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Reviewed work(s):
Source: *Philosophy of Science*, Vol. 71, No. 5, Proceedings of the 2002 Biennial Meeting of The Philosophy of Science Association<break></break>Part II: Symposia Papers<break></break>Edited by Sandra D. Mitchell (December 2004), pp. 669-682
Published by: The University of Chicago Press on behalf of the Philosophy of Science Association
Stable URL: http://www.jstor.org/stable/10.1086/425941

Who Invented the "Copenhagen Interpretation"? A Study in Mythology

Don Howard†

What is commonly known as the Copenhagen interpretation of quantum mechanics, regarded as representing a unitary Copenhagen point of view, differs significantly from Bohr's complementarity interpretation, which does not employ wave packet collapse in its account of measurement and does not accord the subjective observer any privileged role in measurement. It is argued that the Copenhagen interpretation is an invention of the mid-1950s, for which Heisenberg is chiefly responsible, various other physicists and philosophers, including Bohm, Feyerabend, Hanson, and Popper, having further promoted the invention in the service of their own philosophical agendas.

1. Introduction. Niels Bohr has long been inseparably linked to the Copenhagen interpretation of quantum mechanics. Critics portray it as a hopeless muddle and trace its alleged shortcomings to Bohr's supposed obscurity and dogmatism. Friends of the Copenhagen interpretation, fewer in number than critics, credit Bohr with deep insight into the novel way in which the quantum theory approaches the description of nature. Almost no one asks whether what is known as the Copenhagen interpretation was, in fact, Bohr's view.

The present paper argues that what is called the Copenhagen interpretation corresponds only in part to Bohr's view, here termed the complementarity interpretation. Most importantly, Bohr's complementarity interpretation makes no mention of wave packet collapse or any of the other silliness that follows therefrom, such as a privileged role for the subjective consciousness of the observer. Bohr was also in no way a positivist. Much of what passes for the Copenhagen interpretation is found in the writings of Werner Heisenberg, but not in Bohr. Indeed, Bohr and Heisenberg disagreed for decades in deep and important ways. The idea that there was a unitary Copenhagen point of view on interpretation was, it shall be argued, a postwar invention, for which Heisenberg was chiefly

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Philosophy of Science, 71 (December 2004) pp. 669–682. 0031-8248/2004/7105-0002$10.00

responsible. Many other physicists and philosophers, each with his own agenda, contributed to the promotion of this invention for polemical or rhetorical purposes. The list includes David Bohm, Paul Feyerabend, Norwood Russell Hanson, and, most importantly, Karl Popper. Understanding the motivations of these actors will carry us a long way toward understanding how views so alien to Bohr could have been foisted upon him.

Disentangling Bohr's views from the views of those who claimed to speak on his behalf and understanding what complementarity really involves are important, now, because new interest in Bohr and complementarity is being evinced in two different arenas. One arena is that of history. Two recent books, especially, have awakened serious scholarly interest in the history of the interpretation of quantum mechanics. Both James Cushing (1994) and Mara Beller (1999) stress the role of social and institutional context in explaining the triumph of a Copenhagen point of view that the authors, themselves, find intrinsically uncompelling. Each takes as given the existence of a unitary Copenhagen view on interpretation, and neither is flattering to Bohr. More sympathetic to Bohr are Catherine Chevalley (1991, 1994) and Jan Faye (1991, 2002). But, while recognizing a diversity of voices within the Copenhagen community, they too employ the "Copenhagen" designation for a cluster of shared ideas.

Contemporary research in the foundations of quantum mechanics is another arena where Bohr draws attention, complementarity being seen as, perhaps, holding the key to an interpretation that might work, as it must, in the context of quantum field theory (Clifton and Halvorson 1999; Halvorson 2004). It is noteworthy that some of those deploying complementarity in foundations research have been led, themselves, to take the historical turn (Clifton and Halvorson 2002; Dickson 2001, 2002). But here again, understanding the complementarity interpretation is complicated by decades of folklore about Bohr's ideas.

2. Subjectivity or Entanglement? Bohr's Philosophy of Complementarity.

Central to the popular image of the Copenhagen interpretation is the idea that observation-induced wave packet collapse is a mode of dynamical evolution unique to measurement interactions. If one regards measurement interactions as thus different in kind from other physical processes, one easily convinces oneself that the observer plays an active role in the quantum domain unlike the detached observer of classical physics, which view opens the door to the subjectivism also assumed to be part of the Copenhagen interpretation, the post-measurement state of the system being held to depend crucially on the state of the observer's knowledge.

Here, already, is a clue that the Copenhagen interpretation bears only a tenuous relationship to Bohr's complementarity interpretation, for Bohr never mentioned wave packet collapse nor did he accord the observer in his or her subjective aspect any fundamental role. Bohr was always careful to physicalize the "observer," preferring locutions such as "agencies of observation" to emphasize the fact that whatever was novel about observations in quantum mechanics was consequent upon measurement's being just another species of physical interaction.

What was novel about measurement for Bohr was that, as in any other quantum interaction, the post-measurement joint state of the object plus the measuring apparatus is entangled. Bohr did not employ the term, "entanglement," but consider a famous early statement of the point at the heart of the complementarity interpretation, from Bohr's 1927 Como lecture:

> The quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected. Accordingly, an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation. . . .
>
> This situation 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. The very nature of the quantum theory thus forces us to regard the space-time co-ordination 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. (Bohr [1927] 1934, 54–55)

Much in this passage calls for comment. Notice, first, that Bohr does not endorse the antirealism often attributed to him. He does not say that one cannot ascribe reality to quantum phenomena, only that one cannot ascribe an "independent reality" to the phenomena *or* to the "agencies of observation." His point is that, since the object and the apparatus form an entangled pair, they cannot be accorded distinct, separate realities. The emphasis is on the word "independent," not the word "reality."

Second, the version of complementarity here introduced, between "space-time coordination" and the "claim of causality," engenders confusion on the part of both those who, failing to hear the Kantian echoes in Bohr's vocabulary, find the idea inherently obscure and those who want to convict Bohr of changing his mind after the EPR paper's employment of indirect measurements (Einstein, Podolsky, and Rosen 1935) supposedly forced him to abandon a disturbance analysis of measurement. The latter argue that Bohr replaced talk of complementarity between "space-time coordination" and the "claim of causality" with talk of complementarity between conjugate observables. But Bohr never endorsed a disturbance analysis of measurement and so could not have changed his mind. Bohr always criticized Heisenberg for promoting the disturbance analysis, arguing that while indeterminacy implies limitations on measurability, it is grounded in "limitations on definability" (Bohr [1927] 1934). More importantly, Bohr had also already in 1927 asserted the equivalence of the two formulations of complementarity:

> According to the quantum theory a general reciprocal relation exists between the maximum sharpness of definition of the space-time and energy-momentum vectors associated with the individuals. This circumstance may be regarded as a simple symbolical expression for the complementary nature of the space-time description and the claims of causality." (Bohr [1927] 1934, 60)

Can entanglement really have been the foundation of Bohr's complementarity interpretation in 1927? Schrödinger introduced the term only in 1935, and awareness of entanglement's fundamental significance seems to many a post-Bell phenomenon. In fact, in 1935 Schrödinger was simply putting a name on (1935a) and developing a formalism for (1935b, 1936) what had been for a decade widely understood as the chief novelty of the quantum theory. For now, just one illustration of the pervasive awareness of entanglement's importance (for further details, see Howard 1990): In a May 1927 talk to the Berlin Academy, shortly before Bohr's Como lecture, Albert Einstein proposed his own hidden variables interpretation of quantum mechanics. The talk was never published because Einstein discovered that his model, like quantum mechanics itself, failed to satisfy what he deemed a necessary condition on any such model, namely, that joint states of previously interacting systems factorize, a condition that Einstein (following already common practice) wrote: $\Psi_{1,2} = \Psi_1 \cdot \Psi_2$ (Einstein 1927). Ever since his first paper on the photon hypothesis in 1905, Einstein had worried about the fact that quanta do not evince the mutual independence of classical material particles, but by the mid-1920s the ineluctability of entanglement became obvious to all thanks to the discovery of Bose-Einstein statistics and to experimental investigations of electron scattering phenomena, such as the Ramsauer effect, along with experimental confirmation of strict energy and momentum conservation in individual scattering events. The latter, especially the Bothe-Geiger and

Compton-Simon experiments, refuted the Bohr-Kramers-Slater theory's assumption of statistical energy and momentum conservation and therewith the statistical independence of transition processes in distant atoms interacting electromagnetically (for details, see Howard 1990). Bohr reacted to these developments in a letter to Hans Geiger of 21 April 1925:

> I was quite prepared to learn that our proposed point of view about the independence of the quantum process in separated atoms would turn out to be wrong. . . . Not only were Einstein's objections very disquieting; but recently I have also felt that an explanation of collision phenomena, especially Ramsauer's results on the penetration of slow electrons through atoms, presents difficulties to our ordinary space-time description of nature similar in kind to the those presented by the simultaneous understanding of interference phenomena and a coupling of changes of state of separated atoms by radiation. In general, I believe that these difficulties exclude the retention of the ordinary space-time description of phenomena to such an extent that, in spite of the existence of coupling, conclusions about a possible corpuscular nature of radiation lack a sufficient basis. (Bohr 1984, 5:79)

As the concluding sentence makes clear, Bohr's famous skepticism about the photon concept concerned mainly the mutual independence seemingly implied by the corpuscular picture.

How did Bohr get from entanglement to complementarity? The route lies through another famous idea, Bohr's doctrine of classical concepts (see Howard 1994). Here is a succinct statement of the first crucial steps in the argument:

> The elucidation of the paradoxes of atomic physics has disclosed the fact that the unavoidable interaction between the objects and the measuring instruments sets an absolute limit to the possibility of speaking of a behavior of atomic objects which is independent of the means of observation.
>
> We are here faced with an epistemological problem quite new in natural philosophy, where all description of experience has so far been based on the assumption, *already inherent in ordinary conventions of language*, that it is possible to distinguish sharply between the behavior of objects and the means of observation. This assumption is not only fully justified by all everyday experience *but even constitutes the whole basis of classical physics*. . . . As soon as we are dealing, however, with phenomena like individual atomic processes which, due to their very nature, are essentially determined by the

> interaction between the objects in question and the measuring instruments necessary for the definition of the experimental arrangement, we are, therefore, forced to examine more closely the question of what kind of knowledge can be obtained concerning the objects. In this respect, *we must, on the one hand, realize that the aim of every physical experiment—to gain knowledge under reproducible and communicable conditions—leaves us no choice but to use everyday concepts, perhaps refined by the terminology of classical physics*, not only in all accounts of the construction and manipulation of the measuring instruments but also in the description of the actual experimental results. On the other hand, it is equally important to understand that just this circumstance implies that no result of an experiment concerning a phenomenon which, in principle, lies outside the range of classical physics can be interpreted as giving information about independent properties of the objects. (Bohr [1938] 1958, 25–26; my italics.)

As Bohr explained in many essays, objectivity requires "unambiguous communicability," which means ascribing definite properties to individual objects, a mode of description inherent in ordinary language and definitive of "classical" physics. But object-instrument entanglement renders a "classical," disentangled description impossible. What to do?

Bohr's answer is that, in the quantum domain, when one speaks of a "phenomenon," one uses that term "exclusively to refer to the observations obtained under specified circumstances, including an account of the whole experimental arrangement" (Bohr [1949] 1958, 64) and that in describing such quantum phenomena one deploys a "classical" physical description, which involves also one's introducing a "cut," the distinction between object and measuring instrument seemingly disallowed by their forming an entangled pair (see, e.g., Bohr [1961] 1963, 78). Understanding how the contextualized notion of quantum phenomena makes possible our disentangling the object and the instrument requires our first dispelling two additional confusions.

First, in stressing the importance of specifying the total experimental context, Bohr was not endorsing operationalism. Electron momentum is not defined by a procedure for measuring momentum; instead, one is permitted to speak of the electron's having a definite momentum only in a specified experimental context. Second, in coupling an object-instrument "cut" with a "classical" account of the measurement, Bohr did not assert that one describes "classically" all of and only that which stands on the instrument side of the cut. Instead, Bohr asserted that one describes "classically" those degrees of freedom of both instrument and object that are coupled in the measurement. If one is measuring an electron's momentum, one describes "classically" both the electron's momentum and, say, the recoil momentum of a test body. Bohr did not introduce an instrument-object dualism, instruments being described "classically," objects of measurement being described quantum mechanically. What he meant was something more subtle.

What Bohr meant by a "classical" description was not a description in terms of classical mechanics or electrodynamics. It was, instead, a description wherein one assumes that object and instrument are separable, that they do not form an entangled pair. But since they do form an entangled pair, how is such a "classical" description possible?

What I think Bohr meant is this (see Howard 1979, 1994): Given a pure state correctly describing any system, including a joint system consisting of an entangled instrument-object pair, and given an experimental context, in the form of a maximal set of comeasureable observables, one can write down a mixture that gives for all observables in that context exactly the same statistical predictions as are given by the pure state. But then, with respect to the observables measurable in that context, one proceeds *as if* the instrument and object were not entangled. One can speak as if the measurement reveals a property of the object alone, and one can regard the statistics as ordinary ignorance statistics, the experiment being taken to reveal a definite, though previously unknown value of the parameter in question.

One short step now to complementarity, for the mutual incompatibility of the experimental contexts for measuring conjugate observables implies that different contextualized "classical" descriptions in terms of mixtures of the aforementioned kind are required for incompatible observables. Relativizing to experimental context makes possible an unambiguous, "objective" account of the object as not entangled with the instrument and, in so doing, implies complementarity.

No wave packet collapse. No antirealism. No subjectivism. Bohr's complementarity interpretation is not at all what came to be regarded as the Copenhagen interpretation. How, then, did the latter come to be taken as representing a unitary Copenhagen point of view whose author and chief advocate was supposed to be Bohr?

3. The Invention of the "Copenhagen Interpretation." Everything not found in Bohr's complementarity interpretation is found in the writings of Heisenberg, and (so far as I have been able to determine) Heisenberg first introduced the term "Copenhagen interpretation" in 1955. Simply put, the image of a unitary Copenhagen interpretation is a postwar myth, invented by Heisenberg. But once invented, the myth took hold as other authors put it to use in the furtherance of their own agendas.

The setting for the invention is Heisenberg's contribution to a volume of essays in Bohr's honor. Heisenberg begins with a review of developments in the mid-1920s downplaying Bohr's criticisms of Heisenberg's story about the significance of indeterminacy and then takes up "the criticisms which have recently been made against the Copenhagen interpretation of the quantum theory" (Heisenberg 1955, 12). About the latter, he says: "What was born in Copenhagen in 1927 was not only an unambiguous prescription for the interpretation of experiments, but also a language in which one spoke about Nature on the atomic scale, and in so far a part of philosophy" (1955, 16).

Heisenberg's characterization of this interpretation is, in part, a perceptive presentation of some of the subtleties in Bohr's complementarity interpretation and, in part, an insinuation of his own rather different views. What he gets right is the need to describe the instrument-object pair by means of a mixture in order to be able to represent the object as possessing objective real properties, but already here he insinuates his subjectivist reading of quantum mechanics.

A closed system, says Heisenberg, can be represented as a pure case, which is an "objective" description because it is not "connected with the observer's knowlege," but since the state function is defined in configuration space, not three space, this representation is "abstract," "incomprehensible," and "contains no physics at all." How does it become physics?

> The representation becomes a part of the description of Nature only by being linked to the question of how real or possible experiments will result. From this point we must take into consideration the interaction of the system with the measuring apparatus and use a statistical mixture in the mathematical representation of the larger system composed of the system and the measuring apparatus. (1955, 26)

But, says Heisenberg, one employs a mixture to describe the instrument-object pair only because the measuring apparatus must, in turn, be connected with the "external world" for its behavior to be "capable of being registered as something actual." When the observer registers an actual measurement result, "he thereby alters the mathematical representation discontinuously, because a certain one among the various possibilities has proved to be the real one." This discontinuous change corresponds to the "reduction of wave-packets" (1955, 27). Dependence upon the observer's registration of a result is analogous to the selection of a virtual ensemble in which to include a system in Gibbsian statistical mechanics, a mode of description that also "contains information about the extent of the observer's knowledge of the system" and thus contains a "subjective" element (1955, 26). Thanks to this privileging of the observer's knowledge, "such concepts as 'objective reality' have no immediately evident meaning, when they are applied to the situation which one finds in atomic physics" (1955, 23).

Heisenberg's advocating a subjectivist interpretation of quantum mechanics was nothing new in 1955, nor was the conflation of his views with Bohr's or philosophical exploitation of the resulting mix. Already by the mid-1930s, physicist friends of logical empiricism, such as Philipp Frank (1932, [1936] 1950, and 1938) and Pascual Jordan (1936a, 1936b), cited Heisenberg on the role of the observer in order to claim quantum mechanics as yet another scientific vindication of verificationism and a consequent disavowal of questions about atomic reality as ill posed. Frank was the more careful and accurate of the two, Jordan the more forthright propagandist on behalf of a "positivistic" epistemology, which is, he wrote in a widely-used textbook, "the overall epistemological viewpoint that finds expression in the modern quantum theory," from which it derives "its most significant support." Positivism, wrote Jordan, is "the epistemological viewpoint of Bohr and Heisenberg" (Jordan 1936a, vii). But it was Heisenberg (1934), not Bohr, whom Jordan cited when explaining that "the act of observation is what first *creates* the definiteness" in an observed quantity (1936a, 308), from which circumstance follows a "radical disavowal" of the "classical representation of reality" (1936a, 309).

What was new in 1955 was Heisenberg's dubbing his amalgam of ideas the "Copenhagen interpretation," but having so dubbed it, Heisenberg regularly reinforced the invention of a unitary Copenhagen point of view and posed as its chief spokesperson (see, e.g., Heisenberg [1955] 1958, 1958). Why? It helps to recall Heisenberg's situation in 1955, especially the fact that the person who was Bohr's favorite in the 1920s had become a moral exile from the Copenhagen inner circle in the postwar period, mainly because of the bitter rupture in Heisenberg's relationship with Bohr during his ill-fated visit to Copenhagen in September 1941 after taking over the leadership of the German atomic bomb project (see Cassidy 1992). What better way for a proud and once ambitious Heisenberg to reclaim membership in the Copenhagen family than by making himself the voice of the Copenhagen interpretation?

Whatever Heisenberg's motivation, his invention of a unitary Copenhagen view on interpretation, at the center of which was his own, distinctively subjectivist view of the role of the observer, quickly found an audience. Many authors took up Heisenberg's representation of Copenhagen orthodoxy as foil or support for their own programs, the most important enablers of the myth being Bohm, Feyerabend, Hanson, and Popper.

Bohm was once an articulate friend of what he took to be Bohr's interpretation of quantum mechanics (Bohm 1951), but by the mid-1950s he was perhaps the most threatening critic of quantum orthodoxy with his revival of de Broglie's pilot-wave hypothesis in a new, technically more adept hidden variables interpretation of quantum mechanics (Bohm 1952, 1957a). After the turn, Bohm seized upon Heisenberg's version of a Copenhagen interpretation for legitimating his hidden variables program by contrasting its objectivism and realism with the alleged Copenhagen absurdities of observer-induced wave packet collapse and the subjectivism that follows therefrom (see, e.g., Bohm 1957b), a move that becomes a standard rhetorical strategy in all subsequent apologetics for Bohmian mechanics (see, e.g., Cushing 1994).

Feyerabend published an important, not at all unsympathetic review of Bohm's *Causality and Chance in Modern Physics* (1957a) in 1960 (see also Feyerabend 1957, 1958). That the choice between Bohmian hidden variable theory and Copenhagen orthodoxy is empirically undecidable was grist for Feyerabend's mill when his program of epistemological anarchism was in gestation (Feyerabend 1973), and thus representing the choice between Bohm and "Bohr" (i.e., Heisenberg) as illustrating the limitations of an empirical algorithm for theory choice becomes a standard trope in the anti-empiricist literature from Thomas Kuhn forward (see Beller 1997).

Hanson was another important critic of empiricism and precursor of Kuhn. He was drawn to Heisenberg's Copenhagen interpretation, but in a manner different from Feyerabend. What especially interested Hanson was Heisenberg's argument that quantum mechanics, properly interpreted, constitutes a "closed theory," one systematically insulated against refutation by experiments within its intended phenomenal domain (Heisenberg 1948; see Hanson 1958, 1959). Foreshadowed here is an important feature of the Kuhnian notion of a paradigm (see Beller 1999, 294–300).

But it was Popper, an old critic of quantum orthodoxy (Popper 1934a, 1934b), who did more than anyone else, starting in the late 1950s, to cement in the popular mind the idea that Bohr and Heisenberg were together committed to a subjectivist interpretation of quantum mechanics. The Copenhagen interpretation was a foil for advancing Popper's own program, both his realist, "objectivist" alternative to antirealist, positivist inductivism and his objective propensity interpretation of quantum mechanical probabilities (Popper 1957, 1959, 1967, 1982). Here is how he begins his most influential essay on the topic:

> This is an attempt to exorcize the ghost called "consciousness" or "the observer" from quantum mechanics, and to show that quantum mechanics is as "objective" a theory as, say, classical statistical mechanics. . . . The opposite view, usually called the *Copenhagen interpretation* of quantum mechanics, is almost universally accepted.

> In brief it says that *"objective reality has evaporated", and that quantum mechanics does not represent particles, but rather our knowledge, our observations, or our consciousness of particles*. (Popper 1967, 7)

Bohr is named as the possibly fearsome defender of Copenhagen orthodoxy, but, as is the case with every one of Popper's other such characterizations of the Copenhagen opposition, the only citation given to ground this portrayal of the Copenhagen interpretation is to a paper of Heisenberg's (1958).

No one event was more significant in establishing as orthodoxy Heisenberg's version of the Copenhagen interpretation than the 1957 Colston Research Symposium, the proceedings of which appeared in a very widely-cited volume (Körner 1957). Some recall that the conference was organized for the purpose of setting Bohm straight (Michael E. Fisher, personal communication), but it turned into more of a coming-out party for Bohm. A distinguished group of speakers included A. J. Ayer, Bohm, Fritz Bopp, Richard Braithwaite, Feyerabend, Markus Fierz, Michael Polanyi, Popper, Leon Rosenfeld, Gilbert Ryle, and Jean-Paul Vigier. The rapidity with which Heisenberg's invention of a unitary Copenhagen interpretation had succeeded is evidenced by the fact that many of the speakers use terms like "orthodox interpretation" and "present interpretation" in a manner that suggests a settled, unproblematic reference.

Bohm, Feyerabend, Popper, and others launch vigorous attacks on one or another aspect of Heisenberg's Copenhagen interpretation, but most commonly on its supposed subjectivism. It was left to Rosenfeld to try—it seems in vain—to stem the tide. This always sensible friend and collaborator of Bohr's saw how the invention was proceeding. Calling the question of "interpreting" a formalism a pseudoproblem, "a short-lived decay-product of the mechanistic philosophy of the nineteenth century" (1957, 41), Rosenfeld goes right to the heart of the critics' rhetorical maneuvers:

> According to our critics, the epistemological point of view of quantum theory undermines the sound belief in the reality of the external world, in which all physical thinking is rooted, and opens the door to the barren doctrine of positivism. . . . This picture would be alarming if it were true. However, it is just another dream, a nightmare perhaps, of our critics. . . . It is based on the most futile casuistics: the critics diligently excerpt from the writings in which the principles of quantum theory are discussed isolated sentences on which they put arbitrary interpretations. No wonder that they should find . . . some difficulty in "understanding Bohr": which, incidently, does not prevent them from branding him as a positivist. There is

> no difficulty, at any rate, in understanding the critics' philosophy and exposing its unscientific character. (1957, 43)

4. Conclusion. Until Heisenberg coined the term in 1955, there was no unitary Copenhagen interpretation of quantum mechanics. There was a group of thinkers united by the determination to defend quantum mechanics as a complete and correct theory, a group whose members shared what many of them called the "Copenhagen spirit." They agreed that indeterminacy, complementarity, and entanglement were important lessons of the quantum theory, lessons whose import went beyond simple empirical claims to a revision in our thinking about how physical theories represent natural phenomena. But they did not all believe that quantum mechanics entailed observer-induced wave packet collapse, a privileged role for the observer, subjectivism, or positivism. Heisenberg and Bohr, in particular, disagreed for decades about just these issues. That Heisenberg succeeded in convincing us otherwise is unfortunate. It is time to dispel the myth.

REFERENCES

Beller, Mara (1997), "Copenhagen Ideology and the Philosophy of Science: Kuhn and Feyerabend," paper delivered at "Dissent and Orthodoxy in Quantum Mechanics", conference in honor of James T. Cushing's Sixtieth Birthday, September 27, 1997. University of Notre Dame, Notre Dame, IN.

——— (1999), *Quantum Dialogue: The Making of a Revolution*. Chicago: University of Chicago Press.

Bohm, David (1951), *Quantum Theory*. New York: Prentice-Hall.

——— (1952), "A Suggested Interpretation of the Quantum Theory in Terms of 'Hidden Variables,' I and II", *Physical Review* 85: 166–193.

——— (1957a), *Causality and Chance in Modern Physics*. London: Routledge & Kegan Paul.

——— (1957b), "A Proposed Explanation of Quantum Theory in Terms of Hidden Variables at a Sub-quantum-mechanical Level", in Körner 1957, 33–40.

Bohr, Niels ([1927] 1934), "The Quantum Postulate and the Recent Development of Atomic Theory", in *Atti del Congresso Internazionale dei Fisici 11–20 Settembre 1927, Como-Pavia-Roma* (published 1928), vol. 2. Bologna: Nicola Zanichelli. Reprinted (1928) in *Nature* (Suppl.) 121: 580–590; and in Niels Bohr (1934), *Atomic Theory and the Description of Nature.* Cambridge: Cambridge University Press, 52–91.

——— ([1938] 1958), "Natural Philosophy and Human Cultures", reprinted in Bohr 1958, 23–31. Originally published in *Nature* 143: 268.

——— ([1949] 1958), "Discussion with Einstein on Epistemological Problems in Atomic Physics", reprinted in Bohr 1958, 32–66. Originally published in Paul Arthur Schilpp (ed.), *Albert Einstein: Philosopher-Scientist*. Evanston, IL: The Library of Living Philosophers, 199–241.

——— (1958), *Atomic Physics and Human Knowledge.* New York: John Wiley & Sons; London: Chapman & Hall.

——— ([1961] 1963), "The Genesis of Quantum Mechanics", reprinted in *Essays 1958–1962 on Atomic Physics and Human Knowledge*. New York: Wiley, 74–78. Originally published as "Die Entstehung der Quantenmechanik", in Fritz Bopp (ed.), *Werner Heisenberg und die Physik unserer Zeit*. Braunschweig: Friedrich Vieweg und Sohn.

——— (1984), *Collected Works*, Erik Rüdinger (ed.), vol. 5, *The Emergence of Quantum Mechanics (Mainly 1924–1926)*, Klaus Stolzenburg (ed.) Amsterdam: North-Holland.

Cassidy, David C. (1992), *Uncertainty: The Life and Science of Werner Heisenberg*. New York: Freeman.

Chevalley, Catherine (1991), “Introduction: Le dessin et la couleur”, in Niels Bohr, *Physique atomique et connaissance humaine,* Edmond Bauer and Roland Omnès (trans.), Catherine Chevalley (ed.). Paris: Gallimard, 17–140.

——— (1994), “Niels Bohr’s Words and the Atlantis of Kantianism”, in Faye and Folse 1994, 33–55.

Clifton, Rob, and Hans Halvorson (1999), “Maximal Beable Subalgebras of Quantum Mechanical Observables”, *International Journal of Theoretical Physics* 38: 2441–2484.

——— (2002), “Reconsidering Bohr’s Reply to EPR”, in Placek and Butterfield 2002, 3–18.

Cushing, James T. (1994), *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony*. Chicago: University of Chicago Press.

Dickson, Michael (2001), “The EPR Experiment: A Prelude to Bohr’s Reply to EPR”, in Michael Heidelberger and Friedrich Stadler (eds.), *History of Philosophy of Science: New Trends and Perspectives*. Dordrecht and London: Kluwer Academic, 263–275.

——— (2002), “Bohr on Bell: A Proposed Reading of Bohr and Its Implications for Bell’s Theorem”, in Placek and Butterfield 2002.

Einstein, Albert (1927), “Bestimmt Schrödingers Wellenmechanik die Bewegung eines Systems vollständig oder nur im Sinne der Statistik?”, unpublished manuscript, Einstein Archive 2–100.

Einstein, Albert, Boris Podolsky, and Nathan Rosen (1935), “Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?”, *Physical Review* 47: 777–780.

Faye, Jan (1991), *Niels Bohr: His Heritage and Legacy. An Anti-Realist View of Quantum Mechanics.* Dordrecht: Kluwer.

——— (2002), “Copenhagen Interpretation of Quantum Mechanics”, in *The Stanford Encyclopedia of Philosophy* (Summer 2002 Edition), Edward N. Zalta (ed.). http://plato.stanford.edu/archives/sum2002/entries/qm−copenhagen/.

Faye, Jan, and Henry Folse (eds.) (1994), *Niels Bohr and Contemporary Philosophy*. Dordrecht: Kluwer.

Feyerabend, Paul K. (1957), “On the Quantum Theory of Measurement”, in Körner 1957, 121–130.

——— (1958), “Complementarity”, *Supplementary Proceedings of the Aristotelian Society* 32: 75–104.

——— (1960), “Professor Bohm’s Philosophy of Nature”, *British Journal for the Philosophy of Science* 10: 321–328.

——— (1973), *Against Method.* London: New Left Books.

Frank, Philipp (1932), *Das Kausalgesetz und seine Grenzen*. Vienna: Julius Springer.

——— ([1936] 1950), “Philosophic Misinterpretations of the Quantum Theory”, in *Modern Science and Its Philosophy*. Cambridge, MA: Harvard University Press, 158–171. Originally published as “Philosophische Deutungen und Mißdeutungen der Quantentheorie”, *Erkenntnis* 6: 303–316.

——— (1938), *Interpretations and Misinterpretations of Modern Physics*. Paris: Hermann et Cie.

Halvorson, Hans (2004), “Complementarity of Representations in Quantum Mechanics”, *Studies in History and Philosophy of Modern Physics* 35: 45–56.

Hanson, Norwood Russell (1958), *Patterns of Discovery*. Cambridge: Cambridge University Press.

——— (1959), “Copenhagen Interpretation of Quantum Theory”, *American Journal of Physics* 27: 1–15.

Heisenberg, Werner (1934), “Atomtheorie und Naturerkenntnis”, *Universitätsbund Göttingen. Mitteilungen* 16(1): 9–20.

——— (1948), “Der Begriff ‘abgeschlossene Theorie’ in der modernen Naturwissenschaft”, *Dialectica* 2: 331–336.

——— (1955), "The Development of the Interpretation of the Quantum Theory", in Wolfgang Pauli (ed.), *Niels Bohr and the Development of Physics*. London: Pergamon, 1955, 12–29.

——— ([1955] 1958), "The Copenhagen Interpretation of Quantum Theory", the Gifford Lectures at St. Andrews, winter term, 1955–1956. Reprinted in *Physics and Philosophy: The Revolution in Modern Science*. New York: Harper and Row, chapter 3.

——— (1958), "The Representation of Nature in Contemporary Physics", *Daedalus* 87 (Summer): 95–108.

Howard, Don (1979), *Complementarity and Ontology: Niels Bohr and the Problem of Scientific Realism in Quantum Physics*. Ph.D. Dissertation. Boston: Boston University.

——— (1990), "'Nicht sein kann was nicht sein darf,' or the Prehistory of EPR, 1909–1935: Einstein's Early Worries about the Quantum Mechanics of Composite Systems", in Arthur Miller (ed.), *Sixty-Two Years of Uncertainty: Historical, Philosophical, and Physical Inquiries into the Foundations of Quantum Mechanics*. New York: Plenum, 61–111.

——— (1994), "What Makes a Classical Concept Classical? Toward a Reconstruction of Niels Bohr's Philosophy of Physics", in Faye and Folse 1994, 201–229.

Jordan, Pascual (1936a), *Anschauliche Quantentheorie: Eine Einführung in die moderne Auffassung der Quantenerscheinungen*. Berlin: Julius Springer.

——— (1936b), *Die Physik des 20. Jahrhunderts: Einführung in den Gedankeninhalt der modernen Physik*. Braunschweig: Freidrich Vieweg & Sohn.

Körner, Stephan (ed.) (1957), *Observation and Interpretation: A Symposium of Philosophers and Physicists*. New York: Academic Press.

Placek, Tomasz, and Jeremy Butterfield (eds.) (2002), *Non-locality and Modality.* Dordrecht and Boston: Kluwer.

Popper, Karl R. (1934a), *Logik der Forschung*. Vienna: Julius Springer.

——— (1934b), "Zur Kritik der Ungenauigkeitsrelationen", *Die Naturwissenschaften* 48: 807–808.

——— (1957), "The Propensity Interpretation of the Calculus of Probability, and the Quantum Theory", in Körner 1957, 65–70.

——— (1959), "The Propensity Interpretation of Probability", *British Journal for the Philosophy of Science* 10: 25–42.

——— (1967), "Quantum Mechanics without 'the Observer'", in Mario Bunge (ed.), *Quantum Theory and Reality.* New York: Springer, 1–12.

——— (1982), *Quantum Theory and the Schism in Physics*. Totowa, NJ: Rowan and Littlefield.

Rosenfeld, Leon (1957), "Misunderstandings about the Foundations of Quantum Theory", in Körner 1957, 41–45.

Schrödinger, Erwin (1935a), "Die gegenwärtige Situation in der Quantenmechanik", *Die Naturwissenschaften* 23: 807–812, 824–828, 844–849.

——— (1935b), "Discussion of Probability Relations between Separated Systems", *Proceedings of the Cambridge Philosophical Society* 31: 555–562.

——— (1936), "Probability Relations between Separated Systems", *Proceedings of the Cambridge Philosophical Society* 32: 446–452.