist the following more elementary derivation is given.

For stationary Markov processes $\rho = P(x|z,t)$ denotes the conditional probability that, given x at $t=0$, one finds z in the interval $(z, z+dz)$ at time t. From elementary probability considerations it follows that $\int dz P = 1$ and that $P(x|y,t+\Delta t) = \int dz P(x|z,t) P(z|y,\Delta t)$ which is the Smoluchowski equation. The moments of coordinate change during Δt are $\mu_n(z,\Delta t) = \int dy (y-z)^n P(z|y,\Delta t)$. These are assumed to be proportional to Δt if $\Delta t \to 0$ only for $n=1$ and 2. Hence $a(z) = \lim \mu_1/\Delta t$ and $b(z) = \lim \mu_2/\Delta t$ exist. If the arbitrary function $f(y)$ goes to zero for $y \to \pm 0$ sufficiently fast, the integral $I = \int dy f(y)\, \partial\rho(xy,t)/\partial t$, by virtue of the Smoluchowski equation, becomes by interchange of order of integration

$$I = \lim \frac{1}{\Delta t} \int dy f(y) [\rho(x|y,t+\Delta t) - \rho(x|y,t)]$$

$$= \lim \frac{1}{\Delta t} \left[\int dz \rho(x|z,t) \int dy f(y) \rho(z|y,\Delta t) - \int dz f(z) \rho(x|z,t) \right].$$

Expanding $f(y)$ as a Taylor series in $(z-y)$ we obtain

$$I = \int dz \rho(x|z,t) \left[f'(z) a(z) + \frac{1}{2} f''(z) b(z) \right],$$

and after integration by parts, writing y for z,

$$\int dy f(y) \left[\frac{\partial \rho}{\partial t} + \frac{\partial}{\partial y}(a\rho) - \frac{1}{2} \frac{\partial^2}{\partial y^2}(b\rho) \right] = 0.$$

Since $f(y)$ is arbitrary, the general Fokker-Planck equation holds

$$\frac{\partial \rho}{\partial t} = -\frac{\partial}{\partial y}(a\rho) + \frac{1}{2} \frac{\partial^2}{\partial y^2}(b\rho).$$

For an n-dimensional Markov process with a_i and b_{ik} defined analogously to a and b we obtain

$$\frac{\partial \rho}{\partial t} = -\sum_i \frac{\partial}{\partial y_i}\left[(a_i\rho) + \sum_k \frac{\partial}{\partial y_k}(b_k\rho)\right] = -\sum_i \partial_i j_i$$

and hence (27).

STATISTICAL Interpretations

Chapter Ten

10.1. HISTORICAL ORIGINS

In the interpretations of the quantum mechanical formalism discussed so far the state vector or wave function was generally regarded as a description of the individual system (e.g., electron). It was assumed either to be the most complete possible description of an individual physical system, as in the various versions of the Copenhagen interpretation, or to be amenable to further completion with at most only minor modifications of the formalism, as in the hidden-variable interpretations. An interpretation according to which the state vector provides a description not of the individual system but of an ensemble of identically (or similarly) prepared systems will be called a statistical ensemble interpretation or briefly a *statistical interpretation.*[1]

Occasional references to the statistical interpretation have been made in the preceding chapters. In fact, as explained in our account of the 1927 Solvay Congress,[2] it was Einstein who on this occasion proposed a statistical interpretation, his "viewpoint I," to avoid the conceptual difficulties which arise if the reduction of a wave packet is described in terms of an interpretation which associates wave functions with individual systems. It has also been pointed out, in a different context,[3] that all through his life Einstein adhered to the statistical interpretation of the existing formalism even though he hoped that someday a "complete" theory of microphysics would be available which, though established on a conceptual basis different from that of quantum mechanics, would contain the latter as a statistical approximation. Suffice it to recall the statement made by Einstein in 1936: "The ψ function does not in any way describe a condition which could be that of a single system: it relates rather to many systems, to 'an ensemble of systems' in the sense of statistical mechanics."[4] And a few years before his death he wrote in his "Reply to Criticisms," after having admitted that Bohr's interpretation "is certainly by no means absurd from a purely logical standpoint": "One arrives at very implausible theoretical conceptions, if one attempts to maintain the thesis that the statistical quantum theory is in principle capable of producing a complete description of an individual physical system. On the other hand, those difficulties of theoretical interpretation disappear, if one views the quan-

[1]Following Fock, we thus distinguish between "probabilistic" and "statistical" interpretations and apply the former term to all those interpretations which regard elementary processes as not governed by deterministic laws.

[2]Section 5.1.

[3]Ref. 7-3.

[4]Ref. 6-128.

tum-mechanical description as the description of ensembles of systems."[5]

For these historical reasons the thesis that quantum mechanics predicts the relative frequencies of the results of measurements performed on an ensemble of identically prepared systems is sometimes called the "Einstein hypothesis" in contrast with what is called the "Born hypothesis," according to which quantum mechanics predicts the probability of the result of a measurement performed on a single system. This, however, makes sense only if a distinction can be made between a theory that predicts frequencies and a theory that predicts probabilities of individual events. Such a distinction could certainly be drawn if typical quantum probabilities were not identical with relative frequencies, even if the experimental procedures used for the verification of both types of prediction appeared to be identical. Many physicists deny the logical legitimacy of such a distinction.

In this context it is of historical interest to note that Max Born, who generally considered probabilities in quantum mechanics as something *sui generis*, espoused the frequency interpretation on at least, two occasions. Shortly after having proposed his original probabilistic interpretation of the wave function,[6] Born read before the British Association at Oxford on August 10, 1926, a paper in which he declared: "The quantum theoretical description...does not answer...the question of where a certain particle is at a given time. In this respect the quantum theory is in agreement with the experimentalists, for whom microscopic coordinates are also out of reach, and who therefore only count instances and indulge in statistics. This suggests that quantum mechanics similarly only answers properly-put statistical questions, and says nothing about the course of individual phenomena. It would then be a singular fusion of mechanics and statistics."[7] And after the publication of the Schilpp volume in which Einstein made the above-mentioned statement Born wrote to Einstein that he agreed with his interpretation of the ψ-function and that "the difference [in their views] is not essential, but merely a matter of language."[8]

The first among the specialists in quantum mechanics to endorse the statistical view was probably John Clarke Slater, a graduate of Harvard (Ph.D., 1923) who had studied at Cambridge and Copenhagen before he joined the faculty of Harvard University in 1924 and that of M.I.T. in

[5]Ref. 4-9 (1949, 1959, p. 671).

[6]Ref. 2-28.

[7]M. Born, "Physical aspects of quantum mechanics," *Nature* **119**, 354–357 (1927); reprinted in Ref. 6-11 (1956, pp. 6–13); Cf. *Reports of the British Association for the Advancement of Science* (Oxford, August 4–11, 1926), p. 440.

[8]Ref. 5-14 (1969, p. 250; 1971, p. 186).

1930 and who is well known for his numerous contributions to quantum theory. At a symposium on quantum mechanics, held under the auspices of the American Physical Society in New York City on December 31, 1928, Slater declared that "wave mechanics is an extension, not of ordinary Newtonian mechanics, but of statistical mechanics; and this simple observation is enough to explain many of its otherwise puzzling features."[9] As a statistical theory, he added, wave mechanics deals with ensembles which it describes by distribution functions in a space which, though different from the phase space of ordinary statistical mechanics (primarily because of the indeterminacy principle), is nevertheless "essentially" similar to it. Although we do not work directly with such distribution functions but rather with the wave functions ψ when we calculate the changes of state in time, the meaning of ψ, according to Slater, lies in the statistical information which it provides with respect to such distributions for ensembles of systems.

As already mentioned,[10] Edwin C. Kemble fully agreed with his younger colleague. "When we say that the 'state' of an electron in motion is described by $\psi(x,t)$ we mean," said Kemble, "that an assemblage of a very large number of similarly prepared electrons would have statistical properties described by this function, and that we cannot know more about an individual electron than the fact that it belongs to a suitably chosen potential assemblage of this character."[11] Throughout his well-known textbook he applied the statistical interpretation. Thus he wrote, for example, that "we can identify the state of a physical system with that of a Gibbsian assemblage of identical systems so prepared that the past histories of all its members are the same in all details that can affect future behavior as that of the original system."[12] Although in the very *Preface* of his book Kemble emphasized the fundamental importance of Gibbsian ensembles of independent systems for his interpretation of the formalism,[13] his subsequent exposition of the indeterminacy relations seems not to be consistent with his declared position, for there he referred to "observations... made on a particle to determine the simultaneous values of a coordinate q and momentum p" and endorsed Heisenberg's thought-experiment involving the notion of a mutual interference between measurements performed on individual systems. It should be recalled in this

[9] Ref. 6-53.

[10] Ref. 6-52.

[11] Ref. 6-52 (p. 974).

[12] Ref. 6-54 (p. 54).

[13] Ref. 6-54 (p. VII)

context that a formulation of the Heisenberg relations within the framework of the statistical interpretation had already been proposed by Popper[14] in 1934.

Why did Kemble commit this interpretative inconsistency if a presentation of the Heisenberg relations in the spirit of the statistical interpretation was already available? As stated in the Preface, Kemble wrote his book to bridge "the gap between the exacting technique of von Neumann and the usual less rigorous formulations of the theory" since von Neumann's work seemed to him too difficult "for any but the most mathematical students of this subject." Kemble simply followed von Neumann. In fact, von Neumann's endorsement of the Bohr and Heisenberg thought-experiments and their original interpretations[15] conflicts with the general tenor of the statistical ensemble interpretation in which his great classic on the mathematical foundations of quantum mechanics was written. Interestingly, von Neumann seems never to have committed himself *verbis expressis* to the statistical view, although his approach to the mathematical treatment of quantum phenomena often cries out for such an interpretation. In any case, D. I. Blokhintsev's carefully worded statement, "I think that a large contribution to the statistical interpretation of quantum mechanics has been given by von Neumann, who stresses the importance of a clear understanding of the quantum ensemble, contrary to the traditional presentation,"[16] is certainly justified.

10.2. IDEOLOGICAL REASONS

It is well known that for several decades the statistical interpretation, compared with the Copenhagen interpretation, gained little acceptance in spite of the arguments voiced by Einstein, Popper, Slater, Kemble, and a few other theoreticians. Most notable among the latter was the eminent physicist Paul Langevin, who popularized Einstein's theories in France. Langevin's plea for the statistical point of view can be traced back to his criticism of the usual interpretation of the Heisenberg relations which he raised in an address at the Inaugural Session of the Réunion Internationale de Chimie Physique in Paris on October 15, 1933.[17] Still, in practice many physicists availed themselves of the logic and also terminology of the

[14]Section 6.2. Cf. also H. Margenau, "Measurement in quantum mechanics," *Annals of Physics* **23**, 469–485 (1963), which presents a lucid analysis of the interpretation of these relations as statistical dispersion relations.

[15]Ref. 1-2 (Chapter 3, section 4).

[16]Letter from D. I. Blokhintsev to the author, dated May 21, 1970.

[17]P. Langevin, *La Notion de Corpuscule et d'Atome* (Hermann, Paris, 1934), pp. 44–46;

statistical interpretation in their daily work, especially when scattering processes or related problems were involved, even if they aligned themselves with the Copenhagen school.[18]

In Soviet Russia where, as intimated earlier,[19] Bohr's complementarity ideas were interpreted as an endorsement of idealistic philosophy and hence as incompatible with dialectical materialism the statistical interpretation was more favorably accepted as an alternative to the Copenhagen view. One of the first, if not the first, in Soviet Russia to subscribe to the statistical interpretation was Konstantin Vjatseslavovits Nikolskii.[20] When Fock published a Russian translation[21] of the Einstein-Podolsky-Rosen paper in 1936 and commented on it in favor of Bohr, Nikolskii[22] sided with Einstein.

A Russian translation of Einstein's article "Physics and Reality"[23] was also published in 1937 in *Pod Znamenem Marksizma*,[24] the most influential Soviet journal of philosophy at that time. Some physicists and philosophers in Moscow and Leningrad hailed it as a welcome antidote to the Copenhagen philosophy and it encouraged Nikolskii to write a monograph entitled *Quantum Processes*[25] in which he put forward a systematic elaboration of the statistical interpretation of quantum phenomena.

Nikolskii's book was not designed to serve as a text for students. The first comprehensive university textbook on quantum mechanics in the Russian language, Dimitrii Ivanovich Blokhintsev's *Introduction to Quantum Mechanics*,[26] was written in the spirit of Heisenberg's interpretation

reprinted (extract) in *P. Langevin—La Pensée et l'Action* (Les Éditeurs Français Réunis, Paris, 1950), pp. 114–116; P. Langevin, *Izbrannje Proizvedenija* (Foreign Literature Publications, Moscow, 1949), pp. 332 et seq.

[18]A relevant example is Heisenberg's discussion of the notion of the orbit of an electron in which he pointed out that through a collision with a single photon of sufficiently short wave length "only a single point of the hypothetical orbit is thus observable. One can, however, repeat this single observation on a large number of atoms, and thus obtain a probability distribution of the electron in the atom. According to Born, this is given mathematically by $\psi\psi^*$... This is the physical significance of the statement that $\psi\psi^*$ is the probability of observing the electron at a given point" Ref. 3–19 (1930, p. 33).

[19]Ref. 7-87.

[20]Ref. 6-170.

[21]V. Fok, "Mozhno li schitat,' chto kvantomekhanicheskoe opisanie fizicheskoi real'nosti iavliaetsia polnym?," *Uspekhi Fisicheskikh Nauk* **16**, 436–457 (1936).

[22]K. V. Nikolskii, "Otvet V. A. Foku," *Uspekhi Fisicheskikh Nauk* **17**, 555 (1937).

[23]Ref. 6-128.

[24]A. Einstein, "Fizika i real'nost," *Pod Znamenem Marksizma* **1937**, 126–151.

[25]K. V. Nikolskii, *Kvantovye Protsessy* (G.I.T.T.L., Moscow, Leningrad, 1940).

[26]D. I. Blokhintsev, *Vvedenie v Kvantovuiu Mekhaniku* (G.I.T.T.L., Moscow, Leningrad, 1944).

according to which the wave function represents man's knowledge of the state rather than the state of the system itself, let alone that of an ensemble of systems. Five years later Blokhintsev published a revised edition, *Fundamentals of Quantum Mechanics*,[27] which by virtue of its excellent didactic approach became one of the most popular textbooks on quantum mechanics ever written in Russian and which was translated into several other languages.[28] In this edition Blokhintsev categorically rejected the Bohr-Heisenberg interpretation[29] and presented the theory on the basis of the statistical interpretation. In fact, as mentioned in the *Preface* of this edition (1949), "the chapter which concerns the concept of state in quantum mechanics has been changed,...and the idealistic conceptions of quantum mechanics which are now widespread abroad are subjected to criticism."

That the notorious ideological conflict between Markov and Maksimov and Zdanov's programmatic speech[30] may have prompted Blokhintsev to revise his position has been intimated by L. I. Stortschak in his review of the revised edition.[31]

According to Blokhintsev's interpretation, modern quantum mechanics is not a theory of microprocesses but studies their properties by employing statistical ensembles which are described in terms borrowed from classical macroscopic physics (e.g., energy, impulse, coordinates). In his theory of the measurement process measuring instruments are regarded as spectral analyzers of quantum ensembles which select from the given ensemble certain subensembles in accordance with the nature of the instrument or separate an ensemble (pure state) into a mixture of subensembles (mixture of states). The assignment of a new wave function to such a subensemble corresponds to what is usually called the "reduction of a wave packet." "Physically, a reduction means that a particle belongs to a new pure ensemble after a measurement."[32]

Blokhintsev's basic contention that quantum mechanics eliminates the observer and becomes objectively significant due to the fact that the wave function describes not the state of a particle but the particle's belonging to

[27] D. I. Blokhintsev, *Osnovy Kvantovoi Mekhaniki* (G.I.T.T.L., Moscow, Leningrad, 1949).

[28] Ref. 6-12.

[29] Ref. 27 (section 14, pp. 55 *et seq.*).

[30] Refs. 6-175, 6-176, 6-177.

[31] L. I. Stortschak, "Za materialisticheskoje osveshenije osnov kvantovoj mekhaniki," *Voprosy Filosofii* **1950**, 202–205; "Zur materialistischen Deutung der Grundlagen der Quantenmechanik," *Sowjetwissenschaft (Naturwissenschaftliche Abteilung)* **1952**, 176–181.

[32] Ref. 6-12 (English edition, pp. 65–70).

a certain ensemble was criticized by Heisenberg[33] as self-contradictory, for to assign a particle to an ensemble, Heisenberg argued, requires some kind of knowledge about the particle on the part of the observer.

In subsequent publications[34] and especially in a recent monograph[35] on the foundations of quantum mechanics Blokhintsev gave a more detailed account of his statistical interpretation. He defined ensembles as (ideally) infinite sequences of identical microsystems each of which is found in the same macrosetting M (set of macroscopic bodies such as collimating slits, magnetic analyzers); the macroenvironment somehow determines the state of motion, or simply the "state," of the microsystem. It is a "quantum ensemble" if the indeterminacy relation for position and momentum coordinates q,p (averaged over the ensemble) is satisfied, a condition which excludes the existence of a joint probability distribution of p and q in quantum ensembles (in contrast to the classical Gibbs ensembles). To find what replaces such probability distributions for quantum ensembles Blokhintsev, generalizing the Heisenberg relations, had recourse to Bohr's principle of complementarity which, if stripped of all its philosophical trappings, he regarded merely as a "principle of exclusiveness": it divides dynamical variables into mutually exclusive groups which do not coexist in quantum ensembles. Although microsystems cannot be described, according to this principle, in terms of a phase space $R(p,q)$, they can be described in terms of a configuration space $R(q)$ or in terms of a momentum space $R(p)$ or in terms of the space of any other complete set of dynamical variables; but each description yields a different probability measure.

There is, however, a quantity which characterizes the quantum ensemble completely in the sense that its knowledge enables one to calculate all these different probability measures: it is the wave function ψ_M which thus describes the quantum ensemble either in the coordinate representation or in the momentum representation or, for that matter, in the representation of any other complete set of dynamical variables and, as indicated by the

[33]Ref. 3-4.

[34]Ref. 7-63

[35]D. I. Blokhintsev, *Printsipalnye Voprossy Kvantovoi Mekhaniki* (Nauka, Moscow, 1966); *Principes Essentiels de la Mécanique Quantique* (Dunod, Paris, 1968); *The Philosophy of Quantum Mechanics* (Reidel, Dordrecht-Holland; Humanities Press, New York, 1968). According to Blokhintsev, the French translation "is closer to the Russian original" (Letter from Blokhintsev to the author, dated May 21, 1970). The English title is misleading since, as stated explicitly in the Preface, the book "is concerned more with theoretical physics than with philosophy." Misguided by the title, a recent reviewer wrote that the book's contribution to the philosophy of quantum mechanics "is essentially nil." Cf. J. Bub's review in *Philosophy of Science* **37**, 153–156 (1970).

subscript M, depends on the macroscopic setting. Though capable of assuming various representations, or precisely because of that, the wave function is an objective characteristic of the quantum ensemble. Playing for such ensembles a role similar to that of the classical joint probability for Gibbs ensembles—M being analogous to the absolute temperature θ in the canonical distribution—"the wave function is not the quantity that determines the statistics of any particular measurement but one that determines the statistics of a quantum ensemble."[36]

For the sake of historical accuracy it should be pointed out that Leonid Isaakovits Mandelstam, as a close analysis of his lectures[37] indicates, also adopted a viewpoint similar to that of Blockhintsev. Fock, on the other hand, criticized Blokhintsev's approach on the ground that the wave function denotes something potentially possible and not something actually realized; in his view, the notion of statistical ensembles in quantum mechanics is legitimate as far as it refers to the results of measurements, each ensemble being associated with a specific experimental arrangement, but not with the micro-objects themselves.[38] The conflict between the positions of Blokhintsev and Fock became a subject of numerous discussions in Soviet Russia in the 1950s.[39]

10.3. FROM POPPER TO LANDÉ

In the Western countries, at that time, the statistical ensemble interpretation had very few committed adherents. One reason for this state of affairs was undoubtedly the great authority which Bohr, Heisenberg, and the other prominent protagonists of the Copenhagen interpretation enjoyed in the world of quantum mechanics. To understand the second, less obvious reason, we should recall that in those years von Neumann's impossibility proof of hidden variables was still generally regarded as absolutely conclusive. Since, it was argued, it is an incontestable fact that individual physical systems exist in nature as objects of experimental research, it would be reasonable to expect that a statistical interpretation is only the first step toward a theory that describes the behavior of individual sys-

[36] Ref. 6-12 (English edition, p. 25).

[37] L. I. Mandelstam, *Polnoje Sobrannje Trudov* (Academy of Science USSR, Moscow, 1950), Vol. 5, esp. p. 356.

[38] V. A. Fock, "O tak jazyvajemykh ansambljakh v kvantovoj mekhanike," *Voprossy Filosofii* **1952**, 170–174; Ref. 7-92.

[39] Cf., e.g., A. D. Alexandrov, "O smyslje volnovoj funktsii," *Doklady Akademii Nauk SSSR* **85**, 292–294 (1952). G. J. Mjakishev, "V chjem prichina statisticheskovo karaktera kvantovoj mekhaniki?," *Voprossy Filosofii* **1954**, no. 6, 146–159.

tems; but such a theory, to conform with the spirit of the statistical standpoint, would necessarily be a hidden variable theory whose logical validity had been disproved.

There were, however, a few exceptions. Karl R. Popper, who, as we recall,[40] had proposed a statistical interpretation of the Heisenberg relations in the early 1930s never abandoned his point of view. Although he once[41] explicitly declared that a theory which implies statistical consequences and which can be tested only by statistical tests need not have a statistical meaning he always held that the problems to which quantum mechanics is applied are essentially statistical problems and as such require statistical answers. In fact, according to Popper, the vectors in Hilbert space provide statistical assertions from which no predictive inferences can be drawn about the behavior of individual particles. Quantum mechanics qua statistical theory does not rule out, however, the possibility of exact single measurements, for example, of position and momentum; on the contrary, Popper declared, such measurements are indispensable for testing the predictions of the theory: to test, say, the Heisenberg relations which predict the reciprocity in the spreads of conjugate variables the statistical distributions of the latter must be determined; this is possible only if the measurements are far more precise than the range of spread; these highly accurate measurements are precisely those retrodictive or nonprognostic measurements which Heisenberg erroneously regarded as theoretically insignificant.[42]

An even more serious error, in Popper's view, was committed by the Copenhagen school in identifying a statistical distribution with a physical property of the elements of the ensemble: distributions are properties of ensembles but not properties of the elements of ensembles. This illogical conflation of concepts, Popper declared, was "the great quantum muddle" which led to the emergence of the notorious "wave-particle duality" and hence to the complementarity interpretation.[43]

Popper's original conception of the nature of quantum mechanics was significantly modified by his interpretation of probability as a propensity, that is, as a physical property comparable to symmetry or asymmetry or

[40] See Sections 3.1 and 6.2.

[41] K. R. Popper, "Philosophy of science: A personal report," in *British Philosophy in the Mid-Century*, C. A. Mace, ed. (Allen and Unwin, London, 1957), pp. 153–191; reprinted in K. R. Popper, *Conjectures and Refutations* (Routledge and Kegan Paul, London, 1963), pp. 33–96, esp. p. 60.

[42] Ref. 4-39. See also the important footnote on p. 231 in Popper's *The Logic of Scientific Discovery* (1959), Ref. 3-15, where a detailed exposition of this argument is given.

[43] K. R. Popper, "Quantum mechanics without 'the observer,'" in *Quantum Theory and Reality*, M. Bunge, ed. (Springer, New York, 1967), pp. 7–44.

some kind of generalized forces, pertaining to the whole experimental arrangement for repeatable measurements. Although not the first to propose such a conception,[44] Popper developed his propensity interpretation, apparently independently in 1953 as a refinement of the frequency interpretation of probability.[45] In his contribution to the 1957 Colston Symposium[46] he claimed that this probability interpretation "takes the mystery out of quantum theory, while leaving probability and indeterminism in it."

In this interpretation the wave function determines the propensity of the states of the particle, that is, it gives weights to its possible states. Applied to the two-slit experiment, for example, it explains the futility of the wave-particle duality which has so often been inferred from such experiments, for every change in the experimental arrangement such as the shutting of one slit, affects the distribution of weights to the various possibilities and thus produces a different wave function. In Popper's view, the situation is in principle not different from tilting an ordinary pinboard as a consequence of which the new distribution curve of the little balls rolling down differs from its normal shape (when the board is not tilted). In brief, according to Popper, quantum mechanics, if interpreted as "a generalization of classical statistical mechanics"[47] of particles, subject to the propensity interpretation of probability, has nothing more mysterious in it than any classical theory of any game of chance, for the features

[44]According to N. L. Rabinovitch, Moses ben Maimon, better known as Maimonides (1135–1204), already suggested a propensity interpretation of probability when, in the Introduction to Part Two of his *Guide to the Perplexed,* he referred to possibilities that "are inherent in the capacity or propensity of matter to receive given forms" (Premises 23 and 24). Maimonides' conception of probability was subsequently taken up by Albertus Magnus, Thomas Aquinas, and other Schoolmen. Cf. N. L. Rabinovitch, *Probability and Statistical Inference in Ancient and Medieval Jewish Literature* (University of Toronto Press, Toronto, 1973), pp. 74–76; and E. Gilson, *Le Thomisme* (Vrin, Paris, 1923), pp. 60–61. A dispositional theory of probability, to mention a more modern approach precursory to Popper's, was also proposed by Charles Sanders Peirce (1839–1914), who wrote: "The die has a certain 'would-be'...a property quite analogous to any *habit* that a man may have." Cf. Ref. 8–211 (Vol. 2, section 664). This interpretation has also been referred to in B. Braithwaite's book *Scientific Explanation* (Cambridge University Press, Cambridge, 1953), p. 187. Recent protagonists of the propensity interpretation are I. Hacking (*Logic of Statistical Inference,* Cambridge University Press, 1965), I. Levi (*Gambling with Truth,* Knopf, New York, 1967), and D. H. Mellor (*The Matter of Chance,* Cambridge University Press, Cambridge, 1971).

[45]Ref. 41; K. R. Popper, "Three views concerning human knowledge," in *Contemporary British Philosophy,* H. D. Lewis, ed, (G. Allen and Unwin, London, 1956), pp. 355–388.

[46]K. R. Popper, "The propensity interpretation of the calculus of probability, and quantum mechanics," Ref. 7-102 (pp. 65–70).

[47]Ref. 43 (p. 16).

which so far have been regarded as peculiarities of quantum mechanics are but aspects of probability and common even to devices such as a pinboard or a die in a beaker.[48]

Popper's interpretation and his polemics against Bohr were severely criticized by Paul K. Feyerabend in a comprehensive vindication of the complementarity interpretation.[49] As far as the propensity interpretation is concerned, Feyerabend contended, "Popper stands much closer to Bohr whom he attacks than to Einstein whom he defends." For Popper's insistence that the experimental conditions of the whole physical setup determine the probability distribution is, in Feyerabend's view, precisely what Bohr had in mind when he used the notion of a "phenomenon" to include the account of the whole experimental arrangment. But complementarity, Feyerabend continued, goes beyond the propensity interpretation by taking out of the individual physical system and attributing to the experimental arrangement not only probability but also the dynamical variables of the system such as position and momentum: it thus relativizes not only probability but all dynamical magnitudes. Popper's argument that a change in experimental conditions implies a change in probabilities does not suffice to account for the kind of changes encountered, say, in the two-slit experiment or its variations.

To refute Popper's interpretation of the Heisenberg relations Feyerabend reexamined such a diffraction experiment[50] by viewing it as a variation of Popper's pinboard model. Popper's claim concerning the existence of simultaneously definite particle positions and momenta could be consistently maintained, Feyerabend declared, only if the redistribution of the particles' trajectories, as manifested in the "interference" pattern on the screen, can be dynamically accounted for in terms of certain forces and in conformance with the conservation laws whose validity in each individual case had been empirically demonstrated by Bothe and Geiger as well as by Compton and Simon.[51] Since, however, no such forces are known to exist and since, moreover, any attempt even at just conceiving the existence of such forces is bound to raise insuperable conceptual difficulties, as

[48]Interestingly, Lawrence Sklar in an analysis of Popper's propensity interpretation arrived at the almost diametrically opposite conclusion that in classical situations this dispositional view, "ignoring the one objective feature of the world that gives probability its point and usefulness," is untenable while in quantum mechanics—and so far there alone—it may be acceptable. Cf. L. Sklar, "Is probability a dispositional property?," *The Journal of Philosophy* **67**, 355–366 (1970).

[49]P. K. Feyerabend, "On a recent critique of complementarity," *Philosophy of Science* **35**, 309–331 (1968); **36**, 82–105 (1969).

[50]*Op. cit.*, p. 94.

[51]Ref. 1-1 (pp. 185–186).

Einstein and Ehrenfest had shown,[52] it follows that such a dynamical account cannot be given.

The way out of this dilemma was Bohr's renunciation of particle trajectories, his denial that particles possess well-defined positions together with well-defined momenta. By resorting to the wave model and de Broglie's relation (qua empirical law), Feyerabend continued, Bohr arrived at the relation $\Delta x \, \Delta p > h$ in which, contrary to Popper's conception, Δx and Δp are not statistical magnitudes but "describe the extent to which concepts such as position and momentum are still applicable."[53] This relation, Feyerabend emphasized, is not just a restriction of our knowledge but expresses the absence of an objective feature in the world, just as Bohr's not infrequent use of subjectivistic terms, if more closely analyzed, referred not to a state of knowledge but to objective conditions. Bohr's conception of the Heisenberg relations as expressions of some objective indefiniteness of the individual system does not preclude their derivation as statistical formulae in which Δ denotes the root-mean-square deviation. In fact, the relations as conceived by Bohr may be regarded, according to Feyerabend, as a test as well as an explanation of these relations in their statistical interpretation: they explain why measurement results must always agree with the statistical relations, and they are a test insofar as they indicate the agreement between the quantum theory together with Born's interpretation and all the physical laws known to be valid in microphysics. Popper's allegation which he referred to as "the great quantum muddle" is in Feyerabend's opinion "nothing but a piece of fiction" to which Popper was led because he confused classical waves with ψ-waves and neglected the dynamics of the individual particle, being misled by his unwarranted conception that the quantum theory is pure statistics.

Popper's criticism of the Copenhagen interpretation, Feyerabend declared, is "a big and unfortunate step back from what has already been achieved in 1927"[54] for it neglects important facts which proved indispensable for a proper evaluation of complementarity. Thus Popper's claim that the famous "reduction of the wave packet" is "not an effect characteristic of quantum theory but of probability theory in general"[55] is according to Feyerabend based on a serious default. In his ninth thesis—Popper presented his interpretation in the form of 13 theses—Popper referred to the pinboard experiment to explain this "reduction." He called the original specification of the experimental conditions e_1 and the new specification,

[52]Ref. 1-1 (pp. 134–135).
[53]Ref. 49 (p. 95).
[54]Ref. 49 (p. 103).
[55]*Ibid.*, p. 34.

when (say) only those balls are considered (or selected) which have hit a certain pin q_2, e_2. Then it is obvious, Popper pointed out, that the two probabilities for a ball to reach a certain point a at the bottom of the board, $p(a, e_1)$ and $p(a, e_2)$, will not generally be equal because the two experiments specified by e_1 and e_2 are not the same. But according to Popper this "does not mean that the new information which tells us that the conditions e_2 are realized in any way changes $p(a, e_1)$: from the very beginning we could calculate $p(a, e_1)$ for the various a's, and also $p(a, e_2)$; and we knew that $p(a, e_1) \neq p(a, e_2)$. Nothing has changed if we are informed that the ball has actually hit the pin q_2, except that we are now free, *if we so wish*, to apply $p(a, e_2)$ *to this case*; or in other words, we are free to look upon the case as an instance of the experiment e_2 instead of the experiment e_1. But we can, of course, continue to look upon it as an instance of the experiment e_1, and thus continue to work with $p(a, e_1)$: the probabilities (and also the probability packets, i.e., the distribution for the various a's) are *relative probabilities*: they are *relative to what we are going to regard as a repetition of our experiment*; or in other words, they are relative to what experiments are, or are not, *regarded as relevant to our statistical test*."[56]

Popper's conclusion that a change in experimental conditions also changes the probabilities is fully shared by Feyerabend as being absolutely correct—but equally irrelevant! "For what surprises us (and what led to the Copenhagen Interpretation) is not the fact that there is *some change*; what surprises us is the *kind of change* encountered: trajectories which from a classical standpoint are perfectly feasible are suddenly forbidden and are not entered by any particle. It is in order to explain *these* curious occurrences that the Copenhagen interpretation was gradually built up."[57]

This last point was further elaborated by Jeffrey Bub,[58] who was among those to whom Feyerabend had shown an earlier version of his paper. To prove that Popper's interpretation cannot resolve the foundational problems of quantum mechanics Bub referred to Popper's just quoted pinboard example and ascribed to the set of trajectories which hit the pin q_2 and another pin q_3 the symbol b and to the set of trajectories which hit the pin q_2 and a fourth pin q_4 the symbol c. That $p(b, e_1) \neq p(b, e_2)$ and $p(c, a_1) \neq p(c, e_2)$, he pointed out, is not at all puzzling; but that $p(b, e_1)/p(c, e_1) \neq p(b, e_2)/p(c, e_2)$, an inequality which is characteristic for the quantum mechanical reduction of the wave packet, remains incomprehensible in Popper's interpretation. For what Popper ignored, according to Bub, is the

[56]Ref. 43, (pp. 35–36).

[57]Ref. 49 (p. 326).

[58]J. Bub, "Popper's propensity interpretation of probability and quantum mechanics" (Preprint, 1972).

fact that in quantum mechanics the transition of an initial probability distribution into a new probability distribution conditional on certain additional information is not the same as in the Boolean probability calculus for in quantum mechanics additional information invalidates any initial information concerning the relative probabilities of subsets in the phase space used in the phase space reconstruction of quantum statistics. The object of Bub's censure of Popper's reasoning is ultimately the same as that of the criticism raised against Popper by Jauch and Piron in another context,[59] namely the illegitimacy of applying Boolean logic to a non-Boolean logical space.

If for Popper the wave-particle duality principle of the Copenhagen interpretation originated in a *logical* fallacy, for Landé it resulted from a serious misconstruction of purely *physical* arguments. Alfred Landé, whose contributions to quantum theory and atomic structure are of course well known to every student of physics, studied at the Universities of Marburg, Göttingen, and Munich where in 1914 he obtained his Ph.D. degree under Arnold Sommerfeld. After serving as assistant to David Hilbert in Göttingen and as *Privatdozent* in Frankfurt he became in 1922 professor in Tübingen, which, due to his work, became one of the foremost centers for research in spectroscopy. In 1931 he left for Ohio State University, where in 1951 he published a textbook on quantum mechanics,[60] which, like David Bohm's book[61] (also published in 1951), was designed to clarify the physical meaning of the theory. Like Bohm's, it was written in the spirit of the Copenhagen interpretation, Bohm's being inclined more toward Bohr's, Landé's more toward Heisenberg's version.

In his book Landé declared that "it is the task of the quantum theory to reconcile the contradiction between the two classical concepts" of particle and wave by proving the equivalence of the descriptions of physical phenomena in either terms. In fact, Landé showed in great detail how the particle and wave theories lead to identical results for Rutherford scattering of matter waves, for Thomson scattering of light rays, for the normal Zeeman effect, and for many other processes. With special emphasis he demonstrated how Laue's undulatory explanation of X-ray diffraction by crystals can be matched with Duane's corpuscular interpretation, by comparing Bragg's formula $2d\sin\vartheta = n\lambda$ with Duane's equation[62] $2p\sin\vartheta = nP$.

[59]Ref. 8-43.

[60]A. Landé, *Quantum Mechanics* (Pitman, London, 1951). Landé's earlier *Principles of Quantum Mechanics* (Cambridge University Press; Macmillan, New York, 1937) also professed the Copenhagen point of view.

[61]Ref. 7-59.

[62]W. Duane, "The transfer in quanta of radiation momentum to matter," *Proceedings of the National Academy of Science (Washington)* **9**, 158–164 (1923).

On closer inspection, however, one may already discern in Landé's book, especially in the *Retrospect* (pp. 298–300), symptoms of a new approach. Although still admitting that "both wave theory and particle theory are self-consistent schemes" he emphasized that in diffraction experiments "particles produce maximum and minimum intensity of diffraction through a perfectly normal mechanical process which, however, is formally *related* to the wave explanation." It should also be noted that nowhere in his book did Landé mention explicitly Bohr's notion of complementarity.

Shortly after the publication of his book—just as happened with Bohm—Landé withdrew from the Copenhagen school and became one of its most ardent opponents.[63] In his view the orthodox interpretation, by dogmatically declaring duality to be irremovable, elevated it to the rank of a fundamental principle, thus "talking us out of a difficult problem of theoretical physics rather than solving it by the means and methods of theoretical physics itself." Thus, dissatisfied with the explanatory contents of the Copenhagen interpretation, which in his view supplanted constructive research in physics by a refinement of its language, Landé thought it imperative to do for quantum mechanics what Descartes had done for philosophy: to begin anew by laying the foundations on postulates or principles which are "simple, plausible, and almost self-evident." To interpret the theory was for him tantamount to constructing it as a straightforward consequence of a few fundamental principles such as continuity, symmetry, and invariance. In contrast to Schrödinger's *unitary wave interpretation* and Bohr's *dualistic wave-particle interpretation* he called his approach the *unitary particle interpretation*, which turned out to be a particular version of the statistical ensemble interpretation. Since 1952 he has concentrated on elaborating his point of view in a long series of publications.[64]

In contrast to the proponents of hidden variable theories Landé denied that microphysical probability can be reduced to classical determinacy; on the contrary, determinism, he declared, "fails not only in accounting for the results of any honest 'classical' game of chance, there is no possibility

[63]"Just when writing that textbook my conscience concerning dualism became worse and worse. I was also always bothered by the insufficient 'solutions' of the Gibbs paradox." Letter from Landé to the author, dated May 24, 1971.

[64]A. Landé, "Quantum mechanics and thermodynamic continuity," *American Journal of Physics* **20**, 353–358 (1952); "Thermodynamic continuity and quantum principles," *Physical Review* **87**, 267–271 (1952); "Probability in classical and quantum theory," in Ref. 6-111 (pp. 59–64); "Quantum mechanics—A thermodynamic approach," *American Scientist* **41**, 439–448 (1953); "Continuity, a key to quantum mechanics," *Philosophy of Science* **20**, 101–109 (1953); "Thermodynamische Begründung der Quantenmechanik," *Die Naturwissenschaften* **41**, 125–131, 524–525 (1954); "Quantum indeterminacy, a consequence of cause-effect continuity,"

Dialectica **8**, 199–209 (1954); "Quantum mechanics and thermodynamic continuity II," *American Journal of Physics* **22**, 82–87 (1954); "Le principe de continuité et la théorie des quanta," *Journal de Physique et du Radium* **16**, 353–357 (1955); *Foundations of Quantum Theory* (Yale University Press, New Haven, Conn., 1955); "Quantum mechanics and common sense," *Endeavour* **15**, 61–67 (1956); "ψ-superposition and quantum rules," *American Journal of Physics* **24**, 56–59 (1956); "The logic of quanta," *The British Journal for the Philosophy of Science* **6**, 300–320 (1956); "Déduction de la théorie quantique à partir de principes non-quantiqùes," *Journal de Physique et du Radium* **17**, 1–4 (1956); "Quantentheorie auf nicht-quantenhafter Grundlage," *Die Naturwissenschaften* **43**, 217–221 (1956); "ψ-superposition and quantum periodicity," *Physical Review* **108**, 891–893 (1957); "Wellenmechanik und Irreversibilität," *Physikalische Blätter* **13**, 312–316 (1957); "Non-quantal foundations of quantum theory," *Philosophy of Science* **24**, 309–320 (1957); "Quantum physics and philosophy," *Current Science* **27**, 81–85 (1958); "Quantum theory from non-quantal postulates," in *Berkeley Symposium on the Axiomatic Methods* (North-Holland Publishing Company, Amsterdam, 1958), pp. 353–363; "Determinism versus continuity," *Mind* **67**, 174–181 (1958); "Ist die Dualität in der Quantentheorie ein Erkenntnisproblem," *Philosophia Naturalis* **5**, 498–502 (1958); "Zur Quantentheorie der Messung," *Zeitschrift für Physik* **153**, 389–393 (1958); "Heisenberg's contracting wave packets," *American Journal of Physics* **27**, 415–417 (1959); "Quantum mechanics from duality to unity," *American Scientist* **47**, 341–349 (1959); "From dualism to unity in quantum mechanics," *The British Journal for the Philosophy of Science* **10**, 16–24 (1959); *From Dualism to Unity in Quantum Theory* (Cambridge University Press, London, New York, 1960); "From duality to unity in quantum mechanics," in *Current Issues in the Philosophy of Science* (Holt, Rinehart and Winston, New York, 1961), pp. 350–360; "Unitary interpretation of quantum theory," *American Journal of Physics* **29**, 503–507 (1961); "Dualismus, Wissenschaft und Hypothese" in Ref. 2-30 (1961, pp. 119–127); "Ableitung der Quantenregeln auf nicht-quantenmässiger Grundlage," *Zeitschrift für Physik* **162**, 410–412 (1961); "Warum interferieren die Wahrscheinlichkeiten?," *Zeitschrift für Physik* **164**, 558–562 (1961); "The case against quantum duality," *Philosophy of Science* **29**, 1–6 (1962); "Von Dualismus zur einheitlichen Quantentheorie," *Philosophia Naturalis* **8**, 232–241 (1964); "Why do quantum theorists ignore the quantum theory?," *The British Journal for the Philosophy of Science* **15**, 307–313 (1965); "Solution of the Gibbs entropy paradox," *Philosophy of Science* **32**, 192–193 (1965); "Non-quantal foundations of quantum mechanics," *Dialectica* **19**, 349–357 (1965); "Quantum fact and fiction," *American Journal of Physics* **33**, 123–127 (1965), **34**, 1160–1163 (1966); *New Foundations of Quantum Mechanics* (Cambridge University Press, London, New York, 1965); "Quantum theory without dualism," *Scientia* **101**, 208–212 (1966); "Observation and interpretation in quantum theory," in *Proceedings of the Seventh Inter-American Congress of Philosophy* (Presses de l'Université Laval, Quebec, 1967), pp. 297–300; "New foundations of quantum physics," *Physics Today* **20**, 55–58 (1967); "Quantum physics and philosophy," in *Contemporary Philosophy* (La Nuova Italia Editrice, Firenze, 1968), pp. 286–297; "Quantum observation and interpretation," in *Akten des XIV. Internationalen Kongresses für Philosophie* (Herder, Vienna, 1968), pp. 314–317; "Quantenmechanik, Beobachtung und Deutung," *International Journal of Theoretical Physics* **1**, 51–60 (1968); "Dualismus in der Quantentheorie," *Philosophia Naturalis* **11**, 395–396 (1969); "Wahrheit und Dichtung in der Quantentheorie," *Physikalische Blätter* **25**, 105–109 (1969); "Quantum fact and fiction III," *American Journal of Physics* **37**, 541–548 (1969); "The non-quantal foundations of quantum mechanics," in *Physics, Logic, and History* (Plenum Press, New York, London, 1970), pp. 297–310; "Unity in quantum theory," *Foundations of Physics* **1**, 191–202 (1971); "The decline and fall of dualism," *Philosophy of Science* **38**, 221–223 (1971); "Einheit in der Quantenwelt," *Dialectica* **26**, 115–130 (1972); *Quantum Mechanics in a New Key* (Exposition Press, New York, 1973). "Albert Einstein and the quantum riddle," *American Journal of Physics* **42**, 459–464 (1974).

of carrying out a program of reducing classical thermodynamics to deterministic mechanics, notwithstanding the many efforts of deriving the Second Law on a deterministic mechanical basis."[65] Why, then, has indeterminacy to be regarded as a basic feature in physics? In search of an answer to this question Landé was led to the first fundamental principle of his theory, *the principle of cause-effect continuity*. "Indeterminacy for discontinuous transitions between states of an object, the heart of the quantum doctrine, is indeed the necessary counterpart of a principle of broad generality, that of continuity for deterministic cause-effect relations," he contended.

Crediting Leibnitz[66] as the originator of this principle Landé rephrased it as follows: "A finite change of effect requires a finite change of cause."[67] To explain his argument Landé then discussed the example of a ball-knife game in which balls are dropped from a chute upon a knife-edge so that at first all balls fall, say, to the right; the initial conditions are then slowly changed so that finally all balls fall to the left; since there were only infinitesimal (i.e., nonfinite) changes in the cause, no finite changes in the effect could be produced; according to the continuity principle there must have been therefore a situation in which, for a certain range of the initial conditions, the effect (right or left) could not change abruptly; that is, there must be a statistical frequency ratio which varies continuously between the extremes of certainty for right and left. Indeterminacy, resulting from the continuity of the cause-effect relation, must thus be acknowledged as a basic feature of the physical world. Originally Landé justified the principle of thermodynamic continuity, as he called it in the early 1950s, as a necessary prerequisite to solving the Gibbs paradox.

Having thus introduced probability, Landé considered, for a given mechanical system, a physical quantity A (e.g., energy) which, if the system is subjected to a measurement of A by means of an A-meter or A-filter, assumes the values $A_1, A_2, \ldots, A_m$ (the condition of the finiteness of the number of eigenvalues can later be relaxed). If the system in state $A_k (1 \leqslant k \leqslant m)$ is subsequently subjected to the measurement of another physical quantity B, capable of assuming the values $B_1, B_2, \ldots, B_n$, there exists, in conformance with the postulate of reproducibility, a certain statistical frequency or transition probability $P(A_k, B_j)$ for the value of B to be B_j. These experimentally determinable transition probabilities form a matrix

$$\begin{pmatrix} P(A_1,B_1) & P(A_1,B_2) & \cdots \\ P(A_2,B_1) & P(A_2,B_2) & \cdots \\ \cdots & & \end{pmatrix} = (P_{AB})$$

[65]Ref. 64 (1953, p. 59).

[66]G. W. Leibnitz' letter to Pierre Bayle of 1687.

[67]Ref. 64 (1960, p. 18).

in which each row sums up to unity. By defining the "fractional degree of equality" between A_k and B_j as the statistical "passing fraction" of A_k state systems through a B_j filter and invoking the reversibility of processes in classical mechanics[68] Landé provided plausible arguments for "the symmetry principle"

$$P(A_k, B_j) = P(B_j, A_k)$$

from which he deduced that each column in (P_{AB}) also sums up to unity and that the matrix is quadratic.

Landé then studied the mathematical relations that have to hold among the stochastic matrices if (P_{AB}) and (P_{BC}) are to determine (P_{AC}). The above-mentioned summation restrictions in addition to certain group-theoretical requirements such as the condition that (P_{AB}) if combined with (P_{BA}) yield (P_{AA}) as a unit matrix[69] enabled him to construct a "metric of probabilities"[70] which turned out to be the metric of unitary transformations and to be identical with the law of superposition for probability amplitudes. For the conditions imposed are so restrictive that only one correlation law is conceivable. To every $P = P(A_k, B_j) = P(B_j, A_k)$ there correspond two vectors, $\psi(A_k, B_j)$ and $\psi(B_j, A_k)$, both of magnitude $P^{1/2}$ and of opposite directions with respect to a fixed axis in their plane. The correlation

$$\psi(A, C) = \psi(A, B) \times \psi(B, C)$$

where $\times$ denotes matrix multiplication or, in more detail,

$$\psi(A_k, C_m) = \sum_j \psi(A_k, B_j)\psi(B_j, C_m)$$

subject to

$$P = |\psi|^2,$$

which is the only simple and general solution of the metrical problem, expresses the "interference law of probability amplitudes." Conventional quantum mechanics, Landé pointed out, regards this as a basic principle underlying the wave-particle duality. But, as Landé claimed to have shown, it is merely "the only conceivable way for nature to geometrize; that is, to establish a general law between the various transition probabilities, rather than leaving them a chaotic array of unrelated entities."[71]

[68]Ref. 64 (1961, p. 506).
[69]Ref. 64 (1952, p. 268).
[70]Ref. 64 (1957, **24**, pp. 316–318).
[71]Ref. 64 (1961, pp. 358–359).

To derive the wavelike function for the probability amplitude $\psi(q,p)$,

$$\psi(q,p) = \text{const.}\exp\left(\frac{2\pi iqp}{h}\right)$$

and with its help the quantum rules $p = h/\lambda$ and $E = h\nu$ Landé postulated[72] the following invariance principle: Any observable $T(q)$ has matrix elements $T_{pp'}$ which depend only on the difference $p-p'$; similarly, any observable $S(p)$ has matrix elements $S_{qq'}$ which depend only on the difference $q-q'$. That is, there are no preferred zero points in ordinary or in momentum space. Analogous conditions apply to E and t. Now, to derive the above-mentioned wavelike function Landé applied the transformation

$$T_{pp'} = \int \psi_{pq} T(q) \psi_{qp'} dq$$

to the special case of the Dirac function $T(q) = \delta(q-q_0)$. Hence, by virtue of the invariance postulate, the corresponding $T_{pp'}$ or $\psi_{pq_0}\psi_{q_0p'} = \psi_{pq_0}\psi^*_{p'q_0}$ must be a function of $p-p'$ alone. Thus, replacing q_0 by q, we obtain $\psi_{pq}\psi^*_{p'q} = f(p-p')$. Landé now suggested expanding the ψ into power series in p and p', respectively,

$$\psi_{pq} = a(1 + a_1 p + a_2 p^2 + \cdots)$$

$$\psi^*_{p'q} = a^*(1 + a^*_1 p' + a^*_2 p'^2 + \cdots)$$

where a_k are functions of q alone, to multiply the two series and to order the product with respect to terms linear, quadratic, cubic, and so on, in p and p'. This product is a function of $p-p'$ alone if $a_1 = i\alpha(q)$, where $\alpha(q)$ is real, and if $a_2 = \frac{1}{2}(i\alpha(q))^2$, and so on. In other words,

$$\psi_{pq} = a_0(q)\exp[i\alpha(q)p].$$

Interchanging q and p and applying the same considerations again, Landé obtained

$$\psi_{pq} = b_0(p)\exp[i\beta(p)q] = \psi^*_{pq}$$

where $\beta(p)$ is real. Comparing $\psi_{pq} = b^*_0(p)\exp[-i\beta(p)q]$ with $\psi_{pq} = a_0(q)$ $\exp[i\alpha(q)p]$ showed him that $a_0(q) = b^*_0(p)$ and $\alpha(q)/q = -\beta(p)/p$, that is, that a_0 and $\alpha(q)/q$ are constants. He thus obtained the wavelike function $\psi_{pq} = \text{const.}\exp[iqpc]$, as desired, where c is a constant whose value $\hbar^{-1}$ has

[72]Ref. 64 (1954, p. 86; 1960, p. 57; 1961, pp. 410–412).

to be found from experiment.

Finally, to see how Landé derived the quantum rules $p = h/\lambda$ and $E = h\nu$ from his basic principles let us consider a "one-dimensional crystal," periodic in q with period l, so that $T(q)$ assumes the same value at q as at $q+l$, $q+2l$, and so forth. $T(q)$ may therefore be expanded into a Fourier series with periodicities l/n:

$$T(q) = \sum_n c_n \cos\left(\frac{2\pi nq}{l} + \alpha_n\right)$$

or

$$T(q) = \sum_n \frac{1}{2} c_n \exp\left(\frac{\pm 2\pi inq}{l} \pm i\alpha_n\right).$$

In view of the fact that $\psi(q,p) = \text{const.}\exp(2\pi ipq/h)$ the general transformation formula

$$T_{pp'} = \sum_{q,q'} \psi_{pq} T_{qq'} \psi_{q'p'}$$

reduces for a function $T(q) = T_{qq'}\delta_{qq'}$ to

$$T(p,p') = \int T(q)\exp\left[\frac{2\pi i(p-p')q}{h}\right] dq$$

so that with $T(q)$ as given above,

$$T(p,p') = \sum_n c_n \int \exp\left[2\pi i\left(\frac{p-p'}{h} \pm \frac{n}{l}\right)q \pm i\alpha_n\right] dq,$$

which differs from zero only if the coefficient of q in the integrand vanishes, that is, if $\Delta p = p - p' = \pm nh/l$. Thus if l is the distance between parallel lattice planes in a crystal, the momentum perpendicular to these planes will change only by amounts Δp as given by this formula. When these selective momenta are imparted to incident particles, the latter will be deflected, as a simple geometrical consideration shows, precisely into the directions which the undulatory interference theory (von Laue, Bragg) provides by its association of λ with h/p. Duane's purely corpuscular account of diffraction does not need to invoke, as Landé put it, "the supernatural forth-and-back transmutation by a mysterious 'dual manifestation'" of particles into waves and waves into particles and does not confuse the object and its properties in order to explain the process. Since in the double-slit experiment the diaphragm with its slit-structure acts like a crystal, the often discussed interference-type pattern produced on the plate also obtains its natural explanation within the framework of the unitary particle interpretation. Born's renunciation[73] of his original corpuscular-probabilistic interpretation, under the impact of experiments such as that with the double slit, was according to Landé a very grave mistake. Had Duane's explanation of selective diffraction not been forgotten at that time, physics, in Landé's opinion, would have been saved from that "fantastic 'quantum philosophy' supported by great names."

Landé's demystification of quantum mechanics by nonquantal postulates and his iconoclastic attitude toward widely accepted conceptions was generally met with strong opposition by the physicists and particularly, of course, by the advocates of the Copenhagen interpretation, but with warm sympathy and approval by the philosophers of science. In a review[74] of Landé's *Foundations of Quantum Theory*,[75] published in *Nuclear Physics* (edited by L. Rosenfeld), the reviewer (L.R.) expressed extreme disapproval, calling it confusing and "making a muddle of a perfectly clear situation." The periodical also published—a rare exception in scientific literature—Landé's comments[76] on this unfavorable review, giving him the opportunity to defend his position. While Landé's conception of a connection between thermodynamics and the statistical aspects of quantum mechanics which, via a denial of the reality of Gibbs' paradox, led him to construct his notions of fractional likeness and separation filters, was rejected as unwarranted by L.R., it was called "a significant accomplishment" by Boris Podolsky in a review[77] of the same book. Podolsky, however, questioned other assumptions made by Landé, for example, that $f_{E'E''}$ depends only on $E' - E''$; for in Podolsky's view a conjugacy between E and t can be justified only on relativistic grounds, but in relativity it is false to suppose that only energy differences have a meaning. Yet in spite of these and other inadequacies, Podolsky declared, Landé's work is "a worthwhile contribution to a deeper understanding of quantum mechanics."

Another example of conflicting criticisms are the evaluations by Yourgrau and Mehlberg of Landé's *From Dualism to Unity in Quantum Physics*.[78] In his nine-page long review Wolfgang Yourgrau, though criticiz-

[73]See Section 2.5.
[74]*Nuclear Physics* **1**, 133–134 (1956).
[75]Ref. 64.
[76]*Nuclear Physics* **3**, 132–134 (1957).
[77]*Philosophy of Science* **24**, 363–364 (1957).
[78]Ref. 64.

ing a number of points and complaining that Landé "compressed his arguments in such a tight fashion" that "the 'unpacking' of the diverse claims is at times true labour," especially with respect to the metric of probabilities, called the work "a fascinating piece of powerful argumentation, a true requiem for duality, if not a solemn one, then at least an honest one." In particular, Yourgrau pointed out, "the analysis of the meaning of the term 'state' in quantum physics is an example of precise and relevant elucidation."[79] Henryk Mehlberg, on the other hand, speaking in December 1959 in Chicago to the American Association for the Advancement of Science, expressed his regret "that Mr. Landé has refrained from discussing the single and most decisive issue which separates his view of quantum mechanics from the semiofficial position of the Copenhagen school," namely the question whether the state ψ is construed to apply to an individual system or serves as a statistical attribute of a virtual ensemble of such systems. But in spite of the logical gaps which Mehlberg thought it necessary to censure he called Landé's approach an "impressive achievement" which holds out a "significant promise."[80]

Among the physicists who expressed approval were Otto Robert Frisch of Cambridge University, who wrote a cautiously worded appreciative review,[81] J. H. van der Merve of the University of Pretoria, South Africa, who referred to Landé's book as "a *must* for anybody interested in the foundations of quantum theory,"[82] Paul Roman of Boston University who admired "the great skill and eloquence" of the author,[83] as well as Wesley S. Krogdahl, who "enthusiastically recommended the book to any readers interested in the basis of quantum theory."[84] Victor F. Lenzen, who began his career as a philosopher at Harvard before he became professor of physics at Berkeley in 1939, declared that "Professor Landé's original and expert discussion should awaken quantum theorists who may be in a state of 'dogmatic slumber,' and further may herald the demise of the dualistic interpretation of the quantum theory." And in spite of certain reservations, such as that Landé's account of the equations of motion (Schrödinger's equation) is incomplete or that it is false to impute to Bohr and Heisenberg positivism or subjectivism because of their use of the terms particle picture and wave picture, Lenzen declared: "The original method by which Professor Landé establishes the principles of the quantum theory may be

[79] *British Journal for the Philosophy of Science* **12**, 158–166 (1961).

[80] H. Mehlberg, "Comments on Landé's 'From duality to unity in quantum mechanics,'" in *Current Issues in the Philosophy of Science* (Ref. 64), pp. 360–370.

[81] *Contemporary Physics* **2**, 323 (1960–1961).

[82] *South-African Journal of Science* **57**, 114 (1961).

[83] *Mathematical Reviews* **1962**, 305–306 (B 1897).

[84] *American Scientist* **98A**, 210–211 (1962).

the most significant contribution to the problem of foundations since the creation of quantum mechanics in the Nineteen Twenties."[85]

In 1962 an interesting exchange of views[86] between Harry V. Stopes-Roe of Birmingham University and Landé was published in *Nature*. Stopes-Roe claimed that Landé's explanation of the double-slit experiment or of any other diffraction phenomenon in terms of a mechanical interaction between the incident particle and the screen *as a whole* contradicts the very assumption of Landé's localized particle approach because it involves a spread-out or collective and not locally concentrated action. Considering a double-slit experiment in which the three parts of the diaphragm—the two side pieces and the central piece between the slits—are held separately fixed, Stopes-Roe asked whether Landé could consistently declare that "this screen assemblage could act collectively so as to transfer momentum in a quantized manner." Landé replied that in a nonrelativistic theory admitting instantaneous communication the assumption of such a collective action is fully justified; if a particle strikes obliquely a heavy wall the equality of the angles of incidence and reflection also follows from the conservation laws "in reaction to the wall as a whole," the only difference from a crystal being that the wall has no selective periodicities. "But I would disagree," declared Landé, "if a dualist should tell me that this equality is due to a wave interlude, with the particle 'manifesting' itself as spread out over the whole surface and being reflected according to the Huyghens principle."[87] The debate was joined by T. E. Phipps of the U. S. Naval Ordance who suggested that only a modification of the mathematical formalism could resolve this dilemma.[88] Neither Landé's answer nor Phipps' suggestion satisfied Stopes-Roe.[89]

When Landé's *New Foundations of Quantum Mechanics*[90] was published in 1965 his approach again became the subject of many discussions. On the whole, however, the real issues involved were hardly touched upon. Thus, to mention only one typical example, R. C. Whitten,[91] criticizing a number of statements against the complementarity interpretation, wrote that "if one carefully reads the opening sections of the third volume of the Feynman Lectures in Physics, he will find Feynman saying exactly what Landé says, in an even clearer manner but without laying down the

[85] *Philosophy of Science* **29**, 213–216 (1962).
[86] H. V. Stopes-Roe, "Interpretation of quantum physics," *Nature* **193**, 1276–1277 (1962).
[87] *Nature* **193**, 1277 (1962).
[88] T. E. Phipps, "Interpretation of quantum physics," *Nature* **195**, 1088–1089 (1962).
[89] *Ibid.*, p. 1089.
[90] Ref. 64.
[91] *American Journal of Physics* **34**, 1203–1204 (1966).

gauntlet to the Copenhagen School." Nevertheless, and in spite of its "carping" style, the book was regarded by Whitten as "a valuable addition to the literature of quantum theory," replacing the "mumbo-jumbo" about complementarity and wave-particle dualism by physically more reasonable ideas.

A discussion which penetrated much more deeply into the core of the matter was Abner Shimony's review.[92] The first target of Shimony's criticism was not Landé himself but rather his critics, whom Shimony charged with not having been sufficiently critical in assessing Landé's approach. For, according to Shimony, Landé's work is not free of ambiguities in its premises and of errors in its arguments. Against Landé's argument that the acausality of individual events can be inferred from macroscopic phenomena such as games of chance Shimony maintained that, although the ergodic theory is still far from complete, large classes of physical systems are known for which the physically significant quantities have the same statistical distributions along all trajectories, providing thereby an explanation of statistical behavior which Landé completely ignored. Against Landé's application of the cause-effect continuity principle the objection is raised that were it not for quantum theory and the empirical evidence for the superposition of states, other assumptions than those made by Landé would have had greater plausibility.

To refute Landé's contention of the uniqueness of the unitary transformation as a possible law of interdependence between the matrices of transition probabilities Shimony constructed a geometrical model of states which, though satisfying all the requirements imposed by Landé, admits different transformation laws. In fact, this model, although consistent with all the seven axioms postulated in Landé's 1965 book and claimed to suffice for a derivation of quantum physics from nonquantal assumptions, displays no quantum properties at all. Shimony thus claimed to have refuted Landé's contention concerning such a derivation.

In a footnote to his 1967 *Physics Today* paper Landé admitted that "the reviewer's objection to the book version by way of a counter example...-was justified because I did not emphasize enough the requirement of generality and physical appropriateness." In his 1969 *American Journal of Physics* paper Landé accordingly modified the relevant assumption, the last among the seven axioms postulated in his 1965 book, without altering the first six axioms, and he claimed to have closed thereby the logical gap. That even this modification[93] does not yet admit a rigorous derivation of the property of "interference" for the probabilities as defined and

[92] *Physics Today* **19**, 85–91 (1966).
[93] Ref. 64 (1969, p. 544, eq. (4) *et seq.*).

characterized by the unchanged six other axioms has been charged, on mathematical grounds, in an unpublished paper "Quantum Fact and Fiction" by David K. Nartonis of Principia College, Elsah, Illinois, a former student of Shimony.

In August 1968 another exchange of views[94] was published which, though of a much smaller scale, is somewhat reminiscent of the famous controversy of 1715–16 between Newton and Clarke, on the one hand, who defended the ideas of absolute space and absolute time, and Leibniz, who argued against them. In the debate which took place about 250 years later the disputants were Max Born and Walter Biem, a solid-state physicist of the Kernforschungsanlage Jülich, who defended the idea of wave-particle duality, and Landé who argued against it. Like its precedent it had interesting historical sidelights. Born and Biem contended that Bohr and Heisenberg were not the founders of the dualistic view as Landé had claimed, but Albert Einstein was; for his formula for the density fluctuation of radiative energy, which he proposed in 1905, can be understood only if one accepts both the corpuscular and the undulatory aspects of light. Concerning Duane's quantum rule, Landé's favorite subject, they pointed out that it could not have played any important rule in the early discussions because without the de Broglie wave-particle relations it could not really be understood. Landé objected that duality began to be taken seriously only after the electron diffraction experiments seemed to admit no other explanation and that Einstein was never a champion of dualism.

One month after the publication of this dialogue in *Physics Today* Landé discussed his unitary theory at the 14th International Congress of Philosophy in Vienna (September 1968). When his lecture was published in *Physikalische Blätter*[95] Born and Biem were asked to write a rejoinder[96]—to which Landé, in turn, added his reply.[97] Heisenberg's comments[98] with which the discussion was closed introduced a conciliatory tone; for he pointed out that the disagreement was not so much about the contents of the quantum theory as about the language in which it was formulated. The language of "waves" and "particle," he declared, as it is used by the majority of physicists is the product of an organic development of forty years of physics and not the result of a dogmatic preconception; it need

[94]M. Born and W. Biem, "Dualism in quantum theory"; A. Landé, "Replies," *Physics Today* **21**, 51–56 (1968).

[95]Ref. 64, (1969, pp. 105–109).

[96]M. Born and W. Biem, "Zu Alfred Landés Auffassung von der Quantentheorie," *Physikalische Blätter* **25**, 110–111 (1969).

[97]M. Born, "Antwort zu den Einwänden von Born und Biem," *ibid.*, p. 112.

[98]W. Heisenberg, "Zur Sprache der Quantentheorie," *ibid.*, 112–113.

not be altered as long as it does not lead to misconceptions; but no such danger exists if it is used intelligently.

The philosophical importance of Landé's nonquantal derivation of quantum mechanics, whatever its final success, was well characterized by Hermann Bondi,[99] the well-known mathematician and cosmologist, who at the 1969 Denver convention said: "I always hoped that one day we can deduce quantum theory from the mere fact that there are solid bodies... Landé's paper goes a good way in this direction, but I do hope that this can be carried further and that eventually we will see quantum theory as a straightforward deduction from elementary observations."

10.4. OTHER ATTEMPTS

In the fall of 1966 Philip Pearle, a pupil of W. Furry at Harvard University, proposed an interesting modification of the statistical interpretation. To present his ideas in a concise form let us denote by I_c the conventional (orthodox) interpretation according to which the state vector describes the behavior of an individual system (particle), by I_s the statistical interpretation according to which the state vector describes the behavior of an ensemble of identically prepared systems, and by R the reduction procedure (reduction of the wave packet). Pearle's point of departure[100] was the question which of these two interpretations admits more readily the elimination of R.

Analyzing in detail the experiment consisting of a position measurement of a single particle, confined within a given volume V, Pearle showed that I_c, if stripped of R, leads to incorrect predictions, for if an apparatus is set to perform and record the results of two consecutive position measurements, one shortly after the other, it would seldom record the two measured distances as being far from each other; however, I_c without R ascribes to such results the same probabilities as to spatial propinquities. I_s without R, however, gives correct predictions for the outcome of such an experiment, for now an ensemble of volumes V, each containing a single particle, has to be taken into consideration. Does I_s, contrary to I_c, therefore admit the abandonment of R?

[99]See W. Yourgrau and A. D. Breck, *Physics, Logic, and History* (Based on the First International Colloquium held at the University of Denver, May 16–20, 1966), (Plenum Press, New York, London, 1970), p. 307.

[100]P. Pearle, "Alternative to the orthodox interpretation of quantum theory," *American Journal of Physics* 35, 742–753 (1967); "Elimination of the reduction postulate from quantum theory and a framework for hidden variable theories," Preprint, undated.

That without an additional condition the answer is generally negative Pearle demonstrated by analyzing another experiment which consisted of a position measurement followed by an energy measurement. If, however, the "Everett procedure"[101] is adopted according to which the measuring apparatus is to be included in the state vector, I_s without R, as Pearle showed, leads to the same predictions as the conventional interpretation. Pearle claimed that his "alternative interpretation" according to which the state vector refers to ensembles, is never to be "reduced" and always includes the measuring apparatus, gives predictions in full agreement with all experiments hitherto performed. Since any experiments for which the two interpretations may provide different predictions (such as experiments which are designed to determine the quantum state of a system-plus-apparatus) are practically infeasible, Pearle contended, there is no experimental evidence to support the conventional over the alternative interpretation.

Pearle's proposed interpretation was soon criticized by Ingram Bloch[102] of Vanderbilt University as inconsistent and unwarranted. It was inconsistent because the ensembles to which Pearle referred in the experiments had to be defined by him, before he performed the experiments, as those systems-plus-apparatus, out of a more comprehensive set, that have been observed to be in specific states, a definition which cannot avoid the reduction of the wave function. And it was unwarranted because there do exist experiments for which the two interpretations give different predictions, as Bloch illustrated by citing experiments he himself was carrying out together with A. Burba. These claims were called into question by Pearle in his reply to Bloch.[103]

At the same time a careful analysis of the relations between the states of ensembles and the states of individual systems in general statistical theories was carried out by James L. Park, who had just joined Washington State University after having studied under Henry Margenau at Yale. Park intended to prove that quantum mechanics, in contrast to classical physics, does not allow one to draw any inference from ensemble states as to the existence of individual system states. The dominant theme of the quantum theory, Park pointed out at the beginning of his paper,[104] is its probabilistic character. Although the quantum mechanical state concept, ever since the

[101]Everett's "relative state" formulation will be discussed in Chapter 11.

[102]I. Bloch, "Comment on: 'Alternative to the orthodox interpretation of quantum theory,'" *American Journal of Physics* **36**, 462–463 (1968).

[103]*Ibid.*, p. 463.

[104]J. L. Park, "Nature of quantum states," *American Journal of Physics* **36**, 211–226 (1968) (Part of Park's Ph.D. dissertation).

theory was given empirical meaning through Born's probabilistic postulate, had to be regarded as referring *empirically* to statistical ensembles instead of individual systems, the *theoretical* possibility seemed not to be precluded that the state concept could also refer to individual systems.

To find out whether such a theoretical construction is legitimate Park investigated the structure of an abstract paradigm theory, representative of a probabilistic theory in general, and studied the conditions to be satisfied if the theory allows the extracting of a consistent state concept for individual systems. One condition, it turned out, was the uniqueness of the resolution of general ensembles into pure subensembles on which an individual state specification had to be based. Park now pointed out that, contrary to classical physics, quantum mechanics does not satisfy this condition, for a general quantum ensemble can be subdivided in an infinite variety of ways into pure subensembles. To substantiate this point, Park considered an ensemble of spin-$\frac{1}{2}$ particles described in the two-dimensional spinor space by the statistical operator

$$\rho^{(1)}=\tfrac{3}{4}|\alpha\rangle\langle\alpha|+\tfrac{1}{4}|\beta\rangle\langle\beta|$$

where

$$\sigma_z\alpha=\alpha \qquad \text{and} \quad \sigma_z\beta=-\beta,$$

and he obtained in an obvious way two pure subensembles, one consisting of 3/4 of the original ensemble and characterized by α and the other consisting of 1/4 of the original ensemble and characterized by β. A second ensemble described by the statistical operator

$$\rho^{(2)}=\tfrac{3}{8}|\delta\rangle\langle\delta|+\tfrac{5}{8}|\eta\rangle\langle\eta|$$

where

$$\sigma_x\delta=\delta, \qquad \sigma_x\gamma=-\gamma, \quad \text{and} \qquad \eta=\left(\frac{1}{5}\right)^{1/2}(\delta+2\gamma)$$

was similarly divided into two pure subensembles, one consisting of 3/8 of the original ensemble and characterized by δ and the other consisting of 5/8 of the original ensemble and characterized by η. But by expressing $\rho^{(2)}$ as a matrix operator in the α-β representation Park showed, as can be easily checked, that $\rho^{(1)}=\rho^{(2)}$.

Since the statistical operator completely describes all measurement results for an ensemble the two original ensembles were physically one and the same ensemble. Park thus established that resolutions of quantum ensembles into pure subensembles are not unique, an ambiguity which may well lead, as he intimated, to theoretical paradoxes.[105] Moreover, an individual state concept, to be meaningful, should be applicable to the system at all times, a condition which implies that a pure ensemble has to remain pure throughout its temporal development. Since a quantum pure ensemble, when interacting with another pure ensemble, is converted into a mixture, this criterion also is not satisfied in quantum mechanics. Park thus claimed to have proved that "although classical statistical mechanics admits of an unambiguous assignment of individual states, quantum theory fails to satisfy the necessary criteria." Applying the terminology used in statistical theories, Park declared that quantum theory is "a theory with macrostates (of ensembles) for which there are no underlying microstates (of systems)."

Park's claim that the legitimacy of the single state concept can be conclusively disproved has not been shared by all advocates of the statistical interpretation. True, Leslie E. Ballentine of Simon Fraser University (Canada), who in a comprehensive article[106] pleaded persuasively for the need of broadening the Copenhagen interpretation into a statistical interpretation, seems to have fully endorsed Park's contention.[107] On the other hand, Hilbrand Johannes Groenewold of the University of Groningen (Holland), a staunch proponent of the statistical view, who as early as 1946 in a penetrating but not sufficiently known investigation[108] exposed the difficulties of any interpretation other than the statistical one, recently made the following statement:

> In my opinion perhaps all physicists might after an exhausting open minded discussion come to an agreement that it is legitimate to consider statistical ensembles and perhaps even that the statistical interpretation is a consistent one. They might possibly never come to a general agreement about the question whether either also a consistent individual interpretation making specific statement about single individual systems could be given or the statistical interpretation is already maximal in this respect. I would advocate that if we care to keep strictly to the rules of the statistical interpretation and speak about ensembles only, it is consistent indeed. But because this is

[105]In fact, the EPR argument is based precisely on such an ambiguous resolution as the reader will recognize if he recalls the two different expansions of $\psi(x_1,x_2)$ described in Section 6.3.

[106]L. E. Ballentine, "The statistical interpretation of quantum mechanics," *Reviews of Modern Physics* **42**, 358–381 (1970).

[107]"The present paper is consistent with his [Park's] conclusion," *op.cit.* p. 380.

[108]H. J. Groenewold, "Foundations of quantum theory—Statistical interpretation," in *Induction, Physics, and Ethics* P. Weingartner and G. Zecha, eds. (Reidel, Dordrecht-Holland, 1970), pp. 180–199, quotation on p. 182.

extremely fatiguing, we use to handle it sloppy and then we are liable to run into paradoxes. Although I do not know any consistent and adequate individual interpretation, I do not believe that it is possible to prove in general that it is impossible. I merely do not expect that it ever will be found and must in principle stay open to conversion after having been beaten hip and thigh.[108]

Theories of
MEASUREMENT

Chapter Eleven

11.1. MEASUREMENT IN CLASSICAL AND IN QUANTUM PHYSICS

In our account of the interpretations of quantum mechanics measurement has been repeatedly referred to. Measurement, after all, constitutes the link between theory and experience and its analysis is therefore one of the most sensitive parts of any interpretation. However, due to the historical fact that, roughly speaking, the formalism of quantum mechanics preceded its interpretation, the early interpretations—though, of course, bound to deal sooner or later with the problem of measurement—did not regard it as their point of departure.

Physics, according to Norman Campbell,[1] is the science of measurement. Yet before the rise of quantum mechanics the concept of measurement raised little interest. Only a few scientists, such as Hermann von Helmholtz[2] and Otto Hölder,[3] seemed ever to have been aware that the concept of measurement is by no means philosophically innocuous.

In classical physics observation and its quantitative refinement, measurement, were regarded as necessarily involving physical processes: a physical interaction $I_1 = I_{\mathfrak{S}\leftrightarrow\mathfrak{A}}$ between the object $\mathfrak{S}$ under observation or measurement (e.g., planet, heavy body, electric current) and measuring device $\mathfrak{A}$ (telescope, balance, ammeter), and a psychophysical interaction I_2 between $\mathfrak{A}$ and the observer $\mathfrak{O}$ (his sense organs and, ultimately, his consciousness). Classical physics described physical reality as composed of entities devoid of sensuous qualities (extended bodies moving in space or fields); but the theory achieved its validity only by virtue of its verifiability (or falsifiability), that is, by virtue of the fact that its predictions could be tested, an operation which, in the last analysis, had to involve human consciousness. Consequently, ontological and epistemological problems concerning the relationship between physical objects, on the one hand, and human consciousness, on the other, seemed to be involved.

The following stratagem, however, made it possible to ignore these problems. Strictly speaking, $I_{\mathfrak{S}\leftrightarrow\mathfrak{A}}$ implies an action $I_{\mathfrak{S}\to\mathfrak{A}}$ of $\mathfrak{S}$ on $\mathfrak{A}$ as well as an action $I_{\mathfrak{A}\to\mathfrak{S}}$ of $\mathfrak{A}$ on $\mathfrak{S}$. But due to the fact that the order of magnitude of the latter could be considered to be much smaller than that of the former, $I_{\mathfrak{A}\to\mathfrak{S}}$ was regarded as negligible or, at least, as eliminable in

[1]N. Campbell, *An Account of the Principles of Measurement and Calculation* (Longmans and Green, London, 1928).

[2]H. von Helmholtz, "Zählen und Messen erkenntnistheoretisch betrachtet," in *Philosophische Aufsätze, Eduard Zeller gewidmet* (Fues' Verlag, Leipzig, 1887), pp. 17–52; reprinted in *Wissenschaftliche Abhandlungen* (Barth, Leipzig, 1895), Vol. 3, pp. 356–391; *Counting and Measuring* (Van Nostrand, New York, 1930).

[3]O. Hölder, "Die Axiome der Quantität und die Lehre vom Mass," *Leipziger Berichte* **53**, 1–64 (1901).

principle; $I_{\mathfrak{S}\to\mathfrak{A}}$ could not be neglected, for the pointer position of $\mathfrak{A}$ had to depend on the state of $\mathfrak{S}$ if $\mathfrak{A}$ was to serve as a measuring device. In addition, the psychophysical problem concerning the relationship between $\mathfrak{A}$ and $\mathfrak{O}$ was regarded as extraneous to physical theory. This made it possible to "objectivize" classical physics, that is, to treat its processes as independent of observation and to ignore the role of the observer.

With the rise of quantum mechanics it was soon realized that, due to the finite value of Planck's constant h, $I_{\mathfrak{S}\to\mathfrak{A}}$ and $I_{\mathfrak{A}\to\mathfrak{S}}$ may have the same order of magnitude so that the condition for the consistency of the classical conception of measurement failed to be satisfied. Consequently, the foregoing problems could no longer be ignored.

At first, under the impact of philosophical implications of Heisenberg's indeterminacy relations most discussions on measurement tried to establish a rationale for these relations. A typical example of those early theories of measurement was Bridgman's explanation[4] of the indeterminacy relation between position and momentum as being due to the absence of measuring tools finer than complete collisions between elementary particles. Although Bohr, as will be recalled, analyzed in great detail experimental and other methodological aspects of various measurement procedures, his approach was essentially of a heuristic nature.

For Bohr the absence of a formal theory of measurement did not indicate any imperfection or incompleteness of his epistemological analysis of quantum mechanics but was rather required for reasons of consistency. In his view, classical concepts, representing the ultimately immediate data of common experience, are in the last resort not formalizable, for any formal elaboration becomes physically meaningful only if it is interpreted in terms of classical concepts. Bohr's insistence on the logical (though not physical) necessity of drawing a sharp distinction between object and measuring instrument can therefore never be replaced by any formal treatment.

For this very reason Bohr never showed real interest in an axiomatic formulation of quantum mechanics. For such an axiomatization cannot dispense with undefined primitive concepts and relations whose concrete meaning can be conveyed only in terms of the language of ordinary experience. Since an axiomatization of quantum mechanics is intended to ? clarify the latter it is not only sterile, as axiomatizations usually are, but also necessarily circular; it can, at best, serve as a test for the consistency of reasoning but never resolve any epistemological or methodological difficulties.

Also, if we digress to a later stage of the Copenhagen interpretation and

[4] P. W. Bridgman, "The new vision of science," *Harpers Magazine* **158**, 443–451 (1929).

regard, in conformance with its most extreme version, a quantum phenomenon as merely a nonclassically describable functioning of classically describable experimental arrangements, measurements in quantum mechanics are no more or less problematic than in classical physics, for the Hilbert space vector is only a purely formal device for relating the statistics associated with these arrangements to the physics of observations in classical physics.

If, however, Bohr's view is not accepted, the reason for the absence of a systematic treatment of measurement during the early stages of the theory could be explained by the fact that little need was felt for such a treatment when only position, momentum, and energy were the observables discussed and no axiomatic formulation postulating a general correspondence between operators and observables was yet established. Heisenberg, for example, in his classic Chicago lectures[5], discussed in great detail measurements of position, of momentum, and of energy, but he developed no general theory of measurement. Nor did the early texts on quantum mechanics such as those written by Weyl (1928), Fraenkel (1929), Born and Jordan (1930), and Dirac (1930) touch upon such a theory. The only possible exception was a short paper written in 1930 by Madelung which dealt with measurement in a more general way but ignored its epistemological problems.[6]

According to the Copenhagen interpretation in any measurement the state of the observed object is affected by the macroscopic measuring instruments whose existence and mode of operation, though necessary for the possibility of observing quantum mechanical processes, are not accounted for by the quantum theory itself but regarded as logically preceding the theory. It was further assumed that these macroscopic devices could be observed with arbitrary accuracy and that the very act of reading the pointer or the registration of the result had no effect on the outcome of the measurement. At the same time, however, for reasons of consistency it was also assumed that these devices, or parts of them, obey the laws of quantum mechanics—at least as far as any determination of the position or momentum of the center of mass is regarded as being subject to Heisenberg's indeterminacy relations; Bohr's treatment[7] of the moveable diaphragm D_2 in the double-slit thought-experiment may serve

[5]Ref. 3–19. At a later stage Heisenberg partially endorsed von Neumann's theory of measurement but interpreted the reduction of the wave packet as a transition from potentiality to actuality. See the end of his article Ref. 3–4 (1955).
[6]E. Madelung, "Geschehen, Beobachten und Messen im Formalismus der Wellenmechanik," *Zeitschrift für Physik* **62**, 721–725 (1930).
[7]See Section 5.2.

as an illustration. This double nature[8] of the macroscopic apparatus (on the one hand a classical object and on the other hand obeying quantum mechanical laws) by virtue of which, as we have seen, Bohr successfully defended his position, remained a somewhat questionable or at least obscure feature in Bohr's conception of quantum mechanical measurement.

Another problematic aspect whose serious implications were only gradually understood was the fact that as long as a quantum mechanical one-body or many-body system does not interact with macroscopic objects, as long as its motion is described by the deterministic Schrödinger time-dependent equation, no events could be considered to take place in the system. Even such an elementary process as the scattering of a particle in a definite direction could not be assumed to occur (since this would require a "reduction of the wave packet") without an interaction with a macroscopic body. In other words, if the whole physical universe were composed only of microphysical entities, as it should be according to the atomic theory, it would be a universe of evolving potentialities (time-dependent ψ-functions) but not of real events.

Recognition of these epistemological difficulties accentuated of course the importance of a theory of measurement. At first, however, it was primarily the quest for a complete operational interpretation of the formalism of quantum mechanics which led to the search for a systematic theory of measurement, for logical consistency requires that a full-fledged interpreted theory accounts not only for the information it provides but also for the process of acquiring this information. It was also immediately clear that the main concern of such a theory of measurement was not the measurement of structural properties such as electric charges or magnetic moments of physical systems but the measurement of their states.

11.2. VON NEUMANN'S THEORY OF MEASUREMENT

In view of our preceding observations it may be expected that von Neumann's axiomatic foundation of quantum mechanics, though generally based on the Copenhagen interpretation, contained an explicit theory of measurement,[9] which had to transcend in certain points Bohr's interpretation of quantum mechanics. Since von Neumann's conception of the measurement problem became the framework of almost all subsequent theories of measurement we shall discuss it in detail. Von Neumann's point

[8]See Section 6.5.
[9]Ref. 1–2 (Chapters 4–6).

of departure[10] was the assumption that there are two kinds of changes of quantum mechanical states: (1) "the discontinuous, non-causal and instantaneously acting experiments or measurements," which he called "arbitrary changes by measurement" [willkürliche Veränderungen durch Messungen], and (2) "continuous and causal changes in the course of time," which evolve in accordance with the equations of motion and which he called "automatic changes" [automatische Veränderungen]. The former or, briefly, "processes of the first kind" are irreversible whereas the latter, the "processes of the second kind," are reversible.

A measurement, according to von Neumann, is essentially "the temporary insertion of a certain energy coupling into the observed system," or an interaction that establishes a statistical relation between the states of the object $\mathfrak{S}$ and the states of the measuring apparatus $\mathfrak{A}$. The observer turns his attention not directly to $\mathfrak{S}$ but rather to $\mathfrak{A}$ and infers the state of $\mathfrak{S}$ from observing that of $\mathfrak{A}$. If $\mathbb{S}$ is the physical quantity (or observable), represented by the self-adjoint (or hypermaximal Hermitian) operator S, to be measured on $\mathfrak{S}$, its eigenvalues, for simplicity assumed to be discrete and nondegenerate, will be denoted by s_k and the corresponding normalized eigenvectors by $\sigma_k = \sigma_k(x)$, where x symbolizes the system coordinates.

Contrary to Bohr, von Neumann treated the measuring apparatus $\mathfrak{A}$ as a quantum mechanical system. To its observable $\mathcal{A}$ corresponds the self-adjoint (hypermaximal Hermitian) operator A with the nondegenerate and discrete eigenvalues a_k and the normalized eigenvectors $\alpha_k \equiv \alpha_k(y)$ where y is the apparatus coordinate (e.g., pointer position).

If the initial (pure) state $\sigma \equiv \sigma(x)$ of $\mathfrak{S}$ is characterized by the statistical operator $\rho_\sigma = |\sigma\rangle\langle\sigma|$, the discontinuous change ("of the first kind") can be expressed by the formula

$$\rho_\sigma \rightarrow \rho_{\sigma'} = \sum_k \rho_{\sigma_k} \rho_\sigma \rho_{\sigma_k} \tag{1}$$

where $\rho_{\sigma'}$ obviously[11] represents a mixture of the states with weights (statistical probabilities) $w_k = |(\sigma_k, \sigma)|^2$.

The continuous change (of the second kind), on the other hand, can be

[10] *Ibid.*, Chapter 5, section 1.

[11] If the vectors $|\beta_n>$ are a base then

$$(\rho_{\sigma'})_{ij} = \sum_k \langle \beta_i|\sigma_k\rangle\langle\sigma_k|\sigma\rangle\langle\sigma|\sigma_k\rangle\langle\sigma_k|\beta_j\rangle$$

$$= \sum_k w_k (\rho_{\sigma_k})_{ij}$$

$$\text{or } \rho_{\sigma'} = \sum w_k \rho_{\sigma_k}.$$

expressed by the formula

$$\rho_\sigma \to \rho_{\sigma_t} = U_H(t)\rho_\sigma U_H(-t) \tag{2}$$

where $U_H(t) = \exp(-iHt/\hbar)$, H being the Hamiltonian of the system $\mathfrak{S}$. This change is a (reversible) unitary transformation

$$\sigma_t = U_H(t)\sigma \tag{3}$$

in accordance with the Schrödinger equation.

There is an essential difference between $\rho_{\sigma'}$ and ρ_{σ_t}: ρ_{σ_t}, the unitary transform of the pure state ρ_σ, is a pure state whereas $\rho_{\sigma'} = \Sigma w_k \rho_{\sigma_k}$ is a mixture.[12]

In von Neumann's theory the discontinuous change (1) cannot be reduced to the continuous change (2) but is regarded as an irreducible fact. Its reconciliation with the continuous change became a central problem only in later theories of measurement.

The main problem in von Neumann's theory of measurement was the question of whether the results obtained in accordance with (1) are consistent with those obtained by inserting a measuring apparatus $\mathfrak{A}$ and measuring $\mathcal{Q}$. To solve this consistency problem von Neumann defined a well-designed measurement as that which changes continuously the initial state $\sigma_k \alpha$ of the composite system $\mathfrak{S} + \mathfrak{A}$ ($\mathfrak{S}$ is assumed to be in the eigenstate σ_k of the measured observable $\mathbb{S}$) into the final state $\sigma_k \alpha_k$. The consistency problem was thus equivalent to the question whether it is possible to find a Hamiltonian H' of the composite system such that within a time-interval t, $\sigma = \Sigma c_k \sigma_k$ is continuously transformed into

[12]The reader is reminded that a statistical operator ρ describes a pure state (represented by a vector in Hilbert space) if and only if it is idempotent (i.e., $\rho^2 = \rho$). That $\rho_{\sigma'} = \Sigma w_k \rho_k$ (we suppress σ in ρ_{σ_k}) does not satisfy this condition can be seen as follows:

$$\begin{aligned}
\rho_{\sigma'}{}^2 &= \sum_{m,n} w_m w_n \rho_m \rho_n \\
&= \sum_m w_m{}^2 \rho_m + \sum_{m<n} w_m w_n (\rho_m \rho_n + \rho_n \rho_m) \\
&= \sum_m w_m{}^2 \rho_m + \sum_{m \neq n} w_m w_n \rho_m - \sum_{m<n} w_m w_n (\rho_m - \rho_n)^2 \\
&= \sum_m w_m{}^2 \rho_m + \sum_m w_m (1 - w_m) \rho_m - \sum_{m<n} w_m w_n (\rho_m - \rho_n)^2 \\
&= \rho_{\sigma'} - \sum_{m<n} w_m w_n (\rho_m - \rho_n)^2 \neq \rho_{\sigma'}.
\end{aligned}$$

$\Sigma c_k \sigma_k \alpha_k$ or, in other words, such that

$$\epsilon_{H'}(t)\sigma\alpha = \sum_k c_k \sigma_k \alpha_k \tag{4}$$

where $c_k = (\sigma_k, \sigma)$.

To demonstrate (4) von Neumann[13] relabeled σ_j so that $j = 0, \pm 1, \pm 2, \ldots,$ and α_k so that $k = 0, \pm 1, \pm 2, \ldots,$ and defined $U = U_H(t)$ by the following equation:

$$U \sum_{j,k=-\infty}^{\infty} x_{jk} \sigma_j \alpha_k = \sum_{j,k=-\infty}^{\infty} x_{jk} \sigma_j \alpha_{j+k}. \tag{5}$$

Since the $\sigma_j \alpha_k$ as well as the $\sigma_j \alpha_{j+k}$ form a complete orthonormal set in the Hilbert space of the composite system, U is unitary. From the identities $\sigma = \Sigma(\sigma_j, \sigma)\sigma_j$ and $\sigma\alpha = \Sigma(\sigma_j, \sigma)\sigma_j \alpha$ and the definition of U he derived

$$\epsilon\sigma\alpha = \sum_{j=-\infty}^{\infty} (\sigma_j, \sigma)\sigma_j \alpha_j$$

so that indeed $c_j = (\sigma_j, \sigma)$. H' has thus been shown to establish a one-to-one correlation between the states of the object-system $\mathfrak{S}$ and the states of the measuring apparatus $\mathfrak{A}$. The probability that an observer who measures $\mathcal{A}$ on $\mathfrak{A}$ will obtain a_k and hence infer s_k is $w_k = |c_k|^2$ just as in (1). This completes von Neumann's consistency proof.

Having thus shown how the formalism of quantum mechanics is capable of accounting consistently for the operation of the measuring apparatus, von Neumann continued his analysis of the measuring process by regarding it as consisting of two stages: (I) the interaction between the object and the apparatus, and (II) the act of observation.

Stage I. To simplify the account as much as possible let us assume that the quantity $\mathcal{S}$ to be measured is dichotomic, that is, it can assume only two values, either s_1 or s_2, and that the system is capable of existing in only two states σ_1 or σ_2 (e.g., the two orientations of spin in a given direction). Before the interaction of the system (object) with the apparatus, the pure state of the system, $\sigma(x)$, can be expanded in terms of the eigenfunctions $\sigma_1(x)$ and $\sigma_2(x)$ belonging to s_1 and s_2:

$$\sigma(x) = c_1\sigma_1(x) + c_2\sigma_2(x). \tag{6}$$

The expectation value of any physical quantity (observable) represented by

[13]Ref. 1–2 (1932, p. 235; 1955, p. 442).

T is consequently

$$(\sigma, T\sigma)=|c_1|^2(\sigma_1, T\sigma_1)+|c_2|^2(\sigma_2, T\sigma_2)+c_1^* c_2(\sigma_1, T\sigma_2)+c_1 c_2^*(\sigma_2, T\sigma_1). \quad (7)$$

After the object has been coupled to the measuring apparatus, designed to measure whether $\mathfrak{S}$ has the value s_1 or s_2, and the interaction has ceased, the system-plus-apparatus is in the state

$$\psi=c_1\sigma_1(x)\alpha_1(y)+c_2\sigma_2(x)\alpha_2(y). \quad (8)$$

As explained previously, the state function ψ, being causally determined by $\sigma(x)$ and $\alpha(y)$, is a pure state with respect to the combined system-plus-apparatus, and as long as this combined system remains isolated. However, with respect to the apparatus (observable $\mathfrak{A}$ alone), or the system (observable $\mathfrak{S}$) alone, ψ is a mixture.

To see this most easily, consider the operator T which pertains to the system (object) alone. The expectation value of T due to the orthogonality of the eigenfunctions of $\mathfrak{A}$ is

$$\begin{aligned}(\psi,(T\otimes 1)\psi)&=(c_1\sigma_1\otimes\alpha_1+c_2\sigma_2\otimes\alpha_2,\ (T\otimes 1)\ (c_1\sigma_1\otimes\alpha_1+c_2\sigma_2\otimes\alpha_2))\\ &=|c_1|^2(\sigma_1, T\sigma_1)+|c_2|^2(\sigma_2, T\sigma_2). \qquad (9)\end{aligned}$$

The interference terms have disappeared, that is, after the interaction with the apparatus, the system-plus-apparatus behaves like a mixture of σ_1 and σ_2 with respect to T. It is in this sense, and in this sense alone, that a measurement is said to "change" a pure state into a mixture.[14]

[14]This result had been discovered already by L. Landau and applied in 1927 in his paper "Das Dämpfungsproblem in der Wellenmechanik," *Zeitschrift für Physik* **45**, 430–441 (1927). In fact, if

$$\psi=\sum_{j,k} g_{jk}\sigma_j(x)\alpha_k(y)$$

the expectation value of an observable $F(x)$ of $\mathfrak{S}$ in the state ψ is given by the expression

$$\langle F\rangle=\sum_{jkmn} g_{jk}^* g_{mn}\langle\sigma_j|F|\sigma_m\rangle\langle\alpha_k|\alpha_n\rangle=\mathrm{Tr}(\rho_\psi F)$$

where

$$(\rho_\psi)_{mj}=\sum_k g_{mk}g_{jk}^*.$$

With

$$w_k=\sum_m |g_{mk}|^2, \qquad g_m^{(k)}=w_k^{-1/2}g_{mk}, \qquad \text{and} \quad g_{mj}^{(k)}=g_m^{(k)}(g_j^{(k)})^*$$

In the general case the state function of the system-plus-apparatus after the interaction is obviously

$$\psi = \sum c_k \sigma_k \alpha_k \tag{10}$$

and is a pure state as long as the combined system remains isolated.

Stage II. Von Neumann was fully aware that knowledge of the state of the combined system does not suffice to infer the state of the object or the value of $\mathbb{S}$. If it could be ascertained that after the interaction the apparatus is in the state α_j, it would be known that the object is in the state σ_j and $\mathbb{S}$ has the value s_j. But how can we find out whether the apparatus is in state α_j? It may be suggested that one couple the apparatus $\mathfrak{A}$ to a second measuring device $\mathfrak{A}'$. This proposal, however, would lead to an infinite regress since $\mathfrak{A}'$ stands, conceptually, in the same relation to $\mathfrak{A}$ as $\mathfrak{A}$ stands to $\mathfrak{S}$: the state function of the supercombined system would be

$$\psi' = \sum c_k \sigma_k \alpha_k \alpha_k'. \tag{11}$$

But, clearly, von Neumann reasoned, a measurement must be a finite operation; usually it is completed by an act of observing the pointer position of $\mathfrak{A}$. The process leading to this result, von Neumann concluded, can therefore no longer be of the second kind but has to be a discontinuous, noncausal, and instantaneous act. Where and how does this act take place?

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it follows that

$$(\rho_\psi)_{mj} = \sum_k w_k g_{mj}^{(k)}$$

so that

$$\rho_\psi = \sum w_k \rho_k$$

where the statistical operator ρ_k with elements $(\rho_k)_{mj} = g_{mj}^{(k)}$ satisfies $\rho_k{}^2 = \rho_k$ and hence describes a pure state. This, however, shows that ρ_ψ describes a mixture. This conclusion remains valid even if $g_{jk} = c_j \delta_{jk}$ and $g_{mj}^{(k)} = (\rho_k)_{mj} = \delta_{mk}\delta_{jk}$ so that

$$(\rho_\psi)_{mj} = \sum_k |c_k|^2 \delta_{mk}\delta_{jk}.$$

Clearly, ψ is a pure state in the composite Hilbert space $\mathcal{H}_{\mathfrak{S}} \otimes \mathcal{H}_{\mathfrak{A}}$, for a unitary transformation of the pure case $\sigma\alpha$ produces a pure case. ψ is a mixture only with respect to $\mathcal{H}_{\mathfrak{S}}$ (or $\mathcal{H}_{\mathfrak{A}}$) alone or an "improper mixture" in the terminology of D'Espagnat. On this point which caused some confusion in the theory of measurement see B. D'Espagnat, "An elementary note about 'mixtures,' "in *Preludes in Theoretical Physics* (North-Holland Publishing Company, Amsterdam, 1966), pp. 185–191; *Conceptions de la Physique Contemporaine* (Hermann, Paris, 1965); *Conceptual Foundations of Quantum Mechanics* (Benjamin, Menlo Park, Calif., 1971), pp. 82–87.

Von Neumann's answer to this problem was greatly influenced by Leo Szilard (1898–1964), his countryman, five years his senior, with whom he had long conversations on this issue in Berlin. Szilard had just published his influential study[15] on the relation between the intervention of an intelligent being on a thermodynamic system and the second law of thermodynamics, a problem to which he was led by his study of Smoluchowski's paper[16] on the limitations of the second law. Smoluchowski's conception of an intellect that is constantly cognizant of the instantaneous state of a dynamical system and thereby able to invalidate the second law of thermodynamics without performing work marked the beginning of certain thought-provoking speculations about the effect of a physical intervention of mind on matter and thus paved the way toward von Neumann's far-reaching contention that it is impossible to formulate a complete and consistent theory of quantum mechanical measurement without reference to human consciousness.

The fact that at the end of the measurement the observer sees the pointer of the apparatus, say, on n was for von Neumann an indication that the state of the composite system is $\sigma_n \alpha_n$ after the measurement and that of the object is σ_n. The problem von Neumann had to face was therefore this: How does the superposition $\Sigma c_j \sigma_j \alpha_j$ transform into $\sigma_n \alpha_n$? As mentioned above, not only $\Sigma c_j \sigma_j$ but also $\Sigma c_j \sigma_j \alpha_j$ is a pure state even though the latter is a mixture with respect to $\mathfrak{S}$ (or $\mathfrak{A}$) alone. One may be tempted to explain the statistical character of the considered transformation by assuming that the initial state of $\mathfrak{A}$ was not a pure state but a mixture, say, $\Sigma v_k \rho_k$ where $\rho_k = |\alpha_k\rangle\langle\alpha_k|$ is the statistical matrix for α_k and v_k are arbitrary weights, independent of σ. Such a mechanism, von Neumann admitted, might well be conceivable for, if referred to $\mathfrak{O}$ rather than to $\mathfrak{A}$, the state of information of the observer regarding his own state could have, by the law of nature, absolute limitations (expressed by the values of the v_k). But this attempt to solve the problem was easily disproved by von Neumann on the following ground. After the interaction the state of the whole system will indeed be described by a statistical matrix of the form $\Sigma v'_k \rho'_k$ where $\rho'_k = U \rho_k \epsilon$ and $v'_k = v_k$. Quantum mechanics requires $v'_k = |c_k|^2 = |(\sigma, \sigma_k)|^2$ and v_k would depend on σ in contradiction to the above mentioned assumption.

[15] L. Szilard, "Über die Entropieverminderung in einem thermodynamischen System bei Eingriffen intelligenter Wesen," *Zeitschrift für Physik* **53**, 840–856 (1929).

[16] M. von Smoluchowski, "Gültigkeitsgrenzen des zweiten Hauptsatzes der Wärmetheorie," in *Vorträge über die kinetische Theorie der Materie und Elektrizität* (Teubner, Leipzig, Berlin, 1914), pp. 89–121; *Pisma Marjana Smoluchowskiego—Oeuvres de Marie Smoluchowski*, L. Natanson, ed. (Imprimerie de l'Université Jaguellonne, Cracow; Béranger, Paris, 1927), pp. 361–398.

Von Neumann was thus led to regard the transition of $\sum c_j \sigma_j \alpha_j$ into $\sigma_n \alpha_n$ as a process of the first kind, that is, as an irreducible element of the theory. More precisely, he believed that immediately after the measurement of $\mathcal{R}$ yielded the value a_n, the state of $\mathfrak{A}$ is α_n and that of $\mathfrak{S}$ is σ_n. This assumption is now usually called "the projection postulate,"[17] for the process under discussion "projects" the superposition into the eigenspace (i.e., the subspace spanned by the eigenvectors) belonging to the eigenvalue obtained.[18] Von Neumann's argumentation in favor of this assumption, which was based on an analysis of the Compton and Simon experiment (elastic photon-electron collisions), was criticized by Paul K. Feyerabend[19] and by Joseph D. Sneed.[20]

However, as reported by Margenau,[21] von Neumann admitted in personal conversation that the postulate, though "pictorially useful and consistent with the axioms of quantum mechanics," may well be dispensable. In his treatise, however, von Neumann stated explicitly[22] that through the process of the first kind "the measurement transforms $\mathfrak{S}$ from the state σ_0 into one of the states σ_j, the probabilities for which are respectively $|(\sigma_0, \sigma_j)|^2$."

As to any details of the process of the first kind which from now on will be referred to as the "resolution" or "reduction" of the superposition ("collapse of the wave packet") or briefly "reduction" von Neumann was rather reticent. In one passage he wrote: "Now quantum mechanics describes the events which occur in the observed portions of the world, so long as they do not interact with the observing portion, with the aid of the process [of the second kind], but as soon as such an interaction occurs, i.e. a measurement, it requires the application of [a] process [of the first kind]."[23] This argument for the indispensability of processes of the first kind also seems to suggest that these processes do not occur in the observed portions of the world, however deeply in the observer's body the boundary is drawn. They can thus occur only in his consciousness. A complete measurement, according to von Neumann's theory, involves

[17] Ref. 6–125. The term was coined in 1958 by Henry Margenau.

[18] That in the case of degeneracies von Neumann's formulation has to be modified was shown by Gerhart Lüders, "Über die Zustandsänderung durch den Messprozess," *Annalen der Physik* **8**, 322–328 (1951). Cf. also W. H. Furry, "Some aspects of the quantum theory of measurement," in *Lectures in Theoretical Physics*, Vol. 8A, *Statistical Physics and Solid State Physics* (University of Colorado Press, Boulder, 1966), pp. 1–64, esp. pp. 14–16.

[19] P. K. Feyerabend, "On the quantum theory of measurement," in Ref. 7–102 (pp. 121–130).

[20] J. D. Sneed, "Von Neumann's argument for the projection postulate," *Philosophy of Science* **33**, 22–39 (1966).

[21] H. Margenau, "Measurement in quantum mechanics," *Annals of Physics* **23**, 469–485 (1963).

[22] Ref. 1–2 (1932, p. 234; 1955, p. 439).

[23] Ibid., (1932, p. 224; 1955, p. 420).

therefore the consciousness of the observer. In view of the classification of processes into two mutually irreducible categories, corresponding to the partition of the world into the observed and the observing, which, in spite of the mobility of their dividing line, are also mutually irreducible, von Neumann's theory is a dualistic one. In fact, it would be instructive to compare its philosophical content with Anaxagoras' doctrine of Matter and Mind [*Nous*] which was one of the earliest dualistic conceptions of the world and according to which "the things that are in a single world are not parted from one another, not cut away with an axe, neither the warm from the cold nor the cold from the warm"[24] (superpositions!?), but "when Mind began to set things in motion, separation took place from each thing that was being moved, and all that Mind moved was separated" (reduction!?).

11.3. THE LONDON AND BAUER ELABORATION

Since von Neumann's theory of measurement constituted the culmination of his axiomatic presentation of the foundations of quantum mechanics and required a high standard of mathematical knowledge it would not have been easily accessible, especially not to the experimentalists, were it not for the fact that Fritz Wolfgang London and Edmond Bauer published a simplified account of it. London,[25] whose name is well known for his work on the homopolar chemical bond, the Heitler-London theory of co-valency (1927), and his work on the Meissner effect in superconductivity, in collaboration with his brother Heinz London (1935), was all through his life—deeply interested in philosophy. In fact, while a student of physics at Arnold Sommerfeld's Institute in Munich he earned his Ph.D. degree (1921, *Summa cum laude*) for a thesis[26] in philosophy under Alexander Pfänder[27] who was greatly influenced by Theodor Lipps' psychological theory of empathy [*Einfühlung*].[28] According to Lipps, the founder of the Munich Institute for Psychology, the psychic state which we experience when we perceive an aesthetic object is not merely a kinesthetic inference but has an exclusively objective reference, being an "objectivated enjoy-

[24]Simplicius, *Physics*, 175,12; 176,29. Diogenes Laertius, II, 8.

[25]A short biography of Fritz London, written by his widow Edith London, is contained in F. London, *Superfluids* (Dover, New York, 1961), Vol. 1, pp. X–XIV.

[26]F. London, "Über die Bedingungen der Möglichkeit einer deduktiven Theorie," *Jahrbuch für Philosophie und phänomenologische Forschung* **6**, 335–384 (1923).

[27]Cf. A. Pfänder, *Einführung in die Psychologie* (Barth, Leipzig, 1904, 1920).

[28]Th. Lipps, *Psychologische Untersuchungen* (Engelmann, Leipzig, 1907–1912); *Psychological Studies* (Williams and Wilkins, Baltimore, Md., 1926); *Zur Einfühlung* (Engelmann, Leipzig, 1913).

ment of self"; the emphatic act, a peculiar source of knowledge also about other egos, is a blend of inference and intuition. Logic, for Lipps, was a special branch of psychology. Later Pfänder became an advocate of Edmund Husserl's phenomenalism and joined him in editing the *Jahrbuch für Philosophie und phänomenologische Forschung*. Lipps' notion of empathy, as we shall see, was influential on London's ideas about the measurement process in quantum mechanics.

After his emigration from Berlin where he was Privatdozent from 1928 to 1933 and after a three-year stay in Oxford, London moved to Paris to assume the position of *Maitre* (later *Directeur*) *de Recherche* at the Collège de France. There he met Edmond Bauer who was *Sous-Directeur du Laboratoire de Physique*. Eager to cooperate on a project of common interest London and Bauer decided to write a "concise and simple" presentation of von Neumann's theory of measurement."[29] Section 11 (Mesure et observation. L'acte d'objectivation) was written primarily by Fritz London.[30] As a student of philosophy in Munich he had been influenced by his teacher Erich Becher, who always insisted that the mind-body problem is the central question of metaphysics. As documented in the Archives of Munich University (Quästur, "Verzeichnis No. 6316") Fritz London attended in the summer term of 1921 Becher's lectures on Logic and Epistemology [*Erkenntnistheorie*], four lecture hours weekly; in addition, he heard Baeumker on Aristotle and Geiger on the history of philosophy and the philosophy of mathematics. London thus attended 12 lectures per week in philosophy, whereas in physics, during this term, he participated only in Sommerfeld's lecture on hydrodynamics (and exercises) and Sommerfeld's seminar, together only eight hours per week. Becher's interest in this problem stemmed from the time when, under Erdmann in Bonn, he studied Spinoza's doctrine of attributes.[31] Most of Becher's subsequent writings dealt with the mind-body problem.[32] Two of his books are quoted by London in his thesis.[33] Claiming to have demonstrated that the interaction between mind and body obeys the law of

[29]F. London and E. Bauer, *La Théorie de l'Observation en Mécanique Quantique* (Hermann & Cie., Paris, 1939).

[30]While the monograph was being printed London had to leave for France. He finally joined Duke University in North Carolina and stayed there until his death.

[31]E. Becher, *Der Begriff des Attributes bei Spinoza* (Niemeyer, Halle, 1905).

[32]E. Becher, "Naturphilosophie" in *Die Kultur der Gegenwart* (Teubner, Leipzig, 1914), Part 3; "Erkenntnistheorie und Metaphysik" in *Die Philosophie in ihren Einzelgebieten* (Ullstein, Berlin, 1925) pp. 301–392 (esp. Chapter 3: Der Zusammenhang von Seele und Materie-an-sich); *Grundlagen des Naturerkennens* (Duncker und Humblot, Munich, 1938), esp. pp. 69–82.

[33]E. Becher, *Die philosophischen Voraussetzungen der exakten Naturwissenschaften* (Barth, Leipzig, 1906); *Geisteswissenschaften und Naturwissenschaften* (Duncker und Humblot, Munich, 1921).

energy conservation, Becher rejected the doctrines of parallelism, occasionalism, and epiphenomenalism and became a staunch defender of interactionalism. Physical processes, he declared, pervade the brain in a continuous course and produce, in addition to physical effects, psychic effects which in turn decisively affect physical events."[34]

London thus found in quantum mechanics a field where he could meaningfully apply Lipps' and Becher's philosophy and it is not surprising that in the discussion of von Neumann's conception of the resolution of superpositions we read in the London and Bauer monograph:

> The observer has a completely different point of view: for him it is only the object $\mathfrak{S}$ and the apparatus $\mathfrak{A}$ which pertain to the external world, to that which he calls "objective." By contrast, he has *with himself* relations of a very peculiar character: he has at his disposal a characteristic and quite familiar faculty which we can call the "faculty of introspection." For he can immediately give an account of his own state. It is in virtue of this "immanent cognition" ("connaissance immanente") that he lays claim to the right to create for himself his own objectivity, namely, to cut the claim of statistical coordination expressed by $\Sigma c_j \sigma_j \alpha_j \alpha_j'$ by stating: "I am in state α_j'" or more simply: "I see $A = a_j$," or even directly "$S = s_j$."[35]

It will be recalled[36] that not long after the publication of von Neumann's theory of measurement Schrödinger presented his famous thought-experiment now known as "Schrödinger's cat." According to London and Bauer, whether the cat is alive or is dead is decided only at the moment when the observer opens the steel chamber and "looks" at the cat. The von Neumann, London, and Bauer recourse to human consciousness in their quest for a solution of a problem which their physical theory failed to resolve may be compared with the invocation of "God's sensorium" by Isaac Newton in his search for a fundamental inertial reference system which his equations failed to produce.[37]

The idea of regarding mental activity as a physical interaction, as the London and Bauer theory suggested, was, strictly speaking, not novel. In fact, the conception of a physiological-physical nature of mind (which still

[34]Cf. P. Luchtenburg, "Erich Becher," *Kantstudien* **34**, 275–290 (1929).

[35]Ref. 29 (p. 42). Here $\mathfrak{A}'$ denotes the observer himself and α_j' one of his states. The original has different notation.

[36]Ref. 6–107.

[37]M. Jammer, *Concepts of Space* (Harvard University Press, Cambridge, 1954, 1969; Harper and Brothers, New York, 1960), pp. 113–117; *Storia del Concetto di Spazio* (Feltrinelli, Milano, 1963, 1966), pp. 100–103; *Das Problem des Raumes* (Wissenschaftliche Buchgesellschaft, Darmstadt, 1960), pp. 122–125; *Conceptos de Espacio* (Grijalbo, Mexico D. F., 1970), pp. 148–151.

reverberates in modern expressions like "inspiration") predominated early Western thought. According to Aristotle "Sensation consists in being moved and acted on... it seems to be a sort of *change of state*."[38]

In an interesting essay, "On the Hypothesis That Animals Are Automata, and its History," (1874) Thomas Henry Huxley, the eloquent popularizer of Darwinism, traced the development of this idea through the seventeenth to nineteenth centuries. His account[39] of the 27-year-old sergeant F. in the French army who was wounded by a ball that fractured his left parietal bone sounds almost as if it were a description of a superposition of mental states.[40] "Consciousness," Huxley wrote, "is a direct function of material changes...; all states of consciousness... are immediately caused by molecular changes of the brain-substance." And he even declared that "any state of consciousness is the cause of change in the motion of matter of the organism."[41] But, of course, all such interactionalistic or epiphenomenalistic considerations are without solid scientific foundations. An "autocerebroscope" capable of carrying out an experimental test of the London and Bauer theory of mental reduction of superpositions still remains a dream.

To regard states of perceptual vagueness, indecision, ambivalence, or "preconsciousness" (in Freud's sense) as manifestations of superpositions of mental states would lead to serious difficulties. The conception of superposed mental states therefore seemed hardly to be acceptable. Furthermore, the argument adduced by London and Bauer to save intersubjective agreement seemed, on closer analysis, to contradict their initial assumption that the measuring apparatus is ontologically on a par with the microphysical object. In this context it should also be noted that von Weizsäcker based the inseparability between object and observer [die untrennbare Kette zwischen Objekt und Subjekt] not only on the mental act of cognition [Wissen] but also on that of volition [Wollen]: "Two fundamental functions of consciousness underlie every statement in physics: cognition and volition."[42]

Not only can quantum mechanics—and this according to von Weizsäcker is the essential difference between quantum mechanics and classical physics—not make any statement without referring to the subjective state

[38]Aristotle, *De anima*, 416b, 33–35.

[39]T. H. Huxley, *Science and Culture and other Essays* (Appleton, New York, 1893), pp. 206–252.

[40]*Ibid*., pp. 230–233.

[41]*Ibid*., p. 245.

[42]"...zwei Grundfunktionen des Bewustseins gehen in jeden Satz der Naturbeschreibung ein: Wissen und Wollen." C. F. von Weizsäcker, "Zur Deutung der Quantenmechanik," *Zeitschrift für Physik* **118**, 489–509 (1941).

of knowledge; its statements are also always the result of volitional decisions on the part of the observer. This follows most clearly from the fact that "the ψ-function expresses the probability of every possible result of every possible experiment,"[43] where the first "possible" acknowledges the subjective state of ignorance [Nichtwissen] but the second "possible" the voluntary decision to perform an experiment or to do without it. This situation is well illustrated by von Weizsäcker's analysis of Heisenberg's gamma-ray microscope thought-experiment:[44] whether the electron has a sharp momentum (plane wave) or a sharp position (spherical wave) depends on the observer's *volitional decision* where to place the photographic plate.

11.4. ALTERNATIVE THEORIES OF MEASUREMENT

One of the first who rejected von Neumann's theory of measurement was Henry Margenau.[45] We have already discussed his arguments against the projection postulate, how, by rejecting the postulate, he thought he had solved the difficulties encountered in the EPR argument, and how, as a result of his correspondence with Einstein, he was led to distinguish measurements sharply from state preparations. A measurement was defined by him as any "physical operation by means of which the numerical value of a physical quantity can be determined."[46]

The indeterminacy in measurements, according to Margenau, has its origin in the preparation of the state and not in the measurement itself. If in a beta-ray spectrometer, used to measure the momentum of a free electron, the Geiger counter were not present, it would be undetermined whether the electron arrives at all at the place where the counter would have been placed.

We have also mentioned Margenau's objections to the claim that simultaneous measurement of complementary quantities cannot be performed, and also his interpretation of the Heisenberg principle as a statistical dispersion relation. Concerning the resolution of superpositions by a mental act, as suggested by London and Bauer, Margenau wrote:

> One might for instance be looking at a recording device and, while daydreaming, fail to take conscious cognizance of the registration. All *physical* processes, as the term is ordinarily interpreted, are the same as if he were

[43] *Ibid.*, p. 504. Cf. also C. F. von Weizsäcker, Ref. 6–77 (4th ed., p. 89).
[44] Ref. 6–31.
[45] Ref. 6–123.
[46] Ref. 6–126 (pp. 356–357).

> taking conscious notice of the result, but in one case the state function develops continuously, in the other it changes abruptly.... If the absurdity is justified, as is sometimes done, by the observation that processes in the human brain do matter and that ego is to be introduced into the scheme somewhere, the only significant reply is, we believe, that quantum mechanics does not as yet pretend to be a psychological theory. As such it would have to show a little more competence in the purely psychological realm.[47]

Since according to Margenau a measurement, such as that of the polarization of a photon by two polaroids and a photographic plate (without the latter it is no measurement), annihilates not only the *state* of the observed object but quite often also the *object* itself, the assumption of acausal transition of states is, in his view, unnecessary and misleading.

That not every measurement satisfies the conditions that if it yields a certain result it produces, if immediately repeated, the same result with certainty, had been stressed already by Landau and Peierls.[48] A measurement, they pointed out, may determine the state which belongs to the object *after* the measurement has terminated or it may reveal the state in which the object was *before* the measurement began.[49] In classical mechanics, where the interaction between object and apparatus is negligible, no such distinction need be made and every measurement is reproducible; but this is not so in quantum mechanics. In fact, Landau and Peierls found that a measurement is repetitive in the foregoing sense only if the measured observable commutes with the Hamiltonian of the interaction energy (between object and apparatus); and since their interaction energy is always a function of the position coordinates only measurements of such coordinates, they concluded, may be repetitive in the sense explained.

Pauli,[50] also, following Landau and Peierls, distinguished between measurements whose immediate repetitions reproduce the original results, which he called "measurements of the first kind," and measurements which change the state of the system in a controllable way so that a second measurement will not reproduce the result of the first but the value of the observable before the measurement can be unambiguously inferred; these

[47] *Ibid.*, p. 367.

[48] Ref. 5–50.

[49] Landau and Peierls did not elaborate on the methodological importance of this difference: a physical theory is verified (or falsified) by confirming (or disaffirming) its predictions. This requires the availability of measurements which ascertain the values of state variables possessed by the object *before* the measuring act interferes with it. It would be difficult to accept a physical theory as verifiable (or falsifiable) if its principles admit only measurements which yield information about the state of the object after the measuring act unless the premeasurement values can be unambiguously inferred.

[50] Ref. 3–13 (1958, p. 73).

he called "measurements of the second kind." A spin measurement by the Stern-Gerlach methods belongs to the first category, a determination of an atomic energy level by means of inelastic electronic collisions to the second. Every measurement of the first kind—and in particular each measurement which satisfies the projection postulate—is also a state preparation (in the sense of Margenau), but a state preparation is not necessarily a measurement.

Margenau's article "Critical Points in Modern Physical Theory,"[51] in which he presented his unconventional ideas on measurement, prompted Pascual Jordan,[52] in a contribution to a symposium on quantum mechanics held in 1949, to propose a new approach to the clarification of the process of measurement by rejecting the traditional view that "measurement" is a fundamental concept which ought not to be analyzed. Using Margenau's example of the polarization measurement of a photon Jordan pointed out that the reduction of the superposition or "decision," as he called it, has not taken place as long as one or the other of the waves, which are still able to interfere with each other, has not fallen on a photographic plate. But as soon as it has, "this photographic plate makes the decision." A real physical process and not a mental act in the mind of the observer has to turn the pure case into a mixture. "The essential point," Jordan declared, "seems to me to be that this process must be a *macrophysical* one... (for) it belongs to the definition of macrophysics that we are here never faced with the characteristic microphysical features of complementarity." Jordan now pointed out that in each measurement the microphysical object indeed leaves a trace of macrophysical dimensions, usually in form of an avalanche process such as in a Wilson chamber or in a counter; and since a macrophysical system, endowed with objectivation and reality in the sense of classical physics, is a large accumulation of microphysical individuals and from an observation consequences are drawn "about the probabilities of experiments... *afterwards*," a temporal relation that cannot be reversed, the key for the solution of the reduction problem must lie in thermodynamics or thermodynamical statistics in which similar features manifest themselves.

Jordan's idea of explaining the reduction of superpositions as a thermodynamic irreversible process was pursued with great ingenuity by Günther Ludwig to whom reference has been made in Section 8.7. Confining himself to measurements of the first kind (in the sense of Pauli)

[51]Ref. 6–126.

[52]P. Jordan, "On the process of measurement in quantum mechanics," *Philosophy of Science* **16**, 269–278 (1949).

Ludwig[53] tried to demonstrate that the relation between the two changes discussed by von Neumann is the same as the relation between reversible microphysical processes and irreversible macrophysical processes. His argument was based on the thermodynamic properties of metastable states and, in particular, on the ergodic theorem, which under certain assumptions concerning the statistical distribution of energy eigenvalues (Pauli's *Unordnungsannahme*) entails the H-theorem and hence assures the tendency of a metastable system to a stable state. The apparatus is regarded as a macrosystem in a thermodynamically metastable state and hence capable of evolving toward a thermodynamical stable state subjected to a perturbation by a microphysical system. It was of course well known that ionization and cloud chambers, photographic emulsions, electron multipliers, and the more recently invented bubble chambers contain matter in a metastable state which precipitates into a state of stable equilibrium if triggered by a small perturbation. It is the task of the theory of measurement to correlate the states of the microsystem with the stable equilibrium states of the macrosystem.

Due to such thermodynamic processes a measurement is "an inseparable chain, not between object and subject" as von Weizsäcker thought, but between a microphysical and a macrophysical system.[54]

Consequently, Ludwig went on, the essential difference between quantum mechanics and classical physics is not grounded on the varying role of the observer, as claimed by von Weizsäcker, but on the fact that only in classical physics is it possible to infer the form of the object from the form of its effect: if the impression made by an object on a soft (plastic) support is spherical or oblong, classical physics legitimately infers that the object itself is of a spherical or, respectively, oblong form; if, however, an electron interacting with a diffraction device produces a wavelike effect (interference), or if one interacting with a counter produces a corpusclelike effect, quantum mechanics cannot legitimately infer that the electron is a wave or, respectively, a corpuscle.[55]

Although Ludwig thus rejected von Weizsäcker's thesis of the dependence of quantum mechanical statements upon the cognitive act of the observer he admitted their dependence on the volitional interference of the

[53]G. Ludwig, "Der Messprozess," *Zeitschrift für Physik* **135**, 483–511 (1953); *Die Grundlagen der Quantenmechanik* (Springer, Berlin, Göttingen, Heidelberg, 1954), Chapter 5, pp. 122–165.
[54]G. Ludwig, "Die Stellung des Subjekts in der Quantentheorie," *Veritas-Iustitia-Libertas: Festschrift zur 200-Jahrfeier der Columbia University, New York (überreicht von der Freien Universität Berlin und der Deutschen Hochschule für Politik)* (Colloquium-Verlag, Berlin, 1954), pp. 262–271.
[55]Ludwig obviously alluded to Kant's example of a sphere on a pillow. Cf. I. Kant, *Kritik der reinen Vernunft* (2nd ed., pp. 248–249).

observer which, however, is confined to the macroscopic realm and primarily to the technical construction and optional application of measuring devices. The question why human beings cannot directly interfere with microscopic objects is left unanswered.

In 1955 in a very readable essay Ludwig summarized his theory of measurement. Quantum mechanics, he wrote,[56] studies at macroscopic bodies thermodynamically irreversible processes which it regards as caused by microscopic objects and describes the correlations among the occurrences of such effects. Its indeterminism reflects the impossibility of preparing, by means of macroscopic devices, ensembles of such a kind that all their individual members will subsequently produce at all other macroscopic devices the same effects. Quantum mechanics thus presupposes the existence of macroscopic bodies of classically describable properties and, in particular, the existence of the second law of thermodynamics. Since, however, macroscopic bodies are presumably composed of atoms it must be shown how statements about micro-objects inferred from their macroscopic effects explain the behavior of macro-objects just as required, for example, by the second law. More precisely, it must be shown how the reversible change in accordance with Schrödinger's equation develops in the limiting case of macroscopic objects into the irreversible process characteristic of measurements. Only such a proof would assure the logical consistency of quantum mechanical description.

Ludwig's attempt to explain measurement as an irreversible recording process in a macroscopic device, triggered by a microscopic event, led to the introduction of the notion of "macroscopic observables," to a reexamination of the relations between quantum mechanics and macroscopic (classical) physics which cannot simply be regarded as being obtainable by the limit process $h \to 0$, and in particular to the question how far and under what precise conditions classical physics can be derived from quantum physics.

N. G. Van Kampen[57] had just shown, independently of Ludwig's investigations, that certain equations in statistical mechanics (concerning occupation numbers), sufficient to derive Onsager's phenomenological irreversible thermodynamics, can be accounted for on the basis of purely quantum theoretical arguments (even without the theory of perturbations). Ludwig's pupil Hermann Kümmel[58] specified the precise conditions to be

[56] G. Ludwig, "Zur Deutung der Beobachtung in der Quantenmechanik," *Physikalische Blätter* **11**, 489–494 (1955); reprinted in *Erkenntnisprobleme der Naturwissenschaften*, L. Krüger, ed. (Kiepenheuer & Witsch, Köln, Berlin, 1970), pp. 428–434.

[57] N. G. Van Kampen, "Quantumstatistics of irreversible processes," *Physica* **20**, 603–622 (1954).

[58] H. Kümmel, "Zur quantentheoretischen Bergründung der klassischen Physik," *Nuovo Cimento* **1**, 1057–1077 (1955); **2**, 877–897 (1955).

imposed on the energy eigenvalues and eigenfunctions associated with the macroscopic observables that suffice for a quantum mechanical derivation of the dynamics of gases and fluids (Euler's equations) and, in his second paper, of the basic laws of the statistics and thermodynamics of reversible and irreversible processes. In view of these promising results toward the establishment of a quantum theory of macrosystems Ludwig's approach laid the foundations for a number of measurement theories or models of the measuring process.[59]

One of the most elaborate of these is undoubtedly the measurement theory proposed by Antonio Daneri, Angelo Loinger, and Giovanni Maria Prosperi[60] of the Istituto Nazionale di Fisica Nucleare in Milan. Having worked, together with Pietro Bocchieri[61] and Antonio Scotti,[62] on the problem of finding the exact ergodicity conditions for the validity of the ergodic theorem in quantum statistical mechanics, and realizing that these ergodicity conditions are satisfied by that class of Hamiltonians for which Léon Van Hove[63] derived a master equation with its implications of irreversibility, and assuming that these results can be extended to most macrosystems, these theoreticians, prompted by Ludwig's work, thought it possible to establish on these foundations a satisfactory quantum theory of measurement. According to their basic idea, the reduction of the quantum state of the observed microsystem is not carried out by its interaction with the macroscopic measuring device but by a process of ergodic character which takes place in the latter well after the interaction has ceased and which forms a pérmanent mark. In accordance with considerations previously worked out by Prosperi and Scotti[64] the macroscopic measuring device $\mathfrak{M}$ is regarded as a system which possesses, apart from its energy E, a second constant of the motion, J, so that a suitable basis $\{\Omega_{ak\nu i}\}$ of vectors can be introduced spanning manifolds $C_{ak\nu}$ in the Hilbert space of

[59] P. K. Feyerabend, "On the quantum theory of measurement" in Ref. 7–102 (pp. 121–130). H. S. Green, "Observation in quantum mechanics," *Nuovo Cimento* **9**, 880–889 (1958).

[60] A. Daneri, A. Loinger, and G. M. Prosperi, "Quantum theory of measurement and ergodicity conditions," *Nuclear Physics* **33**, 297–319 (1962); "Further remarks on the relations between statistical mechanics and quantum theory of measurement," *Nuovo Cimento* **44B**, 119–128 (1966). Cf. also G. M. Prosperi, "Quantum theory of measurement," in *Encyclopedic Dictionary of Physics*, Supplementary volume 2 (Pergamon Press, Oxford, 1967), pp. 275–280.

[61] P. Bocchieri and A. Loinger, "Ergodic foundation of quantum statistical mechanics," *Physical Review* **114**, 948–951 (1959).

[62] G. M. Prosperi and A. Scotti, "Ergodic theorem in quantum mechanics," *Nuovo Cimento* **13**, 1007–1012 (1959).

[63] L. Van Hove, "The ergodic behavior of quantum many-body systems," *Physica* **25**, 268–276 (1959).

[64] G. M. Prosperi and A. Scotti, "Ergodicity conditions in quantum mechanics," *Journal of Mathematical Physics* **1**, 218–221 (1960).

$\mathfrak{M}$. The indices a and k denote, respectively, the values of E and J, which do not change during the free evolution of the system, and ν denotes the value of the macrovariables which do. Each energy "shell" C_a (i.e., for given a, the manifold of energy eigenvectors corresponding to the energy interval $E_a, E_{a+1} = E_a + \Delta E$) is thus partitioned into orthogonal submanifolds or "channels" C_{ak} (spanned by the eigenvectors of k, corresponding to the interval $J_k, J_{k+1} = J_k + \Delta J$) in which ergodicity conditions are assumed to hold. Hence if the number of dimensions of C_{ak} is denoted by s_{ak} and that of its "cells" $C_{ak\nu}$ by $s_{ak\nu}$, and if within the channel C_{ak} there exists a cell C_{ake_k} such that $s_{ake_k} \gg s_{ak\nu}$ for all $\nu \neq e_k$, the system $\mathfrak{M}$, initially in a state belonging to the channel C_{ak}, will evolve spontaneously toward one of the macroscopic equilibrium states in the cell C_{ake_k}. If, therefore, the measuring device $\mathfrak{M}$ is initially in the equilibrium cell C_{a0e} of a certain channel C_{a0} and the interaction of the microsystem $\mathfrak{S}$ with $\mathfrak{M}$ induced a transition (depending on the state of $\mathfrak{S}$) of the state of $\mathfrak{M}$ from the channel C_{a0} to C_{ak} within the same energy shell, $\mathfrak{M}$ will reach at the end of the measuring process the final equilibrium macrostate C_{ake_k} whereby it registers the initial state of the microsystem $\mathfrak{S}$. (For a more detailed exposition of the Daneri, Loinger, and Prosperi quantum theory of measurement the reader is referred to the original papers[65].)

According to these authors the objection that quantum theory is capable of explaining how microphysical objects behave but not how their behavior can be observed has now been overcome by their theory of measurement which therefore constitutes, in their view, "an indispensable completion and a natural crowning of the basic structure of present-day quantum mechanics."[66] Their view was fully endorsed by Léon Rosenfeld, who declared that with their "very thorough and elegant discussion of the measuring process [these] Italian physicists have conclusively established the full consistency of the [quantum mechanical] algorism, leaving no loophole for extravagant speculations."[67] Their analysis, Rosenfeld declared, shows "quite clearly the origin and the character of the 'reduction' of the initial state of the atomic system, prescribed by the algorism of quantum mechanics," and by applying the ergodic theory to the measurement problem they translated into mathematical language a physical argument "which can be more briefly stated in terms of simple concepts,

[65]For a qualitative presentation see P. Caldirola, "Teoria della misurazione e teoremi ergodici nella meccanica quantistica" ("Theorie de la mesure et theoremes ergodiques en mecanique quantique"), *Scientia* **99**, 219–231 (129–140) (1964).
[66]Ref. 60 (1966, p. 127).
[67]L. Rosenfeld, "The measuring process in quantum mechanics," *Supplement of the Progress of Theoretical Physics,* Commemoration Issue for the 30th Anniversary of the Meson Theory by Dr. H. Yukawa, 1965, pp. 222–231.

referring directly or indirectly to common experience."

Since these words obviously refer to Bohr's attitude toward the measurement problem as outlined above, Rosenfeld's unqualified endorsement of the Daneri, Loinger, and Prosperi measurement theory raises the question whether this theory is really congenial, or at least not incompatible, with the basic tenets of the Copenhagen interpretation. This question has been answered in the negative by Bub. In addition to his criticizing the theory as having failed to accomplish its program, since the macrostates still permit the occurrence of superpositions and hence have not been shown to be "objective" in the sense of classical macrostates, Bub[68] pointed out that the measurement theory under discussion "is basically opposed to Bohr's ideas." For it applies classical concepts at the macrolevel on the basis of purely quantum theoretical arguments and thus regards classical mechanics as an "approximation to a quantum theory of macrosystems" and hence as replaceable in principle by the quantum theory, whereas according to Bohr the conceptual framework of classical mechanics is indispensable for a description of quantum phenomena and hence logically prior to any application of quantum mechanics.

The Daneri, Loinger, and Prosperi theory was severely criticized by Jauch, Wigner, and Yanase.[69] In one of their arguments they pointed out that the theory which, as will be recalled, viewed the microscopic part of the measuring process as a triggering device of an ergodic amplification in the macroscopic measurement apparatus, does not apply to "negative-result measurements" as discussed by M. Renninger. In measurements of this kind superpositions of state functions are resolved apparently without any microscopic triggering process.

Because of the general importance of this category of measurements for the whole theory of measurement a digression on this subject seems to be justified.

Mauritius Karl Renninger, a graduate of the Technische Hochschule in Munich (Dr. rer. techn., 1930) and since 1946 at the Crystallographic Institute of the University of Marburg, had been attracted to the foundational problems of quantum mechanics ever since he attended a lecture course on the quantum theory which was given in Stuttgart by Paul P.

[68] J. Bub, "The Daneri-Loinger-Prosperi quantum theory of measurement," *Nuovo Cimento* **57B**, 503–520 (1968).

[69] J. M. Jauch, E. P. Wigner, and M. M. Yanase, "Some comments concerning measurements in quantum mechanics," *Nuovo Cimento* **48B**, 144–151 (1967). This paper provoked Loinger to publish a rather acrimonious reply which ended with the statement that "in future we shall reply to sensible criticisms only." Cf. A. Loinger, "Comments on a recent paper concerning the quantum theory of measurement," *Nuclear Physics* **108**, 245–249 (1968).

Ewald in the late 1930s. In 1953 Renninger published a paper[70] which caused quite a stir, for it contained a thought-experiment designed to prove that, in contrast to the complementarity interpretation according to which a manifestation of the wave-particle duality requires two different and mutually exclusive experimental arrangements, one and the same experimental arrangement may exhibit both duality aspects. It was a cleverly designed interference experiment involving moveable mirrors, half-silvered plates, and $\lambda/2$-flats,[71] and it seemed to imply that the photon is a particle which traverses a continuous trajectory in space and time but also spreads out like a field or a wave. It led Renninger to conclude that "every quantum is an energy-corpuscle which is 'carried' or 'guided' by an energy-free wave."[72]

Aware of the fact that a similar conception had been proposed as early as 1927 by Louis de Broglie[73] in his theory of the "*onde pilote*," Renninger saw the importance and novelty of his experiment in its presentation of these two components, the corpuscle and the wave features, as "plain experimental data," preceding explanation and theory. In an address[74] at the Innsbruck meeting of German and Austrian physicists attended by Heisenberg, Schrödinger, and von Laue, Renninger pointed out that it was not his intention to replace the orthodox interpretation by proposing a new theory but only to disclose what a *complete* theory of microphysical phenomena should be able to explain.

Having sent to Einstein a preprint of his 1953 paper Renninger received this reply: "It gave me great pleasure to read your careful investigations the result of which fully agrees with my own view on this matter: in the concrete individual case one has to ascribe real existence both to the wave field and to the (more or less) localized quantum unless he is ready to admit a telepathic coupling between objects in different regions of space."[75] Later, Einstein wrote to Renninger: "I think it very reasonable

[70]M. Renninger, "Zum Wellen-Korpuskel-Dualismus," *Zeitschrift für Physik* **136**, 251–261 (1953).

[71]For details the reader is referred to Ref. 70.

[72]"Jedes Quant besteht aus einer Energiekorpuskel, die von einer energielosen Welle 'getragen' oder 'geführt' wird." *Op. cit.*, p. 253.

[73]Renninger translated into German the introductory chapters of de Broglie's *La Physique quantique restera-t-elle indéterministe*? (Gauthier-Villars, Paris, 1953) and published this translation under the title "Wird die Quantenphysik indeterministisch bleiben?" in *Physikalische Blätter* **9**, 488–548 (1953).

[74]"Experimentalphysikalische Überlegungen zum Wellen-Korpuskel-Dualismus" (unpublished), read at the Insbruck meeting (September 20–25, 1953).

[75]"*Ich hatte viel Freude am Studium Ihrer sorgfältigen Überlegungen, deren Ergebnis mit dem meiner eigenen durchaus übereinstimmt: Man muss für den realen Einzelprozess sowohl dem Wellenfeld als auch dem (mehr oder weniger) lokalisierten Quant reale Existenz zuschreiben,*

that with your thought-experiment you drew attention again to the fact that the wave-particle duality is a reality which cannot be dodged by metaphysical tricks."[76] From Max Born Renninger obtained this answer: "Your view is precisely the same as the one I have espoused from the beginning (1926): both particles and waves have some sort of reality, but it must be admitted that the waves are not carriers of energy or momentum."[77] However, as it transpires from their later correspondence, Born did not agree with Renninger's conclusions, nor did Pascual Jordan, with whom Renninger had a long exchange of letters.

In 1960 Renninger published a paper[78] that also touched on deep-seated problems of the interpretation of quantum mechanics and became the subject of much discussion. Renninger began his paper by stating that Heisenberg's relations were generally interpreted as arising from the fact that in a measurement the interference with the object by the measuring device cannot be eliminated even in principle, and as testimonies he quoted, apart from earlier writings by Bohr and Jordan, statements made to this effect by Heisenberg in 1958 and by Léon Brillouin in 1959. He thought it therefore imperative to point out that there exist certain kinds of measurement which, in his opinion, do not interfere at all with the object of the measurement. They are characterized by the fact that they produce the measurement result not through the occurrence of a physical event as in the ordinary type of measurements, but *through the very absence of such an occurrence.*[79]

The following thought-experiment, according to Renninger, is a simple example of such a "negative-result experiment." At time $t=0$ a photon is

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wenn man telepathische Kupplung zwischen in verschiedenen Raumteilen vorhandenen Dingen nicht anzunehmen bereit ist." Letter from Einstein to Renninger, dated May 3, 1953.

[76]"Ich finde es recht vernünftig, dass Sie durch Ihr Gedankenexperiment die Menschen aufs Neue darauf aufmerksam gemacht haben, dass die Dualität Welle-Korpuskel eine Realität ist, über die man sich nicht mit metaphysischen Kunststücken hinwegtäuschen sollte." Letter from Einstein to Renninger, dated February 27, 1954.

[77]"Ihre Auffassung ist genau dieselbe, die ich von Anfang an (1926) vertreten habe: Dass sowohl die Teilchen wie die Wellen eine bestimmte Art Realität haben, wobei man in Kauf nehmen muss, dass die Wellen nicht Träger von Energie und Impuls sind." Letter from Born to Renninger, dated May 23, 1955. In the sequel Born referred to pages 103 and 105 of his book *Natural Philosophy of Cause and Chance* (Clarendon Press, Oxford, 1949) where he expressed such ideas and added that the conception of two complementary aspects being involved is a later misinterpretation of Bohr's notion of complementarity which refers to the measurement of conjugate variables and their corresponding arrangements.

[78]M. Renninger, "Messungen ohne Störung des Messobjekts," *Zeitschrift für Physik* **158**, 417–421 (1960).

[79]The idea of "negative-result experiments" was, of course, conceived long before Renninger. An interesting example of such an experiment and in many respects similar to Renninger's is the moveable mirror experiment described by P. S. Epstein, Ref. 6–129.

emitted from a point P which is partially surrounded at a radial distance R_1 by a spherically shaped screen S_1 of solid angle $4\pi-\Omega$ and is also completely surrounded at a greater distance R_2 by a closed spherical screen S_2. If at $t=R_1/c$ no scintillation has been observed on S_1, the wave packet of the photon, which prior to t_1 was a superposition of two components, one associated with the impinging on S_1 and the other with that on S_2, has been resolved to the second component even before (i.e., for $t_1<t<t_2$) the photon arrives at S_2 at time t_2. Renninger did not deny that the very *existence* of S_1, that is, the *possibility* of observing photons by means of S_1, affects the object even if it does not hit S_1; the very diffraction, produced by S_1, of the image on S_2—especially if Ω is small—proves this fact. According to Renninger this disturbance has not been caused by the measuring process itself but rather has its origin in the initial form of the wave packet.

Heisenberg, having read a preprint of this paper and having been asked for his comments, wrote[80] to Renninger that the thesis of the Copenhagen interpretation concerning the unavoidability of the interference with the object in a measurement does not necessarily refer to the actual measurement procedure [*Mess-Vorgang*]; the mere existence of the measuring apparatus already constitutes such an interference. "The act of recording, on the other hand, which leads to the reduction of the state, is not a physical but rather, so to say, a mathematical process. With the sudden change of our knowledge also the mathematical presentation of our knowledge undergoes of course a sudden change." Heisenberg's reaction to Renninger's thought-experiment was similar to Bohr's reply to the Einstein-Podolsky-Rosen argument with which Renninger's thought-experiment has certain features in common.

That in such "negative-result measurements" quantum ergodity plays a substantial role even if no "triggering" occurs, has been argued by Loinger in his reply, mentioned in reference 69; his argument is based on an analysis of a negative-result version of a Stern-Gerlach spin-component measurement.

The preceding development originated, as we have seen, with Jordan's criticism of the epistemological implications of von Neumann's theory of measurement. A more formal criticism of this theory was launched in 1952 by Eugen P. Wigner[81] who pointed out that von Neumann's basic assumption $\epsilon_H(t)\sigma_k(x)\alpha(y)=\sigma_k(x)\alpha_k(y)$ [cf. (4)] can be strictly valid only for the measurement of an observable whose corresponding operator commutes

[80]Letter of February 2, 1960.

[81]E. P. Wigner, "Die Messung quantenmechanischer Operatoren," *Zeitschrift für Physik* **133**, 101–108 (1952).

with that of any observable which represents a conserved quantity of the system. To prove that the presence of a conservation law imposes such a limitation on measurability Wigner considered the simple case of measuring the spin x-component of a spin-$\frac{1}{2}$ particle while the z-component of the total angular momentum is assumed to be conserved. His proof has been generalized by Huzihiro Araki and Mutsuo M. Yanase,[82] two Japanese physicists who worked for some time at Wigner's Institute in Princeton. The following is a simplified proof of the general theorem.

If $\mathcal{H}_1$ and $\mathcal{H}_2$ are, respectively, the Hilbert spaces of the observed system $\mathfrak{S}$ and the measuring apparatus $\mathfrak{A}$ so that the unitary operator $U = U_H(t)$ operates in the product space $\mathcal{H}_1 \otimes \mathcal{H}_2$ of the composite system, (4) can be written

$$U\sigma_k(x) \otimes \alpha(y) = \sigma_k(x) \otimes \alpha_k(y)$$

where

$$(\sigma_k, \sigma_{k'}) = (\alpha_k, \alpha_{k'}) = \delta_{kk'}. \tag{12}$$

The conserved quantity, represented by the self-adjoint operator B, assumed to be additive in the sense that

$$B = B_1 \otimes 1 + 1 \otimes B_2, \tag{13}$$

satisfies

$$UB - BU = 0.$$

Hence

$$(\sigma_{k'} \otimes \alpha, B(\sigma_k \otimes \alpha)) = (U(\sigma_{k'} \otimes \alpha), UB(\sigma_k \otimes \alpha)) = (\sigma_{k'} \otimes \alpha_{k'}, B(\sigma_k \otimes \alpha_k))$$

or

$$(\sigma_{k'}, B_1\sigma_k)(\alpha, \alpha) + (\sigma_{k'}, \sigma_k)(\alpha, B_2\alpha) = (\sigma_{k'}, B_1\sigma_k)(\alpha_{k'}, \alpha_k) + (\sigma_{k'}, \sigma_k)(\alpha_{k'}, B_2\alpha_k),$$

which shows that

$$(\sigma_{k'}, B_1\sigma_k) = 0 \qquad \text{if} \quad k \neq k'. \tag{14}$$

With S decomposed into projection operators

$$S = \sum s_k P_k \tag{15}$$

where

$$P_k = P_{[\sigma_k]}$$

[82]H. A. Araki and M. M. Yanase, "Measurement of quantum mechanical operators," *Physical Review* **120**, 622–626 (1960).

is the projection operator whose range is the subspace spanned by σ_k so that

$$P_k \sigma_{k'} = \delta_{kk'} \sigma_{k'}, \tag{16}$$

and from (2), (4), and the self-adjoint nature of P_k it follows that

$$(\sigma_{k'}, P_k B_1 \sigma_{k''}) = (P_k \sigma_{k'}, B_1 \sigma_{k''}) = \delta_{kk'}(\sigma_{k'}, B_1 \sigma_{k''}) = \delta_{kk'}\delta_{k'k''}(\sigma_{k'}, B_1 \sigma_{k''})$$

$$(\sigma_{k'}, B_1 P_k \sigma_{k''}) = \delta_{kk''}(\sigma_{k'}, B_1 \sigma_{k''}) = \delta_{kk''}\delta_{k'k''}(\sigma_{k'}, B_1 \sigma_{k''}). \tag{17}$$

Since every P_k commutes with B_1, S itself commutes with B. Hence (4) can hold only if S commutes with all additively conserved observables.[83]

However, this limitation, as Wigner pointed out earlier, loses its stringency if the measuring apparatus $\mathfrak{M}$ is large in the sense that its state is a superposition of sufficiently many states with different quantum numbers of the conserved quantity B or, in other words, if it contains many quanta of the conserved observable. Araki and Yanase[84] showed that even if S and B do not commute, an approximate measurement of S can be carried out to an arbitrary degree of accuracy provided B has a discrete spectrum and B_1 only a finite number of eigenvalues. Soon afterward Yanase[85] was able to define, in the general case, for the accuracy of measurement an upper limit in terms of the "size" of the apparatus, this "size" being defined as a function (mean square value) of the measured quantity of the apparatus.

In the early 1960s Wigner became deeply interested in the physical, and perhaps even more so in the epistemological aspects of the measurement problem. He chose this theme for his contribution to the 98th Annual

[83]Objections against these results of Wigner, Araki, and Yanase were raised by Yakir Aharonov in a conversation with Wigner in Princeton, 1961. Aharonov denied that the limitations on interactions imposed by conservation laws restrict measurability in nonrelativistic quantum mechanics. If the variables, he claimed, are defined *relative* to some reference frame—as they must be defined, for in view of the invariance principle they cannot be given any absolute meaning—they can be measured accurately without violating the invariance principle. Wigner expressed great interest in Aharonov's comments ("there is something of great value in your observation," letter to Aharonov from Wigner, dated April 18, 1961) but found his reasoning incomplete. Aharonov has meanwhile elaborated his theory of measurement and has lectured on it on various occasions (Hebrew University, Jerusalem, spring term 1972; Max-Planck-Institute, Munich, summer 1972). Cf. also Y. Aharonov and A. Petersen,"Definability and measurability in quantum theory" (preprint). Aharonov intends to publish his ideas on this issue and other aspects in a book, tentatively called *"A New Approach to Quantum Mechanics."*
[84]Ref. 82.
[85]M. M. Yanase, "Optimal measuring apparatus," *Physical Review* **123**, 666–668 (1961).

meeting of the U.S. National Academy of Science and for his talk at the Vienna Congress of German and Austrian physicists (October 16-22, 1961).[86] Particularly thought-provoking was his short essay "Remarks on the Mind-Body Question,"[87] in which he proposed what became known as "the paradox of Wigner's friend." Accepting the London and Bauer subjectivist interpretation of measurement according to which the reduction of a superposition is actualized only by the consciousness of the observer, Wigner considered the following situation.

The observable of an object S, having only two eigenstates σ_1 and σ_2, is observed by a "friend," it being assumed that the initial state of the object is a linear combination $c_1\sigma_1 + c_2\sigma_2$. The state of the system object-plus observer after the interaction is consequently $c_1\sigma_1\alpha_1 + c_2\sigma_2\alpha_2$ where α_1 and σ_2 denote states of the friend. If Wigner now asks his friend whether he saw S_1 (the eigenvalues corresponding to σ_1) in which case, say, a light-flash would be visible, the answer will be positive with a probability $|c_1|^2$ and negative with probability $|c_2|^2$; in case the answer is positive, the object will also give Wigner the responses as if it were in the state σ_1. The situation is thus that discussed by London and Bauer[88] $\left(\sum_{j=1}^{2} c_j\sigma_j\alpha_j\alpha_j'\right)$ but with $\mathfrak{A}$ replaced by a conscious observer, the "friend." If now, after having completed the whole experiment, Wigner asks his friend, "What did you see before I asked you?" he will answer, "I told you already, I did [did not] see S_1," as the case may be. Since the question whether he did or did not see S_1 was therefore already decided in his mind before Wigner's question, it has to be concluded that the state immediately after the interaction of friend and object was already either $\tau_1 = \sigma_1\alpha_1$ or $\tau_2 = \sigma_2\alpha_2$ and not $\tau = c_1\sigma_1\alpha_1 + c_2\sigma_2\alpha_2$ (which has properties different from those of the former) before any interference with the natural time development of the two interacting systems (objects + friend) has occurred. If we substitute for the "friend" an atom which may or may not be excited by the light-flash, Wigner wrote, this difference would have observable effects and there can be no doubt that τ alone describes the properties of the combined system correctly, and Wigner added:

[86]E. P. Wigner, "Theorie der quantenmechanischen Messung," in *Physikertagung, Wien 1961* (Physik Verlag, Mosbach, 1962).

[87]Published in *The Scientist Speculates—An Anthology of partly-baked Ideas*, I. J. Good, ed. (W. Heinemann, London, 1961; Basic Books, New York, 1962), pp. 284–302; reprinted in E. P. Wigner, *Symmetries and Reflections* (Indiana University Press, Bloomington and London, 1967), pp. 171–184.

[88]Ref. 35.

> If the atom is replaced by a conscious being the wave function τ appears absurd because it implies that my friend was in a state of suspended animation before he answered my question. It follows that the being with a consciousness must have a different role in quantum mechanics than the inanimate measuring device: the atom considered above. In particular, the quantum mechanical equations cannot be linear if the preceding argument is accepted. This argument implies that "my friend" has the same types of impressions and sensations as I—in particular, that, after interacting with the object, he is not in that state of suspended animation which corresponds to the wave function τ. It is not necessary to see a contradiction here from the point of view of orthodox quantum mechanics, and there is none if we believe that the alternative is meaningless, whether my friend's consciousness contains either the impression of having seen a flash or of not having seen a flash. However, to deny the existence of the consciousness of a friend to this extent is surely an unnatural attitude, approaching solipsism, and few people, in their hearts, will go along with it.[89]

Wigner saw in this argument an indication that consciousness or mind influences the physicochemical conditions of living systems just as it is influenced by them.

Since then the mysteries of the coexistence of mind and matter and of their possible mutual interaction have preoccupied Wigner's thought.[90] Shortly after he published his primarily philosophical essay containing "the paradox of the friend" Wigner wrote a more technical paper,[91] "The Problem of Measurement," in which he reviewed von Neumann's theory of measurement and showed that essentially it is the only one that is compatible with quantum mechanics as generally conceived. His main thesis in this paper was to prove that the assumption that the state of the combined system of object-plus-apparatus is after the measurement a mixture of states each with one definite position of the pointer, conflicts with the equations of motion in quantum mechanics. If one therefore insists on the necessity of such a type of measurement procedure quantum mechanics in its generally accepted formulation can have only limited validity and, in particular, its linear equation of motion (Schrödinger equation) has to be replaced by a nonlinear one.

A dramatic description of the conceptual difficulty, characterized by Wigner's "paradox of the friend," was given by Hugh Everett III in a paper entitled "Theory of the Universal Wave Functions," which was

[89]Ref. 87 (p. 294).

[90]E. P. Wigner, "Two kinds of reality," *The Monist* **48**, 248–264 (1964); reprinted in Ref. 87 (1967, pp. 185–199); "Epistemology of quantum mechanics—Its appraisal and demands," *Psychological Issues* **6**, (No. 2, Monograph 22), 22–49 (1967–1968).

[91]E. P. Wigner, "The problem of measurement," *American Journal of Physics* **31**, 6–15 (1963).

published in 1973 (see Ref. 109 below) and which will be discussed in a later context. Everett considered the following situation.

In a laboratory L, isolated in space, an observer A performs a measurement of an observable X on a system S in a state σ which is not one of the eigenstates ψ_k of X, the probability of obtaining the result x_j being $|(\psi_j,\sigma)|^2$. Immediately after the measurement at time t_1, A records his result, say x_n, in a notebook. A second observer B, outside L, is supposed to know the state φ of L and its contents (including S,A, the measuring devices and the notebook) at a time t_0 just prior to t_1. Interested in what will be found in the notebook at time t_2, a week after t_1, B computes in accordance with the Schrödinger equation the state φ of L at time t_2. Clearly, $\varphi(t_2)$ must have nonzero probabilities for several notebook entries, for otherwise A would have been wrong in assuming indeterminacy of the outcome of his measurement. Immediately after t_2, B enters L and looks at the notebook. As an orthodox quantum theorist he informs A that since φ, just prior to his entry into L, had nonzero probabilities for other than the recorded result, the latter must have been decided only when he looked at the notebook. He tells A that "his notebook entry, and his memory about what occurred one week ago had no independent objective existence" until he (B) intervened. "In short, B implies that A owes his present objective existence to B's generous nature which compelled him to intervene on his behalf. However, to B's consternation, A does not react with anything like the respect and gratitude he should exhibit towards B, and at the end of a somewhat heated reply, in which A conveys in a colorful manner his opinion of B and his beliefs, he rudely punctures B's ego by observing that if B's view is correct, then he has no reason to feel complacent, since the whole present situation may have no objective existence, but may depend upon the future actions of yet another observer." Like Wigner, Everett saw in solipsism a logically consistent, but philosophically repugnant, solution of this difficulty. Everett's suggested alternative, his own interpretation, will be discussed in due course.

A proposal to modify quantum mechanics more in the spirit suggested by Wigner was made by Ludwig in his programmatic essay[92] on the quantum mechanical measurement problem by postulating an "inverse correspondence principle," that is, a principle which leads to a definition of macrophysical observables as an extension of quantum mechanical ones so that certain macroscopic observables of the quantum mechanically conceived macroscopic systems represent classical objective properties. This program was based partly on his previously mentioned investigations

[92]G. Ludwig, "Gelöste und ungelöste Probleme des Messprozesses in der Quantenmechanik," in Ref. 2-30 (1961, pp. 150–181).

and partly on subsequent elaborations.[93] Although Ludwig based his exposition on the ensemble interpretation, he admitted the possibility of translating all his statements into the language of any interpretation preferred by the reader, and added that it is anyhow hard to convince somebody else on questions of interpretation. Just as for Wigner, also for Ludwig the measurement problem touched on far-reaching philosophical issues; on these he published a nontechnical essay.[94]

Ludwig's ambitious project of constructing a novel approach to the Hilbert space formalism of quantum mechanics which is based exclusively on "objectively given facts" has been outlined above.[95]

In his attempt to base the quantum theory solely on nonsubjective elements Ludwig derived the physical meaning of its basic notions through logical deductions from macroscopic phenomena which exhibit nonclassical features. He thus began his conceptual reconstruction of quantum mechanics with a theory of measurement without however, adopting, a priori any definite ontology. Precisely the opposite direction of approach, though toward the same aim, namely the complete elimination of all subjectivist elements, was proposed by Mario Bunge, a prolific philosopher of science. In his monograph *Causality* and in his book *Metascientific Queries* which he had written when he was at La Plata and Buenos Aires universities, before he joined in the mid 1960s the Philosophy Department of McGill University in Montreal, Canada, some philosophical problems of quantum mechanics had already been discussed. But his more mature and technically more elaborate ideas on this subject were published[96] primarily in 1967.

Criticizing the usual interpretation as "teeming with the ghostly" and relegating all references to an observer or his activities as belonging to psychology, Bunge claimed to have purged quantum mechanics of its "ghosts" by reconstructing its formalism and its interpretation on the basis of critical realism. By definition, he declared, physics studies physical

[93]G. Ludwig, "Zum Ergodensatz und zum Begriff der makroskopischen Observables," *Zeitschrift für Physik* **150**, 346–374 (1958), **152**, 98–115 (1958).

[94]G. Ludwig, *Das naturwissenschaftliche Weltbild des Christen* (A. Fromm, Osnabrück, 1962), especially Chapter 7 ("Mikrokosmos und Makrokosmos"), pp. 72–89.

[95]Ref. 8-222.

[96]M. Bunge, "Strife about complementarity," *British Journal for the Philosophy of Science* **6**, 141–154 (1955); "Physics and Reality," *Dialectica* **19**, 195–222 (1965); reprinted in *Erkenntnisprobleme der Naturwissenschaften*, L. Krüger, ed. (Kiepenheuer & Witsch, Cologne, Berlin, 1970), pp. 435–457. *Foundations of Physics* (Springer, Berlin, Heidelberg, New York, 1967), Chapter 5; "Analogy in quantum theory: From insight to nonsense," *British Journal for the Philosophy of Science* **18**, 265–286 (1967); "A ghost-free axiomatization of quantum mechanics," in *Quantum Theory and Reality*, M. Bunge, ed. (Springer, Berlin, Heidelberg, New York, 1967), pp. 105–117.

systems and physical systems are, by definition, self-existent and observer-free entities even if they cannot be grasped just as they are. His formulation of quantum mechanics is based on an axiomatized formalism containing 17 primitive terms. In accord with his strict antisubjectivism the meaning of the fundamental notions of this formalism is not specified in terms of measurements or observations but is obtained, rather, by the fact that some of the abstract symbols are assumed to denote objectively existing physical entities just as in classical physics. In spite of its realistic approach Bunge's reconstruction of quantum mechanics led to a probabilistic and hence deterministic theory. In fact, one of the logical consequences he derived from his formalism is Born's probabilistic interpretation, provided it is understood to refer to the probability, not of *finding* the particle within a given region of space, but of the particle's *being* in that region.

As another consequence Bunge also derived the Heisenberg inequalities which appear in his theory as objective scatter relations but not of ensembles, as in the statistical interpretation, but rather as relations pertaining to individual particles. According to Bunge, a physical theory about a class of objectively existing entities should be sharply distinguished from the set of partially theoretical, partially practical precepts and procedures designed to test the theory; nor should the theoretical parts of this set be conceived as a one-to-one translation of the relevant parts of the theory itself into observational language as if the theses of the latter could find their confirmation or refutation in the former. The theory of measurement which Bunge constructed along these lines is, in his view, not an integral part of the theory but only, so to say, an appendix to it. Moreover, this theory of measurement has, as he expressed it, the same fatal shortcomings as the usual theories of measurement: it is generic and therefore concerns no real measurement.

To the class of measurement theories which, like those proposed by Jordan, Ludwig, and the Italian physicists, claim that statistical properties inherent in the necessarily macroscopic measuring device make it possible to explain measurement within the framework of the quantum theory and without recourse to human consciousness and which may therefore be also called objective theories of measurements belongs also the theory suggested by Wolfgang Weidlich of the Technische Hochschule Stuttgart. Weidlich[97] claimed that the main problem, the transformation of the initial state of the combined system object-plus-apparatus characterized by the statistical operator $\rho_{\mathfrak{S}\mathfrak{A}}$ into a mixture characterized by a statistical opera-

[97]W. Weidlich, "Problems of the quantum theory of measurement," *Zeitschrift für Physik* **205**, 199–220 (1967).

tor of the form

$$\bar{\rho}_{\mathfrak{S}\mathfrak{A}} = \sum_k |d_k|^2 \rho_{\mathfrak{S}\mathfrak{A}}^{(k)} \qquad \text{where} \quad d_k = \langle \sigma_k | \sigma \rangle,$$

being the initial state of the object and $|\sigma_k\rangle$ the eigenvectors of the observable measured, can be accounted for by the following postulate. Let $\rho = \sum w_k \rho_k$ describe an ensemble E composed of subensembles E_k of systems $\mathfrak{S}$ in state σ_k, $w_k \geqslant 0$ being the relative frequencies or probabilities with which σ_k and hence S_k, the eigenvalue belonging to σ_k of the measured observable, appears in E; if $\bar{\rho}$ describes a mixture and differs from ρ so that $\bar{\rho} = \rho + \rho'$ and if for arbitrarily chosen fixed states ψ

$$\langle \psi | \rho' | \psi \rangle \ll \langle \psi | \rho | \psi \rangle$$

the w_k retain their meaning; in other words, small deviations in ρ do not change its interpretation. By showing that the nondiagonal terms in the evolving statistical operator converge to zero (in the sense of weak convergence), provided certain conditions for the macroscopic structure of the apparatus and for its interaction with the object systems are met, the preceding condition is shown to hold. Thus, although the phase relations between the expansion coefficients of σ are not destroyed by the unitary time development Weidlich claimed to have shown that the measurement results have an objective (subject-independent) meaning. In collaboration with F. Haake[98] he subsequently set up a simple model (a number of two-level atoms in interaction with an electromagnetic field) of a measuring process to corroborate his approach.

11.5. LATENCY THEORIES

How intimately the interpretation of quantum mechanical measurements is related to the general interpretative problem of quantum mechanics as a whole is strikingly accentuated by Henry Margenau's theory of latent variables or "latency interpretation" of quantum mechanical measurements. It is not a detailed theory of such measurement processes but rather a philosophy of quantum mechanics based on his scientific methodology (P-plane, C-field, etc.) outlined in his magnum opus *The Nature of Physical Reality*.[99] Invited to give the 23rd Joseph Henry lecture before the Philo-

[98]F. Haake and W. Weidlich, "A model for the measuring process in quantum theory," *Zeitschrift für Physik* **213**, 451–465 (1968). Cf. also W. Weidlich and F. Haake, On the quantumstatistical theory of the measuring process," *Journal of the Physical Society of Japan* **26**, Supplement, 231–232 (1969).

[99]H. Margenau, *The Nature of Physical Reality* (McGraw-Hill, New York, 1950).

sophical Society of Washington, on March 26, 1954, Margenau evaluated the advantages and disadvantages of the prevailing interpretations of quantum mechanics.[100]

At the end of his lecture Margenau presented an interpretation which he found most congenial in view of all available evidence: What is real is the state function ψ whereas the classical state observables, such as position, momentum, or energy may be said to be latent in ψ in the sense that their values emerge only in response to measurement. Margenau saw in this view the culmination of a philosophical development of long standing and the logical extension of Galileo's distinction between primary qualities (e.g., shape, size, location), regarded as inalienable and inseparable from the object which "possesses" them, and secondary qualities (e.g., color, sound, taste) which, as John Locke defined them, are "nothing in the objects themselves, but power to produce various sensations in us by their primary qualities." According to Margenau, the distinction between primary and secondary qualities is superseded by that between "possessed" and "latent" qualities or observables, the former exemplified by mass or charge, the latter by the observables with which the quantum mechanical theory of measurement is concerned. As to these Margenau declared: "I believe they are 'not always there,' that they take on *values* when an act of measurement, a perception, forces them out of indiscriminacy or latency, 'just as' happiness, equanimity are observable qualities of man, but they are *latent* qualities which need not be present at all times; they, too, can spring into being or be destroyed by an act of inquiry, a psychological measurement." Margenau even speculated that eventually all physical qualities, with the inclusion of mass and charge, would become "latent observables" just like position, momentum, energy, or spin.[101] Margenau's latency theory offered a simple interpretation of the Heisenberg principle by relating the cause of the spread to the application of noncommuting operators and not to the dynamic variables themselves. Although founded on a different epistemology and methodology it showed a striking similarity to one of the radical formulations of the complementarity interpretation as discussed above.

In fact, Heisenberg's version of the Copenhagen interpretation as expounded in his *Physics and Philosophy*[102] also envisaged the state of a quantum system prior to a measurement as a set of tendencies comparable to the Aristotelian *potentia*; since the measurement of an observable is a

[100]H. Margenau, "Advantages and disadvantages of various interpretations of the quantum theory," *Physics Today* **7**, 6–13 (1954).
[101]Cf. also Ref. 3-72 (1968, esp. p. 219).
[102]Ref. 6-86.

"transition from the possible to the actual" Heisenberg's notion of the "possible" may well be likened to Margenau's conception of "latency."

Margenau's latency interpretation was elaborated by John Lacy McKnight[103] and also served as the methodological background for Loyal Durand's analysis of the measurement process in quantum mechanics. Although admitting that the observed microphysical object is inextricably connected with the means used to observe it, Durand[104] thought it possible to consider the observer as effectively external to the system observed, basing this conclusion on the existence of the correspondence limit where quantum mechanical description approaches classical description and thus permits a quasi-classical description, that is, a description in which observables are treated classically for all practical purposes. According to Durand "peculiarly quantum mechanical effects are small relative to the accuracy with which classical observations may practically be made" and the measurement of quasi-classical observables uses the language of classical physics, and hence is a matter to be discussed within the confines of the epistemology of classical physics and is of no specific relevance to quantum mechanics.

For Durand, therefore, the only real problem of the quantum theory of measurement consisted of the "delineation of the circumstances in which the correlation of a peculiarly quantum mechanical observable A to a classically measurable observable B could be said to result in a measurement of A." To solve this problem he proposed suitable restrictions on the types of apparatus to be used and showed how appropriate correlations between object and apparatus observables "permit the translation of theoretical statements about the expectation values of abstract operators in a Hilbert space, evaluated in the initial object state, into statistical assertions about the results which will be found in an ensemble of quasi-classical observations."

Durand's theory of measurement, based essentially on filters, is wholly congenial with the principles of Margenau's interpretation of quantum mechanics and, in particular, with his rejection of the projection postulate; on closer analysis, however, as shown by Arthur I. Fine,[105] Durand's theory faces insuperable difficulties.

[103] J. L. McKnight, "Measurement in quantum mechanical systems" (Ph.D. thesis, Yale University, 1957); "The quantum theoretical concept of measurement," *Philosophy of Science* **24**, 321–330 (1957), "An extended latency interpretation of quantum mechanical measurement," *Philosophy of Science* **25**, 209–222 (1958).

[104] L. Durand III, "On the theory and interpretation of measurement in quantum mechanical systems" (Institute for Advanced Study, Princeton, N. J., 1958, unpublished); "On the theory of measurement in quantum mechanical systems," *Philosophy of Science* **27**, 115–133 (1960).

[105] A. I. Fine, "On the general quantum theory of measurement," *Proceedings of the Cambridge Philosophical Society* **65**, 111–122 (1969).

In addition to Margenau's arguments against the projection postulate, the untenability of this postulate was recently demonstrated indirectly by the construction of models of simultaneous measurement procedures for noncommuting observables, which were referred to at the end of Chapter 3. These models do not conflict with the remaining postulates of the quantum theory. That such counterexamples to the generally accepted thesis of the incompatibility of noncommuting observables are fully legitimate was claimed by Margenau and his research student James L. Park in a series of publications.[106] That such measurements, if physically acceptable, disqualify the projection postulate follows from the mathematical fact that state functions cannot generally be simultaneous eigenvectors of noncommuting operators. In the statistical ensemble interpretation a state vector, instead of being associated with a single system, describes an ensemble of identically prepared systems. Park therefore examined the legitimacy of what he called "the weak projection postulate": If measurements of an observable, represented by the operator A, are performed on an ensemble, the postmeasurement subensemble consisting of those systems for which the measurement yielded a certain value a_k has the density operator ρ_{α_k} where $A\alpha_k = a_k\alpha_k$. But, as Park has shown, if von Neumann's general theory of measurement, though without the (strong) projection postulate, is adopted, even the weak projection postulate precludes the simultaneous measurement of noncommuting observables. Park thus concluded that the conventional view of quantum mechanical measurements, initiated by von Neumann, has to be rejected. Park's own approach distinguished between two kinds of "measurement": one is essentially the specification of a rule of correspondence between concepts and percepts and as such just outside the P-plane of direct sensations or "protocols" and constitutes a universal feature of scientific method; the other, peculiar to quantum mechanics, describes the relation between an observable and its latent values.

11.6. MANY-WORLDS THEORIES

The early quantum theories of measurement, as we have seen, were dualistic in the sense that they postulated two fundamentally different modes of the behavior of state functions (von Neumann's changes of the first and second kind) and made the possibility of observation or measurement contingent on the existence of an extraneous macroscopic apparatus or of an ultimate human observer.

[106]Ref. 3-72. J. L. Park, "Quantum theoretical concepts of measurement," *Philosophy of Science* **35**, 205–231, 389–411 (1968).

When in the 1950s interest was focused—particularly by the Princeton and Chapel Hill groups of relativists—on the various approaches for a formulation of a quantum theory of general relativity[107] it was recognized that the idea of a quantization of a closed system like the universe of general relativity or the idea of a state function of the whole universe might be a physically acceptable, or even necessary conception. But the available dualistic theories of measurement denied to such notions, to say the least, any operational meaning; for neither is there room for any external observer nor for any classical measuring apparatus.

The quantum theory of general relativity thus demanded a far-reaching modification of the quantum theory of measurement: It was necessary to identify the observer, regarded as an automaton, or the measuring apparatus as part of the total system and to reject altogether the idea of a discontinuous change or "collapse" of the wave packet.

A monistic quantum theory of measurement of this type was indeed proposed in 1957 by Hugh Everett III. This "relative state" formulation, as it was originally called, was in fact a reformulation of quantum mechanics designed to eliminate the need not only of classical (macroscopic) observing devices or extraneous (ultimate) observers but also of an a priori operational interpretation of its formalism. It claimed that the mathematical formalism defines its own interpretation and that, in spite of the revolutionary novelty of its conceptual scheme, the statistical results of the conventional theory are fully reproduced.

Hugh Everett III did his undergraduate study in chemical engineering at the Catholic University of America where he also attended a course entitled "Introduction to Epistemology," the only formal course in Philosophy or psychology he ever attended. He did his graduate studies in mathematical physics at Princeton University. Studying von Neumann's and Bohm's textbooks on quantum mechanics he was struck by the apparent paradoxes raised by the unique role which the measurement process plays in the conventional presentation of quantum mechanics. It seemed to him "unreal that there should be a 'magic' process in which something quite drastic occurred (collapse of the wave function), while in all other times systems were assumed to obey perfectly natural continuous laws."[108] Trying to resolve what appeared to him to be inherent inconsistencies in the conventional interpretation, he discussed these problems with two fellow residents of the Princeton Graduate College, Charles Misner, who became later professor at the University of Maryland, and Aage

[107]Cf., e.g., Ch. W. Misner, "Feynman quantization of general relativity," *Reviews of Modern Physics* **29**, 497–509 (1957).
[108]Letter from H. Everett to the author, dated September 19, 1973.

Peterson, Bohr's assistant, who was spending at that time a year at Princeton. In the course of these discussions he conceived the basic ideas of his new interpretation. John A. Wheeler, to whom he presented his ideas, encouraged him to pursue this matter further as a Ph.D. thesis. Preprints (January 1956) of the thesis were circulated to several physicists, among them Bohr, Groenewold, Stern, and Rosenfeld. On March 1, 1957, Everett submitted his thesis to Princeton University and in July of that year *The Physical Review* published it in a condensed version.[109] Wheeler added to it an evaluation in which he emphasized the self-consistency of Everett's theory even though it implies a fundamental revision of our traditional conception of physical reality.[110]

Everett's theory was first generally ignored, so much so indeed that a recent reviewer referred to it as "one of the best kept secrets in this century." It was only 10 years after its publication that the "relative state" formulation attracted, due primarily to Bryce S. DeWitt[111] and his school at Chapel Hill, the attention of wider circles. An extension of the Everett interpretation was also the subject of a thesis written by R. Neill Graham[112] under the guidance of B. S. DeWitt. Part of Graham's conclusions —in particular those concerning the derivability of the probability interpretation from the mathematical formalism—have been independently obtained by J. B. Hartle[113] of the University of California at Santa Barbara. The theory proposed by Leon N. Cooper and Deborah Van Vechten[114] of Brown University, which places the measurement process "wholly within the quantum theory" and considers the entire system including the apparatus and even the mind of the observer as developing

[109]H. Everett, III, "'Relative state' formulation of quantum mechanics," *Reviews of Modern Physics* **29**, 454–462 (1957); Ph. D. thesis, Princeton University: reprinted in *The Many-Worlds Interpretation of Quantum Mechanics*, B. DeWitt and N. Graham, eds., (Princeton University Press, Princeton, 1973); "The theory of the universal wave function," *ibid.*, pp. 1–140.

[110]J. A. Wheeler, "Assessment of Everett's 'relative state' formulation of quantum theory," *Reviews of Modern Physics* **29**, 463–465 (1957).

[111]B. S. DeWitt, "The Everett-Wheeler interpretation of quantum mechanics," in *Battelle Rencontres 1967—Lectures in Mathematics and Physics*, C. DeWitt and J. A. Wheeler, eds. (Benjamin, New York, 1968), pp. 318–332; "The many-universes interpretation of quantum mechanics," Lecture delivered at the Varenna International School of Physics "Enrico Fermi," July 1970; "Quantum mechanics and reality," *Physics Today* **23**, (September issue), 30–35 (1970).

[112]N. Graham, "The Everett interpretation of quantum mechanics," Ph.D. thesis, University of North Carolina at Chapel Hill, submitted 1970.

[113]J. B. Hartle, "Quantum mechanics of individual systems," *American Journal of Physics* **36**, 704–712 (1968).

[114]L. N. Cooper and D. Van Vechten, "On the interpretation of measurement within the quantum theory," *American Journal of Physics* **37**, 1212–1221 (1969).

according to the Schrödinger equation, has much in common with Everett's ideas.

In the "relative state" formulation the wave mechanics of only continuous changes obeying a linear wave equation everywhere and at all times is a complete theory, capable of describing every isolated physical system as well as every system subject to observation which it regards as part of a larger isolated system. The observer is treated as a physical system interacting with other subsystems. If

$$\psi = \sum_{j,k} g_{ik}\sigma_j\alpha_k \tag{18}$$

is the general state of a composite system $\mathfrak{S}+\mathfrak{A}$, $\mathfrak{S}$ and $\mathfrak{A}$ being subsystems ($\mathfrak{A}$ may be the observer) with σ_j, α_k as complete orthonormal sets of states for $\mathfrak{S}$ and $\mathfrak{A}$, respectively, the subsystems do not posses definite states independently of one another (it is assumed that at least two g_{jk} are nonzero). However, to any state of $\mathfrak{S}$, say σ_m, a corresponding *relative* state of $\mathfrak{A}$, denoted by $\psi(\mathfrak{A};\ \mathrm{rel}\,\sigma_m,\ \mathfrak{S})$ can uniquely be assigned.

$$\psi(\mathfrak{A};\ \mathrm{rel}\,\sigma_m, \mathfrak{S}) = N_m \sum_k g_{mk}\alpha_k \tag{19}$$

where N_m is a normalization constant. This relative state is independent of the choice of basis $\{\sigma_j\}$ $(j \neq m)$ for the orthogonal complement of σ_m and is thus uniquely determined by σ_m alone; in the conventional theory it gives the conditional probability distributions for the results of all measurements on $\mathfrak{A}$, provided $\mathfrak{S}$ has been found to be in state σ_m.

Since the observer $\mathfrak{A}$ is regarded as a physical system which "observes" other systems by interacting with them, Everett thought it necessary "to identify some present properties of such an observer with features of the past experience of the observer." Thus, to say that $\mathfrak{A}$ has observed the result s_m, the state of $\mathfrak{A}$ must change to a new state which is dependent on s_m. If the initial state of the composite system is represented by the normalized vector

$$|\psi_0\rangle = |\sigma\rangle|\alpha\rangle \tag{20}$$

the postinteraction state is

$$|\psi\rangle = U|\psi_0\rangle \tag{21}$$

where U is a certain unitary operator.

In contrast to our previous assumptions, it is convenient to assume, with DeWitt,[115] that the eigenvalues a, associated with the observer system $\mathfrak{A}$,

[115]In the sequel we follow mainly DeWitt's elaboration of the theory.

form a continuous spectrum whereas, as before, the eigenvalues s, associated with the object system, form a discrete set. By expansion,

$$|\sigma\rangle = \sum_s c_s|s\rangle \qquad \text{where } c_s = \langle s|\sigma\rangle \tag{22}$$

$$|\alpha\rangle = \int e_a|a\rangle da \qquad \text{where } e_a = \langle a|\alpha\rangle \tag{23}$$

and with $|s,a\rangle = |s\rangle|a\rangle$

$$|\psi_0\rangle = \sum_s c_s e_a|s,a\rangle da. \tag{24}$$

Orthonormality and completeness require that

$$\langle s,a|s',a'\rangle = \delta_{ss'}\delta(a-a') \tag{25}$$

$$\sum_s \int |s,a\rangle\langle a,s|da = 1. \tag{26}$$

In accordance with Everett's postulate concerning the particular property of the "observing" system the action of the unitary operator U must be such that

$$U(|s\rangle|a\rangle) = |s\rangle|a+gs\rangle \tag{27}$$

where g is some kind of adjustable coupling constant. The last equation, if translated into conventional language, expresses the facts (1) that the interaction resulted in the "observation" by $\mathfrak{A}$ that $\mathfrak{S}$ is in the state $|s\rangle$, and (2) that due to the shift from $|a\rangle$ to $|a+gs\rangle$, this observation is "stored" in the "memory" of $\mathfrak{A}$. From the preceding equations it follows that

$$|\psi\rangle = \sum_s c_s|s\rangle|e[s]\rangle, \tag{28}$$

where

$$|e[s]\rangle = \int e_a|a+gs\rangle da \tag{29}$$

and from the normalization of σ and α that

$$\sum_s |c_s|^2 = 1 \qquad \text{and} \qquad \int |e_a|^2 da = 1. \tag{30}$$

The final state (28) of the total system is a superposition of states $|s\rangle|e[s]\rangle$, each of which indicates that the object system has assumed one of the possible values of the object observable and that the observer system has "observed" just this value. The observer state $|e[s]\rangle$ is precisely the relative state $\psi(\mathfrak{A};\ \mathrm{rel}|s\rangle, \mathfrak{S})$ and thus uniquely defined by $|s\rangle$. If Δs denotes the spacing between adjacent values of the object variable and Δa is defined by

$$(\Delta a)^2 = \int (a - \bar{a})^2 |e_a|^2 da, \tag{31}$$

where

$$\bar{a} = \int a |e_a|^2 da, \tag{32}$$

then the condition

$$\Delta a \ll g \Delta s \tag{33}$$

guarantees that the "observation" is good in the sense that it distinguishes adjacent object values; the condition (33) implies that

$$\langle e[s] | e[s'] \rangle = \delta_{ss'} \tag{34}$$

so that the initially single wave packet of the observer system separates into mutually orthogonal packets, each being correlated with a different value of the object system.

With the exception of the "memory configurations" recorded in the observer states, the relative state formulation has, up to this stage, only followed the trodden path of the conventional theory. But now, instead of assuring that the superposition (28) "collapses" immediately after the interaction and "reduces" to one of its components, say $|s\rangle|e[s]\rangle$ with probability $|c_s|^2$, as claimed by the conventional theory in complete disregard of the Schrödinger equation, the relative state formulation asserts that *the superposition never collapses*. To reconcile this assumption with ordinary experience which ascribes to the object system (or the correlated apparatus system) after the measurement only one definite value of the observable, the relative state formulation makes the bold suggestion that the "world" initially described by $|\psi_0\rangle$ has been split, as a consequence of the interaction, into a multitude of equally real "worlds" each of which corresponds to a definite component of the superposition $|\psi\rangle$. Hence in each separate "world" a measurement yields only one result, though this result differs, in general, from "world" to "world." Just as in the ancient Moslem school of the Mutakallimun (Kalām),[116] the daring assumption of a continual dissolution and recreation of the universe reconciled the apparent continuity of macroscopic phenomena with the atomic doctrine of space, time, and matter, so in the modern theory of Everett, Wheeler, Graham, and DeWitt,

[116]Ref. 37 (1954, 1969, p. 61 *et seq.*; 1960, New York, p. 60 *et seq.*; 1963, 1966, p. 61 *et seq.*; 1960, Darmstadt, p. 65 *et seq.*; 1970, p. 89 *et seq.*).

or briefly EWG theory, the no less daring hypothesis of a continual splitting of the world into a stupendous number of branches reconciled the continuity of microphysical processes with the experience of distinct outcomes in measurements.

In disregard of more demanding criteria (such as those proposed by Popper or Feyerband) to be satisfied by a viable scientific theory, the minimum requirements to be imposed are those of logical consistency and agreement with experience. The "relative state" formulation or "many-universes interpretation," as it also may be called, had therefore to meet the obvious objection which points out that we never experience such a splitting of the world. This objection is refuted by proving that the very laws of quantum mechanics preclude the possibility of "observing" such a splitting of the world. Again, a historical analogy may be quoted. The objection raised against the Copernican theory of the rotation of the earth, namely, that we do not feel the effects of such a rotation, was met by the demonstration that the laws of Newtonian physics (inertia, gravitation) preclude (with sufficient approximation for ordinary experience) such perceptible effects.[117]

To prove the consistency, a second apparatus (or "observer") $\mathfrak{A}$ is introduced which "observes" both the object system $\mathfrak{S}$ and the original apparatus (or "observer") $\mathfrak{A}$. Its state vector $|a_1, b_1\rangle$ is consequently characterized by two indices. a_1, which plays the same role as did a for $\mathfrak{A}$, and b_1, which is used to measure $\mathfrak{A}$. To find the outcome of the interaction involving $\mathfrak{S}$, $\mathfrak{A}$, and $\mathfrak{A}_1$, we assume that at first both apparatuses observe the system variable s so that in generalization of (27)

$$U'(|s,a\rangle|a_1,b_1\rangle) = |s\rangle|a+gs\rangle|a_1+gs,b_1\rangle. \tag{35}$$

In the second step, $\mathfrak{A}_1$ observes the memory bank of $\mathfrak{A}$ so that

$$U''(|s,a\rangle|a_1,b_1\rangle) = |s,a\rangle|a_1,b_1+g_{11}a\rangle. \tag{36}$$

If the state of the total system was initially

$$|\psi_0\rangle = |\sigma\rangle|\alpha\rangle|\alpha_1\rangle \tag{37}$$

the first unitary transformation U' changed it into

$$|\psi'\rangle = U'|\psi_0\rangle = \sum_s c_s|s\rangle|e[s]\rangle|e_1[s]\rangle \tag{38}$$

[117]A footnote, added in proof (Ref. 109, 1957, p. 460), in which Everett drew this kind of comparison with the Copernican theory was written in response to a critic who objected that the alleged splitting could not possibly be true because the observer is unaware of any such splitting effect.

where

$$|e[s]\rangle = \int e_a |a+gs\rangle da \qquad \text{with} \quad e_a = \langle a|\alpha\rangle \tag{39}$$

and

$$|e_1[s]\rangle = \int da_1 \int db_1 e_{a_1,b_1} |a_1 + g_1 s, b_1\rangle \tag{40}$$

with

$$e_{a_1,b_1} = \langle a_1, b_1 | \alpha_1\rangle. \tag{41}$$

The final state (after the measurement) is consequently

$$|\psi''\rangle = U''|\psi'\rangle = \sum_s \int c_s |s\rangle e_a |a+gs\rangle |e_1[s, a+gs]\rangle da \tag{42}$$

where

$$|e_1[s,a]\rangle = \int da_1 \int db_1 e_{a_1,b_1} |a_1 + gs, b_1 + g_{11} a\rangle. \tag{43}$$

If

$$\Delta a \ll g\Delta s, \qquad \Delta a_1 \ll g_1 \Delta s, \qquad \Delta a \ll \frac{\Delta b_1}{g_{11}}, \qquad \text{and} \quad \Delta b_1 \ll g_{11} g \Delta s, \tag{44}$$

it follows that (to a good approximation) the postinteraction state is given by

$$\sum_s c_s |s\rangle |e[s]\rangle |e_1[s, \langle a\rangle + gs]\rangle \tag{45}$$

So that the final state of the triple composite system is again a linear superposition of elements, each of which indicates that both apparatuses "observe" the same object value characteristic of this element, and no other value at all, and agree that the results of their observations are identical. No experiment in a given branch of the splitting universe can therefore ever reveal the outcome of a measurement obtained in another branch of the universe. This completes the proof of the consistency theorem.

Through an investigation of the statistical frequency of obtaining the same result for the object variable in the case of repeated measurements on an ensemble of identical systems in identical states, Everett claimed to *derive* the interpretation of $|c_s|^2$ to which no a priori interpretation was given, and to prove that it plays precisely the role ascribed to it by conventional quantum mechanics. To derive the probability interpretation of $|c_s|^2 = |\langle s|\sigma\rangle|^2$ we consider, following Graham and DeWitt, an ensemble of identical independent systems which are in the initial state

$$|\psi_0\rangle = |\sigma_1\rangle|\sigma_1\rangle|\sigma_2\rangle \cdots |\alpha\rangle \tag{46}$$

where

$$c_s = \langle s|\sigma_k\rangle \qquad \text{for all } k. \tag{47}$$

After a sequence of N measurements, the state is

$$|\psi_n\rangle = \sum_{s_1, s_2, \ldots} c_{s_1} c_{s_2} \cdots |s_1\rangle|s_2\rangle \cdots |e[s_1, s_2, \ldots, s_n]\rangle \tag{48}$$

where

$$|e[s_1, \ldots, s_n]\rangle = \int da_1 \int da_2 \cdots e_{a_1, a_2, \ldots} |a_1 + gs_1, \ldots, a_n + gs_n\rangle \tag{49}$$

and

$$e_{a_1, a_2, \ldots} = \langle a_1, a_2, \cdots |\alpha\rangle. \tag{50}$$

Although every system was initially in the same state, the apparatus, in general, "observes" different system values even in a single component of the superposition so that each "memory sequence" $[s_1, s_2, \ldots, s_n]$ yields a certain distribution of these values. The "relative frequency function," characterizing this distribution, is defined by

$$f(s; s_1 \cdots s_n) = n^{-1} \sum_{k=1}^{n} \delta_{ss_k} \tag{51}$$

The function

$$\delta(s_1 \cdots s_n) = \sum_s [f(s; s_1 \cdots s_n) - w_s]^2 \tag{52}$$

where

$$w_s = |c_s|^2, \tag{53}$$

measures the variance between the sequence $s_1 \cdots s_n$ and a random sequence with weights w_s. For $\epsilon > 0$ but arbitrarily small, the sequence $s_1 \cdots s_n$ is "random" if $\delta(s_1 \cdots s_n) < \epsilon$, and "nonrandom" otherwise. If in (48) all elements with nonrandom memory sequences are omitted, and the surviving superposition is denoted by $|\psi_n^\epsilon\rangle$ it can be shown that

$$\lim_{n \to \infty} (|\psi_n\rangle - |\psi_n^\epsilon\rangle) = 0 \qquad \text{for all } \epsilon. \tag{54}$$

Hence in the limit $N \to \infty$ the nonrandom memory sequences in (48) have "measure zero" in the Hilbert space. The preceding considerations, it is claimed, show that the conventional probability interpretation (Born's interpretation) is a consequence of the formalism itself, namely, that the $|c_s|^2$ are the relative frequencies or probabilities with which the various eigenvalues of the system observable are "observed."

In response to Bryce DeWitt's presentation of the Everett-Wheeler-Graham theory in *Physics Today* that journal published a few months later letters[118] by Leslie E. Ballentine, Philip Pearle, Evan Harris Walker, Mendel Sachs, Toyoki Koga, and Joseph Gerver which criticized this view. For Ballentine, an advocate of the ensemble interpretation, the unusual features of the EWG interpretation illustrate the illegitimacy of the assumption that the state vector provides a complete description of an individual physical system instead of an ensemble of systems; the claim that the formalism itself generates its interpretation is, in his view, "unfounded and misleading." At best it may only *suggest* a certain interpretation for the semantics of a formalism always requires specific interpretative assumptions. Pearle, in general concurring with Ballentine, regarded the notion of an enormous multiplicity of unobservable universes as "uneconomical." For Walker the assumption that as a result of an isolated event the entire universe undergoes a simultaneous splitting is a postulate just as problematic as that of the Copenhagen interpretation according to which by changing our mind about which measurement to perform on one of two independent systems "we are able to alter the state of the other system."

Sachs, though admitting the consistency of DeWitt's statements with his mathematics, questioned the necessity of going "to such extreme lengths of straining physical sensibilities to resolve the logical difficulties of the quantum theory" instead of possibly generalizing the formalism of quantum mechanics as done, for example, by himself. Koga, referring to Landé's elaboration of William Duane's ideas of particle diffraction,[119] as a promising program for establishing a deterministic interpretation, doubted whether "it is healthy for quantum mechanics to enshrine itself" as proposed in the EWG theory. Finally, Gerver challenged the proposed interpretation as leading to inconsistencies as soon as one considers worlds branching off in the backward direction of time, a process which in view of the time symmetry of the Schrödinger equation must be just as real as that in the forward direction of time.

In his reply DeWitt pointed out that when claiming preference for the

[118]"Quantum-mechanics debate," *Physics Today* **24**, 36–44 (April issue) (1971).
[119]See Section 10.3.

EWG view he confined himself entirely to those interpretations which accept the conventional formalism without (essentially) changing its rules (an answer to Sachs and, partly to Koga). In fact, he reemphasized, "it is the only conception that, within the framework of presently accepted formalism, permits quantum theory to play a role at the very foundations of cosmology." Since we make measurements on single systems, DeWitt replied to Ballentine and Pearle, he could not regard the ensemble interpretation as satisfactory. Concerning the philosophically important question whether the formalism really yields its own interpretation, he admitted, however, to "having somewhat overstated the case...in implying that the EWG metatheorem has been rigorously proven," and confessed that this remains a task for the future, "to be carried out by some enterprising analytical philosopher." Still, in his view, no other interpretation exists that does not add anything to the mathematical formalism as it stands and regards the latter as a complete description of quantum phenomena.

The multiuniverse theory is undoubtedly one of the most daring and most ambitious theories ever constructed in the history of science. In its boldness it surpasses even the atomistic doctrine of the Kalam according to which with every atomic process, as already mentioned, the universe—but only *one* universe—is continually created anew. In the scope of its contentions it is virtually unique. Indeed, if its claim that the mathematical formalism entails its own interpretation could be literally true, the multiuniverse theory would not only confound our ideas about formalism and interpretation which we presented in Chapter 1, it would also imply that all nonmultiuniverse theories, and that is all the other interpretations described in this book, are *logically* false, for they all refer virtually to the same formalism.

These considerations alone seem already to suggest that the just-mentioned claim cannot be maintained. Indeed, it may be questioned, for example, whether the very fact that only numerically large coefficients as taken as physically significant in contrast to numerically small coefficients, as done in the limit considerations expressed by (54), does not involve an implicit interpretative assumption.

Another criticism, voiced originally by Ballentine,[120] touches on the possible ambiguity in defining the branches of the splitting universe. Should, say, the quantum state $\exp(i\mathbf{k}\cdot\mathbf{r})$, which can also be written, if expanded in Legendre polynomials,[121] as $\Sigma(2l+1)i^l j_l(kr)P_l(\cos\theta)$, be in-

[120] L. E. Ballentine, "Can the statistical postulate of quantum theory be derived?—A critique of the many-universes interpretation," *Foundations of Physics* **3**, 229–240 (1973).

[121] See, e.g., L. I. Schiff, *Quantum Mechanics* (McGraw-Hill, New York, 3rd ed., 1968), p. 119, or G. N. Watson, *Theory of Bessel Functions* (Macmillan, New York, 1944), p. 128.

terpreted as a single-branch universe containing a system with linear momentum $\hbar k$ or should it be interpreted as a universe of denumerably infinitely many independent branches, each containing a system with a different angular momentum? To postulate, in order to obtain uniqueness, that only that representation should count which diagonalizes the observable to be measured and to give thus to the measurement a privileged position, would be "contrary to the spirit of Everett's program, which was motivated in part by a reaction against the privileged status of measurement (reduction of state vector) in the orthodox interpretation."

Ballentine also showed that Born's probabilistic interpretation[122] or its generalization by Dirac[123] according to which in a (normalized) state $|\psi>$ the probability of an observable S having the value s is given by $|\langle s|\psi\rangle|^2$ where $|s>$ is the (normalized) eigenvector corresponding to s can be derived from a more elementary set of postulates, which, however, is not a purely mathematical formalism but incorperates the interpretative assumption that the state function represents the statistical distribution of all observables for an ensemble of similarly prepared systems.

The many-worlds interpretation may be criticized also for not giving a sufficient explanation of why the observer is localized in that branch of the splitting universe in which he happens to be. If, for example, the theory is applied to the Bohm version of the EPR argument (see Section 6.7), the two terms on the right-hand side of eq. 6.60, $\psi_+(1)\psi_-(2)$ and $\psi_-(1)\psi_+(2)$, represent two different branches: in one branch the measurement result is that corresponding to $\psi_+(1)\psi_-(2)$ and in the other that corresponding to $\psi_-(1)\psi_+(2)$. Thus, although the EPR argument itself would have lost its point, the question is left unanswered why in any experimental realization of this situation an observer finds himself in one and not in the other of the two branches. A large-scale indeterminism which moreover includes the observer seems to involve problems that differ considerably from those raised by microphysical indeterminism.

Finally, an attempt to disprove the tenability of the EWG interpretation on purely logico-mathematical grounds should be mentioned. By showing that the mathematical formalism of quantum mechanics, stripped of any interpretative element, is formally compatible with a whole class of probability spaces conceivable a priori—among which the quantum mechanical probability space realizes just one possibility—Miora Mugur-Schächter claimed to have proved that this formalism does not entail syntactically the quantum mechanical probability space as originally contended by the

[122]See Section 2.4.

[123]P. A. M. Dirac, *The Principles of Quantum Mechanics* (Clarendon Press, Oxford, 2nd ed., 1935), p. 65.

proponents of the EWG interpretation.[124]

Although quite a few physicists seem to sympathize, though often with reservations,[125] with the principles of the many-worlds interpretation, it can certainly not claim to have gained wide acceptance. For the majority of physicists the problem of finding a consistent and plausible quantum theory of measurement is still unsolved. The endless stream of publications suggesting new theories of measurement or modifications of ideas earlier proposed as well as the unending discussions and symposia[126] on this topic are an eloquent indication of the general discomfort felt among physicists on this issue. It is no exaggeration to say that it is currently the most discussed problem in foundational research. Our account on this matter could consequently only skim the surface and throw perhaps some light on the historical origins of the main currents of opinion. It would be a most difficult, though certainly not uninteresting, task to classify systematically, and to compare with each other the different theories of measurement such as those advocated by J. Albertson, S. Amai, W. Band, J. M. Burgers, J. Earman, G. G. Emch, I. Fujiwara, K. Gottfried, H. J. Groenewold, M. N. Hack, P. Jesselette, A. Komar, H. P. Krips, E. Lubkin, V. Majernik, H. Mehlberg, P. A. Moldauer, T. E. Phipps, H. Schmidt, H. Stein, J. Voisin, B. L. van der Waerden, H. Wakita, and G. C. Wick, to name only a few authors who have not been explicitly mentioned in this chapter.

Let us add some brief remarks on some recent more general papers in the quantum theory of measurement which seem to be particularly interesting for the philosopher of science. Arthur Fine[127] of Cornell University claimed to have shown that "the measurement problem," that is, the question of whether a measurement interaction can lead to a mixture of object-plus-apparatus states in each of which the apparatus itself is in a pure state (it being supposed that its initial state was a mixture), cannot be solved affirmatively. Based on what is claimed to be the most general possible theory of quantum measurements consistent with elementary quantum mechanics, Fine's insolubility proof of the quantum measure-

[124] M. Mugur-Schächter, "The quantum mechanical Hilbert Space Formalism and the quantum mechanical Probability Space of the Outcomes of Measurements," Ref. 8-220 (1974, pp. 288–308).

[125] See, e.g., H. D. Zeh, "On the interpretation of measurement in quantum theory," *Foundations of Physics* **1**, 69–76 (1970).

[126] Out of the 25 papers presented at the 1970 Varenna meeting on the "Foundations of Quantum Mechanics," 13 were papers on the problem of measurement in quantum theory. See Ref. 7–1.

[127] A. I. Fine, "Realism in quantum measurements," *Methodology and Science* **1**, 210–220 (1968); "On the general quantum theory of measurement," *Proceedings of the Cambridge Philosophical Society* **65**, 111–122 (1969); "Insolubility of the quantum measurement problem," *Physical Review* **2D**, 2783–2787 (1970).

ment problem contains Wigner's negative solution of this problem for the von Neumann type of measurement and D'Espagnat's, Earman's, and Shimony's negative solution for the Landau type of measurement as special cases.

For those interested in the relations between logic and quantum mechanics Bas C. van Fraassen's[128] "modal interpretation of measurement" may be of some interest. On the problem of the consistency of the use of classical concepts for the account of quantum phenomena the general treatment of large quantal systems by C. George, I. Prigogine, and L. Rosenfeld[129] seems to throw new light. By introducing a superspace formed by the direct product of the Hilbert space with itself and constructing therein a projection superoperator whose range contains the asymptotic time evolution of the density operator for time intervals very large compared with those typical for atomic processes, they were able to show that the asymptotic subdynamics thus obtained manifests the characteristic features of macroscopic behavior. Since their superspace representation allows characterization of precisely the macroscopic level of description and mathematical formulation of the conditions which a given physical system has to satisfy in order that its properties belong to this level, it proves to be particularly well adapted to offer a concise and general account of the measurement process.

In contrast to such attempts at extending the conventional formalism of quantum mechanics to incorporate a consistent quantum theory of measurement it has been argued that quantum mechanics should be reformulated to exclude altogether the notion of measurement. An argument to this effect was advanced by Nicholas Maxwell[130] of University College at London primarily on the ground that orthodox quantum mechanics does not formulate the conditions which specify whether an object-plus-apparatus system undergoes a deterministic transformation in accordance with the Schrödinger time-dependent equation or whether it undergoes a probabilistic transformation associated with the reduction of the wave packet. According to Maxwell, it is this inability of the orthodox theory to specify precisely such mutually exclusive conditions that lies at the root of

[128]B. van Fraassen, "Measurement in quantum mechanics as a consistency problem" (preprint, 1970).

[129]C. George, I. Prigogine, and L. Rosenfeld, "The macroscopic level of quantum mechanics," *Kongelige Danske Videnskabernes Selskab Matematisk-fysiske Meddelelser* **38** (12), 1–44 (1972).

[130]N. Maxwell, "A new look at the quantum mechanical problem of measurement," *American Journal of Physics* **40**, 1431–1435 (1972). For a critique of this paper see also W. Band and J. L. Frank, "Comments concerning 'a new look at the quantum mechanical problem of measurement,'" *American Journal of Physics* **41**, 1021–1022 (1973), and for a response N. Maxwell, "The problem of measurement—real or imaginary," *ibid.*, 1022–1025.

the insolubility of the measurement problem. To solve it, "a new fully objective version of quantum mechanics needs to be developed which does not incorporate the notion of measurement in its basic postulates at all."

The immense diversity of opinion and the endless variety of theories concerning quantum measurements, as illustrated in this chapter, are but a reflection of the fundamental disagreement as to the interpretation of quantum mechanics as a whole. The establishment of a fully consistent and adequate theory of quantum measurement and the achievement of a satisfactory interpretation of quantum mechanics as a whole are ultimately identical. As long as one of them remains unsettled, so too does the other.

The reader who has had the endurance to reach the end of this book will not expect to find final answers to the questions it raises. For what is told here is essentially a story without an ending. This may be a disappointment for the reader who expected too much. But let us recall the advice once given by the French moralist Joseph Joubert: "It is better to debate a question without settling it than to settle a question without debating it."

LATTICE Theory Appendix

Definition 1. A set $S = \{a,b,c,\ldots\}$ is *partially ordered* if in S a reflexive, transitive, and antisymmetric binary relation $a \leqslant b$ (read "a is smaller than, or equal to, b" or "a is contained in b," or "b contains a") is defined, that is, for any a,b,c of S $a \leqslant a$; $a \leqslant b$ and $b \leqslant c$ imply $a \leqslant c$; and $a \leqslant b$ and $b \leqslant a$ imply $a = b$. $a < b$ means $a \leqslant b$ and $a \neq b$ (read "a is properly contained in b" or "b properly contains a"). $a \prec b$ (read "b covers a") means $a < b$ and no c exists such that $a < c < b$.

In the sequel S denotes a partially ordered set and T one of its subsets.

Definition 2. If an element of T is contained in every element of T it is a *least element* of T. If an element of T contains every element of T it is a *greatest element* of T. The least element of S, if it exists, is denoted by 0 ("zero"); the greatest element of S, if it exists, is denoted by 1 ("unity").

THEOREM 1. A least element of T, if it exists, is unique. A greatest element of T, if it exists, is unique.

Definition 3. If an element is contained in every element of T it is a *lower bound* of T. If an element contains every element of T, it is an *upper bound* of T. The least element of the set of all upper bounds of T, if it exists, is the least upper bound or *supremum* of T, sup T, or $\bigvee T$. The greatest element of the set of all lower bounds of T, if it exists, is the greatest lower bound or *infimum* of T, inf T or $\bigwedge T$.

THEOREM 2. sup T, if it exists, is unique; inf T, if it is exists, is unique.

THEOREM 3. $\sup\{a, \sup\{b,c\}\} = \sup\{a,b,c\}$;
$\inf\{a, \inf\{b,c\}\} = \inf\{a,b,c\}$.

Definition 4. A *lattice* is a partially ordered set (with zero and unity) in which every pair of elements has a supremum and infimum. A lattice is *σ-complete* if every nonempty denumerable subset of it has a supremum and infimum. A lattice is *complete* if every nonempty subset of it has a supremum and infimum. A lattice is *finite* if the number of its elements is finite.

THEOREM 4. A finite lattice is complete.

In the sequel $a \cup b$ [read "the union (or join, or disjunction) of a and b"] denotes $\bigvee\{a,b\}$; $a \cap b$ [read "the intersection (or meet, or conjunction) of a and b"] denotes $\bigwedge\{a,b\}$; and L denotes a lattice.

Lattices may be graphically represented (Hasse diagrams) as follows: if

$a < b$, a is plotted lower than b and a segment is drawn from a to b. A lattice of five elements, for instance, is represented by one of these diagrams:

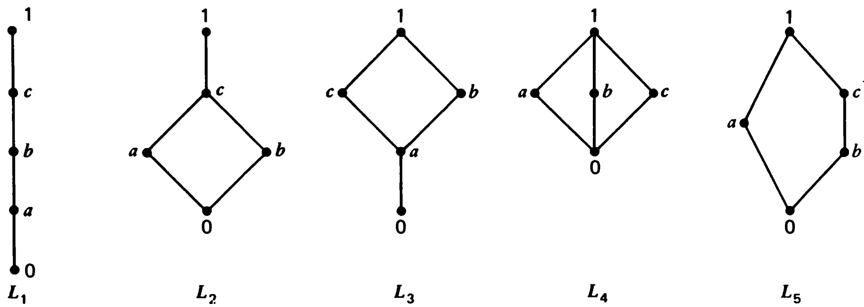

Figure 8.

Definition 5. An element a of L is an *atom* if $0 \prec a$. L is *atomic* if every nonzero element of L contains an atom.

THEOREM 5. If a,b,c are elements of L then

1. $a \cup a = a$	$a \cap a = a$	(idempotency)
2. $a \cup b = b \cup a$	$a \cap b = b \cap a$	(commutativity)
3. $a \cup (b \cup c) = (a \cup b) \cup c$	$a \cap (b \cap c) = (a \cap b) \cap c$	(associativity)
4. $a \cup (a \cap b) = a$	$a \cap (a \cup b) = a$	(absorptivity)
5. $a \leqslant b$, $a \cup b = b$, and	$a \cap b = a$	imply each other
6. $a \leqslant b$ implies	$a \cup c \leqslant b \cup c$ and	$a \cap c \leqslant b \cap c$.

THEOREM 6. A set in which two binary operations $\cup$ and $\cap$ are defined which satisfy the preceding conditions 2, 3, and 4 is a lattice with respect to the partial order $a \leqslant b$ defined by $a \cup b = b$.

Definition 6. b is a *complement* of a if $a \cap b = 0$ and $a \cup b = 1$. If every element of L has at least one complement L is *complemented.* If every element of L has exactly one complement L is *uniquely complemented.*

THEOREM 7. In any L, $(a \cap b) \cup (a \cap c) \leqslant a \cap (b \cup c)$ and $a \cup (b \cap c) \leqslant (a \cup b) \cap (a \cup c)$.

Definition 7. L is *distributive* if $(a \cap b) \cup (a \cap c) = a \cap (b \cup c)$ and $a \cup (b \cap c) = (a \cup b) \cap (a \cup c)$ for all a, b, c of L. L is *Boolean* if it is complemented and distributive.

THEOREM 8. In a distributive lattice $a \cup b = a \cup c$ and $a \cap b = a \cap c$ imply $b = c$ (cancellation rule). A Boolean lattice is uniquely complemented.

THEOREM 9. In a Boolean lattice (a' denotes the complement of a):

1. $0' = 1 \; 1' = 0$
2. $(a')' = a$
3. $a = b$ if and only if $a' = b'$
4. $(a \cup b)' = a' \cap b'$, $(a \cap b)' = a' \cup b'$
5. $a \leqslant b$ if and only if $b' \leqslant a'$
6. $a \leqslant b$, $a \cap b' = 0$, and $a' \cup b = 1$ imply each other.

THEOREM 10. In any lattice $a \leqslant c$ implies $a \cup (b \cap c) \leqslant (a \cup b) \cap c$ for all a, b, c of L.

Definition 8. (b, c) is a *modular pair* or $(b, c)M$ if for every $a \leqslant c$ $a \cup (b \cap c) = (a \cup b) \cap c$. A lattice is *modular* if every two elements of it are a modular pair, that is if $a \leqslant c$ implies $a \cup (b \cap c) = (a \cup b) \cap c$ for all a, b, c of the lattice.

THEOREM 11. Every distributive lattice is modular.

Definition 9. A *homomorphism* is a mapping $h : L_1 \to L_2$ of a lattice L_1 into a lattice L_2 such that $h(a \cup b) = h(a) \cup h(b)$ and $h(a \cap b) = h(a) \cap h(b)$ for all a, b of L_1. An *isomorphism* is a one-to-one homomorphism. An *automorphism* is an isomorphism of a lattice with itself. A *dual-isomorphism* is a one-to-one mapping $d : L_1 \to L_2$ such that $a \leqslant b$ implies $d(b) \leqslant d(a)$ for all a, b of L_1. A *dual-automorphism* is a dual-isomorphism of a lattice with itself. An *involutive dual-automorphism* of L is a dual-automorphism d such that $d(d(a)) = a$ for all a of L.

THEOREM 12. An involutive dual-automorphism of L satisfies $d(a \cup b) = d(a) \cap d(b)$ and $d(a \cap b) = d(a) \cup d(b)$ for all a, b of L.

THEOREM 13. For an involutive dual-automorphism of L the following three statements imply each other: (1) $a \leqslant d(a)$ implies $a = 0$, (2) for all a of L $a \cap d(a) = 0$, (3) for all a of L $a \cup d(a) = 1$.

Definition 10. An involutive dual-automorphism of L which satisfies one of the preceding three conditions is an *orthcomplementation*, L is *orthocomplemented* and $d(a)$ is the *orthocomplement* $a^{\perp}$ of a. Being a complement $a^{\perp}$ will be denoted simply by a'. $a \perp b$ means $a \leqslant b'$.

THEOREM 14. L is orthocomplemented if and only if there exists a mapping $a \to a'$ of L onto itself such that $(a')' = a, a \leqslant b$ implies $b' \leqslant a'$, $a \cap a' = 0$, and $a \cup a' = 1$ for all a, b of L.

THEOREM 15. $a \perp b$ implies $b \perp a$ (a and b are *orthogonal* or *disjoint*).

Definition 11. A lattice is *weakly modular* if it is orthocomplemented and $a \leqslant b$ implies $b = a \cup (a' \cap b)$. It is *quasi-modular* if it is orthocomplemented and $a \leqslant b \leqslant c'$ implies $a = (a \cup c) \cap b$. It is *orthomodular* if it is orthocomplemented and $a \perp b$ implies $(a,b)M$.

THEOREM 16. An orthocomplemented lattice is weakly modular if and only if $a \leqslant b$ implies $a = b \cap (a \cup b')$.

THEOREM 17. Each of the three lattice properties, weakly modularity, quasi-modularity, and orthomodularity, implies the other two.

THEOREM 18. An orthocomplemented modular lattice is orthomodular. In the sequel, L_o denotes an orthocomplemented lattice and L_{om} an orthomodular lattice.

THEOREM 19. In L_o $(a \cap b) \cup (a \cap b') \leqslant a$ for all a, b of L_o.

Definition 12. In L_o a is *compatible* with b or $a \leftrightarrow b$ if $(a \cap b) \cup (a \cap b') = a$; a is *commensurable* with b or $a \Leftrightarrow b$ if $a \cup (a' \cap b) = b \cup (b' \cap a)$; a is *commeasurable* with b or $a \sim b$ if there exist in L_o three mutually orthogonal elements a_1, b_1, and c such that $a = a_1 \cup c$ and $b = b_1 \cup c$.

THEOREM 20. In L_o $a \leftrightarrow b$ implies $a \leftrightarrow b'$.

THEOREM 21. In L_{om} $a \leftrightarrow b$ implies $b \leftrightarrow a$, and $a \leqslant b$ implies $a \leftrightarrow b$.

THEOREM 22. An L_o in which $a \leqslant b$ implies $b \leftrightarrow a$ is an L_{om}.

THEOREM 23. In L_o $a \Leftrightarrow b$ implies $b \Leftrightarrow a$.

THEOREM 24. In L_{om} each of the three lattice-element relations, compatibility, commensurability, and commeasurability, implies the other two.

Definition 13. A subset of a lattice L is a *sublattice* of L if it is itself a lattice with respect to the lattice operations of L.

THEOREM 25. The (set-theoretical) intersection of a family of sublattices of L is a sublattice of L.

Definition 14. If T is a subset of L the *lattice generated* by T is the intersection of all sublattices of L which contain T.

THEOREM 26. In L_o a is commeasurable with b if and only if the lattice generated by a, a', b, and b' is Boolean.

Definition 15. In L_o an element is *central* if it is commeasurable with all elements of L_o; the set of all central elements is the *center* of L_o; L_o is *irreducible* if its center is $\{0, 1\}$.

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