What Makes a Classical Concept Classical?

Toward a Reconstruction of Niels Bohr's Philosophy of Physics

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> "Every description of natural processes must be based on ideas which have been introduced and defined by the classical theory."
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> — Niels Bohr, 1923[1]

> "There must be quite definite and clear grounds, why you repeatedly declare that one *must* interpret observations classically, which lie absolutely in their essence. . . . It must belong to your deepest conviction—and I cannot understand on what you base it."
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> — Erwin Schrödinger to Niels Bohr, 13 October 1935[2]

1. *Introduction*

There was a time, not so very long ago, when Niels Bohr's influence and stature as a philosopher of physics rivaled his standing as a physicist. But now there are signs of a growing despair—much in evidence during the 1985 Bohr centennial—about our ever being able to make good sense out of his philosophical views.[3] I would not beg the question of whether or not Bohr's philosophy of physics can be given a coherent interpretation, but I think that the despair is premature. What has come unraveled is the *illusion* of understanding given to us by Bohr's self-appointed spokespeople in various philosophical camps—the logical positivists are chiefly to blame—who sought vindication for their own views more than an accurate reading of Bohr's. And this does not imply that understanding is impossible. What is needed at the present juncture is really quite simple. We need to return to Bohr's own words, filtered through no preconceived philosophical dogmas. We need to apply the critical tools of the historian in order to establish what those words were and how they changed over time. We need to assume, at least provisionally, that Bohr's words make sense.

And we need to apply the synthetic tools of the philosopher in order to reconstruct from Bohr's words a coherent philosophy of physics. The present paper is intended as a contribution to these efforts.

The chosen starting point is, by design, somewhat unorthodox: Bohr's *doctrine of classical concepts*. Partly, the choice is dictated by the fact that the doctrine of classical concepts turns out to be more fundamental to Bohr's philosophy of physics than are better-known doctrines, like complementarity. But equally important is the need to find fresh perspectives on Bohr, new questions to put to the evidence of his words. Answers to the old questions are inevitably suspect, because we, as inheritors of at least two generations of misinterpretation, can no longer easily distinguish a reading of Bohr's own words from a reading of the misreadings of those words.

Another good reason for starting with Bohr' s doctrine of classical concepts is that it is, at first reading, so very puzzling. Here is a typical statement of the doctrine:

> It must above all be recognized that, however far quantum effects transcend the scope of classical physical analysis, the account of the experimental arrangement and the record of the observations must always be expressed in common language supplemented with the terminology of classical physics. (Bohr 1948, p. 313)

Does this mean that classical physics has a privileged epistemological status? Does it imply that experimental arrangements are to be described in terms fundamentally different from those applied to other physical systems? Is it merely a restriction on the observational vocabulary of physics? Does it preclude the introduction of new descriptive predicates, such as “spin,” “strangeness,” “color,” and “charm”? Interpretations of the doctrine and the larger philosophy of physics to which it is central run the gamut from a “positivism of higher order” (Feyerabend 1958), through Kantianism (von Weizsäcker 1963), “neo-Kantianism” (Murdoch 1987), or “transcendental philosophy” (Honner 1987), to critical realism (Folse 1985) and “objective anti-realism” (Faye 1991). Some commentators see in Bohr's doctrine of classical concepts the assertion that we are trapped inside a linguistic framework that imposes its structure upon our description of the world (Petersen 1968); others take it to be a vindication of Oxford ordinary language philosophy (Bergstein 1972); and still others see it as having an "affinity" with P. F. Strawson's argument for the indispensability of the common-sense conceptual scheme (Murdoch 1987). In the end, however, all of these interpretations are equally unconvincing, and often for the same reason. They assume, uncritically, that we know exactly what Bohr intended regarding: (*i.*) what a classical description is, and (*ii.*) where a classical description is to be employed; the only question is where the doctrine of classical concepts places Bohr in the philosophical tradition. But both assumptions turn out to be wrong.

My aim in the present paper is to develop a new interpretation, or better, a reconstruction of the doctrine of classical concepts that seeks to be faithful to Bohr's words, and, at the same time, to make both physical and philosophical sense. At the heart of this reconstruction are proposals regarding both of the supposedly non-problematic issues: one regarding what it means to describe a system classically, and one regarding where a classical description, is to be employed. As regards the latter, it is widely assumed that Bohr's intention was that a classical description be given to the measuring apparatus in its entirety, a quantum description being given, presumably, to the observed object in its entirety. On this view, the classical/quantum distinction would coincide with the instrument/object distinction; hence, its designation in what follows as the "coincidence interpretation" of the doctrine of classical concepts. I will argue, instead, that the two distinctions cut across one another, that Bohr required a classical description of *some*, but not necessarily *all*, features of the instrument and, more surprisingly, perhaps, a classical description of some features of the observed object as well. More specifically, I will argue that Bohr demanded a classical description only of those properties of the measuring instrument that are correlated, in the measurement interaction, with the properties of the observed object that we seek to measure; and that this implies, as well, a classical description of the associated measured properties of the observed object itself. A quantum description would be possible for the remaining properties of instrument and object, the properties not crucially involved in the measurement. The properties requiring a classical description will vary from one experimental context to another, but in a manner determined by physical considerations alone, indeed, by quantum mechanics itself.

My claim about the nature of a classical description is that Bohr did not mean simply the application of classical physics the—physics of Newton, Maxwell, Boltzmann, and Einstein—in some combination appropriate to the occasion. I will argue instead that a classical description, in the sense of "classical" relevant to Bohr's concerns, is a description in terms of what physicists call "*mixtures*" (as opposed to what are termed "pure cases"), a formal device that permits us to proceed as if the systems being described were in well-defined, if unknown, intrinsic states, at least with respect to those properties requiring a classical description. The device of mixtures also permits one to give a classical, ignorance interpretation to any statistics that one encounters. Which mixture to employ in a given classical description will depend upon the kind of measurement being performed, the "appropriate mixture" being one constructed out of simultaneous eigenstates of all the observables measurable in a given experimental context. Thus conceived, a classical description is a kind of special case of a quantum mechanical description, special in the sense of agreeing with the latter when employed in the appropriate experimental context, though disagreeing with the quantum mechanical description when employed more generally. It follows that the distinction between classical and quantum modes of description, being just the distinction between appropriate mixtures and pure cases, is implicit in quantum mechanics itself, and thus is not a mark of some fundamental ontological or epistemological distinction.

The interpretation of Bohr's doctrine of classical concepts proposed here is termed a "reconstruction," because one can no longer pretend merely to interpret Bohr' s words as if they stand there unadorned, waiting for an informed and sympathetic eye to read their author's intentions. The history of misreadings of Bohr has so obscured his intentions, that one must first deconstruct the misreadings, so that one can then reconstruct both Bohr's words and their meanings. But another reason for speaking of a "reconstruction" is that there are places where one is forced to go beyond the record of Bohr's words, to ask what Bohr would have said, in certain contexts, consistent with what he says elsewhere. Such interpolation is necessary because Bohr did not always choose his examples and illustrations as we might have wished, and we can now confront his words with new examples that bring out better their intended meaning. In what follows, the places where interpretation passes over into reconstruction will be carefully noted.[4]

2. *Objectivity and Unambiguous Description. Why Are Classical Concepts Important?*

The logical basis of Bohr's philosophy of physics is a novel thesis about *objectivity.* Most of Bohr's physicist colleagues, especially the realists among them, assumed that a necessary condition for scientific objectivity is the mutual independence of the scientist, as knowing subject, from the object of investigation. But Bohr made a break with this traditional conception of objectivity, arguing that the most important necessary condition for objectivity is what he termed the “unambiguous communicability” of the scientist's descriptions of experiments and their results.

In the early twentieth century, one of the foremost proponents of the idea that objectivity requires independence was Max Planck. A typical expression of his views is found in his essay, *Positivismus und reale Aussenwelt:*

> Positivism, when carried through consistently, denies the idea and the necessity of an objective physics, that is, *a physics independent of the individuality of the researcher*. It is forced to do that because, on principle, it recognizes no other reality than the experiences of the individual physicist. I need not say that once this is established, the question whether positivism suffices for the construction of physical science is unequivocally answered, for a science that denies to itself, in principle, the title of objectivity, thereby passes judgment on itself. The foundation that positivism provides for physics is, indeed, firmly established, but it is too narrow; *it must be broadened through an addition, the significance of which consists in the fact that as far as possible science is freed from the contingencies that are introduced through reference to particular human individuals.* (Planck 1931, p. 234; my emphasis)

Planck's point is clear and noncontroversial: Scientific objectivity requires a measure of independence, both for theories and for observations, from the individual scientist. But Planck goes on to make quite another claim. The previous quotation continues:

> And this is done by means of a fundamental step into the metaphysical, demanded not by formal logic, but by the healthy human reason; that is to say, by means of the hypothesis that *our experiences do not themselves constitute the physical world*, rather *that they only give us information about another world that stands behind them and is independent of us*, in other words, the hypothesis that *a real external world exists.* (Planck 1931, p. 234; my emphasis)

The independence demanded in the first part of the quotation is *sociological:* The content of science must be invariant from one researcher to another, unaffected by shifts of perspective. But the second part of the quotation advances a much stronger, metaphysical independence claim, to the effect that the putative objects of scientific description and explanation belong to a world existing independently of the scientist, independent not in the trivial sense of being there whether or not human observers exist, but in the sense of being the way it is whether or not it is observed and regardless of who does the observing. Talk of an "independent reality," without qualification, is ambiguous between the sociological and metaphysical meanings, and the shift from one to the other in Planck's argument verges on equivocation.

Rarely is the nature of this metaphysical independence clearly explained. Planck certainly supplies no such account. But there is, arguably, a necessary physical condition for metaphysical independence, a condition made explicit by Einstein in a late essay, "Quanten-Mechanik und Wirklichkeit":

> If one asks what is characteristic of the realm of physical ideas independently of the quantum-theory, then above all the following attracts our attention: the concepts of physics refer to a real external world, i.e., ideas are posited of things that claim a "real existence" independent of the perceiving subject (bodies, fields, etc.), and these ideas are, on the one hand, brought into as secure a relationship as possible with sense impressions. Moreover, it is characteristic of these physical things that they are conceived of as being arranged in a space-time continuum. Further, it appears to be essential for

> this arrangement of the things introduced in physics that, at a specific time, these things claim an existence independent of one another, insofar as these things "lie in different parts of space." Without such an assumption of the mutually independent existence (the "being-thus") of spatially distant things, an assumption which originates in everyday thought, physical thought in the sense familiar to us would not be possible. Nor does one see how physical laws could be formulated and tested without such a clean separation. (Einstein 1948, p. 321)

Elsewhere, I have labeled Einstein's principle of the "mutually independent existence . . . of spatially distant things" the *separability principle* (Howard 1985; see also Howard 1989). It asserts that all spatio-temporally separated physical systems, whether interacting or not, are to be regarded as possessing separate, intrinsic states. These states will, of course, change as a result of interaction, but they are always separately definable.

Why is separability a necessary condition for metaphysical independence? It is because, whatever else they might be, the observing scientist and the observed object are both physical systems, and so the observation interaction, like all physical interactions, must obey the separability principle.[5] For if observer and observed were to lose their separate *physical* identities, then it could hardly be claimed that they are independent in the strong, *metaphysical* sense.

A fundamental difference between classical physics and quantum mechanics is, precisely, the latter's denial of separability: According to the quantum theory, two previously interacting systems are to be described by a single, non-decomposable joint state, regardless of their spatio-temporal separation, until one of the two undergoes a subsequent interaction with another system. Since an observation is a physical interaction, and since quantum mechanics purports to describe all physical interactions, it follows that quantum mechanics denies the primary necessary physical condition for the metaphysical independence of observer and observed that Planck and Einstein believed to underlie scientific objectivity. I believe that Bohr understood quite well this line of reasoning, and that he saw its entailing the need for an alternative analysis of objectivity.[6] It was in developing this alternative that Bohr articulated the doctrine of classical concepts.

In order to grasp Bohr's point, think again about Planck on objectivity. Entirely apart from what we now know about the invalidity of the physical presuppositions underlying Planck's analysis of objectivity, Planck's argument can be faulted for the ambiguity in its conception of independence. And the cogency of the shift, hardly a valid inference, from sociological to metaphysical independence can be questioned: Is the assumption of the metaphysical independence of observer and observed the *only* way to guarantee the shareable, public character of science?

In a sense, Bohr answers both "yes" and "no." Bohr acknowledges the need for a public science, one independent and hence objective in the sociological sense; however, he chooses to ground such objectivity not upon the nature of the observer-observed relation, but upon the unambiguous communicability of scientific theories and of the results of scientific observations: "Our task must be to account for [human] experience in a manner independent of individual subjective judgment and therefore *objective in the sense that it can be unambiguously communicated in the common human language*" (Bohr 1963b, p. 10; my emphasis). Bohr regarded the doctrine of classical concepts as a direct consequence of his doctrine of objectivity, holding that the use of classical concepts (in a manner yet to be spelled out) is a necessary condition for unambiguous communicability. More often than not, the argument is given in the condensed form typical of Bohr. For example:

> Faced with the question of how under such circumstances [the investigation of atomic systems] we can achieve an *objective* description, it is decisive to realize that however far the phenomena transcend the range of ordinary experience, the description of the experimental arrangement and the recording of observations must be based on common language. (Bohr 1963b, p. 11; my emphasis)

The brevity of such statements misleads by concealing a complicated train of thought. Bohr seems simply to be saying that in order to communicate we must use common language, which appears so obviously true that we wonder how it could enlighten us about so difficult a matter as the interpretation of quantum mechanics.

A rare, more complete version of the argument is found in Bohr's 1937 address, “Natural Philosophy and Human Cultures”:

> 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 experiences has so far been based upon the assumption, *already inherent in ordinary conventions of language*, that it is possible to distinguish sharply between the behaviour 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 infomation about independent properties of the objects. (Bohr 1938, pp. 25-26; my emphasis)

Notice the order of topics in this passage, Bohr starts and ends with reminders that classical assumptions about the mutual independence of observer and observed, or better, about the separability of instrument and object, must be rejected when dealing with quantum phenomena, that is, with phenomena lying “outside the range of classical physics, where one encounters the unavoidable interaction between the objects and the measuring instruments.” But then, Bohr goes on to claim that the assumption of such independence is, nevertheless, “inherent in ordinary conventions of language” and “even constitutes the whole basis of classical physics,” and that, moreover, the demand for unambiguous communicability—“to gain knowledge under reproducible and communicable conditions"—"leaves us no choice" but to use the ordinary language and classical concepts founded on the literally false assumption of independence in describing the design and results of our experiments. Savor the irony! *Physics* forces us, in principle, to deny the independence of observers and quantum objects; *philosophy*, in the guise of the demand for objectivity and, thus, unambiguous communicability, compels us, in principle, to reintroduce the assumption of independence in our choice of a descriptive language.

What exactly is Bohr's argument for the necessity of classical concepts? Clearly, Bohr regards the use of classical modes of description as necessary for an unambiguous and hence objective account of any phenomenon; equally clearly, he regards the assumption of observer-observed independence as an inherent feature of such classical descriptions. But is the use of classical concepts necessary *because* objectivity requires the physical independence of instrument and object and it is classical physics—not quantum mechanics—that is based upon the independence or separability assumption? Or are we compelled to employ ordinary language, supplemented by classical physical terminology, simply because communication requires the use of the common language, in which case the assumption of observer-observed independence would be just an incidental consequence of our having to communicate in our accustomed tongue? Some of Bohr's remarks suggest this latter interpretation, as when he says:

> Just the requirement that it be possible to communicate experimental findings in an unambiguous manner implies that the experimental arrangement and the results of the observation must be expressed in the common language adapted to our orientation in the environment. Thus, the description of quantum phenomena requires a distinction in principle between the objects under investigation and the measuring apparatus by means of which the experimental conditions are defined. (Bohr 1961, p. 78)

But to read Bohr as saying merely that we have to speak our mother tongue is to interpret the necessity of classical concepts as a contingent, historical necessity—we have to use classical concepts because we happen to speak a language in which those concepts are at home. And such a reading leaves open the possibility that, as our language develops, we might outgrow this dependence

The former interpretation—that we must use classical concepts because they embody the instrument-object separability assumption—is preferable, because Bohr intended the necessity of classical concepts to be an enduring one, not to be overcome at a later stage in the evolution of language. Reflect on the meaning of the term, "unambiguous," in Bohr's phrase, "unambiguous communicability." In one essay, Bohr writes:

> The argument is simply that by the word "experiment" we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and of the results of the observation must be expressed in unambiguous language with suitable application of the terminology of classical physics. (Bohr 1949, p. 209)

Classical physical concepts facilitate an unambiguous description, because, by assuming the separability of instrument and object, they enable us to say that *this definite object* possesses *this definite property*. If instrument and object were not regarded as independent, we would not be justified in regarding measurement results as reports about the intrinsic properties of the observed object alone. But then, as Einstein warned us, it would not be clear "how physical laws could be formulated and tested without such a clean separation." If this is why classical concepts are necessary, then the necessity will be abiding.

Our conclusion, then, is that, for Bohr, classical concepts are necessary because they embody the assumption of instrument-object separability, and that such separability must be assumed, in spite of its denial by quantum mechanics, in order to secure an unambiguous and thus objective description of quantum phenomena. In this regard, Bohr agrees with Planck and Einstein. But what these classical concepts are, where and how they are to be employed, and how the seeming contradiction between quantum mechanics and the demands of unambiguous communication is to be resolved—all of this has yet to be explained. Let us turn first to the second of these questions: Where are classical concepts properly employed?[7]

3. *Instruments and Objects of Investigation. Where and How Are Classical Concepts to Be Employed?*

The common view, for which there is, prima facie, considerable textual evidence, is that Bohr demands: (*i.*) that there be "a distinction in principle between the objects under investigation and the measuring apparatus," and (*ii.*) that the measuring instrument be described "in common language supplemented with the terminology of classical physics." In one of many such remarks, Bohr links these two claims:

> The essentially new feature in the analysis of quantum phenomena is, however, the introduction of a *fundamental distinction between the measuring apparatus and the objects under investigation.* This is a direct consequence of the necessity of accounting for the functions of the measuring instruments in purely classical terms, excluding in principle any regard to the quantum of action. (Bohr 1958b, pp. 3-4)

If one spells out the implicit assumption that the measuring instrument, *in its entirety*, is to be described classically, then one gets what I called above the "coincidence interpretation": the classical/quantum and instrument/object distinctions coincide. It is also commonly assumed that Bohr has a criterion for where to draw the two coincident distinctions: The measuring instrument is distinguished from the object both by its relative "size," and by the occurrence within it of irreversible amplification effects. Typical of Bohr's remarks supporting this view is the following:

> In actual experimental arrangements, the fulfillment of such requirements [describing unambiguously the apparatus and results of measurement] is secured by the use, as measuring instruments, of rigid bodies sufficiently heavy to allow a completely classical account of their relative positions and velocities. In this connection, it is also essential to remember that all unambiguous information concerning atomic objects is derived from the permanent marks—such as a spot on a photographic plate, caused by the impact of an electron—left on the bodies which define the experimental

> conditions. Far from involving any special intricacy, the irreversible amplification effects on which the recording of the presence of atomic objects rests rather remind us of the essential irreversibility inherent in the very concept of observation. The description of atomic phenomena has in these respects a perfectly objective character, in the sense that no explicit reference is made to any individual observer and that therefore . . . no ambiguity is involved in the communication of information. (Bohr 1958b, p. 3)

To summarize, then, the common view of Bohr's position is that measuring instruments are to be described entirely by classical concepts and are to be distinguished from objects of investigation by their size and by their being the locus of the irreversible amplification effects characteristic of all observations.

But the cluster of ideas making up this common view cannot be the whole story, because, by itself, this view gives rise to too many difficulties. First, the coincidence interpretation introduces a new dualism into our ontology, and, in consequence, a new interaction problem. Instruments obey classical laws, objects of investigation obey quantum mechanical laws; but they must interact in order for a measurement or observation to be made. How is one to give a physical explanation of this interaction when the two systems are described by fundamentally different physical theories? Another problem derives from the fact that, according to Bohr, the placement of the instrument/object division is variable, depending less upon physics and more upon the pragmatics of observation, that is to say, upon the aims and interests of the experimenter in a given situation. One would like to think that the classical/quantum distinction corresponds to an objective feature of the world, like the vague distinction of “size” just mentioned; but if the instrument/object and classical/quantum distinctions coincide, then the latter inherits the variability of the former, leaving Bohr open to a charge of subjectivism, if not also of inconsistency. Finally, the “size” criterion itself is open to criticism, because, as we shall see, the instrument/object division it entails is different from the one Bohr draws in his detailed comments on certain experiments; indeed, in some cases, subatomic particles themselves must be considered as part of the instrumentation. “Size” might be a sufficient condition for the instrument/object distinction, but it cannot be a necessary one.[8]

I want to argue that the common view, built around the coincidence interpretation, is not the whole story. Bohr's many remarks suggesting this point of view cannot be ignored, but Bohr makes other remarks about measurement that point to a more profound rethinking of the nature of classical descriptions and of their role in accounts of observation, to a conception that might comprehend the core of the coincidence interpretation, while simultaneously refining and correcting it. Since important evidence for this alternative view comes from Bohr's comments on the two-slit diffraction experiment, it is to this belabored but still poorly understood example that we next turn.

Consider the experimental arrangement illustrated below in figure 1. On the left, we have a source of monochromatic radiation, that can be regarded as a beam of particles with a precisely defined component of momentum in the x-direction, p_x. A and B represent diaphragms normal to the incident radiation and containing one and two slits, respectively; C represents a photographic plate. We assume, to begin with, that A, B, and C are firmly attached to a common support, an arrangement that yields a characteristic interference pattern on the plate, illustrated on the right.

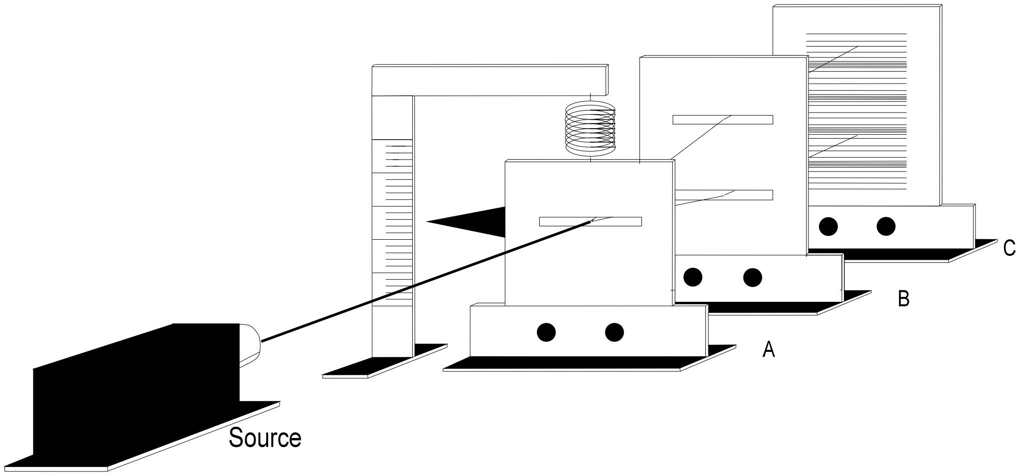

Fig. 1.

The Two-Slit Experiment

Imagine the intensity of the beam reduced to a point where only one particle at a time passes through the apparatus, and consider the possibilities for using the apparatus, in particular diaphragm

A, to measure either the particle's position along the *y*-axis or the *y*-component of its momentum. If *A*, *B*, and *C* are firmly connected through a common support, which defines a spatial frame of reference, then diaphragm *A* provides us with a fairly accurate measurement of the particle's position along the *y*-axis, relative to this reference frame, at the moment of its passage through the slit. The uncertainty in this measurement, Δq_y, will be identical to the slit width. This arrangement cannot be used, however, to determine the particle's momentum in the *y*-direction, because the momentum imparted to the diaphragm by its interaction with the particle is imparted also to the entire apparatus, through the common support that defines the frame of reference. We are thus prevented from studying the momentum of diaphragm *A* relative to this frame, and studying the momentum of the apparatus as a whole, relative to some other reference frame, would be of no interest.

If, on the other hand, we detach diaphragm *A* from the common support, so that it can move freely in the *y*-direction, then a measurement of the particle's momentum becomes possible (but we destroy the interference pattern). Prior to the particle's passage through the slit, we measure the *y*-component of diaphragm *A*'s momentum, and after the particle's passage we measure it again. Both measurements can, in principle, be carried out to any degree of accuracy. A straightforward application of the law of conservation of momentum allows us, then, to infer the particle's momentum after it passes the slit, provided that we knew its momentum in the *y*-direction before passage. But this experimental arrangement precludes an accurate determination of the particle's *y*-position when it passes through the slit, because the *y*-position of the diaphragm is made imprecise by the diaphragm's movement in response to its interaction with the particle.

Bohr uses the two-slit experiment primarily to illustrate the relationship of complementarity, by the example of the phenomena associated with two experimental arrangements—diaphragm *A* either fixed or movable—that reveal equally essential properties of the object under investigation, though the two arrangements are mutually exclusive (see, for example, Bohr 1935, p. 699). But our immediate concern is with the instrument/object and classical/quantum distinctions. Thus, we ask: In each experimental arrangement, which elements are to be considered parts of the object of investigation, and which as parts of the measuring instrument? And, furthermore, what manner of description—quantum mechanical or classical—is to be employed for each element?

In one of his most detailed discussions of the two-slit experiment, this in his reply to the Einstein, Podolsky, and Rosen (EPR) incompleteness argument (Einstein, Podolsky, and Rosen 1935), Bohr says the following:

> The principal difference between the two experimental arrangements under consideration is, however, that in the arrangement suited for the control of the momentum of the first diaphragm [movable diaphragm A], this body can no longer he used as a measuring instrument *for the same purpose as in the previous case* [fixed diaphragm A], but must, *as regards its position* relative to the rest of the apparatus, be treated, like the particle traversing the slit, as an object of investigation, in the sense that the quantum mechanical uncertainty relations regarding its position and momentum must be taken explicitly into account. (Bohr 1935, p. 698; my emphasis)

This is an important passage, because it is the earliest evidence that Bohr did *not* believe that the measuring instrument must be described entirely in classical terms. Ironically, however, it has been used as evidence for the coincidence interpretation.

For example, in one of the more careful analyses of Bohr's remarks on the two-slit experiment, Erhard Scheibe interprets the quoted passage as follows:

> This therefore presents a typical case in which a part of the experimental arrangement [diaphragm *A*] that is initially described in classical terms is converted into the object and thus must be described in quantum-mechanical terms, whereas the requirement of classical description is applied to different parts of the experimental arrangement, in this case the parts used in measuring the momentum of [*A*]. The quantum-mechanical description of [*A*] is expressed . . . precisely in the fact that the uncertainty relation in the y direction for [*A*] is taken into account. (Scheibe 1973, p. 48)

The key to Scheibe's analysis is his assumption of the coincidence interpretation, which implies that the instrument must be described, in its entirety, in classical terms. For when Scheibe reads that the position of diaphragm *A* must be described quantum mechanically, he concludes immediately that the diaphragm, as a whole, must now be part of the object.

Bohr's remarks might at first seem compatible with Scheibe's interpretation, but not upon closer scrutiny. Look again at the first italicized phrase in the quotation from Bohr. Bohr does not say simply that diaphragm *A* can no longer he used as a measuring instrument, as Scheibe suggests. Rather, he says that it can no longer be used as a measuring instrument "*for the same purpose as in the previous case*" (emphasis mine). What Bohr means to say, in my opinion, is that the movable diaphragm *A is* still to be regarded as part of the instrumentation for the purpose of measuring the particle's *momentum*, but not for measuring its *position*. Look also at the second italicized phrase. Bohr says that, in the second arrangement, *A* will be considered an object of investigation, but this only "*as regards its position*" (emphasis mine). Bohr's words actually suggest something quite different from the coincidence interpretation of the doctrine of classical concepts. In *both* experimental arrangements, diaphragm *A* will be regarded as part of the instrumentation, but for different purposes. And in neither arrangement will the whole of diaphragm *A* be given a classical description. In the second arrangement, the position is described quantum mechanically; in the first, we may infer, the momentum will be so described. What will be described classically are, by implication, only those properties of diaphragm *A* that are correlated with the observed system in the measurement. This means that, in the first arrangement, with fixed diaphragm *A*, the diaphragm's *position* would be described classically, since it is correlated with the photon's position, and in the second arrangement, with movable diaphragm *A*, the diaphragm's *momentum* would be described classically, because it is the property correlated with the photon's momentum.

A likely cause of confusion is Bohr's invocation of the uncertainty relations. His words suggest, without implying, that in the second arrangement—the momentum measurement—the movable diaphragm *A as a whole* is to be described quantum mechanically, because the "uncertainty relations regarding its position *and momentum* must be taken explicitly into account" (my emphasis). Proponents of the coincidence interpretation would ask why the quantum uncertainty in diaphragm *A*'s *momentum* must be taken into account, if, with respect to its momentum, *A* is to be described classically, as I have just suggested. Scheibe, for example, stresses this argument.

But the role of the uncertainty relations is not as simple as one might think. Clearly, the quantum mechanical uncertainty relation for the *particle's* position and momentum must hold in both arrangements. But what about the diaphragm? In a trivial sense, the quantum mechanical uncertainty relations apply in both arrangements, because, in principle, any system, regardless of its size, can be described quantum mechanically. However, the question is not whether the diaphragm *can* be so described, but when and how it *should* be. The facts regarding uncertainty are these: In the *first* arrangement, the position measurement, there will be a negligible *classical* uncertainty in the diaphragm's *position*, owing to the high, but limited accuracy with which we can measure its position relative to the common support to which it is attached. At the same time, there will be a non-negligible *quantum* uncertainty in the diaphragm's *momentum*, non-negligible from the point of view of our using its momentum to infer the momentum of the particle, together with a corresponding quantum uncertainty in the diaphragm's *position*. In the *second* arrangement, the momentum measurement, there will be a negligible *classical* uncertainty in the diaphragm's *momentum*, again owing to margins of error in measurements of the diaphragm's momentum. And, at the same time, there will he a nonnegligible *quantum* uncertainty in its *position*, non-negligible from the point of view of our using this position to infer the particle's position, together with a corresponding quantum uncertainty in the diaphragm's *momentum*. With regard to the uncertainty relations, the two situations are thus symmetrical.

If one wonders why Bohr did not explain matters more clearly, one should remember that Bohr's immediate aim in the quoted passage is the elucidation not of the doctrine of classical concepts, but of the doctrine of complementarity (the quoted words are taken from his reply to EPR). He wants to argue that position and momentum together constitute an example of a pair of complementary attributes; and his way of showing this is to demonstrate that, while they are equally essential attributes, the arrangements suited to measure them—fixed or movable diaphragm *A*—are mutually exclusive, in the sense that, for clear-cut physical reasons, the two arrangements cannot be realized simultaneously (diaphragm *A* cannot be *both* fixed and movable simultaneously), nor will either alone suffice for the determination of both parameters. The non-negligible *quantum* uncertainty in *A*'s position in the second arrangement is what demonstrates that arrangement's unsuitability for a measurement of the particle's position. But, of course, uncertainties come in pairs, and, therefore, the quantum uncertainty in *A*'s position must be coupled with an uncertainty in *A*'s momentum. The latter cannot be great enough to invalidate the second arrangement's usefulness for measuring momentum—hence the reference to a *negligible classical* uncertainty in *A*'s momentum—but it must be large enough to ensure satisfaction of Heisenberg's principle.

My alternative interpretation of Bohr's doctrine of classical concepts holds that a measuring instrument, which, on the coincidence interpretation, would be described entirely in classical terms, need only be described classically with respect to those of its properties that are correlated in the measuring process with the properties of the object that we seek to measure. Further evidence for this interpretation comes from Bohr's most careful published remarks on the measurement problem, which are found in a little-known paper of Bohr's from 1939, “The Causality Problem in Atomic Physics,” where Bohr says:

> We must recognize that a measurement can mean nothing else than the unambiguous comparison of some property of the object under investigation with a corresponding property of another system, serving as a measuring instrument, and for which *this property is directly determinable according to its definition in everyday language or in the terminology of classical physics.* (Bohr 1939, p. 19; my emphasis)

A few pages later, Bohr adds:

> In the system to which the quantum mechanical formalism is applied, it is of course possible to include any intermediate auxiliary agency employed in the measuring process. Since, however, *all those properties of such agencies which, according to the aim of the measurements have to be compared with the corresponding properties of the object, must be described on classical lines*, their quantum mechanical treatment will for this purpose be essentially equivalent with a classical description. (Bohr 1939, pp. 23-24; my emphasis)

With regard to the second system, the one serving as the measuring instrument, Bohr does *not* say that *all* of its properties are directly determinable according to their classical definition; rather he says that the property of the instrument corresponding to the measured property of the object, must be so determinable. This means that the only *essential* use of classical methods of description will be in connection with that property of the instrument that is correlated with the property of the object that the instrument is designed to measure. Whether to describe the remainder of the instrument's properties classically or quantum mechanically is basically a practical matter, though even in this respect one's freedom in drawing the classical/quantum distinction is not without limitation.

What the textual evidence suggests, therefore, is that when Bohr talks about the need to describe the measuring instrument in classical terms, he does not mean that the *whole* instrument need be so described. But what, then, can he possibly mean by a classical description? Surely, it cannot be just a straightforward application of the physics of Newton, Maxwell, Boltzmann, and Einstein.[9] Classical mechanics, electrodynamics, statistical mechanics, and even relativity theory cannot be applied to just one of a pair of conjugate parameters, like position or momentum; one cannot construct a classical phase space out of position alone. But what else could be intended by talk of a "classical" description? And, what kind of "classical" description could be, as Bohr remarks, at the end of the last quoted passage, "essentially equivalent" to a quantum mechanical description. In the sense intended by the *correspondence principle*, quantum mechanics might agree with Newtonian mechanics or with Maxwell's electrodynamics in the limit of large quantum numbers, but that is not an "essential" equivalence. Moreover, the kind of convergence between quantum and classical descriptions demanded by the correspondence principle is a wholesale convergence, not an equivalence between selected sets of properties. Whatever Bohr means, it must be something quite different from what we commonly take him to mean.

4. *Of Mixtures and Pure Cases. What Makes a Classical Description Classical?*

Some of our original questions about the doctrine of classical concepts are still to be answered; along the way we have accumulated a few new ones; and there are some that we only now pose:

(1) How does the use of classical concepts guarantee a description that is unambiguous?
(2) How do classical concepts embody the separability principle?
(3) Why does Bohr so often say that "the unambiguous account of proper quantum phenomena must, in principle, include a description of all relevant features of the experimental arrangement" (Bohr 1958b, p. 4)?
(4) In the description of a measuring instrument, why is the only essential use of classical concepts in the account of those parameters of the instrument that are correlated with the measured property of the object?
(5) How can one give a classical description of only one, out of a pair of conjugate parameters?
(6) How can a classical description be "essentially equivalent" to a quantum mechanical one?

My strategy for finding an interpretation of the doctrine of classical concepts that answers these questions will be to look for a *formal* model that makes sense of Bohr's position, by explaining the difference between a classical and a quantum description, filling in, where necessary, the gaps in Bohr's words, but in a way that remains true to Bohr's own words--*all* of Bohr' s words. The very fact that the model is a formal one means that it goes beyond what one will find in Bohr's writings, which is one reason why, as I said above, this interpretation is more accurately described as a reconstruction. But a reconstruction is what is needed.

Our search for this formal model will he aided by the use of another *Gedankenexperiment*, first suggested by David Bohm (Bohm 1951, pp. 615-619). Consider the following situation (see figure 2). A spin-zero particle with positive parity decays at time t_0 into two electrically neutral spin-1/2 particles, L and R, that have the same intrinsic parity, by means of a parity conserving interaction. Collimators select pairs of decay products traveling in opposite directions without affecting their spins, and a pair of Stern-Gerlach apparatuses that can be rotated around their longitudinal axes enable us to measure the spins, along either the *z*- or *y*-axes, of those decay products that pass through the collimators.

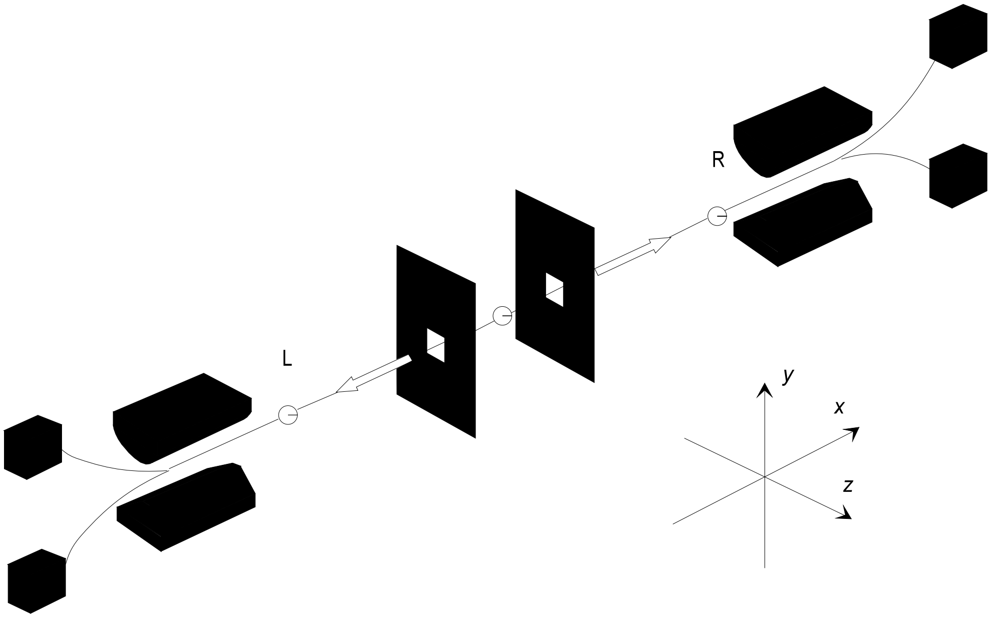

Fig. 2. The Bohm *Gedankenexperiment*

If the decay interaction conserves spin, then the state of the composite system, $L+R$, at some time, $t_1>t_0$, is represented according to the orthodox quantum mechanical interaction formalism as a superposition in the tensor product space $H^L \otimes H^R$ of the Hilbert spaces of the separate systems:

$$(L, R) = 2^{-\frac{1}{2}}(u^z_+(L)u^z_-(R) - u^z_-(L)u^z_+(R)), \qquad (1)$$

where $u^z_+(L)$ represents the state of system L wherein the spin along the z-axis is +1/2; $u^z_-(L)$ represents the state of L corresponding to a z-spin of -1/2; and likewise for $u^z_+(R)$ and $u^z_-(R)$. The basis states, $u^z_\pm(L)$ and $u^z_\pm(R)$, are the eigenstates of the spin operators, $\mathbf{s}^z(L)$ and $\mathbf{s}^z(R)$, respectively.

We can regard the Bohm experimental arrangement as a device for measuring the spin of one of the decay products, say system L. Of course, it is a needlessly complicated and indirect way to measure a particle's spin, but then we are doing philosophy, not physics, so a little artificial complication is perhaps a virtue. The possibility of using the Bohm arrangement for a spin measurement is secured by the fact that the measured spin of system R is a reliable index to the spin of system L, since the principle of spin conservation embodied in (1) implies that if, for example, one measures the z-spin of system R and finds a value of +1/2, any subsequent measurement of the z-spin of L would reveal a value of -1/2 (assuming that L undergoes no other interactions in the meantime).[10]

Unfortunately, nowhere in his published writings does Bohr discuss the Bohm *Gedankenexperiment* as an example of quantum mechanical measurements. My approach, then, is to ask what Bohr would have said about it, consistent with his other remarks about measurement. Our topic being Bohr' s doctrine of classical concepts, let us begin by comparing the quantum and "classical" accounts of the Bohm *Gedankenexperiment.*

No doubt the most *non-classical* feature of the orthodox quantum mechanical account of interactions is its *nonseparability.* What this means is that no factorizable state function of the form, $(L,R) = (L) \circ (R)$, where (L) and (R) are separate state functions for the systems, L and R, can reproduce all of the statistical predictions derivable from a state function of the form of (1). A factorizable state function would be appropriate were the interaction a separable, "classical" one, where the interacting systems are assumed always to possess separate, intrinsic states. Another important, non-classical feature of the quantum account of interactions is that it precludes our assuming that the interacting systems are in definite, but unknown states. Quantum superpositions are, in this sense, characterized by an objective indefiniteness that is resolved only when, by means of a new measurement, the superposition is reduced to one of its component basis states. And the existence of this objective indefiniteness implies that quantum statistics cannot be understood as resulting from an ignorance of objectively definite, but unknown properties of a system, which means that there is a fundamental difference between quantum statistics and classical Boltzmann statistics.

For our purposes, the important differences between the "classical" and quantum mechanical descriptions of the Bohm experiment are best brought out, though, by using the density matrix formalism.[11] Consider an ensemble E of N identically prepared composite systems, L+R, each of which results from a decay interaction of the sort postulated in the Bohm experiment. According to the orthodox quantum theory, which denies the general validity of the separability principle, this ensemble is a pure case described by the density matrix:

$$\begin{aligned} \mathrm{W}_{qm} = \tfrac{1}{2}\big(&|u^z_+(L)\rangle|u^z_-(R)\rangle - |u^z_-(L)\rangle|u^z_+(R)\rangle\big) \\ &\cdot \big(\langle u^z_+(L)|\langle u^z_-(R)| - \langle u^z_-(L)|\langle u^z_+(R)|\big). \end{aligned} \tag{2}$$

If, on the other hand, the separability principle holds, then both components of the composite system must be regarded as assuming separate, definite states immediately after the decay at time t_0. Moreover, the conservation of spin implies that these separate states are correlated, in the sense that, if system L goes into state $u^z_-(L)$, then R goes into $u^z_+(R)$. But that means that we can divide the ensemble E into two sub-ensembles, E^z_+ and E^z_-, where E^z_+ consists of the N^z_+ elements of the original ensemble in which system L is in state $u^z_+(L)$, and E^z_- consists of the N^z_- ($\approx N^z_+$) elements in which system L is in state $u^z_-(L)$. E^z_+ and E^z_- will each be pure cases, described by the density matrices:

$$\mathrm{W}^{z}_{+} = |u^z_+(L)\rangle|u^z_-(R)\rangle\langle u^z_+(L)|\langle u^z_-(R)| \tag{3}$$

and

$$\mathrm{W}^{z}_{-} = |u^z_-(L)\rangle|u^z_+(R)\rangle\langle u^z_-(L)|\langle u^z_+(R)|, \tag{4}$$

respectively. When such a division of an ensemble is feasible, the entire ensemble is a *mixture* of the elements of the sub-ensembles, which in this case is described by the density matrix:

$$\begin{aligned} \mathrm{W}^{z}{}_{cl} = {} & |u^{z}{}_{+}(L)\rangle|u^{z}{}_{-}(R)\rangle N^{z}{}_{+}/N\langle u^{z}{}_{+}(L)|\,\langle u^{z}{}_{-}(R)| \\ & + |u^{z}{}_{-}(L)\rangle|u^{z}{}_{+}(R)\rangle N^{z}{}_{-}/N\langle u^{z}{}_{-}(L)|\,\langle u^{z}{}_{+}(R)|. \end{aligned} \tag{5}$$

The subscript, "*cl*," in $\mathrm{W}^{z}{}_{cl}$, is intended as a reminder that a description in terms of mixtures is "classical" in at least those two senses alluded to above where the "non-classical" character of the quantum mechanical description was discussed: (a) mixtures embody the separability assumption, and (b) they license an ignorance interpretation of the resulting statistical predictions.

For a wide range of possible measurements, the two descriptions, in terms of W_{qm} and $\mathrm{W}^{z}{}_{cl}$, yield exactly the same predictions. They agree, in particular, about all possible measurements, both separate and joint measurements, of the z-spins of L and R. But differences emerge if we ask about spin measurements along other axes. For example, the probability of finding a y-spin of -1/2 for system L, given that we have already found that value for the y-spin of R, is 0 according to W_{qm}, as one expects from spin conservation. $\mathrm{W}^{z}{}_{cl}$ however, yields a probability of 0.5, which, in fact, turns out to be the wrong value, as we find when we perform the experiment, because, of course, spin is conserved.

But while $\mathrm{W}^{z}{}_{cl}$ gives the wrong result for joint measurements of the y-spin of L and R, there is another mixture whose density matrix yields the correct value. $\mathrm{W}^{z}{}_{cl}$ was constructed upon the division of the ensemble E into sub-ensembles, $E^{z}{}_{+}$ and $E^{z}{}_{-}$, corresponding to the two possible definite values of the z-spin of L. We could just as well assume that E is divided into sub-ensembles $E^{y}{}_{+}$ and $E^{y}{}_{-}$, corresponding to the two possible definite values of the y-spin of L. The density matrix for this mixture would be:

$$\mathrm{W}^{y}{}_{cl} = |u^{y}{}_{+}(L)\rangle|u^{y}{}_{-}(R)\rangle N^{y}{}_{+}/N\langle u^{y}{}_{+}(L)|\,\langle u^{y}{}_{-}(R)| \tag{6}$$

$$+\ |u^{y}{}_{-}(L)\rangle |u^{y}{}_{+}(R)\rangle N^{y}{}_{-}/N \langle u^{y}{}_{-}(L)|\ \langle u^{y}{}_{+}(R)|.$$

$W^{y}{}_{cl}$ yields the same values as W_{qm} for all separate and joint measurements of *y*-spins, whereas $W^{z}{}_{cl}$ did not. But $W^{y}{}_{cl}$ is no better than $W^{z}{}_{cl}$ as a "classical" alternative to W_{qm}, because $W^{y}{}_{cl}$ disagrees with W_{qm} for other measurements. In particular, if we have measured the *z*-spin of *R*, obtaining a value of -1/2, W_{qm} implies a probability of 0 for finding the *z*-spin of *L* to be -1/2, whereas $W^{y}{}_{cl}$ implies a probability of 0.5.

One might simply regard the differences between the predictions derived from $W^{z}{}_{cl}$ and $W^{y}{}_{cl}$, along with the discrepancies between each of these sets of predictions and those derived from W_{qm}, as demonstrating the futility of any attempt to describe decay events, or quantum interactions, measurement interactions included, along "classical" lines. Not only are both of the "classical" descriptions incompatible with the quantum mechanical one, they are also mutually inconsistent.

However, to argue thus would be to misjudge the lesson that nature is teaching us here. The interesting fact about mixtures is that, *within a specific experimental context*, what one might call the mixture "appropriate" to that context gives all of the correct predictions *for the results of measurements possible in that context*. Thus, for a specific orientation of the Stern-Gerlach apparatuses, say along the *y*-axis, a mixture constructed out of *y*-spin basis states gives the right predictions for all measurements of *y*-spin.

This fact is but a special case of a general relationship between mixtures and pure cases. If we define an *experimental context* as a set of compatible (and, thus, co-measurable) observables, then in every such context there exists a mixture *appropriate* to the context that yields all of the same predictions for measurements possible in that context as are implied by the pure case that is otherwise the proper quantum mechanical description. The mixture is appropriate in the sense that it will be a mixture over a set of basis states that are simultaneous eigenstates of all of the observables defining the context, that is, of all of the observables measurable in that context.[12]

It is upon this disarmingly simple mathematical fact—the equivalence, context by context, of pure cases and mixtures—that I build my interpretation of Bohr's doctrine of classical concepts. I claim that we make the clearest sense out of Bohr's stress on the importance of a classical account of experimental arrangements and of the results of observation, if we understand a classical description to be one in terms of appropriate mixtures.

More specifically, I would reconstruct the doctrine of classical concepts as follows. Given any measurement interaction, a description in terms of a pure case is correct, in the sense that it yields all of the right predictions. This is the proper quantum mechanical account of the interaction, and such an account can always be given for all aspects of the interaction, including all parts of both instrument and object. Such a description reflects the essential nonseparability of the quantum mechanical interaction formalism, the nonseparability that Bohr stresses as a fundamental lesson of the quantum mechanical account of the instrument/object interaction; it reflects, too, the non-classical character of quantum statistics. On the other hand, precisely because of its nonseparability, a description in terms of a pure case does not permit us to distinguish instrument and object in the way that Planck and Einstein thought necessary to ensure objectivity. But here is where the concept of an appropriate mixture finds its place. Once we specify the kind of measurement being performed, an appropriate mixture can be constructed that gives all of the right predictions for the parameters involved in such a measurement; and, at least with respect to those parameters, we can separate the states of the instrument and the object and give a purely classical, ignorance interpretation of their statistics. The proper "classical" description, then, is a description in terms of an appropriate mixture. Of course, different "classical," descriptions would have to be given in different contexts, but that is entirely consistent with Bohr's remarks, quoted at the end of section three, to the effect that "all those properties of such agencies which, according to the aim of the measurements have to be compared with the corresponding properties of the object, must be described on classical lines."

5. *Does the Reconstruction Work?*

For this reconstruction of Bohr's doctrine of classical concepts to be acceptable, it must give satisfactory answers to all of the questions collected at the beginning of this section. Let us examine them, one by one. First, how do classical concepts guarantee that a description employing them will be unambiguous? From Bohr's point of view, a quantum mechanical description is ambiguous precisely because of its nonseparability. In one essay, he speaks of "the essential ambiguity involved in a reference to physical attributes of objects when dealing with phenomena where no sharp distinction can be made between the behavior of the objects themselves and their interaction with the measuring instruments" (Bohr 1949, p. 234). Bohr's worry seems to be that if we cannot separate the state of the instrument from the state of the object, then we cannot regard measurement results as reflecting intrinsic properties of the object, independent of its interaction with the instrument. But then a "classical" description in terms of an appropriate mixture resolves the ambiguity by allowing us to assume that instrument and object are separable, at least with regard to the properties at issue in the given measurement. We can thus say, unambiguously, that *this* definite system possessed *this* definite property.

Our second question has already been answered: How do classical concepts embody the separability principle? The answer is that a mixture is constructed upon the assumption that the ensemble it describes can be divided into two or more sub-ensembles; and for an ensemble of previously interacting pairs of systems, such a decomposition is possible if and only if the joint state of each pair can be factorized as a product of definite, separate states, if and only if, that is, the joint state can be "disentangled."[13]

Our third question was this: Why does Bohr say that in the description of a proper quantum phenomenon, all features of the experimental arrangement must be specified? The answer given by the appropriate mixtures reconstruction of the doctrine of classical concepts is clear. Only after the total experimental context is specified, only, that is, after we have said exactly what kind of measurement is being performed, can we select the appropriate mixture. In the quantum universe, one cannot assume that observed objects have a separate identity independent of any particular context. It is only within a specific context, determined by the total experimental arrangement, that the object's separate identity can be affirmed. As Bohr himself says in a comment on the EPR paradox:

> In fact, the paradox finds its complete solution within the frame of the quantum mechanical formalism, according to which no well-defined use of the concept of "state" can be made as referring to the object separate from the body with which it has been in contact, until the external conditions involved in the definition of this concept are unambiguously fixed by a further suitable control of the auxiliary body. (Bohr 1939, p. 21)

The fourth question asked why, in the description of a measuring instrument, the only *essential* use of classical methods of description is in the account of that parameter of the instrument that is correlated with the measured property of the object. The answer is that only in connection with these properties need we assume the separability of instrument and object. And the appropriate mixture reconstruction reflects this fact by its context dependence: A different mixture is appropriate to every different context, in the sense that an appropriate mixture yields the correct predictions only for those parameters measurable in that context. All other parameters—of both object and instrument—are correctly described only quantum mechanically, in terms of the pure case density matrix.

Our fifth question was: How can one give a classical description of only one out of a pair of conjugate parameters? The answer, now, is simple. An appropriate mixture describes correctly only those observables that determine the experimental context. By definition, the conjugates of these observables are excluded, though in a context where the latter are observable, they too can be given a correct "classical" description, in terms of their own appropriate mixture.

The sixth and final question probed the connection between quantum and classical descriptions: How can a classical description be "essentially equivalent" to a quantum mechanical one? Bohr's *correspondence principle* is what first comes to mind, but it cannot provide the answer, for two reasons. First, the correspondence principle asserts that quantum and classical descriptions agree in the limit of large quantum numbers, that, is, in phenomena where the quantum of action is negligible. But the Bohm experiment is not such a case, certainly not if we regard one of the particles as a crucial part of the instrumentation for measuring the spin of the other particle, indeed, the success of this and many other measurements depends upon the occurrence of subtle quantum effects. Second, what the correspondence principle says about the relationship between classical and quantum descriptions is that they give *approximately* the same predictions in the limit of large quantum numbers. But approximate agreement is hardly essential equivalence. The appropriate mixtures model gives a quite different answer. A quantum mechanical description, in terms of a pure case, and a "classical" description, in terms of an appropriate mixture, give *exactly* the same predictions for those observables measurable in the context that determines the appropriate mixture. Moreover, this equivalence is a consequence of the quantum mechanical description itself. Think back now to what Bohr said about the relation between quantum and classical descriptions. He did not say that they are equivalent in all respects. Instead, he said:

> Since, however, all those properties of such agencies which, according to the aim of the measurement, have to be compared with corresponding properties of the object, must be described on classical lines, their quantum mechanical description will *for this purpose* be essentially equivalent with a classical description. (Bohr 1939, pp. 23-24)

That is, the quantum and classical descriptions must be equivalent for those instrument parameters crucially involved in the measurement. But that is exactly what the appropriate mixtures model implies.

The appropriate mixtures interpretation of the doctrine of classical concepts thus answers all of the questions posed at the beginning of this section, and it is consistent with Bohr's remarks on observation and classical concepts. But there is no evidence of Bohr's ever having considered explicitly such a model. This is not surprising, given his notorious aversion to the employment of formal methods in the solution of what are, properly, philosophical problems, but it does limit the claims that can be made on behalf of this model as a divining of Bohr's intentions. This is, again, why I speak only of a reconstruction of Bohr's views: The appropriate mixtures model is true to Bohr's words, but goes beyond those words where necessary in order to clarify the direction in which they were tending.

Let me conclude with two final considerations regarding the doctrine of classical concepts and its reconstruction by means of appropriate mixtures. First, there is an as yet unnoted consequence of this reconstruction that is, at the very least, surprising and that might even be taken as a reason for rejecting it. Consider again the mixture appropriate to z-spin measurements, W^{z}_{cl}, and notice that the basis states $u^{z}_{+}(L)$ and $u^{z}_{+}(R)$ occur within it in exactly the same way. The point is that such a mixture treats both instrument and object identically, so it is as much a "classical" description of the object as of the instrument. I can find nothing in Bohr's published writings that contradicts this implication of the appropriate mixtures reconstruction. But neither can I find any confirmation for it. Still, I find it consistent with one feature of Bohr's larger philosophy of physics, that being his stress on the fact that the new features of the quantum mechanical account of measurements affect the instrument and the object equally. For example, in the famous "Como" lecture, where he first introduced the complementarity interpretation, Bohr writes:

> Now 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. (Bohr 1927, p. 54)

But if the quantum description affects both instrument and object equally, then the alternative classical description should do the same. In a classical description, we ought to be able to ascribe an independent reality to both object and instrument, as we do, implicitly, when we describe their state as a mixture.

Finally, now, we must pose a question that has been in the background all along, but could not be posed in a clean way until we had developed a reconstruction of the doctrine of classical concepts: Is the doctrine of classical concepts correct? My opinion is that it is not. I believe that Bohr concedes too much to the world view of classical physics, the world view of Planck and Einstein, when he says that unambiguous communicability and, hence, objectivity, require our distinguishing the instrument from the object after the manner of classical physics. Even when Bohr qualifies his concession by pointing out that such a distinction can be effected not wholesale, but only context by context, he still concedes too much. He was worried, I think, that quantum mechanics, by itself, affords no objective criterion for individuating physical systems. In this, however, he was wrong. The recent work inspired by Bell's theorem has taught us that, for practical purposes, at least, a perfectly objective criterion is available precisely in the absence of the peculiar non-classical quantum correlations whose existence underlies the quantum mechanical violations of the Bell inequality.[14] However, the fuller consideration of this issue would carry us beyond the scope of the present paper, whose aim is not to criticize Bohr, but merely to understand him.

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NOTES

[1] Bohr 1923, p. 117, as quoted in Pais 1991, p. 196, from the English translation, Bohr 1924, p. 1.

[2] As quoted in Moore 1989, p. 313.

[3] For a brief discussion of this attitude of despair, see Howard 1987.

[4] One serious shortcoming of the following analysis, which I hope to correct in the future, is that no effort is made here to place Bohr's views on the role of classical concepts and complementarity, more generally, in their proper historical context, especially as regards the relevant philosophical context. Much nonsense has been written about alleged philosophical influences on Bohr by thinkers like Søren Kierkegaard and William James. Happily, however, some progress is finally being made toward a more adequate historical understanding of the philosophical context in which Bohr worked. I would recommend, in particular, Chevalley 1991, 1992a, 1992b, and Faye 1991.

[5] It is preferable to speak only of *instrument* and *object*, rather than of observer and observed, in order to avoid confusion about the role of a human observer's subjective consciousness. Much of the literature on Bohr goes astray in assuming that observation is a relation between a physical object and a conscious subject, an assumption fostered by Bohr's occasional talk of the "subject-object" relationship, especially where he is seeking psychological analogies to complementarity. But when it comes to observation in physics, Bohr is explicit in insisting, time and time again, that the crucial questions concern the relation between measuring instruments and observed objects, a relation located entirely within the physical realm, and that all talk of "subjects" should be avoided. He says, furthermore: "Since, in philosophical literature, reference is sometimes made to different levels of objectivity or subjectivity or even reality, it may be stressed that the notion of an ultimate subject as well as conceptions like realism and idealism find no place in objective description as we cave defined it" (Bohr 1955, p. 79). Surely, one can always include the human observer, as another physical system, in the instrumentation, but the important point is that, in Bohr's view, human *consciousness* plays no role in elucidating the observational situation in quantum mechanics.

[6] For more on the historical background to debates about separability and the independence of

physical systems in the context of the developing quantum theory, see Howard 1990.

[7] Here I have focused almost exclusively on the physical reasons for Bohr's linking of separability, objectivity, and unambiguous communicability. There is also an interesting historical and philosophical context for this linkage. One important part of that context is the neo-Kantian tradition of *Erkenntnistheorie* in which Bohr and most of his contemporaries learned their philosophy of science. Catherine Chevalley has made a good start at exploring this tradition as it relates to Bohr; see Chevalley 1991, 1992a, 1992b; see also Faye 1991. Another part of the context is the turn-of-the-century debate about what was then termed “Das Gesetz der Eindeutigkeit” (the law of “univocity” or “non-ambiguity”); for more on this debate, see Howard 1992.

[8] Does this mean that Bohr's talk of the size of our instruments in ordinary experiments and of the central place of irreversibility in observation is just a mistake? No. The mistake is ours in supposing that he intended size and irreversibility as necessary criteria for classifying a system as an instrument. My hypothesis is that Bohr meant these criteria to be employed instead in characterizing the closure property necessary for the definition of a quantum mechanical phenomenon. Bohr says, in one essay: “The circumstance that such marks are due to irreversible amplification effects endows the phenomena with a peculiarly closed character pointing directly to the irreversibility in principle of the very notion of observation” (Bohr 1958c, p. 98). For a fuller discussion of the concept of a “phenomenon,” which plays a central role in Bohr's philosophy of physics, see Bohr 1949, pp. 237-238, and Howard 1979, pp. 178-204. That Bohr did not see irreversibility as playing a crucial role in the solution of the measurement problem is evident from his remarks in a letter to Pauli of 16 May 1947, where Bohr writes: “Here, I have in mind such considerations about the complementary relationships between thermodynamical and mechanical concepts as I tried to indicate in my old Faraday lecture. Just as such considerations offer a consistent attitude to the well-known paradoxes of irreversibility in thermal phenomena, so it appears to me that, notwithstanding the obvious quantitative relationship between such phenomena and the irreversibility of observations, we may more adequately regard thermodynamical considerations and the essence of the observational

problem as different complementary aspects of the description" (Bohr 1985, p. 454).

[9] In at least one place, however, Bohr does seem to suggest that the physics of Newton and Maxwell is what he has in mind. A note of 11 February 1930 includes these words: "By classical physical theories we mean the usual mechanics and electrodynamics which have shown in a wonderful way how to explain ordinary phenomena; these theories are tied very closely to our ordinary attitudes to nature" ("Kvanteteorien og de klassiske fysiske Teorier," Niels Bohr Scientific Manuscripts, Niels Bohr Archive, Reel 12, p. 1; as quoted in Honner 1987, p. 62). But even this remark is not inconsistent with the interpretation of the notion of "classical concepts," developed below.

[10] As a model of a measurement, the Bohm experiment enjoys at least two advantages over the two-slit experiment. For one thing, only discrete spin observables are involved, in contrast to the continuous position and momentum observables; this avoids inessential mathematical complications. But more importantly, the fact that, at the time of the spin measurements, the decay products may even be separated by a space-like interval, and the fact that no physical interaction takes place between *L* and *R* after the decay itself, together imply that any novelties of quantum mechanical observation revealed by consideration of the model cannot be the result of any "disturbance" of the object by the instrument (of *L* by *R*), contrary to the suggestions of many commentators, starting with Heisenberg (see Heisenberg 1930, pp. 20ff.). For Bohr's criticism of the disturbance analysis, see, for example, Bohr 1958b, p. 5. There is, of course, the possibility of a non-local, or superluminal disturbance in such an experimental arrangement; but such disturbances must be excluded if we are to preserve consistency with special relativity.

[11] For a fuller account of such an analysis of the Bohm experiment, see, for example, d'Espagnat 1976, pp. 76-91.

[12] For a more detailed statement and proof of this claim, see Howard 1979, pp. 382-386.

[13] "Entanglement" is Arthur Fine's translation of Schrödinger's wonderfully apt expression for non-decomposable joint states, "Verschränkung"; see Fine 1986, p. 67.

[14] But such a criterion of individuation is not without its problems; see Howard 1989, pp. 248-249.

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