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Copenhagen Interpretation of Quantum Theory *

NORWOOD RUSSELL HANSON
Department of Philosophy, Indiana University, Bloomington, Indiana

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The theoretical and experimental context within which the "Copenhagen Interpretation" of quantum theory was generated is underemphasized by recent critics of the Bohr-Heisenberg philosophy. When an interpretation of a theory has been as successful as this one has been, there is little practical warrant for the "alternative interpretations" which have, since Bohm, been receiving prominence. Indeed, these are not even genuine alternatives; although rich in provocative prose, they provide not a scrap of algebra with which to organize the practical physicist's thinking. Several objections to the Bohr interpretation are critically examined, as is also a particular use of the correspondence principle which has seemed to cast doubt on the Copenhagen ideas.

IT has become fashionable amongst philosophers of science to attack the "Copenhagen Interpretation" of quantum theory as being either unrealistic,[1] unreflective,[2] or unnecessary.[3] The present paper may be vulnerable to the same objections, but it aims to locate this "interpretation" in its historical and conceptual context, and to argue for orthodox quantum theory as it now stands. Certainly no reinterpretation yet suggested by philosopher or physicist presents a case for abandoning Bohr's view.

I.

The Bohr theory of the Twenties issues from seeds a century old. Controversy over the nature of light was analogous to our present discussions about interpreting $|\psi(q)|^2$. Grimaldi's undulatory theory,[4] developed by Huygens,[5] speculated about by Hooke,[6] and confirmed by Young,[7] Fizeau,[8] and Foucault,[9] encountered the opposition of corpuscularians like Newton,[10] Biot,[11] Boscovich,[12] and LaPlace.[13] The plot is intricate, but it resolves in the 19th century, when the work of Young and Foucault came to be seen as decisive *against* the particulate theory.

Young's work, of course, proves *only* that light is wave-like, —not that it is in no way corpus-

* A revised version of a paper delivered at the 1958 meeting of the American Philosophical Association, Western Division, in the Symposium "Philosophical Problems of Quantum Mechanics."

[1] H. Mehlberg, "The Observational Problem of Quantum Theory," read at the May, 1958, meeting of the American Philosophical Association, Western Division.

[2] D. Bohm, Phys. Rev. **85**, 166, 180 (1952).

[3] P. Feyerabend, "The quantum theory of measurement," in *Observation and Interpretation* (Butterworths Scientific Publications, Ltd., London, 1957).

[4] Franciscus Maria Grimaldi, *Physico-mathesis de lumine coloribis et iride* (Bologna, 1665).

[5] Christian Huygens, *Traité de la lumière* (Leiden, 1690).

[6] Robert Hooke, *Micrographia* . . . (London, 1664).

[7] Thomas Young, *Lectures on Natural Philosophy* (London, 1807).

[8] A. H. Fizeau, Ann. de Chim. et Phys. **29** (1849).

[9] J. B. Foucault, Rec. trav. sci. (Paris, 1878).

[10] Isaac Newton, *Philosophiae Naturalis Principia Mathematica; Opticks* (London, 1687; 1704).

[11] M. A. Biot, *Traité de physique experimentale et mathematique* (Paris, 1816).

[12] R. J. Boscovich, *Philosophiae Naturalis Theoria* (Venice, 1763).

[13] P. S. LaPlace, *Oeuvres* (Paris, 1878–1912).

cular. The latter conclusion follows only from assuming, in addition to Young's data, an exclusive use of the disjunction "light is wave-like or corpuscular (but never both at once)." Newton would not have accepted this rider. Foucault, however, needed no such logical preface. He crushed a cornerstone of Newton's *Opticks* by proving that the velocity of light decreases as the density of its medium increases. This refutes the theory of particulate attraction with which Newton[14] accounted for Snell's law.

A logical monument was built by the wave theorists to mark this defeat. Poisson, Green, MacCullagh, Neumann, Kelvin, Rayleigh, Kirchhoff, and most notably, Clerk Maxwell[15] developed the heritage of Young and Fresnel. In all their work the ideas of particle and wave came to be fashioned in logical opposition. Particle dynamics and electrodynamics matured as mutually exclusive and incompatible theories. Why? Because of (1) the apparent logic of the crucial experiment of Foucault, and (2) the conviction that either one of these two theories could explain every type of energy transfer. Yet the theories could never be applied simultaneously to the same event. A particle is an entity with ideally sharp coordinates, i.e., it is in one place at one time. No two particles can share the same place at t, —this is the logic of punctiform masses in Newton's *Principia*. They collide and rebound, with a precisely calcuable energy exchange. A wave disturbance, however, essentially lacks sharp coordinates. It spreads boundlessly through the undulating medium. The expression "wave motion at a geometrical point" would be, for Maxwell and Newton both, unintelligible.[16] Moreover, two waves can be in the same place at once, as when surf waves cross at a point. Nor is there in wave motion anything strictly like particulate collision, impact, and recoil. (This follows from the wave-theoretic law of linear superposition.) So obvious was this to our great-grandfathers, and so precise its expression in the algebra of Maxwell and Lorentz, that one could treat any class of wave properties α, β, γ as the obverse of some comparable class of particulate properties, $\sim\alpha$, $\sim\beta$, $\sim\gamma$. It was unthinkable that an event should be at once describable both ways: this means not just *unimaginable*, but *notationally* impossible. In the only languages available for describing particle and wave dynamics such a joint description would have constituted a contradiction. Wave and particle ideas were now become conceptual opposites.

There is the kernel of the Copenhagen interpretation, because the fact is that nature refused to live up to 19th century expectations. One need only consider the discontinuous emission of energy from radiant blackbodies,[17] the discovery that photoelectron energy rises with the *frequency* of the incident light, and is independent of intensity,[18] the photon theory of Einstein,[19] the effects discovered by Compton[20] and Raman,[21] and the first confirmations of the de Broglie-Schrödinger wave theory of matter[22] by Davisson, Germer[23] and G. P. Thomson.[24] All this showed that microparticles could be described only jointly,—in particulate and wave-like terms simultaneously. Yet the only such terms available were the inflexible legacy of Maxwell's successors. From the necessity of describing nature thus arises all the conceptual constraints of quantum theory, including the Copenhagen interpretation.

In microphysics it is arbitrary whether one uses a wave or a particle language for descriptions, —just so one is aware that *both are jointly valid*. Several conclusions follow, which it is the merit of the Copenhagen school boldly to have adopted. The fact is that the microphenomenon is a conspiracy of wave and particle properties. But after admitting this (as one must) one must then also maintain a symmetry between these modes of description in all further theoretical work. Thus in a two-electron interaction, the description may

[14] Reference 10, Book I, Sec. XIV; Propositions XCIV–XCVIII; *Opticks*, Book I, Part I; VI.

[15] James Clerk Maxwell, *Treatise on Electricity and Magnetism* (Clarendon Press, Oxford, 1881); *Scientific Papers* (Dover Publications, New York, 1952).

[16] Cf. reference 10, Prop. XLII, Theorem XXXIII; cf., also, Phil. Trans. Roy. Soc. (London), **88**.

[17] M. Planck, Ann. Physik **4**, 553 (1901); Verh. Deuts. phys. **2**, 176 (1900).

[18] P. E. A. Lenard, Ann. Physik **8**, 149 (1902).

[19] Albert Einstein, Ann. Physik **17**, 132 (1905); R. Millikan, [Phil. Mag. **34**, 1 (1917)] indirectly confirms this theory.

[20] A. H. Compton, Phys. Rev. **21**, 483 (1923).

[21] C. V. Raman, cf. Pringsheim, Naturw. **16**, 597 (1928).

[22] L. de Broglie, theses (Paris, 1924); Erwin Schrödinger, Ann. Phys. **79**, 361 (1926).

[23] C. J. Davisson and L. H. Germer, Nature **119**, 558 (1927); Phys. Rev. **30**, 705 (1927).

[24] G. P. Thomson, Proc. Phys. Soc. (London) **117**, 600 (1928).

run: electron creates field; field acts on another electron. But we can always find a parallel particulate description: electron emits photon; photon is absorbed by another electron. Consider also proton-neutron interaction. In wave notation: neutron creates field; field acts on proton. But we will often say: neutron emits electron plus neutrino; electron and neutrino are absorbed by proton.[25]

This resolution not to sacrifice either notation, the facts being what they are, leads to a qualitative appreciation of the uncertainty relations. This is how it came to Heisenberg.[26] Because if the microphenomenon—an orbiting electron—is provisionally descibed as a cluster of the interferences maxima of an otherwise undefined wave group, then precisely to locate it (as a "punctiform mass") at the intersection of four coordinates would require introducing an infinite number of further waves (of infinitely varying amplitudes and frequencies) so as to increase destructive interference along the line of propagation and "squeeze" the packet to a "vertical" line (in configuration space, of course). This renders unknowable the particle's energy, which is intimately associated with the amplitude and frequency of the component phase waves. But if we would determine the particle's energy, then the phase waves must be decreased in number, allowing the "wavicle" to spread "monochromatically" through the whole configuration space. Thus $\psi(x,0)=(\frac{1}{2}\pi\hbar)^{\frac{1}{2}}\iiint_p \alpha(p)e^{(i/\hbar)\mathbf{p}\cdot\mathbf{x}}dp$. That is, the more narrow $\psi(x,0)$ is chosen, the broader the bracket of linear momenta p —the quicker the component plane waves get out of phase, and the "peaked" packet distintegrates. So also, the square of $\psi(p)$ represents the probability of finding our particles with certain momenta if we carry out an experiment measuring linear momenta. This interpretation shows qualitatively the impossibility of determining with precision at t all the 2^n canonical coordinates of a quantum-mechanical system.

Thus when micronature forced a wedding between the concepts classical physics had sundered, three disconcerting notions emerged as issue of the marriage: 1. Physicists were obliged not to overstress either phase of the new joint notation unless nature dictated this (present experience provides no basis for expecting such a dictation). 2. They became aware of a deep conceptual limitation—the uncertainty relations. 3. They saw the need of a single formalism which could integrate these inharmonious ideas, "wave" and "particle," into one algorithm.

Of the "old" quantum theories of 1913[27] and 1916[28] I shall remark only that they were classical models of the "Saturnian" atom proposed by Nagaoka[29] and Rutherford,[30] into which was forced (without reasons) the idea of quantizing electronic orbits. Then de Broglie simply hammered together the wave and particle notations.[31] He had no clear notion of a physical interpretation of these waves, and perhaps still has not. Schrödinger then took de Broglie's *ondes de phase* literally as classical fields of the Maxwell type.[32] This interpretation was punctured by Born[33] as we shall see in the following. So the elegant wave mechanics of Schrödinger, and the observationally equivalent matrix mechanics of Heisenberg,[34] had to float for a time in a cloud of uncertainty concerning just what experimental sense there is in several parameters connected with $|\psi(q)|^2$. Born dispelled this cloud with the ingenious suggestion that the waves be taken as a measure of the probability of locating particles within a given volume element.[35] Because it was operationally clear, and corroborated by every known experiment, Born's view was quickly adopted, and generalized for multiparticle distributions by Bohr, Heisenberg, Gordon, Jordan, Klein, Pauli, and, most significantly, by Dirac.[36]

In 1928 there appeared the greatest contribution to physical theory of our time. Just as Newton's *Principia* forged together the five independent laws of Kepler and Galileo, all of

[25] Cf. W. Heisenberg, *Nuclear Physics* (London, 1953), p. 98.
[26] W. Heisenberg, Z. Physik **41**, 239 (1927).

[27] Neils Bohr, Phil. Mag. **26**, 1 (1913).
[28] A. Sommerfeld, Ann. Physik **51**, 1 (1916).
[29] H. Nagaoka, Nature **69**, 392 (1904).
[30] E. Rutherford, London, Edinburgh and Dublin Phil. Mag. and J. Sci. **21**, 669 (1911).
[31] L. de Broglie, theses (1924); London, Edinburgh and Dublin Phil. Mag. and J. Sci. **47**, 446 (1924).
[32] E. Schrödinger, *Collected Papers on Wave Mechanics* (Blackie and Son Limited, London, 1928).
[33] M. Born, Z. Physik **38**, 11 (1926).
[34] W. Heisenberg, (Born and Jordan) Z. Physik **33**, 35 (1925).
[35] Max Born, Z. Physik **37**, 863 (1926).
[36] P. A. M. Dirac, Proc. Phys. Soc. (London) **112**, 661 (1926); **113**, 621 (1926); **114**, 710 (1927); **117**, 610 (1928); **118**, 351 (1928).

hydrodynamics and every known fact of astronomy, ballistics, and optics, —so also did Dirac's theory of the electron unite in one formally beautiful, and experimentally powerful theory every idea of the particle physics of the Twenties. He provided a comprehensive model for the hydrogen atom, explained the Compton scattering of electrons, the Zeeman effect (doublet atoms), and the empirically required electron spin; all these independent elements were forged into an algorithm whose purpose was to achieve a relativistically invariant theory for fast electrons. Dirac's mastery is clear from his elegant adaptation of Jordan's operator calculus (itself a generalization of Heisenberg's matrix mechanics), to make the qualitative uncertainty relations a formal property of the notation. Dirac took an idea of Graves,[37] developed in Heaviside's "operational" calculus,[38] and already used in quantum theory by Born and Jordan, wherein an ordinary algebra is modified by the law $PQ-QP=n$ (some number). The properties of such a noncommutative system were well understood by 1900. But to translate this formal innovation into a systematic expression of the uncertainty relations implicit in the wave-particle fusion was pure genius. Dirac's paper established quantum mechanics as a unified description of nature. The theory's stature was even more elevated when one of its consequences—first thought a blemish by Dirac [and even earlier (1926) by Gordon,[39]]—entailed unobserved entities with queer properties. This "blemish," which Dirac,[40] Schrödinger, Weyl, and Oppenheimer[41] tried to eradicate, was seen by Blackett to describe the new antielectrons which he and Occhialini[42] and Anderson[43] observed in 1932. Dirac's theory did everything; it integrated all available facts, provided a well-formed formalism, and was fertile in predictions; e.g., the antiproton and the antineutron, have only recently been detected.[44]

Many early objections to, and some present groans about, the Copenhagen interpretation, arise from not appreciating the historical and conceptual role played by Dirac's paper. Here is the notational key to all subsequent quantum physics. Yet, in that paper (Dirac tells me) the Copenhagen interpretation figured essentially—not as some philosophical afterthought Dirac appends to his algebra, but basic to every operation with the notation. Feyerabend[45] provocatively suggests that this need not have been so, that it would be possible to have a "minimum" (i.e., non-Copenhagen) interpretation of quantum theory, and hence of Dirac's paper. But as a matter of fact this is not the way in which this fundamental paper was written. This would not *be* the same paper were its assumptions "purified" as Feyerabend suggests. Largely because of these tough-minded and realistically practical suppositions Dirac's theory had complete success (at least before the era of the meson). What critics of the Copenhagen interpretation often fail to see is that just to ask for an alternative account of micronature without actually providing one which works, is to reinvite the chaos which it is Dirac's triumph to have ended. The way to command a practicing physicist's attention with counter proposals is to provide a better scientific theory, not just a restatement of the orthodox formalism plus some metaphysical asides. Perhaps it *is* possible, as Feyerabend ingeniously moves, to have a minimum statement of quantum theory, with no more "interpretation" than is required barely to describe the facts. But, rightly or wrongly, this is what Dirac felt he had, and what Copenhagen feels it now has, and why it views most counter proposals as observationally irrelevant superstructures. In 1952, Bohm conceded that his reinterpretation affected no known facts, but only added extra philosophical notions of heuristic value.[46] *Bohr et Heisenberg n'ont pas besoin de cette hypothèse.*

This is not to say that philosophers ought to discontinue all attempts to develop proposals which counter the Copenhagen interpretation,

[37] Dating from 1854; referred to me by Dirac.
[38] O. Heaviside, *Electromagnetic Theory* (London, 1894–1912), Appendix K.
[39] W. Gordon, Z. Physik **40**, 117 (1926).
[40] P. A. M. Dirac, Proc. Roy. Soc. (London) **126**, 360 (1930).
[41] J. R. Oppenheimer, Phys. Rev. **35**, 461, 562 (1930).
[42] P. M. S. Blackett and G. P. S. Occhialini, Proc. Roy. Soc. (London) **139**, 699 (1933).
[43] Carl Anderson, Phys. Rev. **41**, 405 (1932).
[44] E. Segrè, Phys. Rev. **100**, 947 (1955); Phys. Rev. **101**, 909 (1956); Phys. Rev. **102**, 1659 (1956); Nuovo cimento **3**, 447 (1956).
[45] See reference 3, p. 49.
[46] D. Bohm, Phys. Rev. **85**, 166, 180 (1952).

but only that they ought to be less enthusiastic in their own evaluations of such activity. What is certaily objectionable is introducing "reinterpretations" via references to the Copenhagen school as holding the field by a kind of authoritative dictatorship. As if there were several clearly formulable *alternative* interpretations of quantum theory which were being forcibly suppressed in favor of the naive metaphysics of personalities like Bohr and Heisenberg! This is patent nonsense. There is as yet *no* working alternative to the Copenhagen interpretation. Ask your nearest synchrotron operator. It therefore seems a questionable procedure to make every new and tentative speculation sound as if it *were* a clear alternative which could easily revolutionize the foundations of physics, if only the elder statesmen would stop backing their favorite horse so uncritically. The issue is this: *until you formulate a new interpretation which works in every particular as well as does the old one, call your efforts by their proper name, "speculations."* This makes them no less worthwhile. And if it be riposted that the Copenhagen interpretation is itself but a speculation, then please let us distinguish those speculations which have proven themselves to *work* in theory and practice from those which have not yet even been put to any rigorous test.

Consider an analogous uneasiness felt by 19th century scientists. By 1860 the inability of celestial mechanics to explain aberrations in Mercury's perihelion became obvious. Leverrier had explained the perturbations of Uranus via Newtonian mechanics by supposing another planet, "Neptune," gravitationally responsible for the anomaly. Six years later this same man detected Mercury's precession. Leverrier appealed again to the "hidden planet" hypothesis.[47] The unseen object was confidently christened "Vulcan" and made responsible for the perturbations. But Vulcan does not exist. It cannot exist (since it would require a straightline solution to the three-body problem, earth—sun—Vulcan, a solution demonstrably unstable). Although many 19th century thinkers were upset by this failure, no one proposed that Newtonian astronomy be abandoned. To do that then, would have been to stop thinking about celestial phenomena altogether. One had to provide some equally useful astronomy, or provisionally accept the otherwise successful orthodox theory; similarly in quantum theory (although its failings are nowhere so grave as was the Newtonian impasse).

Thus it is arguable that the Copenhagen limitations, far from being the result of philosophical naiveté, are built into the very wave-particle duality micronature has forced on us, and built also into the symmetry of explanations in terms of that duality. At least it remains clearly to be shown that this is not so. Dr. Feyerabend would not agree. He distinguishes "Born's interpretation" which gives the formalism a physical meaning, from "Bohr's interpretation" which he characterizes as being itself a metaphysical addition to the bare theory. Let us suppose that Feyerabend is correct: would it follow that "Admitting this implies that we are . . . free to invent and to consider other 'metaphysical' interpretations."[48] Not at all! For this obscures the historical, conceptual, and operationally successful role of the Bohr view (even granting it to be "metaphysical") *as opposed to other interpretations.* The metaphysics in Newton's *Principia* is not to be rated equally with the wooley harangues of Hooke and *unintelligibilia* of Benton. The freedom for inventing alternative interpretations which Feyerabend imagines to follow on the discovery of a metaphysical strain in Bohr's view is unwarranted in the absence of an alternative formalism, and concrete experimental suggestions, on which to build the "new metaphysics."

There is a related point which concerns the experimental situation in which the quantum physicist works. An exposure to laboratory problems of microphysics (e.g., the design of apparatus), makes clear that the only way of learning about particles is to interact with them at our macrophysical level. This is not merely a comment on experimental technique. The proposition, "to learn anything about a particle we must interact with it," has the same logical force as "nothing can move faster than light," or "there cannot be a *perpetuum mobile* of the first type," or "a super-Carnot engine is nonconstruc-

[47] U. J. J. Leverrier, *Recherches astronomiques* (L'observatoire nationale de Paris Annales, 1855–1877).

[48] Brit. J. Phil. Sci. 7, 356 (1957).

tible," or "a temperature-registration of less than −273°C is impossible." None of these state *mere* matters of fact. *Each involves the conceptual principles of entire physical theories.* Similar is the proposition that one must interact with microparticles in order to learn about them. The negation of this, although not self-contradictory, is physically unintelligible.

What does this entail? Just what many philosophers of science persist in being unhappy about—that in particle physics the data can never come to us packed with invariant properties, and undistorted by the observing instrument. Data in microphysics can never be less than a compound of the microevent and some macrophysical system (a detector, or just ourselves). We have no concept of an alternative to this, as a whole cemetery of dead *gedankenexperiments* proves.[49] Such an alternative would have to rest on the possibility of using a detector whose quantum of perturbation is h, to get information about micronature in units smaller than h! Interaction is *the* information concept in quantum physics. If the basic unit of interaction is h (which no hidden-variable theorist could deny), then all information patterns with which we describe the world must also be quantized in the units of h imposed by the detector. Anything "beyond" this is undetectable—unknowable. The alternative to this view can scarcely be made intelligible. Yet many early critics of quantum theory readily supposed they could have clear ideas of what electrons, and protons are "really" like, technical limitations notwithstanding. They pointed to classical statistical mechanics. There, *experimental* limitations could affect the confidence with which we described, e.g., each new thermodynamical event. But they never altered our *concepts* of the matter involved, a hidden symmetry was always felt to lurk behind the statistics. (This confidence results, of course, from confounding, e.g., "phenomenological thermodynamics" with "deductive thermodynamics," developed from the two principles of impotence.) It is however, *the* feature of experimental microphysics that the degree and manner of the perturbation of the system by the detector is *in principle* incalculable.

[49] W. Heisenberg, cf., his essay in *Niels Bohr and the Development of Physics* (Pergamon Press, London, 1955).

This situation is not novel. Let's take seriously this reference to statistical mechanics. The names of Boltzmann and Gibbs spring to mind. In his irresolvably statistical gas theory[50] Boltzmann often objects to Newtonian concepts of observation, as contrasted with what earlier physicists were really entitled to count as such. Boltzmann did not feel that gas theory required statistical formulation *only* because of experimental limitations. No; he viewed statistical mechanics as the primary discipline; it is directly connected with observable parameters. Punctiform masses and ideal particle populations Boltzmann construed not as the starting point of physics, but as abstractions of heuristic value merely: these were dubious variables hidden within the total laboratory exploration of a physical event. They *are* notationally simpler and as such are indispensable in calculation. But the simplicity is merely formal, and should not be in any way confounded with thinking about actual observational data. The development of kinetic theory might have proceeded more smoothly had early mechanics attended to what is observable and not only to what is easier to conceive and calculate. There are no *special* mechanical problems about gases other than that they *must* be described as observed *en masse.* They will not be experimentally or theoretically reduced to metaphysically prior abstractions. [If one tries this, random phenomena (e.g., Brownian motion) must be abandoned as inexplicable.] Boltzmann saw every observation as a function of the design of the apparatus and of the observer's knowledge of the previous state of the observed system.

A more forceful illustration, often invoked by Bohr, is Gibbs' thermodynamics.[51] Imagine a hot metal emitting electrons. A measuring apparatus registers, by the blackening of a point on a photographic plate, the emission of any electron with a velocity greater than v. Now adjust the apparatus so that such electrons are emitted but very infrequently. This type of measurement of the metal's temperature leads to an "objective" determination of one of its properties. We express this mathematically by

[50] L. Boltzmann, *Vorlesungen über gastheorie* (Johann Ambrosius Barth, Leipzig, 1910).

[51] W. Gibbs, *Collected Works* (Yale University Press, New Haven, 1948).

regarding the "metal system" as a sample arbitrarily selected from a canonical esemble.

But what does "objective" mean here? It means that *any* thermometer can be used as measuring instrument, and that measurements do not depend on either thermometer or observer. Now if the total system, metal-plus-apparatus, is completely isolated from the rest of the universe, it has a constant energy (whose value is not exactly known because of the canonical distribution). If, however, the metal is not isolated (as must be the case) its energy varies with time and oscillates in temperature equilibrium about a mean value as indicated by the distribution. If now the canonical ensemble of the total system follows a Newtonian pattern, the ensemble evolves, containing an increasing proportion of states in which points on the plate are blackened. The probability that the measuring apparatus will respond at a certain place at t can be calculated, but the exact instant and place of an actual event cannot be predicted. Were every detail of this experiment known at the start, *à la LaPlace*,[52] we should be able to determine beforehand each actual point-blackening, *provided that the experimental system were cut off and isolated from the rest of the universe.* But in this case the statement of the temperature would be operationally unintelligible. If, however, the experimental system is really connected with, and part of, the external world, then even complete knowledge of its details would not allow precise predictions, since we cannot possibly know every detail of the remaining universe.

This total experimental system also contains a "subjective" element, despite the assumptions of those who suppose thermodynamics to carry no thumbprints of the observing instrument. In the absence of a detecting apparatus, the descriptive mathematics of the system changes continuously, as outlined. But introject a detector; it will *suddenly* register that a point on the plate is blackened. The representation is altered discontinuously, because here we have moved from the former situation containing a great range of mathematical possibilities to a new ensemble containing only the plate blackened at one point. For *that* event, *ex post facto*, the formal statistical representation originally appropriate has no further descriptive utility. This discontinuous change is not contained or even hinted at in the Gibbs' equations characterizing the ensemble. It corresponds to the "reduction of wave packets" in quantum theory (the relevance of the Einstein-Podolsky-Rosen *gedankenexperiment* here should be clear). Thus, characterizing an *experimental* system as a Gibbs' ensemble not only specifies *its* properties, but is also contingent on an observer's knowledge of it, or (what is the same) on a detector's presence within that system. Hence the word "objective" in this context is philosophically questionable (and the same may be said of "subjective," too).

To summarize: one can assign an "objective" temperature to a body on the evidence of the average velocity of its particles, some of which escape (altering thereby "the body") and are recorded by a detector whose own properties are inextricably involved in the "reading." Now, if one had a microphysical knowledge of the particles, one could predict every actual recording by the detector—place and time. But in that case one could no longer assign an *objective* temperature to the body, since the very concepts of temperature and entropy presuppose statistical disorder. Objective temperature and actual recordings are thus mutually exclusive, though complementary: the former requires absolute randomness. The latter, by determining an actual event, curtails randomness to that extent. Again, every such recording, by changing the ensemble, reduces the probability function in the Gibbs' representation, a reduction nowhere written in the equations of motion for the particles.

Against all this consider quantum mechanics itself. A microsystem can be represented by a wave function or by a statistical mixture of such functions (i.e., by a density matrix). This corresponds to a Gibbs' ensemble. If the system interacts with the world, only the mixture (or matrix) representation is possible; we cannot know all details of the total macrophysical system (i.e., the universe). If the microsystem is closed, we may have a "pure case," represented by a vector in Hilbert space. This representation is completely "objective." It contains nothing connected with any observer's knowledge or with any detector's reaction. Calculations concerning

[52] P. S. LaPlace, *Essai philosophique sur les probabilitès* (Paris, 1920 edition).

this vector are certifiably valid or invalid according to objective rules. But such a representation is completely abstract. Its various mathematical expressions $|\psi(q)|^2, |\psi(p)|^2$, do not refer to experimental space or to observable properties: they contain no physics whatever. The microsystem *qua* vector group in Hilbert space becomes a description of physical nature only when linked to how possible experiments will result. Here we *must* consider the interaction of the system with the measuring apparatus, the detecting instrument, the observer. We *must* use a statistical mixture in representing this joint, and discontinuously fluctuating, system.

Could this be avoided by isolating the microsystem and the observing apparatus from the macrophysical universe? No, its connection with, and placement within, the universe is a necessary condition for the apparatus to perform its intended function; its behavior must register in actual experiments if we are to get information from the apparatus. Again, this is not logical necessity. The denial of this does not reduce to $P.{\sim}P$. *P-and-not-P*. (If it did, we would not be discussing physics, —only the formal properties of an algorithm.) Still, this denial describes nothing intelligible. It is inconceivable ("systematically unintelligible") that we should ever encounter a *perpetuum mobile*. So also is it inconceivable that in experiment the measuring apparatus should both perform its intended function, and also be isolated from the macrophysical world. The joint system is therefore describable only by a statistical mixture of wave functions—or a density matrix. It *inevitably* contains statements about the observer or the detector.

To deny this is to rob us of the very concepts needed to experiment in microphysics at all. When detectors mark certain behavior in the experiment, the mathematical representation is thereby altered discontinuously. One among various statistical possibilities has proved actually to obtain. This discontinuous "reduction of wave packets," underivable from any form of Schrödinger's equation, is (as in Gibbs' theory) an effect of shifting from mathematical descriptions to experimental actualities. That is, an actual observation reduces the original ψ representing an ensemble of possibilities of future particle behavior, to a new ψ, whose future possibilities will have been altered irreversibly by the first observation. This shift from original ψ to new ψ is not in the wave equation. One *can* imagine all this projected back in time, but never so far back that the joint system can be thought separate from the macrophysical world. (This assumption is incompatible with the validity of quantum mechanics for closed systems and hence leaves us no concept with which we can genuinely think about quantum phenomena.)

Hence a system isolated from the macrophysical world cannot be described in classical terms. We *may* say that the state of the closed system described by a Hilbert vector is objective, although it does not obtain. Here, however, the classical idea of objectivity must be abandoned for Meinongean mountains. The description of a microsystem by its Hilbert vector group complements its description in classical terms (just as a Gibbs' description of a microscopic state complements a statement of its temperature). The description of an event can be effected by classical concepts to just that approximation to quantum theory one needs for predictions in a given context; this is one version of the correspondence principle. Quantum theory can also be used however. So the boundary between object-observed and instrument-observing can be pushed indefinitely toward the latter, just as I can always regard the temperature of a body as but a property of some thermometer placed in a specified physical context containing the original body. In this case the statistical nature of the laws of microphysics is again seen to be unavoidable.

So much for the history of the Copenhagen interpretation.

II.

Consider now some representative types of attack on this interpretation. The opposition has moved against Copenhagen on four fronts. The earliest discomfort concerned the statistical nature of quantum laws. How should one interpret the ψ function in the fundamental Schrödinger equation? [$\hbar/i \cdot \partial\psi/\partial t = \hbar^2/2m\nabla^2\psi$; i.e., a linear, homogeneous, partial differential equation for the de Broglie wave function $\psi(x,t)$]. In his dissertation[22] de Broglie's *ondes de phase* are never clearly defined. Are they but algebraic fictions, or are they physical existants? His theory of the pilot wave, a wave supposed to accompany every microparticle, suggests the latter view. But no experiment has ever revealed such pilot waves in electrons, protons, or neutrons. Exit de Broglie.

Abstract configuration space is not for Schrödinger either. He imagines an infinite number of interfering Maxwell waves, whose resultant wave maximum just *is* the particle in question.[53] For him an electron is an "energy smear"; $|\psi(q)|^2$ gives the measure of the spread and intensity of that smear. This view collapses immediately in any multibody collision problem. Here Schrödinger must entertain actual spaces of an indefinite number of dimensions (for N particles ψ is a function of $3N$ coordinates)—an idea with no experimental interpretation whatever (save in the "degenerate" case of one particle, where configuration space and physical space coincide). De Broglie, Schrödinger, and also Einstein[54] and Jeffreys[55] have always contended that quantum theory has not yet settled down. It is like mechanics in 1600, phenomenological and chaotic. Schrödinger seeks to rewrite microphysics as a classical field theory. Others, like Bunemann,[56] pursue hydrodynamic models in which the singularities are localizations in a continuous substratum. These attempts, however, have all proved disappointing so far, which is another way of saying that at present we have in fact *one* way of construing quantum phenomena, and this is in terms of the orthodox Copenhagen formulation. Show the physicist a clear, detailed, physically intelligible alternative and he will readily try it. But, in general, a physicist will use a formalism only so far as its parameters are testable; hence he usually takes the ψ function to measure either the probability of finding a particle within a given volume element, or the probability that certain areas on a target detector will be more affected by particle impact than other areas; or ψ may be related to determinations of the density of particles within a parallelepiped of the particle beam, etc. This doesn't minimize the reality of the wave aspect of microparticles. A pencil of β rays scattered by metal foil will leave target patterns describable only by the distribution implicit in the ψ function of equations appropriate for β particles in such a situation. We possess direct evidence of the correctness of de Broglie's approximation in the diffraction of particle beams by crystal lattices. The resulting patterns obey Bragg's law and all other laws of diffraction by spatial gratings as are observed in x-ray difffration, and with the exact wavelengths required by the law $\hbar K = p$ (K = wave propagation vector). The "ultimate" property of individual particles responsible for this distribution remains for the Copenhagen theorist as it might have been for Newton: *I have not been able to discover the cause of these properties of the distribution; it is enough that a particle will more probably strike in one place rather than in another, and that I have a formula for describing this probability.*[57] Or in Bohr's own words: "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 and specified by means of parameters entering into the algebraic or differential equations. . . . These symbols themselves are not susceptible to pictorial interpretation."[58]

The second charge against Bohr's view has concerned uneasiness about the uncertainty relations. But it has often been shown that this discomfort usually consists in simply misunderstanding the conceptual structure of the theory, and in failing to comprehend the reasons for our needing such a theory at all. It need only be remarked that one could generate all of quantum theory from a suitable statement of these relations alone. So they cannot be a peripheral blemish. They are the heart of quantum theory itself. How else to understand how Dirac's noncommutative operators have achieved so much?

A third discomforture concerns just this noncommutative algebra. Some can feel no confidence in a theory whose mathematics are managed according to: $QP - PQ = n$. This is

[53] Erwin Schrödinger, *Four Lectures on Wave Mechanics* (Blackie and Son Limited, London, 1928).

[54] Albert Einstein, Science News **17**, (1948).

[55] H. Jeffreys, *Scientific Inference* (Cambridge University Press, London, 1957), second edition.

[56] O. Bunemann, in a series of lectures and (unpublished) papers delivered at Cambridge University.

[57] See reference 10, *Principia*, Conclusion: "I have not been able to discover the cause of those properties of gravity . . . it is enough that gravity does really exist, and act according to the laws we have explained."

[58] N. Bohr, Dialectica II (1947).

reminiscent of the dissatisfaction many felt with Heaviside's decision to represent dx/dt by p, treating p as any ordinary algebraic quantity. This also leads to a noncommutativity. Critics thought the operational calculus but a sloppy approximation to some more refined-but-then-uninvented theory. Still, the calculus does its job (e.g., in alternating current circuit theory) if one learns the rules of the algorithm. Similarly the Dirac calculus: it was more powerful in prediction and in explanation of microphenomena than any previous theory. Noncommutativity *per se* is no blemish; here it is an ingenious expression of what the data oblige us to think. Landé indeed, may even have an argument which relates noncommutativity to the "natural" postulates of theoretical continuity and symmetry, as these arise in thermodynamics, especially the second law.[59]

There are *genuine* formal improprieties within quantum theory, e. g., "renormalization" and the unintelligible negative probabilities ("ghost states"). True, some physicists (e.g., Heisenberg) have tried to relate these mathematical inelegancies to the structure of what is fundamentally the Dirac theory. But even Heisenberg's most recent work is couched in a noncommutative algebra.

A final kind of discomfort with the Copenhagen School concerns an asymmetry in microphysical explanation and predition. In classical physics explaining X is symmetrical with predicting X. If I explain X via the laws of a system S by reference to initial conditions $\alpha, \beta, \gamma \ldots$ then I might as easily have predicted X on the basis of $\alpha, \beta, \gamma \ldots$ and operating on these via the rules of S. Thus a retrogradation of Mars is explained via celestial mechanics by referring to Mars' mean angular velocity, distance from the sun, mass, and coordinates at some past time. But conversely, at this time past, to have known Mars' coordinates, mass, distance, and velocity would have allowed one to predict via S that at certain future times Mars would be in apparent retrograde movement. For those who take this as the paradigm of explanation and prediction, disappointment with the quantum-theoretic situation is inevitable. Thus Leibniz would have felt disappointment: "The mark of perfect knowledge is that nothing appears in the thing under consideration which cannot be accounted for, and that nothing is encountered whose occurrence cannot be predicted in advance."[60]

After a microphysical event X has occurred within our purview, of course, 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*. This meshes with our earlier points. Expressions of discomfort at this juncture are often just a covert way of announcing that one likes his physics deterministic, objective, orthodox, —in short, Newtonian. But then would such a one also assent to the second law of thermodynamics? If he does (and what else can he do?), is this not inconsistent, as Landé has argued, with any classical determinism?

III.

Heisenberg has examined in detail several typical counterinterpretations of quantum theory.[61] These fall, roughly, into four classes:

(1) The amorphous sighs of yesterday's great men who have not offered one scrap of algebra to back up their grandfatherly advice. This is all reminiscent of Hooke's annoying charge that Newton had merely provided a formula for gravity, but had said nothing about its causes; to which Newton retorted that if Hooke had some *mathematical* contribution to make, he would listen—but not to metaphysical poetry alone (Einstein, Schrödinger, von Laue, de Broglie, Jeffreys).[62]

(2) *Hidden variable* formalisms each of which ultimately destroys the symmetry properties which have been the power and glory of quantum theory (Bohm, Bopp, Fenyes).[63]

[59] A. Landé, *Foundations of Quantum Theory* (Yale University Press, New Haven, 1955).

[60] G. Leibniz, *De la Sagesse* (1693).

[61] W. Heisenberg, in *Niels Bohr and the Development of Physics* (Pergamon Press, London, 1955).

[62] Albert Einstein, *Library of Living Philosophers* (Evanston, Illinois, 1949), Vol. 7; Erwin Schrödinger, in Brit. J. Phil. Sci. **3**, 109, 233 (1952); M. von Laue, *History of Modern Physics* (Academic Press, Inc., New York, 1950); Naturwiss. **38**, 60 (1951); L. de Broglie, *Revolution in Physics* and *La physique quantique restera-t-elle indeterministe?* (Gauthier-Villars, Paris, 1953); H. Jeffreys, *Scientific Inference* (Cambridge University Press, 1957).

[63] D. Bohm, Phys. Rev. **85**, 166, 180 (1952); *Causality and Chance in Modern Physics* (D. Van Nostrand Company, Inc., Princeton, New Jersey, 1957); Bopp, Z. Naturforsch.

(3) Prose passages which fail to grasp the experimental necessity of interaction (Alexandrow).[64]

(4) Pure mathematics, offering no hint about what an experiment which could abrogate, e.g., the uncertainty relations would really be like (Janossy).[65]

The symmetry sacrifice is the significant consideration, however. These counterproposals all sacrifice symmetry in one form or another. Perhaps a Copenhagen-type interpretation is unavoidable if things like wave-particle duality and Lorentz invariance are genuine features of nature. Every known experiment tends to support this idea.[66] The theoretician always favors the theory which saves such symmetries (Yang and Lee included). Recall Schrödinger's attempt to restate the Dirac electron theory, eliminating the negative energy solutions. Both Weyl and Oppenheimer showed his attempt to vary with the choice of Lorentz frames; exit Schrödinger. If we decide thus the fate of genuine theories, why not the same criterion for interpretations of theories?[67]

IV.

Professor Mehlberg has considered[68] Jordan's powerful argument against holding any of the early versions of the correspondence principle.[69] He rejects Jordan's reasoning by first accepting perfect correspondence between classical and quantum mechanics, and then concluding that therefore the latter cannot ultimately be limited in the "nonclassical" ways Bohr and Heisenberg have stressed. This is by now a familiar type of anti-Copenhagen argument which has been used with force, e.g., by Jeffreys.[70]

Thus Mehlberg says:

> . . . since the validity of quantum theory is . . . admitted to range over the whole physical universe, unobjectifiability would be a common feature of all physical concepts should unobjectifiability be an inescapable consequence of quantum theory. . . .
>
> . . . quantum theory also includes large scale bodies, physical concepts would become unobjectifiable throughout the physical universe and the epistemological consequences of this pervasive unobjectifiability would appear to be crippling for the whole empirical method of acquiring knowledge of physical objects on the basis of observation.

In other words, Mehlberg has adopted and exploited the sentiments explicitly expressed by Weyl:

2a (4), 202 (1947); **7a**, 82 (1952); **8a**, 6 (1953); Fenyes, Z. Physik **132**, 81 (1952). Bopp light-heartedly dismisses this symmetry condition which Bohr, Heisenberg, and Dirac have always treated as indispensable [*Observation and Interpretation* (Butterworths Scientific Publications, Ltd., London, 1957)]. But not only have the promised alternative systems (exclusively preferential either to particulate or undulatory notions) not yet been developed enough to have had any impact on the thought of practicing physicists, it remains difficult even to form a detailed concept of what such an alternative would be like.

[64] Alexandrow, Doklady Akad. Nauk. **84**, 2 (1952).

[65] Janossy, Ann. Physik (6) 11, 324 (1952).

[66] The recent parity experiments exposed no exception to Lorentz invariance. They only showed the impossibility of extending the "proper" Lorentz invariance (to continuous transformations), so as to include the discontinuous ("improper") mirroring transformation. In all experiments it has been immaterial whether or not the mirroring process embraced "charge configuration." The parity experiments showed that mirroring without charge configuration (particles—antiparticles) does not lead to invariant results for weak (decay) interactions. Present indications are that if mirroring is redefined to include the charge configuration (so that the mirror image of an electron is a positron), then Lorentz invariance does hold. I owe this point to Professor Konopinski.

[67] There is an analogy between this controversy and that over material implication in mathematical logic. The paradoxical features of material implication are unavoidable once its truth conditions are identified with those of $\sim p \vee q$. This is often termed the cause of the supposed error. It is easy, using well-known deductive rules, to show that $p \supset q$ and $\sim p \vee q$ are mutually deducible. The "paradox" is thus implicit in these rules. In fact, practicing logicians, and students also, never think the *rules* paradoxical. Anyone objecting to material implication, must therefore consider the rules concerning alternations, the equivalence of alternations and conjunctions, double negatives, conditional and *reductio ad absurdum* proof; and *then* say which of these he would abandon or modify and, if the latter, what modification he proposes. Similarly quantum theory. The "paradoxes" of the Copenhagen interpretation follow directly upon a cluster of experimental facts, and established formal techniques for dealing with these facts. An objector to this interpretation must also say which facts or operations on the facts he would abandon or modify, and, if the latter, what that modification should be. And here we need detailed, alogorithmic discussion, not just speculations about what might or might not become possible if only certain experimental dreams could come true. Quantum theory has not yet had its C. I. Lewis. [*Survey of Symbolic Lodge* (University of California Press, California, 1918) and with Langford, *Symbolic Logic* (Appleton-Century-Crofts, Inc., New York, 1932).]

[68] H. Mehlberg, in the symposium "Philosophical problems of quantum mechanics," at the May, 1958, meeting of the American Philosophical Association, Western Division.

[69] P. Jordan, Phil. Sci. **16**, (1949).

[70] H. Jeffreys, *Scientific Inference* (Cambridge University Press, London, 1957), pp. 215–221.

> Thus we see a new quantum physics emerge of which the old classical laws are a limiting case in the same sense that Einstein's relativistic mechanics passes into Newton's . . . when c the velocity of light tends to ∞.[71]

Theoretical treatises intend something special when they speak thus.[72] In such contexts no one is misled. But in contexts like that of Mehlberg's remarks, misconceptions can arise. He seems to have this perplexity: the motions of planets are described and explained in terms of "the old classical laws." In practice it is not possible to determine planetary states by sharp coordinates and momentum vectors, nor can one eliminate the observer's error. Still, it is always legitimate to speak of the planet as *having* exact coordinates and moments. In classical mechanics uncertainties in state determination are in principle eradicable. Thus *point particles are conceptual possibilities within classical particle physics.*

Within Dirac's theory however, to speak of the exact coordinates and momentum of a particle at t is to make no assertion at all. What could such an assertion consist in? That a wave packet has been compressed to a point? This cannot even be false. There is no such concept. *Point particles, therefore, are not conceptual possibilities within quantum physics.*

Yet we are told, by Weyl and Mehlberg, that quantum theory embraces classical physics as a limiting case. The justification for this is usually given in the classical connection between radiation and electrical oscillation when we consider the orbital frequency of the hydrogen atom's electron. ($\omega/2\pi=\gamma=4\pi^2me^4/h^3n^3$.) Quantum theory gives a formula for this analagous to the classical connection. [$\gamma=(2\pi^2e^4/h^3)(n_i^2-n_f^2/n_i^2n_f^2)$ —for frequency of radiation connected with the transition $n_i\rightarrow n_f$. If this transition is small as against n_i, we write $\gamma=(4\pi^2e^4m/h^3n_i^3)(n_i-n_f)$.] In the limiting case of large quantum numbers $\Delta N=1$ gives a frequency identical with the classical frequency. The transition $\Delta N=2$ gives the first harmonic.

Here a perplexity arises which may have affected Mehlberg. A cluster of symbols S expresses an intelligible assertion in classical mechanics, yet that same S does not do so in quantum mechanics. Could $(d^2s|dt^2)=d^2n/dt^2$ be transferred into Dirac's notation? No; still, classical and quantum languages are said to be continuous—arbitrarily distinguished clusters of statements in but one language.

However, statements and language don't work this way. A well-formed sentence S, if it makes an intelligible assertion in one part of a language, does so in all parts. Technical notations are defined by rules determining which symbol-combinations can make intelligible assertions. When S can express an intelligible statement here, but not there, one concludes that the languages in these contexts were different and discontinuous. Finite and transfinite arithmetics, Euclidean and non-Euclidean geometries, the language of time and the language of space, of mind and of brain; these show themselves different and discontinuous on just this principle. What may be meaningful in the one case may be unintelligible in the other. Thus $(d^2s/dt^2)=d^2n/dt^2$ may express the state of a fast particle in classical physics, but these symbols in Dirac's physics make no assertion at all. This ought to prove that the two languages are logically discontinuous. The correspondence principle apparently instructs us otherwise: quantum theory embraces the old classical laws as a limiting case. Jordan's argument, and also those of Bohr and Heisenberg, fail because microphysics "contains" macrophysics.

But how can intelligible assertions become unintelligible as quantum numbers get smaller; how do unintelligible symbol clusters become meaningful just because quantum numbers get larger? The idea of a single formula S ranging within a language from "meaningless" to "meaningful" is difficult to conceive. Either S makes an intelligible assertion throughout the language in which it figures, or else the latter is really more than one language. It's a simple matter of syntax. Either the noncommutativity of position and momentum operators holds (i.e., the S of classical physics makes no assertion whatever in quantum physics), or the correspondence principle holds (i.e., the S of classical physics is a limiting case of quantum physics). *But not both.* Or else we

[71] Weyl, *Philosophy of Mathematics and Natural Science* (Princeton University Press, Princeton, 1949).
[72] P. Ehrenfest, Z. Physik **45**, 7–8 (1927).

misconstrue one (or both) of the principles.[73] Mehlberg may have misinterpreted the second. Jordan's anticorrespondence argument should be given another hearing.

So the alternatives seem to be: (1) quantum physics cannot embrace classical physics, or (2) quantum physics is not permanently restricted as the Copenhagen interpretation suggests, or (3) classical physics itself should be regarded (à la Boltzmann) as incapable of precise state determination, just as in quantum physics.

Dismiss alternative 3. We may grant with Boltzmann that it is more faithful to actual observation. But it is a self-denying ordinance of little theoretical value. A classical mechanics *sans* punctiform masses, rigid levers, and ideal gases would be too difficult to handle to justify its purity (even so, perhaps we ought still to insist on treating rational mechanics as but an ideal abstraction out of the observationally more fundamental statistical mechanics). Alternative 2 we have already considered.

I suggest there is no ultimate *logical* connection between classical physics and quantum physics, any more than between a sense datum language and a material object language. Consider: the punctiform mass, a primarily kinematical idea, is the springboard of classical theory. The wave pulse, a primarily dynamical idea, is the fountainhead of quantum theory. Languages springing from such different stock will show this difference throughout their whole structure. Notwithstanding the correspondence principle, the languages of classical and quantum mechanics diverge.

Suppose I am charged to say how hydrogen atoms with large quantum numbers behave as they do? Well, this example, and others, may be misunderstood. Such different conceptual structures cannot simply mesh in this way just because of physical facts. Their logical gears are not of the same type. Propositions get their force from the total language system in which they figure. That a particular formula gives a classical frequency for the transition $\Delta N=1$ in the case of the hydrogen atom's electron proves at most that there are *formal analogies between certain reaches of quantum theory and certain reaches of classical theory.* That this is only an analogy is obscured by the fact that identical symbols, "$4\pi^2 me^4/h^3n^3$," are used here in both languages. This is not more a logical identity than the uses of "+" and "−" for both valence theory and number theory shows these to have an identical logic with respect to addition and subtraction. Permit an analogy.

Men are made of cells. Whereas it is true to assert that men have brains, personalities, financial worries, it might be no assertion at all to say such things of cells, especially if cell-talk were constructed *ab initio* as logically different from man-talk, —a move similar to the Jordan-Dirac formulation of quantum theory. The two idioms could then never merge. "It has schizophrenia and an overdrawn account" would express nothing in such a circumscribed cell-language. Even though a complex conspiracy of cells could be spoken of in ways analogous to how we speak of a man, this would not conflate the two languages, not even when both idioms characterize the same object, e.g., me. If someone speaks of me as a man but another speaks of me as a collection of cells, although the *denotatum* of both discourses be identical, the two speakers diverge conceptually. They are not speaking the same language; the logic of their speech differs. The two languages are no more identified than wave mechanics and matrix mechanics are proved identical just because the observable consequences of the one are isomorphic with those of the other. The structures of the Schrödinger and Heisenberg systems differed (as did their interpretations).[74] These differences are logical, and independent of the facts that such utterances may be made truly in superficially similar contexts, and may even be expressed in the same symbols. The continuity suggested by incautious statements of the correspondence principle may be illusory. I do not claim that it *is* illusory, but only that the matter is too rarely discussed. An exception is the work of Jordan.

[73] The same difficulty arises in terms of probability distributions. Classical theory allows joint probabilities (in determining pairs like time-energy, and position-momentum). It allows these to increase simultaneously to the limit l. In quantum theory this is illegitimate. But as quantum numbers get larger, this illegitimacy seems to decrease and ultimately to vanish. Ergo, the same perplexity.

[74] P. Bergmann, *Basic Theories of Physics* (Prentice-Hall, Inc., Englewood Cliffs, New Jersey, 1951), p. 277.

So perhaps the correspondence principle does not make classical physics a limiting case of quantum physics, even though the two formalisms are completely analogous at points. What the principle does show is that when quantum numbers are high the hydrogen atom can be regarded either as a small macrophysical body set in a classical space-time, or as a large "quantum body" exemplifying to but a vanishing degree the dynamics of elementary particles. In the first case $S[\text{e.g.}, (d^2r/dt^2) = d^2n/dt^2)]$ will constitute an assertion. In the second case it will not. Against this Dr. Feyerabend offers a most unconvincing argument.[75] "We may," he says, "admit that macroscopic systems can be described in terms of wave functions if we assume at the same time that a macroscopic observer has never enough information at his disposal in order to set up such a wave function." Feyerabend makes it sound as if "*never having enough information to set up a macroscopic wave function*" is like "*never having enough information to verify assertions about the moon's far side.*" But there is a difference in principle here which parallels just that difference between quantum and classical languages I have marked. We never *could* have enough information of the sort Feyerabend remarks. And it is in the very logic of this "never could" that the Copenhagen interpretation lives.

We may treat the hydrogen atom as we please, we may interchange Poisson brackets for quantum brackets when we wish, depending on our problem. But it need not follow that there is a logical staircase running from regions of the order of 10^{-13} cm to 10^{-13} light year. There may be one logical break; that is why we can make assertions about the exact state of Mars, but not about the elementary particle nearest Mars' classical center of gravity. As an indication of how quantum *mathematics* can be managed, the correspondence principle is clear. But when treated as by Professor Mehlberg, it might mislead. Hence I cannot agree that the anticorrespondence argument fails. It fails only if one takes the correspondence principle over-literally. But we can continue to allow chemists and engineers to treat their data realistically. This is just how the quantum physicist regards his laboratory apparatus, even while insisting on a Copenhagen interpretation for the foundations of the theory. Just as the uncertainty principle holds no consequences for ballistics (bullet's uncertainty in position $= 10^{-28}$ cm) so quantum theory as a whole need have no conceptual consequences for classical mechanics. Certainly no physicist is *obliged* to generalize quantum physics as Mehlberg and Weyl do.

[75] P. Feyerabend, in *Observation and Interpretation* (Butterworths Scientific Publications, Ltd., London, 1957), p. 127.

V.

Finally, I would distinguish *Philosophical Problems of Quantum Mechanics* from another subject: *Problems of Philosophers Concerning Quantum Mechanics*. It is a sociological fact that most working quantum physicists do not bother with general problems concerning, e.g., the interpretation of the ψ function, not to mention the abstract philosophical matters recently raised concerning "realistic" or "nonrealistic" interpretations of quantum theory as a whole. This is not because the working physicist refuses to have, or is intellectually incapable of having, philosophical problems; he has plenty. One cannot be exposed to current discussions in quantum field theory and meson theory without feeling their logical, analytical character. This is not imported into physics from other academic contexts. I remember a discussion between Dirac, Heisenberg, and Bethe concerning whether an otherwise successful algorithm containing inconsistencies (as does renormalization) requires immediate examination—or whether danger looms only when predictions of such a theory fail. Historically this is a conceptually significant problem: remember the aether! Another is the question of interpreting the so-called "negative probabilities." Heisenberg argues that their existence in a renormalized calculation indicates a flaw in the very technique and approach to quantum theory set out in the Dirac notation. (Renormalization requires non-Hermitean operators which ruin the unitary character of the scattering matrices, require negative probabilities, and invoke physically unintelligible "ghost" states.) Others (e.g., Bethe, Peierls, Hamilton, and Salam) appraise the matter wholly differently.

Another physical practice of philosophical interest is Gell-Mann's almost taxonomical attack on meson theory. Much of the Dirac theory is unsatisfactory in this region. So Gell-Mann has proceeded like Linneaus, or Mendeléeff, or indeed, even like a natural historian—drawing up "phenomenological" charts of particle properties and allowing generalizations to stand forth from the data. Methodologists' morals about physical theory cannot ignore this "John Stuart Mill"-type approach. Despite Toulmin's campaign, it does exist in physics and can be important.[76] Many such difficulties are the daily fare of practicing quantum physics. I only point to their existence to mark how little attention philosophers pay them. This is no reason for philosophers to cease talking about what they wish. But no one should think that because most quantum physicists are unperturbed by the type of question brought to prominence by Bohm, that therefore they are unreflective, resigned, Berkleyan, computer-ridden predicting machines. What the practicing physicist is likely to find difficult in many philosophical papers concerning the foundations of quantum theory, is a facile use of terms like "realism," "objectivism," and "subjectivism." One might actually be inclined to suppose that philosophers had settled what a discussion of realism, objectivism, and subjectivism was a discussion about. Which reminds me of the question with which an Oxford undergraduate once staggered his tutor: "What is the external world *external to*?" When I see the full sense, or nonsense, in that question, I may see also how quantum theory is going to help, or be helped by, the cracking of such old philosophical chestnuts.

[76] S. E. Toulmin, *The Philosophy of Science* (Hutchinson's University Library, London, 1953).