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Statement

and

Readings

Bohr's Reply to EPR—A New Look

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Abstract

This talk provide a fresh analysis of Bohr's reply to EPR, placing it in the context of the varied reactions to the EPR paper that emerge from a careful reading of sources such as Bohr's 1935 correspondence with Schrödinger and with Heisenberg, and the latter's draft reply to EPR. In particular, the analysis shines light on the question of Bohr's supposed positivistic turn and challenges other aspects of the received view of Bohr's thinking in this time period.

MARA BELLER AND ARTHUR FINE

BOHR'S RESPONSE TO EPR*

> While imagining that I understand the position of Einstein, as regards the EPR correlations, I have little understanding of the position of his principal opponent, Bohr. Yet most contemporary theorists have the impression that Bohr got the better of Einstein in the argument and are under the impression that they themselves share Bohr's view.
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> Bell, 1987a, 155

The EPR paper (Einstein, Podolsky and Rosen, 1935; hereafter "EPR") appeared in the May 15, 1935 issue of *Physical Review*. The paper's impact was due in large part to their demonstration of an incompatibility between quantum mechanics (if regarded as both correct and complete) and plausible physical principles regarding physical reality. Two other items appeared in *Physical Review* before Bohr's own response: a note by Edwin C. Kemble (Kemble, 1935), and a letter by Arthur E. Ruark (Ruark, 1935). Both authors attempted, in a different way, to rescue quantum mechanics from the EPR conclusion by questioning the concept of reality that underlay the EPR argument. Similarly, Schrödinger wrote to Pauli: "For me this note [the EPR paper] was the cause to rethink once again the issue (which we know essentially for a long time already) . . . that the expressions 'to have a value really', 'to be actually constituted so and so' and similar [expressions] are senseless phrases" (von Meyenn, *et al.*, eds., 1985, Vol. 2, 406).[1]

While the EPR argument and the early responses of Kemble and Ruark generated some excitement (and confusion), Pauli remained unimpressed. Both Einstein's example and Bohr's response to it contain nothing new, wrote Pauli to Schrödinger.[2] Bohr himself was anything but calm. He first indicated his response (Bohr, 1935a) in a brief letter to the editor of *Nature* on June 29, 1935 and the response was spelled out in an article in the October 15, 1935 issue of *Physical Review* (Bohr, 1935b). Rosenfeld, who was working with Bohr at the time, recalls that Bohr reacted very strongly to the EPR paper ("This onslaught came down on us as a bolt from the blue. Its effect on Bohr was remarkable."). Together with Rosenfeld, Bohr worked obsessively ("day after day, week after week") to fashion a reply (Rosenfeld, 1967, 128–29).

J. Faye and H. J. Folse (eds.), Niels Bohr and Contemporary Philosophy, 1–31.

Just what was it about EPR that generated such a strong reaction from Bohr? What did Bohr think EPR put in jeopardy and does the answer to that shed light on the elements of Bohr's own understanding of the quantum theory? Can Bohr's paper be considered an adequate reply to EPR? Can we say, as Bell reminds us is often asserted, that Bohr clearly "won" this round of the battle? In pursuing these questions we will pay particular attention to Bohr's rhetorical strategies and also try to assess the relevance of Bohr's reply to the contemporary issues of locality and separability. Our strategy is to give a close reading of Bohr's response to EPR, setting it in historical and conceptual perspective.

1. EPR AND BOHR'S EPR

In four densely written pages EPR formulates a rather complex argument for the conclusion that quantum mechanics is incomplete.[3] The heart of the argument involves the example of a pair of physical systems that interact and then move apart. In certain cases EPR show that, momentarily at least, although the systems are spatially separated, quantum mechanics allows there to be two conjugate physical quantities (*i.e.*, quantities whose operators do not commute) which are such that if a measured value is obtained for either quantity on one system, then one can predict with certainty what value would be obtained if the same quantity were measured on the other, distant system. They illustrate this possibility with an example (redesigned by Bohr in his response) where the quantities are a position coordinate and linear momentum (in the same direction). Measuring position on one system would enable one to predict with certainty the result of a position measurement on the other system. The linear momenta between the two systems would be similarly linked.

In his response, Bohr begins with a summary of the EPR argument for incompleteness, and then proceeds to question it. It will be useful to have Bohr's summary in full.

> The extent to which an unambiguous meaning can be attached to such an expression as "physical reality" cannot of course be deduced from *a priori* philosophical conceptions, but – as the authors of the article cited themselves emphasize – must be founded on a direct appeal to experiments and measurements. For this purpose they propose a "criterion of reality" formulated as follows: "If, without in any way disturbing a system, we can predict with certainty the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity". By means of an interesting example, to which we shall return below, they next proceed to show that in quantum mechanics just as in classical mechanics, it is possible under suitable conditions to predict the value of any given variable pertaining to the description of a mechanical system from measurements performed entirely on other systems which previously have been in interaction with the system under investigation. According to their criterion the authors therefore want to ascribe an element of physical reality to each of the quantities represented by such variables. Since, moreover, it is a well-known feature of the present formalism of quantum mechanics that it is never possible, in the description of the state of a mechanical system, to attach definite values to both of two canonically conjugate variables, they consequently deem this formalism to be incomplete, and express the belief that a more satisfactory theory can be developed. (Bohr, 1935b, par. 2)[4]

Briefly, then, Bohr sees EPR as using their criterion of reality[5] to infer, in the example at hand, that each of two canonically conjugate variables has a definite value. Since no quantum state description allows this, the quantum theory is descriptively incomplete. We can expand this brief account along the lines that Bohr proposes in his response (Bohr, 1935b, par. 10). Consider the case of interacting particles 1 and 2 which move apart in such a way as to link a position coordinate variable, say Q_1 in one particle with the position variable Q_2 in the other particle, and similarly for variables over the same coordinates of linear momentum, P_1 and P_2. Suppose our measurements will be made on particle 1. According to the assumed linkage, it is possible to predict the value of Q_2 on particle 2 following a measurement of Q_1 on particle 1. Similarly, if we measure P_1 on particle 1, we could predict the value of P_2 on particle 2. These two particles are supposed to be spatially separated at the time of the proposed measurement, so presumably the measurement on particle 1 would not disturb particle 2 (we will see that this is not the case in the specific construction that Bohr proposes). Bohr does not actually refer to this no-disturbance condition in his summary. Later he allows that in the situation employed by EPR the distant measurement process "does not directly interfere with the particle concerned;" *i.e.*, with particle 2 in our case (*Ibid.*, par. 10). Referring to the same circumstances, he also says that there is "no question of a mechanical disturbance" of the unmeasured particle (*Ibid.*, par. 12). So it appears that we can apply the EPR criterion of reality and "ascribe an element of physical reality to each of the quantities represented by such variables". Thus according to Bohr's summary, in the posited circumstances the criterion of reality implies that both Q_2 and P_2 have definite, simultaneous values. But according to quantum mechanics it is "never possible, in the description of the state of a mechanical system, to attach definite values to both of two canonically conjugate variables", variables such as Q_2 and P_2. Hence, in such a case, the quantum description would be incomplete.

2. INCOMPLETENESS AND INCONSISTENCY

While the EPR paper presents an argument for the incompleteness of quantum mechanics, Bohr's reply focuses on the theory's "soundness", "rationality", "lack of contradiction", and "consistency". Thus, Bohr writes:

> Such an argumentation [EPR], however, would hardly seem suited to affect the soundness of the quantum-mechanics description, which is based on a coherent mathematical formalism covering automatically any procedure of measurement like that indicated. The apparent contradiction in fact discloses only an essential inadequacy of the customary viewpoint of natural philosophy for a rational account of physical phenomena of the type with which we are concerned in quantum mechanics. (Bohr, 1935b, par. 3)

Where does this discrepancy between the argument for incompleteness in EPR and defense of consistency in Bohr's response come from?

One can read the EPR argument in different ways, depending on whether one considers the uncertainty relation only as a limitation on exact simultaneous measurability of conjugate variables, or as a prohibition on the "real" existence of simultaneous sharp values for such variables. (From an verificationist perspective these two versions merge into one, and the early writings of the architects of the quantum revolution often fail to distinguish them.) In the first case, where it is meaningful to talk about possible simultaneous sharp values, and the prohibition is only on their simultaneous measurability, the EPR argument can be seen as an argument for the incompleteness of quantum mechanics. The authors of EPR demonstrate the existence of a certain state of affairs (simultaneous existence of sharp values of P and Q for the second particle) that the quantum formalism is not capable of describing. If quantum mechanics were thus incomplete two options would be open. One could suggest that the theory may be completed from within ("hidden variables"), without changing its basic presuppositions and statistical predictions (a view that is often wrongly attributed to Einstein). Or, one could suggest that quantum mechanics is not the ultimate theory of the microworld, and that eventually it will be superseded by a substantially different theory that contains quantum mechanics as a limiting case – the view that Einstein actually held (see Fine, 1986 and 1993). Note that the closing sentences of EPR are compatible with both options.

> While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. We believe, however, that such a theory is possible. (EPR, 1935, 780)

There is another way to read the EPR argument, however, if – as Heisenberg and Bohr did – one considers the uncertainty relation as a prohibition not merely on simultaneous measurability, but on the simultaneous existence of sharp values for conjugate variables. In this case, the EPR assignment of

simultaneous sharp values for both P and Q would simply be inconsistent with the uncertainty relations.

Initially, Heisenberg's uncertainty paper was a response to Jordan's claim of incompleteness – there are situations, such as a continuous path of a particle, that quantum mechanics is not capable of describing (Beller, 1985). Heisenberg responded to this claim by deducing the uncertainty relations, redefining the path of a particle in statistical terms and defending the "completeness" of the quantum formalism.

Yet from the beginning Heisenberg also perceived the uncertainty relations as a test for the consistency of quantum theory. If one could transcend in a thought experiment the limits of the uncertainty relations, quantum theory would be wrong, or inconsistent. Bohr accepted this meaning of the uncertainty relations, as well as Heisenberg's analogy between the role of the uncertainty relations in quantum theory and the limit on the velocity of light in relativity: ". . . Heisenberg has rightly compared the significance of this law of reciprocal uncertainty for estimating the self-consistency of quantum mechanics with the significance of the impossibility of transmitting signals with a velocity greater than that of light for testing the self-consistency of the theory of relativity" (Bohr, 1929, 95). It was natural therefore for Bohr to see the demonstration of violations of the uncertainty relations in EPR as an accusation of inconsistency, a claim that is not actually made in the EPR paper itself.

Another point is in place here. At least in one of its different meanings, Bohr's complementarity between space-time and causality is a direct translation of the uncertainty relations into the terminology of space-time and energy-momentum (Beller, 1992). In quantum, as opposed to classical mechanics, the space-time and energy-momentum (causality) descriptions are not simultaneously applicable. Quantum mechanics thus might seem to be "incomplete" as compared with classical theory. While the former allows only a partial description at a time, the latter allows one to combine these partial descriptions simultaneously into a "complete" picture. Bohr argues that such an "incompleteness" of quantum mechanics is only apparent.

> [T]he action of the measuring instruments on the object under investigation cannot be disregarded and will entail a mutual exclusion of the various kinds of information required for a complete mechanical description of the usual type. This apparent incompleteness of the mechanical analysis of atomic phenomena issues ultimately from the ignorance of the reaction of the object on the measuring instruments inherent in any measurement. (*APHK*, 7)[6]

It appears then that consistency and completeness are as organically interwoven in Bohr's mind as uncertainty and complementarity. This is why, we suggest, Bohr's response to EPR moves freely between the two just as if they were the same.

3. SIMULTANEOUS POSITION AND MOMENTUM IN EPR

The critical step in Bohr's version of the EPR argument occurs when one applies the criterion of reality to infer definite values for both position and momentum on the second particle. It is clear that something is missing here in Bohr's account. Assuming the no-disturbance condition to be satisfied, if we measure Q_1 then the criterion of reality allows us to assign a definite value to Q_2; likewise, if we measure P_1 then the criterion of reality allows us to assign a definite value to P_2. Yet the EPR criterion of reality does not prescribe definite values for both at the same time unless one measures Q_1 and P_1 simultaneously. The simplified version of the EPR argument that Bohr constructs thus contains an obvious gap. Since Bohr's version is not faithful to EPR, it is instructive to compare it with how EPR proceed at this point in their argument. EPR look at the state function of the two-particle system and how it reduces on measurement to state functions for the component systems. Applying the standard procedure of "reduction of the wave packet", they conclude that "as a consequence of two different measurements performed on the first system, the second system may be left in states with two different wave functions" (EPR, 1935, 779). EPR now invoke a subsidiary assumption, not the criterion of reality but only its no-disturbance antecedent: "no real change can take place in the second system in consequence of anything that may be done to the first system" (*Idem*). (In correspondence, Einstein would refer to this as a principle of separation. See Fine, 1986, 35ff. and 46ff., and Howard, 1985.) They conclude that whichever of the two measurements are performed, the "reality" of the distant and unmeasured system would be the same; hence that in the posited circumstances "*it is possible to assign two different wave functions . . . to the same reality*" (*Idem*). Note that this conclusion actually requires a further assumption, which is tacit in the exposition of EPR; namely, that independently of any performed measurements, there actually is some "reality" that pertains to the second, unmeasured system. For if there were no reality to the unmeasured system, then the no-disturbance condition would be satisfied vacuously but the conclusion about the assignment of different wave functions false. In later writings Einstein made this added assumption explicit and, in correspondence with Schrödinger, he found circumstances where it could be avoided (see Fine, 1986, Chap. 5). EPR show that in their example the two different state functions assigned to the reality of the unmeasured system may be eigenfunctions of non-commuting observables, like position and linear momentum. To continue with this version of their argument, then, we may consider the different state functions ψ_2 and ϕ_2 for particle 2 to be "eigenfunctions" (respectively) of operators for momentum and position with respective eigenvalues p_2 and q_2. EPR now argue that by measuring either P_1 or Q_1 on the first particle, one can predict with certainty and without disturbing the second particle, either the value of P_2 (*i.e.*, p_2) for the second particle, in the one case, or the value of Q_2 (*i.e.*, q_2) in the other case. Applying the criterion of reality in the first case, one concludes that the value of P_2 is an element of reality; in the second case that the value of Q_2 is an element of reality. Notice that we are now back in the situation from which we began. For we still have not shown that these two elements apply simultaneously. EPR are aware of this and to achieve that crucial further step they remark, "But, as we have seen, the wave functions [ψ_2 and ϕ_2] belong to the same reality" (EPR, 1935, 780). Thus in EPR the inference to simultaneous values does not follow from the application of the criterion of reality, as Bohr portrays it, but from the fact that particle 2 has a reality that is describable both by an eigenfunction of position and also by an eigenfunction of momentum.

EPR do not say exactly how to use these state descriptions to draw the conclusion about simultaneous values. Their argument ends with the cited remark, whose bearing on the issue may not be apparent. Here is one suggestion. At this stage of the overall argument EPR have made the assumption (for purposes of a *reductio*) that the quantum state function is a complete description, one that leaves out no element of reality. The use of the criterion of reality, it seems, is supposed to insure that values of position and momentum, if a system has those values, are elements of reality – as distinct perhaps from artifacts, constructions or some other sort of 'non-real' thing. They seem to be suggesting that if there were a value of position or momentum and that value were not part of the quantum state description, that description would be incomplete. (Their argument is vulnerable here, since the criterion of reality only guarantees this in special circumstances.) To see that both p_2 and q_2 actually apply together, however, we must look to the state functions, not to the criterion of reality. Early in the article EPR discuss the relevant rule. It is that when the state of a system is an eigenstate of a physical quantity, then that quantity takes a definite value in that state; namely, the corresponding eigenvalue. They show that the criterion of reality is consistent with this eigenstate/eigenvalue rule, whose applications they suggest constitute accepted "quantum mechanical ideas of reality" (EPR, 1935, 778).

Indeed, this is the only other reference to the criterion of reality in the entire article. Referring back to our example, then, where particle 2 is described by the eigenstates ψ_2 and ϕ_2 of P_2 and Q_2, respectively, one can now conclude that particle 2 has definite momentum p_2 and position q_2. The criterion of reality insures that these are elements of reality. It follows that the quantum state description is incomplete.

As we warned, this is a complex argument. Let us summarize its key features. (1) The criterion of reality is only used to certify that when a value is inferred on the unmeasured system, that value constitutes an element of reality (*i.e.*, that it must be included in a complete description). (2) The demonstration of simultaneous P and Q values depends on the state descriptions (not on the criterion of reality) in accord with the eigenstate/eigenvalue principle. (3) EPR make the tacit assumption that some "reality" pertains to the unmeasured component of the two-particle system. (4) EPR assume a principle of separation according to which, after the two particle are far enough apart, the measurement of particle 1 does not affect the reality that pertains to particle 2. (5) EPR employ the standard state vector reduction formalism (von Neumann's projection postulate).

What is striking about EPR, by contrast with Bohr's summary, is their emphasis on the state of a system and its characterization by means of wave functions. Indeed, the point of the EPR paper was to question the adequacy of that characterization. In Bohr's summary and discussion, however, the wave functions and the concept of the state of a system play a minor role. Bohr clearly had his own interpretive agenda. Of the five items listed above, the only one to which Bohr pays attention is the criterion of reality. Although he criticizes that principle, as we shall see, his criticism does not extend to the very limited use to which the principle is put by EPR. Despite some general remarks about physical reality, however, Bohr does not challenge the key assumptions concerning reality (*i.e.*, assumptions (3) and (4)) which are the heart of the EPR argument.

Not all readers of EPR ignored the role of these critical assumptions. In a paper submitted on November 12, 1935 and published in the March 1, 1936 issue of *Physical Review*, Wendell Furry focused exclusively on just this aspect of EPR. He argued that "the assumption that a system when free of mechanical interference [like particle 2 above] necessarily has independently real properties is contradicted by quantum mechanics" (Furry, 1936, 399). It turns out that Furry's formulation of the idea of "independent reality" begs the question, since he assumes that if (after interaction) each of the two EPR systems did have an independent "reality", it would be described by some particular quantum state function. Thus Furry assigns a certain proper mixture to the pre-measurement state of the composite system. An easy calculation (although perhaps it was not so transparent in Furry's day) shows that this assignment contradicts quantum theory. Nevertheless, Furry sees clearly that assumptions about the reality of the component systems in the EPR situation carry the burden of the EPR proof. Curiously, Furry attributes this observation to Bohr. With reference to Bohr's response to EPR, Furry writes: "Bohr has again clearly called attention to this circumstance [the role of the measuring instruments], and has remarked that one must be careful not to suppose that a system is an independent seat of 'real' attributes simply because it has ceased to interact dynamically with other systems" (*Ibid.*, 393). In line with Furry's attribution, the tradition has it that Bohr's critique of EPR centers on their assumptions about the reality of the component systems and that Bohr sees the EPR situation in more holistic terms.

In EPR the inference to simultaneous applicability of both P and Q is subtle and indirect. The authors acknowledge that "one would not arrive at our conclusion if one insisted that two or more physical quantities can be regarded as simultaneous elements of reality only when they can be simultaneously measured or predicted." The authors leave no doubt as to their opinion on such a restrictive definition of reality. "This makes the reality of P and Q depend upon the process of measurement carried out on the first system, which does not disturb the second system in any way. No reasonable definition of reality could be expected to permit this" (EPR, 1935, 780).

These sentences point to a simple, positivist way to get around EPR, a way the authors move to block by their rhetoric of unreasonableness. That rhetorical move suggests that an answer by Bohr along strictly positivist lines would merely point to a difference in metaphysical presuppositions. Bohr wanted more. He wanted to prevail in the confrontation, to "convince" Einstein rather than politely to disagree (*AHQP*, letter from Bohr to Heisenberg, 2 July 1935). Bohr's answer, therefore, appears to meet Einstein on his own ground, for he seems to be accepting EPR's criterion of reality and only finding "ambiguity" in the notion of non-disturbance. In the next section we will show that this move in Bohr's reply is unsuccessful, and that a positivistic shift is the only salvageable version of Bohr's reply.

Not everybody was as reluctant as Bohr to take such a straightforward positivistic stand. Not intimidated by the "unreasonableness" of such a position, Ruark countered that "this conclusion [EPR's] can be attacked by anyone who prefers to say that P and Q could possess reality only if [they both] could be simultaneously measured" (Ruark, 1935, 466). The EPR conclusion is invalid and, claims Ruark, is "directly opposed to the view held by many theoreticians, that a physical property of a given system has reality only when it is actually measured". Ruark's reply to EPR shows both the advantages and the shortcomings of such a positivistic stand. Ruark's paper is lucid, short, and focused. Yet the conclusion Ruark reaches is the very one Bohr is eager to avoid. "It seems . . . that in the present state of our knowledge the question cannot be decided by reasoning based on accepted physical principles. The arguments which can be advanced on either side seem to be far from conclusive, and the issue involved appears to be a matter of personal choice or of definition" (Ruark, 1935, 467).

4. BOHR'S CONCEPT OF DISTURBANCE: EPR AND BEFORE

As we mentioned, Bohr seems to have no quarrel with the EPR criterion of reality, choosing to criticize the "ambiguity" connected with inferring a value without disturbing the system in question. After all, Bohr had long argued that the epistemological situation in the quantum theory derived in large part from measurement disturbance, an uncontrollable interaction between the measured object and the measuring instrument. In his writings prior to EPR, Bohr often wrote as though this measurement disturbance were symmetric: the instrument disturbs the object, and also vice versa. Characteristic phrases like "mutual interaction", "exchange of energy between atom and instrument," and "unavoidable influence on the phenomena" convey this symmetry (or ambiguity). Sometimes, Bohr would invoke the image of a massive instrument disturbing a tiny object, thus feeding intuitions about how different the observational situation was in the realm of atomic physics from that, for instance, in astronomical observations of the moon. "If we will observe anything about the atom, we must create an interaction with it, which has a material influence on the state of the atom . . . The point is that these observations claim an interaction, which cannot be smaller than the quantum of action and therefore will change the state of the atom and will change it in a way which is completely out of our control" (*AHQP: MSS* 12: "Philosophical Aspects of Atomic Theory", 1931). Phrases like "the action of the measuring instrument on the object under investigation", "the reaction of the object to the measuring instrument", and so on, convey this disturbance of the object and regularly occur in Bohr's writings prior to EPR (a fuller discussion is in Beller, forthcoming). On the other hand, in Bohr's many expositions of the double slit experiment, he would often emphasize that a particle reflected in passing through a diaphragm also deflects the diaphragm. Here, although there is certainly a mutual influence, the focus is on the reaction of the instrument. The clear challenge of the EPR example was that it assigned values at a distance, thereby undercutting the idea of a robust physical disturbance of the object under investigation. Surely Bohr felt that if his construction of the quantum problematic were viable, there must be some sort of proper disturbance in the EPR situation as well. Indeed, highlighting the disturbance there is the heart of his response to EPR.

A large part of Bohr's response is devoted to a repetition of his "simple, and in substance well-known considerations" of complementarity between space-time and causality. Bohr's way to argue for such complementarity was to analyze measurement procedures and to demonstrate that the uncertainty relation for position and momentum cannot be transcended in a variety of relevant thought-experiments. By the repetition of these considerations, Bohr attempted to argue for the consistency, or "rationality", of the quantum-mechanical description, by demonstrating that "such rational discrimination between essentially different experimental arrangements and procedures . . . are suited either for an unambiguous use of the idea of space location, or for a legitimate application of the conservation theorem of momentum" (Bohr, 1935b, par. 9). The key concept in these thought-experiments is an "essential" uncontrollability of the measurement interaction, which Bohr demonstrates using a few mechanical set-ups. The first set-up is one for a position measurement. Here we must use a diaphragm, rigidly bolted to a support in order to define the frame of reference. In such a set-up we "voluntarily cut ourselves off from" (*Ibid.*, par. 5) the possibility of following the momentum exchange between the particle and the diaphragm (it will get "buried" in the common support). Our lack of knowledge of the particle's momentum in such a set-up is indeed consistent with the uncertainty relations.

Notice that there is nothing particularly "quantum" in this example. It only prepares our intuitions for realizing the impossibility of a causal space-time description, which, for Bohr, results from – and is "in harmony with" – "the impossibility of a closer analysis of the relations between the particle and the measuring instrument" (*Ibid.*, par. 5). While the position measurement only suggests the peculiarities of the quantum description, the momentum measurement relies on this description directly by employing the uncertainty relations for the movable diaphragm. We measure a particle's momentum by measuring the momentum of a movable diaphragm (by using an appropriate test body) before and after the passage of a particle through a slit of this diaphragm. By applying the law of conservation of momentum to a system consisting of a particle and a moving diaphragm, we can calculate the momentum of a particle passing through the slit. However, because we have exact knowledge of the momentum of the diaphragm during, or immediately after, the particle's passage, we block the possibility of also knowing the position of the diaphragm, in accord with the uncertainty relations. Consequently, we are denied the knowledge of the particle's position which is identical with that of the diaphragm's location.

As Bohr emphasizes, in such a set-up we are free to choose whether we want to know the position or the momentum of a particle "immediately" after it passed through a diaphragm. We can either "catch" and "freeze" the diaphragm in space, thus determining the position of a particle (but giving up the possibility of calculating its momentum because we cannot determine the diaphragm's momentum by its impact on a test body), or we can measure the diaphragm's momentum, and thus calculate the particle's momentum, denying ourselves knowledge of both the diaphragm's and particle's locations.

These examples are intended to persuade the reader that in quantum mechanics one is dealing not with some "arbitrary picking up of elements of reality", but with a "rational discrimination" between different – in fact mutually exclusive – measuring arrangements. Several features of Bohr's analysis stand out.

1. Bohr's analysis implies a limitation on the accuracy with which the conjugate quantities (position and momentum) can actually be co-measured. Unless one accepts an extreme positivist attitude, identifying measurability and meaning (which Bohr had been reluctant to do just a few years earlier – see below), it is not clear what Bohr's analysis has to do with the "unambiguous definition" of physical attributes.

2. While Bohr asserts that "the impossibility of a closer analysis" of the measurement interaction in the quantum domain applies for any conceivable measurement (*Ibid.*, par. 5), the two specific examples he provides hardly lead to this conclusion. Bohr's explanation of the "uncontrollability" in a position measurement and in a momentum measurement are of a basically different nature. One employs some classical intuitions, the other uses the uncertainty relations. It is by no means clear how one can generalize from these examples, as would be the case, say, if one could generalize on some common features or presuppositions in both cases. Thus, while Bohr asserts that this "uncontrollability" is "no peculiarity of the experimental procedure described, but is rather an essential property of any arrangement suited to the study of the phenomena of the type concerned" (*Ibid.*, par. 5), he provides no argument for this general and far-reaching claim.

3. Actual physical disturbances still seem to underlie Bohr's argument for the uncontrollability of the measurement interaction. In the case of a momentum measurement, the diaphragm suffers an "uncontrollable displacement" during each collision process with the test bodies (*Ibid.*, par. 7), so we lose the knowledge of the diaphragm's position and of the position of a particle that passes through its slit. Although Bohr attempts to imply that consequently the particle's position cannot be "defined", the supposition of an "uncontrollable displacement" actually implies that both the particle and the diaphragm do possess definite positions before and after the collision, even if we may not be able to know them.

The crucial step in Bohr's response to EPR is contained in Bohr's reconstruction, or "physical actualization" (through a specific measurement arrangement) of EPR's mathematical reasoning. Bohr claims that the measurement arrangement that he proposes does not "actually involve any greater intricacies than the simple examples discussed above" (*Ibid.*, par. 10) and that it faithfully represents the example suggested by the authors of the EPR paper. Let us see whether this is indeed the case.

In proposing a physical realization for EPR Bohr goes back to the double slit interference experiment, which had long been his touchstone for treating the conceptual situation in the quantum theory. Thus the measurement arrangement Bohr proposes is that of a diaphragm with two parallel slits through which two particles pass simultaneously. At the time of passage we know the difference of the particles' position $X_1 - X_2$ which is equal to the distance between the slits, we can also know the sum $P_1 + P_2$ of the corresponding components of the two particles' momenta, by measuring the momentum of the diaphragm before as well as after the passing of the particles. Because $X_1 - X_2$ commutes with $P_1 + P_2$, we can, by measuring X_1, calculate X_2 from the knowledge of $X_1 - X_2$. We can also choose to measure P_1 and then calculate P_2. Why can we not do both?

Bohr suggests two different answers. The first answer runs as follows. After the two particles pass the first (the two slit) diaphragm, we employ somewhere later a diaphragm (which can be spatially separated from the earlier one) to measure, let's say, the momentum of the first particle P_1, and deduce from it the momentum P_2 of the second particle. For measuring P_1, we must use an arrangement (movable diaphragm) which excludes in principle the possibility of measuring X_1, and thus excludes the possibility of predicting X_2. We can say therefore that by measuring P_1 on the first particle, we exclude the conditions for predicting the position X_2 of the second particle, so the measurement of the first particle implies "an influence on the very conditions which define the possible types of predictions regarding the future behaviour of the system" (*Ibid.*, par. 11). If, in a positivistic vein, we just equate measurability (or predictability) with definability, then we obtain Bohr's stricture on the simultaneous applicability of both momentum and position for the unmeasured particle. This is the answer that Bohr gives later when he recounts his response to EPR (Bohr, 1949).

While this reading is consistent with Bohr's 1935 reply (and with his increasingly positivistic attitude, especially after EPR) a still closer reading of Bohr's text is possible, a reading that makes good sense of more of the details of his discussion of the actual physical arrangement.[7]

For the momentum measurement of the first particle, we use a movable second diaphragm – as discussed. We obtain the value of $P_1 + P_2$ from the two-slit diaphragm; *i.e.*, the one through which both particles passed. We can then predict the momentum of the second particle. Let us emphasize two important points. The measurement of the momentum of the two-slit diaphragm, necessary for our calculation of $P_1 + P_2$, implies that this diaphragm must be movable (suspended by weak springs or the like). Because total momentum is conserved, the diaphragms can be well separated, with the second diaphragm very far from the first.

Consider now the position measurement. In Bohr's set-up, $X_1 - X_2$ has a definite value at the time (and only at that time) when the particles pass through the two slits of the first diaphragm. After this time the value becomes indefinite, according to the Schrödinger equation. In parity with the treatment of the momentum measurement, the correct simulation of the EPR situation for a position measurement should involve a second diaphragm that is rigidly fixed to a space-frame of reference and may be well separated from the first one. The measurement using the second diaphragm should give us, say, X_1. The problem, however, is that one cannot now calculate X_2, because by now $X_1 - X_2$ is no longer definite. Thus we have no choice but to measure X_1 at the very moment of passage of the two particles through the first diaphragm. That means either that the second diaphragm must be infinitely close to the first one, or that they actually merge into one single diaphragm. That this is what Bohr has in mind is confirmed by the sentence "Under the experimental conditions described such a [position] measurement will therefore also provide us with the knowledge of the location, otherwise completely unknown, of the [two slit] diaphragm with respect to this space frame when the particles passed through the slits" (Bohr, 1935b, par. 11). Since we know now the position of the first diaphragm, the knowledge of its momentum is precluded (either due to the uncertainty relations or to the fact that both diaphragms

merge into one, and this common diaphragm must be rigidly fixed now for the X_1 measurement). So we cannot know, in the case of a position measurement of the first particle, $P_1 + P_2$. In Bohr's words: "... we have by this procedure cut ourselves off from any future possibility of applying the law of conservation of momentum to the system consisting of the diaphragm and the two particles" (*Ibid.*, par. 11).

Thus in Bohr's set-up we have a choice of measuring either position or momentum on one particle only at the moment when both particles pass through the two-slit diaphragm. This leads to an arrangement, however, that is contrary to the state function for the composite system posited in the EPR paper (equation 9, p. 779). EPR consider a composite system in a state where, at least for a moment,[8] both the relative position $X_1 - X_2$ and the total momentum $P_1 + P_2$ are co-measurable. Moreover, in EPR both of these quantities are simultaneously determinable with either the position or the momentum (not both) of particle 1. Bohr's double slit arrangement does not satisfy this requirement. In Bohr's example only one of $X_1 - X_2$ or $P_1 + P_2$ could be co-determined together with the variable one chooses to measure on particle 1. Indeed, we actually have to change the set-up of the two-slit diaphragm depending on whether we intend to measure position or momentum on particle 1. In the first case the two-slit diaphragm must be immovable; in the second case, it must be movable. Thus, by altering what we measure on particle 1, we change the mechanical set-up of the two-slit diaphragm, and hence the physical interaction between particle 2 and the (two-slit) diaphragm through which both particles passed!

The EPR condition that "no real change can take place in the second system in consequence of anything that may be done to the first system" is not satisfied in Bohr's realization, which contains an "indirect" disturbance due to the fact that measuring X_1 or P_1 requires different mechanical arrangements for the two-slit diaphragm with which the particles interact. While Bohr may be technically correct in saying that there is "no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure" (Bohr, 1935b, par. 12) the set-up that Bohr proposes does involve a mechanical disturbance just before this last stage, where we either bolt down the slitted diaphragm or suspend it freely. We see therefore that Bohr's physical realization of the EPR case is unsuccessful. It involves mechanical effects not present in EPR. In terms of his set-up, we can also understand why Bohr thinks that "the criterion of physical reality proposed by Einstein, Podolsky and Rosen contains an ambiguity as regards the meaning of the expression 'without in any way disturbing a system'" (*Ibid.*, par. 12). While Bohr has no quarrel with their criterion of reality, as such, we would suggest that he sees an "ambiguity" in applying the concept of disturbance to the case at hand precisely because of the disturbances that exist in his own flawed assimilation of EPR to a double slit experiment.

Bohr's reply to EPR constitutes a turning point in his thought, for with EPR his ability to unravel the quantum puzzles by relying on the idea of a robust physical disturbance runs into a dead-end. The only option that remains, and the one that Bohr embraces, is to fall back on a positivism of Ruark's type. Later, Bohr refers to all these thought-experiments as "semi-serious" (Bohr, 1949, 220). Yet there is hardly anything light-hearted or humorous in the tone of Bohr's original reply to EPR. His language is rather apocalyptic: "essential inadequacy" and "essential ambiguity", "final renunciation", "radical revision". This is the language of extremes rather than the rhetoric of balanced judgment. There is no room for dissent in Bohr's framework for thinking about the quantum world. There is also hardly anything "semi-serious" in Bohr's stern insistence on eliminating every "ambiguity".

5. AMBIGUITY AND DEFINITION

Doing things unambiguously is a central theme in Bohr's writings, and functions centrally in his response to EPR. As we have seen, he talks there of defining physical quantities unambiguously. He charges that there is an essential "ambiguity" in the EPR phrase "without in any way disturbing the system" (*Ibid.*, par. 12). In related passages, he speaks of "the extent to which an unambiguous meaning can be attributed to such an expression as 'physical reality'" (*Ibid.*, par. 2) and of there being "no question of any unambiguous interpretation of the symbols of quantum mechanics other than that embodied in the well-known rules which allow to predict the results to be obtained by a given experimental arrangement described in a totally classical way" (*Ibid.*, par. 15). Finally there is reference to "the mutually exclusive character of any unambiguous use in quantum theory of the concepts of position and momentum" (*Ibid.*, par. 14). One might suppose that Bohr's conception of complementarity represents his own personal tolerance of ambiguity, perhaps even his predilection for it. But even from this brief collection of citations in this one short piece, we can see that Bohr regards ambiguity as a defect. He looks instead for unambiguous forms of expression, which he promotes. Contrary to what one might suppose, it seems that Bohr himself had little tolerance for ambiguity.

Curious then his use of 'define', for that word has a wide spectrum of uses. Two of these figure prominently in Bohr's writings. One is where 'define' is used to indicate discrimination, in place of terms like 'specify' or 'determine' or 'fix'. We say, "I can define the costs better if you give me more information". "The weight of this last parcel will define the load limit". "One cannot define the means without considering the ends". And so on.

A different family of uses connects definition with semantic notions like meaning, interpretation, necessary and sufficient conditions for the use of an expression, and the like. Bohr's use of 'define' seems to trade on a possible ambiguity between these two different uses. He maintains, as above, that when we measure position the accuracy with which we can discriminate momentum is limited. He summarizes this as an inability to define momentum unambiguously. That way of putting it, however, rings with the different idea that we cannot clearly interpret or attribute an unambiguous meaning to 'momentum'. We believe that Bohr chose his language thoughtfully, and that this way of describing the outcome of his arguments about an uncontrollable measurement disturbance represents a deliberate rhetorical strategy. Bohr did not conflate measuring and meaning. Rather, by employing the ambiguous language of definition, he wanted to incline his readers to assimilate one to the other. Suppose we follow Bohr's direction, then, and treat the physical magnitudes of the quantum theory operationally, holding that the conditions that limit the measurement of a quantity (its 'definition' in one sense) restrict its meaning (its 'definition' in the other sense). In accord with this verificationist prescription, when we measure the position of a particle we set up conditions that give attributions of momentum no clear sense ("unambiguous meaning"). There is no clear sense either to the question as to whether, in these circumstances, the particle really has momentum.

Indeed on this reading, assuming what Bohr says is correct, we have "not merely to do with an ignorance of the value of certain physical quantities, but with the impossibility of defining these quantities in an unambiguous way" (*Ibid.*, par. 9). With regard to the physical reality of momentum, then, when position is being measured, the very question about momentum makes no clear sense. So "the extent to which an unambiguous meaning can be attributed to such an expression as 'physical reality' cannot be deduced from a priori philosophical questions, but . . . must be founded on a direct appeal to experiments and measurements" (*Ibid.*, par. 2). One might well be tempted to describe this as "a radical revision of our attitude as regards physical reality" (*Ibid.*, par. 16). In the same way, when we recognize that no clear meaning accrues to "momentum" here, we recognize a limitation on applying conservation of momentum. If, as Bohr usually does, we identify the applicability of the conservation law for momentum with causality, then we might agree with him on "the necessity of a final renunciation of the classical ideal of causality" (*Ibid.*, par. 3). Thus the positivist turn, the use of an operational and verificationist conception of linguistic meaning, is implicit in Bohr's ambiguous use of the concept of definition. Making the positivism explicit pulls together the various reservations and injunctions that characterize his point of view in the EPR response. From this positivist point of view, indeed there is "no question of any unambiguous interpretation of the symbols of quantum mechanics other than that embodied in the well-known rules which allow to predict the results to be obtained by a given experimental arrangement described in a totally classical way" (*Ibid.*, par. 15).

6. POSITIVISM AND ITS PUZZLES

In attributing to Bohr a verificationist doctrine about meaning, we are not suggesting that Bohr appropriated this doctrine from the writings of the neo-positivists. It was hardly necessary for him to do so since the doctrine had been very much in the air in German speaking scientific circles for some time. Heisenberg's uncertainty paper (1927) was built on operational foundations. Initially, Heisenberg tried to reconcile the novel quantum mechanical formalism with experiment by redefining all the classical-kinematical concepts operationally. Bohr of course was well familiar with Heisenberg's endeavors – in fact part of his heated debates with Heisenberg in 1927 concentrated exactly on this point! Bohr strongly objected then to Heisenberg's conflation of definition and observation. Physical concepts, Bohr argued, are independent of, and have a well-defined meaning prior to any procedure of measurement. Bohr originally held that the only way to connect the abstract quantum formalism with observable space-time phenomena was through the wave-theoretical imagery, the de Broglie-Schrödinger wave packet, which also set a limit to visualization in the quantum domain. Thus originally, there was no identity between definition and observation for Bohr (see Beller, 1992, for a fuller discussion). In fact there was a mutual exclusivity, or complementarity between the two (Bohr, 1928). As Bohr's complementarity principle gradually evolved – from Bohr's initial attempts to make some intuitive sense of the peculiarities and "irrationalities" of the quantum description, to an overarching principle for all knowledge – a verificationist doctrine about meaning also emerged in Bohr's writings. Although initially Bohr was ambivalent about this treatment of concepts and meaning, and came to endorse it slowly and with some reluctance, adopt it he did, eventually characterizing as "old fashioned" the idea that one could talk meaningfully about quantities without making presuppositions about their measurement. (Bohr, 1937c, Lecture 5) Indeed, just at the time of EPR, in a manuscript entitled 'Space and Time in Nuclear Physics', Bohr put it as plainly as can be, "When we speak of space and time, it only means certain words that we use in connection with measuring instruments and clockworks, and we have to pay a price for what they are – namely, that these measuring instruments enter an uncontrollable interaction" (*AHQP: MSS* 13, 21 March 1935).

Thus Bohr's discussions of complementarity moved away from an analysis via wave-theoretical imagery to a demonstration of the mutual exclusion of experimental arrangements for measuring the conjugate variables of position and momentum. The mutual exclusivity of two experimental arrangements, and the uncontrollability of the measurement interaction on which such mutual exclusivity ultimately rests, became the key concepts in Bohr's epistemological analysis. Hand in hand, positivistic pronouncements took center stage:

> In fact, it is only the mutual exclusion of any two experimental procedures, permitting the unambiguous definition of complementary physical quantities, which provides room for new physical laws, the coexistence of which might at first sight appear irreconcilable with the basic principles of science. It is just this entirely new situation as regards the description of physical phenomena, that the notion of complementarity aims at characterizing. (Bohr, 1935b, par. 12)

Logical positivists were of course delighted to obtain such a prominent and powerful ally. Philipp Frank carefully studied the EPR paper and Bohr's response to it, and at the Colloquium in Prague he delivered two lectures on the subject (*AHQP*, letter from Frank to Bohr, 9 January 1936). Frank perceived the essence of the exchange between Bohr and Einstein as a confrontation between the "metaphysical" and "positivistic" conceptions of the "logic of science". According to Frank, the metaphysical outlook, that of Einstein and Planck, recognizes three components of a physical theory: (1) the reading of the measuring instruments, (2) the mathematical formalism, and (3) physical reality. The positivistic approach simply dispenses with (3). Evidently, writes Frank, Bohr shares this positivistic outlook.

Not only is such a metaphysical approach superfluous, according to Frank, but it can be downright harmful, leading to fallacies and misconceptions. The importance of Bohr's response to EPR is interesting precisely because it illustrates this point in a particular physical case. The confrontation between Einstein and Bohr, concludes Frank, shows the inadequacy of the Einsteinian conception of reality.[9] In his reply to Frank, Bohr confirmed that the latter did indeed grasp the essence of his aspirations correctly.[10]

Encouraged by Bohr's positivistic stand, Frank attempted to enlist Bohr into the camp of committed and outspoken positivists. In a letter to Bohr, Frank issued a plea for positivistic purity and urged Bohr to express himself so carefully and clearly that no misuse (mystical interpretations – "mystischen Deutungen") of Bohr's words would be possible. Such care is especially important in order to avoid a misuse by national-socialistic forces, who support reactionary philosophy of science and barbaric political regimes. The duty of every physicist is to insulate his words from such a misuse. Only a consistent positivistic stand can ensure this. Precisely because of Bohr's great scientific authority, Frank urged Bohr to share the striving of positivists.[11] (In addition to operationalism, Bohr's emphasis on "unambiguity" of expression also must have found an echo in positivists' hearts.)

We do not have Bohr's reaction to Frank's plea. Yet after the debate over EPR, Bohr's position, more often than not, seems to be indistinguishable from that of the positivists. In the Hitchcock lectures we get the following line, "When we speak about . . . space, time then we must have some experimental arrangement which will allow us to establish the sequence of connections between the behavior of the object and some measuring instruments which . . . serve to define the frame of reference, for the two words, to give the words 'space' and 'time' a definite sense" (Bohr 1937c, Lecture 6).

These proclamations ring with the sound of similar phrases about "time" and its measurement from Einstein's 1905 special relativity paper. As Einstein did then (and later regretted) Bohr here has clearly taken a verificationist line on meaning. As we have seen, this line pulls together the general conclusions that Bohr articulates in his response to EPR. Still, a doubt lingers.

For what of the momentum that gets "buried" in the coordinate reference frame? Bohr insists that it is an "uncontrollable" amount and that this reflects the "feature of individuality" that marks the quantum character of the phenomena. Here is "the finite interaction between object and measuring agencies conditioned by the very existence of the quantum of action" (Bohr, 1935b, par. 3). Nevertheless, the question remains as to whether this physical-sounding description of the object-apparatus interaction is consistent with the operational point of view and with the subsequent inference about the uncertainty in the very meaning of "momentum". If the exchange of momentum is uncontrollable then, even if we knew the initial momentum of the particle very accurately, the momentum transferred from the particle to the apparatus could not be measured. Bohr is quite clear and emphatic on its being in principle unobservable (*Ibid.*, par. 5). What cannot be measured

(what is unobservable) cannot be assigned a clear meaning. So, what one really ought to say here is that talk of an "exchange of momentum" cannot be given a clear meaning. In that case the quantum uncertainty could not be regarded as arising from any meaningful feature of the physical interaction between object and apparatus. Thus Bohr's own operational treatment of the presuppositions for the use of physical magnitudes seems to turn against him.

One might respond to this difficulty as follows. Although it is true that we cannot define the exchange of momentum precisely, still we know that it is "inseparably connected" (*Ibid.*, par. 4) with the uncertainty in the momentum, in accord with the uncertainty relations. This connection, which we cannot trace out in detail, is nevertheless sufficient to enable one to talk meaningfully about the "exchange". Thus Bohr might adopt a liberal operationalism that allows one to talk meaningfully about a quantity if there are experimentally determinable bounds on the possible values of the quantity. But this liberalization will not do, for it is inconsistent with Bohr's strictures on the meaningfulness of momentum talk when a position measurement is made. After all, except in the unrealizable case of a perfectly accurate position measurement, the post-measurement value of momentum is likewise bounded in a determinable way. It is just that the range of possible values cannot be smaller than the uncertainty in momentum. So if the existence of measurable bounds for a quantity warranted meaningful talk of the quantity itself, then one could talk meaningfully about that momentum, contrary to Bohr's dicta.

There is another move to suggest on behalf of Bohr. From the beginning, Bohr regarded the recourse to probabilistic description in the quantum theory as bound to the uncontrollable interaction of the object and apparatus. "[I]f in order to make observation possible we permit certain interactions with suitable means of measurement, not belonging to the system, a rigorous definition of this system is naturally no longer possible, and its description will consequently exhibit a statistical character" (*AHQP: MSS* 12, 'Philosophical Aspects of Atomic Theory' 1927).

Notice the "consequently". In his reply to EPR Bohr suggests a similar connection when he says (Bohr, 1935b, par. 5) that if we could trace out the details of the object-instrument interaction in a many slit experiment, then we would be unable to derive the probabilities characteristic of the expected interference pattern. These considerations suggest that, in the case of a position measurement, although we are unable to "define" the momentum, it is nevertheless meaningful to talk of a probability distribution for momentum. Indeed, it is a "consequence" of the uncontrollable momentum exchange that we must talk here of probabilities. Now the probabilistic talk is measurable;

i.e., we can determine the momentum distribution by repeated measurements. Thus if we link the words "transfer of momentum" or "momentum exchange" to the measurable statistics, so that the latter constitute the operational presuppositions for the meaningfulness of the former, then we can talk about an "uncontrollable interaction" in a way that accords with the verificationist attitude Bohr adopts toward other physical quantities. This proposal also accords with Bohr's insistence that the exchange of momentum is "inseparably connected" to the measurable uncertainty in momentum. The suggestion is that this rigid connection alone warrants its meaningfulness, a proposal that fits nicely with Bohr's overall rhetoric of necessity and entailment. What it means, however, is that measurable quantum uncertainties become linked by relations of necessity and entailment with the uncontrollable disturbance that Bohr requires the object to have on the apparatus. Thus reference to an uncontrollable interaction between object and apparatus derives its meaningfulness from the measurable quantum uncertainty. The "interaction", therefore, does not provide an independently meaningful physical grounding for that uncertainty.

If we tie the language of "exchange of momentum", "transfer of momentum" (and the like) to measurable probabilities and uncertainties, then we can see why Bohr would not be bothered by the following puzzle: how momentum could be transferred (uncontrollably, of course) at the same time a position measurement was being made, without our having to countenance the simultaneous application of the concepts of position and momentum. (Recall item (3) in Section 4.) The answer implicit above is that the experimental conditions required for the applicability of transfer-of-momentum and the experimental conditions required for the applicability of momentum are different, indeed they are complementary. Exactly when we can talk meaningfully of an exchange-of-momentum (namely, when a position measurement is underway) we cannot also talk meaningfully of momentum. This interpretation accords well with what Bohr says and it enables him to slip out of the puzzle. At the same time, however, it makes it plain that his "exchange-of-momentum" has little to do with momentum, in the usual sense – as little as "catacomb" has to do with cats and combs.

We have argued that, from the positivist perspective that Bohr eventually adopted, the idea of an uncontrollable exchange of momentum, which is supposed to ground his physical picture of quantum uncertainty, is problematic. The only way around the problem seems to be to turn the grounding upside down, and to make the measurable uncertainty the operational basis for the language of uncontrollable exchange. Thus despite the lively imagery, when Bohr talks of an exchange or transfer of momentum, there is literally nothing (and in particular, no momentum) that is transferred or exchanged. Bohr conjures up a robust physical picture: the feature of wholeness or "individuality" of the quantum phenomena connected to an uncontrollable interaction between object and apparatus – all giving rise to the quantum uncertainty. Upon scrutiny, however, this impression turns out to be the effect of a conjuring trick. Only the quantum uncertainty itself is independently meaningful. From the positivist point of view, the rest is a word picture constructed around the experimentally verifiable uncertainty formulas, like a collage of printed words glued on to a radiant object.

In section 4 we noted that one could give a positivist or a non-positivist reading of Bohr's reply to EPR. We showed there that the non-positivist reading, which involves genuine physical disturbances that arise during measurement, relies on a double slit arrangement that does not meet the requirements of the EPR case. Here we have argued that the positivist reading, while adequate to EPR and certainly endorsed by the later Bohr, undermines Bohr's story about uncontrollable disturbances providing the physical basis of quantum uncertainty. Thus EPR drives the concept of a measurement disturbance, the central ingredient in Bohr's philosophy of complementarity, onto the horns of a dilemma.

EPR distressed Bohr. It should continue to distress those who are tempted by Bohr's response.

7. LOCALITY AND SEPARABILITY

After Bell, we are accustomed to linking the EPR experiment with locality and separability. These issues come up in EPR through their assumptions, discussed above in section 2, that (separability) the unmeasured particle has some reality which (locality) is not disturbed when the other, distant system is measured. (This is the terminology suggested by Howard, 1985.)

Einstein, we are told, opted for a conception of local causality with respect to EPR that the Bell theorem shows to be untenable. Bohr, on the other hand, saw intuitively the nonlocality (or holism) appropriate for the EPR situation. Whatever his other failings, in this important regard Bohr is said to have won out over Einstein. This is not the place to discuss Einstein on locality and EPR. Suffice it to say, we find that none of the above portrays Einstein's attitudes accurately (see Fine, 1986). There is an opportunity here, however, to discuss Bohr, and what we would urge in this regard is exactly the same conclusion. We have pointed out that there are two different ways to read Bohr's reply to EPR, which are superimposed, we believe, in Bohr's thought at the time. Both readings speak against ascribing to Bohr any reservations about, or willingness to dispense with, locality. On one reading, a mechanical disturbance is of a local nature, due to the need to employ two different mechanical setups of the two-slit diaphragm through which both particles pass before the "last crucial" stage of measurement is performed. In the second case, the positivistic reading, the very question of the reality of the unmeasured system (prior to a measurement on the other system) is carefully bracketed off. It is not that Bohr denies that the unmeasured system has some real physical state; he simply does not discuss it. Instead he addresses himself to specifying what measurements can be made under what circumstances, and he takes the attitude that insofar as the quantum theory can give a satisfactory account of these measurements then nothing more need be said.

The quantum mode of description, he urges, is as complete as it is reasonable to demand. As we saw above, Bohr's reservations about the no-disturbance clause do not involve the conception of a nonlocal interaction, where some real feature of the unmeasured system is disturbed by the distant measurement. Bohr only argues that the actual measurements performed need to be included in any description of the real phenomena; that is, in an accounting of the measurement results. It would be difficult to inflate this lean, positivistic point of view into a holism of real properties or entities. To regard Bohr as endorsing a nonlocal or nonseparable conception of reality strains his carefully tailored language of measurement and his picture of the operational presuppositions on physical magnitudes posed by conditions of measurement.

As for locality itself, there is only a passing allusion to it in Bohr's response to EPR. In discussing a multiple slit experiment Bohr emphasizes that the probability governing where the particle is detected on the photographic plate depends on the "positions of all the slits" and not on any particular one. He argues that this dependence on the whole array of slits is "incompatible" with our being able to say through which slit the particle passed, and hence with the possibility of tracking the course of the particle and the transfer of momentum to the apparatus (Bohr, 1935b, par. 5). However, Bohr discusses this same situation in several other places where he is more explicit about the source of the incompatibility. In "Space and Time in Nuclear Physics" (*AHQP: MSS* 14, March 21, 1935) Bohr refers to the multiple slit experiment several times. Concerning the idea that the path of a photon might depend on the entire array of slits, Bohr says, "So it is completely incomprehensible that in its later course it should let itself be influenced by this hole down there being open or shut". Later, with reference to an electron experiment, he

repeats this, referring to the possibility of such an influence at a distance as "unreasonable".

There is another passage on this theme from the same lecture that anticipates the language of the EPR criterion, and is worth quoting in full.

> If we only imagine the possibility that without disturbing the phenomena we determine through which hole the electron passes, we would truly find ourselves in irrational territory, for this would put us in a situation in which an electron, which might be said to pass through this hole, would be affected by the circumstance of whether this [other] hole was open or closed; but we must be more resigned than usual with the description of the ordinary physical phenomena. We have learned that we are forced to resign because there is nothing at all, because it is only an illusion, seeing that a resignation is the completely deliberate price for the use of measuring instruments in that way. (*Idem*)

Similar considerations are expressed in Bohr's "Light and Life" essay (Bohr, 1933).

We should not conclude that these remarks anticipated either the intricate nature of the EPR argument, or our present concerns about locality. Rather Bohr is wrestling here with another issue, one central for him and around which he gradually weaves his framework of complementarity. This is the paradoxical issue of the applicability of both the "individuality" of particles (their particle nature), and of the "superposition" principle (wave-theoretical nature) in the microdomain. In short, in his considerations about the many slit experiments Bohr wrestles with the wave particle duality. Light quanta or electrons cannot have well defined kinematical paths in space and also lead to interference phenomena. In the many slit experiment Bohr argues for the inapplicability of the classical idea of motion in the quantum domain, asserting instead the consistency of complementarity in resolving the wave-particle dilemma. This is the way Bohr put it in his 1933 essay:

> [I]t should be emphasized that light quanta cannot be regarded as particles to which a well defined path in the sense of ordinary mechanics can be ascribed. Just as an interference pattern would completely disappear if, in order to make sure that the light energy travelled only along one of the two paths between the source and the screen, we would stop one of the beams by a non-transparent body, so is it impossible in any phenomenon for which the wave constitution of light is essential to trace the path of the individual light quanta without essentially disturbing the phenomenon under investigation. Indeed the spatial continuity of our picture of light propagation and the atomicity of the light effects are complementary aspects . . . (*APHK*, 5)[12]

Such a resolution of the wave particle duality in terms of exclusive experimental arrangements assures the consistency of the quantum mechanical interpretation precisely because it satisfies locality and thus avoids 'spooky' non-local effects. Bohr expressed this resolution succinctly and clearly in his contribution to the Schilpp volume:

> This point is of great logical consequence, since it is only the circumstance that we are presented with the choice of either tracing the path of a particle or observing interference effects, which allows us to escape from the paradoxical necessity of concluding that the behavior of an electron or a photon should depend on the presence of a slit in the diaphragm through which it could be proved not to pass. (Bohr, 1949, 217–18)

We see that Bohr persistently considers any option of nonlocality as unacceptable. His own words are: "incomprehensible", "unreasonable", and "irrational". He is not in the business of forming a picture of reality that allows for some kind of nonlocal action. To the contrary, Bohr set about the task of reconstructing and limiting the language of reality in such a way that no phenomena that one could properly describe as "real " would be affected nonlocally. It was not because he regarded the criterion of reality as mistaken that it stood out so strongly in Bohr's mind. Rather the opposite. That criterion was his very own, one he respected and used in trying to craft a proper conception of complementarity that would place limits on the language of "real" phenomena. Tying what is real to what can be measured, and settling finally on measurement disturbance as passing from the object to the instrument, allowed Bohr to accomplish that task to his own satisfaction, although he recognized that doing it his way entailed "renunciations" and "radical revisions".

When Einstein accused the quantum theorists of playing a risky game with reality, this is the game he had in mind. There was no disagreement between Einstein and Bohr with regard to their respective tolerance for nonlocal measurement effects. Neither could tolerate them. Their disagreement was over the role of measurement itself. For Einstein, measurements were probative, indicating some reality already there to be measured. For Bohr, measurements became constitutive of reality.

If Bohr did not chose nonlocality, however, does it mean that he embraced some nonseparability alternative, some kind of quantum holism? Does Bohr's language of "wholeness", "inseparability", or "indivisibility" indicate this choice? While Bohr's terminology is to be found in some current discussions, the conceptual object of Bohr's struggles is not.[13] For Bohr, expressions such as "indivisibility", or "individuality" apply either to the "finitude" of Planck's quantum of action, or to the impossibility of "subdividing" (*i.e.*, of following more closely) the actual behavior of individual micro-objects without violating the "rationality" of the quantum mechanical description (as would be the case if we could follow the path of an individual photon or electron between a particular slit in a many slit diaphragm and the photographic plate). Similarly, Bohr's use of "wholeness" is a positivistic one, referring to the necessity of specifying the entire experimental setup in describing quantum phenomena. The "inseparability" that concerned Bohr was between micro-objects and macroscopic measuring devices.

Before 1935 this "inseparability" was said to be due to robust physical disturbances, and after 1935 it has to do with the positivist semantical link between the micro-object and macroscopic measuring instruments.

In the EPR situation one might try to extend Bohr's inseparability to the micro-objects themselves – the "inseparability" or holism at issue in current discussions – by regarding one particle as a measuring device for the other. Indeed Ruark (1935) considered just this idea. But, in Bohr's case such an extrapolation of "wholeness" will not do, because for him the measuring device must – in principle – be "heavy" and classical. (Look at the surreal figures in Bohr (1949) with their thick bolts and springs and angle brackets.) In principle, the measurement interaction is not to be treated quantum mechanically. Hence Bohr's "wholeness" cannot extend to composite systems consisting of micro-objects, who might be said to lose their individuality when their state functions become entangled.

In rejecting the quantum mechanical treatment of the measurement interaction, Bohr's "wholeness" turns away from the representation of object-apparatus interactions by entangled state functions. Indeed, as we noted earlier, Bohr's answer to EPR avoids any discussion in terms of wave-functions and their properties. Similarly, Bohr later refused to rely on features of the quantum mechanical formalism in his comment on von Neumann's proof concerning indeterminism and hidden variables, maintaining that his simple considerations of intuitive thought experiments were sufficient to unravel all the quantum mechanical mysteries (Bohr, 1939).[14]

Current discussions of inseparability and quantum holism, however, are precisely about the significance of entanglement in the formalism for interacting systems. Thus, however closely Bohr's language resembles that of our present day concerns we should be careful not to assimilate it to our post-Bell questions. Bohr's struggles belong to a different historical and conceptual generation. Just as Bell's theorem did not "toll" to refute Einstein (Fine, 1986), neither does it ring to vindicate Bohr.

8. CONCLUDING REMARKS

The primary challenge that Bohr saw in EPR was not the issue of hidden variables, or locality, or the conception of an independent reality for separated components of a composite system, or any of the other topics that we have come to associate with EPR. What Bohr saw was a specific challenge to the concept of disturbance, the concept around which he had built his philosophical setting for the quantum theory and out of which he had crafted his conception of complementarity. Prior to EPR Bohr had been able to mobilize the compelling physical picture of observation in the quantum domain as a confrontation between a tiny quantum object and a giant instrument of measurement. Bohr skillfully utilized that image to motivate the view that the quantum domain presented a new observational situation, one that limited the classical idea of inferring the initial state of the object from the object-instrument interaction. EPR, however, was designed so that the unmeasured system was not disturbed, and this feature threatened to undercut the whole edifice that Bohr had built. No wonder then that Bohr was troubled by EPR. Bohr's response was to find a "hidden" disturbance in his physical reconstruction of the EPR situation, one that occurs not in the "last crucial" stage of the measurement, but in a preceding stage – thus exemplifying the need to pay attention to the "entire" physical setup. This construction accorded well with Bohr's insistence on the "wholeness" of the experimental situation; it also enabled Bohr to assert that he had found an "ambiguity" in Einstein's claim of non-disturbance – a weak link in Einstein's reasoning.

The exact location of this "ambiguity", however, is not easy to track down from reading Bohr's paper. Moreover, at the time, most physicists had little need to go into the details of Bohr's intricate argumentation (or the patience to do so). For example, Pauli – a prominent champion of operationalism (Hendry, 1984) – presented a brief and lucid summary of Bohr's response to EPR to Schrödinger in exclusively positivistic terms (von Meyenn, *et al.*, eds., 1985, Vol. 2, Pauli to Schrödinger, July 1935). Heisenberg, on the other hand, had long since convinced himself (at least since his uncertainty paper, Heisenberg, 1927) that if there were a consistent and empirically confirmed mathematical scheme, you could always reconcile it with nature, provided you were willing to pay a price in redefining 'intuition' and revising 'reality'. Ironically, despite Bohr's skepticism about the power of mathematics to penetrate into the deep secrets of nature, Bohr's *a priori* conviction that Einstein's objections *could not* undermine the "rationality" of quantum mechanics also relied on the same feeling about the formalism (Bohr, 1935b, par. 3). It is this shared belief in the finality of the successful mathematical formalism of the quantum theory that explains Pauli's and Heisenberg's complacency and impatience toward Einstein's ongoing attempts to locate some fault in the quantum theory. This attitude was apparent in their reactions to Einstein's critiques at the Solvay meetings: "ah, well, it will be all right, it will be all right" (*ach was, das stimmt schon, das stimmt schon*, Pais, 1991,

318). This same shared assumption enabled working quantum physicists to go on with their job at hand, leaving the handling of Einstein's critique to Bohr.[15]

Thus for those around Bohr, the master's reply to EPR was more of a reinforcement than a revelation. Yet, while everybody in Bohr's circle was convinced that EPR posed no "real problem", nobody, to Schrödinger's dismay, could clearly explain why. Thus Schrödinger appealed to Pauli:

> I would very much like to know, what your opinion is on this matter. And if you really think that Einstein's case – let us call it so – does not provide anything to think about, but is completely clear and easy and self-evident. (All those with whom I spoke on this matter for the first were of this opinion, because they had learned well their Copenhagen Credo in unum sanctum) . . . But I did not get a clear answer to why everything is clear and simple. (von Meyenn, *et al*., eds., 1985, Vol. 2, Schrödinger to Pauli, June 1935)[16]

While it may still be asserted that Bohr "won" the battle over EPR, the reasons for such a claim are no clearer today than they were at the time. Bell's words (our epigraph) testify eloquently to this fact. Surely, the positivistic reply that is ascribed to Bohr (and that was first offered, let us recall, by Ruark), can hardly be thought of as a glorious victory after the weeks of Herculean intellectual effort described by Rosenfeld (1969). The feeling of triumph that Bohr and Rosenfeld experienced, and subsequently transmitted, followed from their conviction that in their physical reconstruction of EPR they had found a weakness in Einstein's reasoning. Yet this part of Bohr's reply, as he must eventually have realized (for Bohr never again repeats it), turns out to be fallacious. Nevertheless, like the smile on the Cheshire cat, the legend remained of a triumphant reply that capped an intense intellectual effort.

In his recollections, Bohr (1949) simply quotes the more philosophical part of his reply to EPR, rather than – as he did with the other aspects of his life long dialogue with Einstein – illuminating or explaining the physical analysis. Following the failure of his analysis of measurement disturbance to deal concretely with EPR, Bohr moved from detailed considerations of measurement interactions and disturbances to broad discussions of the "general epistemological lessons" of quantum theory. Bohr's apocalyptic language, his repeated declaration that we have arrived finally at the only possible rational course for physics, shows how completely the challenge of EPR shifted complementarity away from physical analysis and into the realm of philosophical counsel. As a result of EPR, Bohr eventually turned from his original concept of disturbance, to make a final – and somewhat forced – landing in positivism.

If the dialogue around EPR eventually led to Bell, it is perhaps more in spite of, than because of, Bohr's response to their challenge. In particular, Bohr's positivistic attitude was not favorable to the type of questions that interested Bell, and Bell himself found little enlightenment in or inspiration from Bohr's way of thinking. Rather, the argument of the EPR paper itself clearly pointed to the problem of reconciling quantum mechanics with accepted notions of physical reality and local causality. Despite Bohr's announcements of the need for a "radical revision" of the concept of physical reality, his own positivistic strictures could hardly facilitate the search for a new conception.

It is not because Bohr's reply to EPR led to (or still guides) our present discussions that it is important. Nor is it because Bohr's argument or insight won the day. Rather, Bohr's reply constitutes a fascinating document because it represent a decisive turning point in the evolution of Bohr's own epistemological thought.

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NOTES

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[1] Mir war diese Note der Anlass, mir den Fall (den wir ja alle im wesentlichen längst kennen . . . dass 'wirklich einen Wert haben', 'wirklich so und so beschaffen sein' und dgl. nicht sinnvolle Redenwendungen sind. (von Meyenn, *et al*, eds., 1978, Schrödinger to Pauli, July 1935)

[2] Inzwischen hat mir Bohr das Manuscript einer Note geschickt [Bohr, 1935b], das als Erwiderung an Einstein nun ans Physical Review abgegangen ist. Vielleicht skizziere ich zuerst das wichtigste des Inhaltes, der übrigens nichts mir Neues enthält. Ich glaube übrigens in der Tat, dass "Einsteinfall" nichts enthält, als sehr elementare-directe Konsequenzen der Unbestimmtheitsrelation. (von Meyenn, *et al*., eds., 1978, Pauli to Schrödinger, July 9, 1935)

[3] See Fine (1986), Chap. 3, for an analysis of the argument.

[4] Here and below we refer to Bohr (1935b) by citing the relevant paragraph(s).

[5] It is important not to associate the criterion of reality with Einstein. For it was Podolsky who wrote the EPR article and in a manner that Einstein regarded as unsatisfactory. In his own published accounts of the correlated EPR situation Einstein never refers to or makes use of this "criterion". See Fine (1986).

[6] The passage cited here is from the revised version of 'Light and Life' which appears in *APHK*, Bohr (1958a); the parallel sentences in the original version, Bohr (1933), are somewhat different.

[7] The possibility of this reading was suggested to M.B. by Alon Drori, Physics Department, The Hebrew University. M.B. wants to express her gratitude to Alon for very valuable discussions on this issue.

[8] See the critique of EPR due to Epstein, in Jammer (1974), 234–5.

[9] Es scheint mir hier nämlich ein Punkt berüht, der für eine positivistische Auflassung der Physik im Gegensatz zu einer metaphysischen charakteristisch ist . . . Nun scheint es mir ganz deutlich, dass Sie diese letztere Auffassung vertreten und Einstein die erstere.

Ihre Polemik gegen Einstein ist mir gerade deshalb so interessant, weil Sie hier auf einen Fall hinweisen, wo dieses dritte, überflüssig in die Theorie eingeführte Element, die physikalische Realität, zu Trugschlüssen führt. . . . so wäre mir das sehr wertvoll, weil man dann sieht, dass die Einführung einer derartigen Terminologie sehr gefährlich für einen logischen Aufbau der Physik ist . . . Er [Einstein] schliesst darauss, dass die Unvollständigkeit der quantenmechanischen Beschreibung beweisen ist. Man kann aber logisch ebenso schliessen, dass daraus folgt, dass sein Begriff der physikalischen Realität sich nicht eignet, in eine physikalische Theorie eingeführt zu werden." (*AHQP: BSC* 19, Frank to Bohr, 9 January 1936)

[10] Ich glaube auch, dass Sie ganz den Sinn meiner Bestrebungen getroffen haben. (*AHQP· BSC* 19, Bohr to Frank, 14 January 1936)

[11] Wenn ich trotzdem in Ihrer Ausdruck weise oft eine Gefahr des Missverständnisses sehe, so liegt das in folgendem: heute sind überall Krafte am Werk, die an Stelle der modernen Naturwissenschaft etwas von der Art der mittelalterlichen Scholastik setzen wollen, um damit auch die verschiedenen mittelalterlich-barbarischen politischen Systeme geisting zu untermauern. . . . Ich glaube dass es die Pflicht jedes Physikers ist, sich immer so auszudrücken, dass ein Missbrauch seiner Arbeiten unmöglich ist. Meiner Ansicht nach Kann das nur durch die Ausarbeitung einer konsequent positivistishen . . . Terminologie geschehn . . . gerade bei Ihrer grossen Bedeutung für die gegenwärtige Physik wäre es schön, wenn Sie sich auch an den Bestrebungen beteiligen würden, die darauf ausgehn, überall eine konsequent wissenschaftliche ausdruckweise einzuführen und so jeden Missbrauch . . . unmöglich zu machen . . . "(*AHQP: BSC* 19, Frank to Bohr, undated letter, most likely 1936)

[12] The passage cited here is from the revised version of 'Light and Life' which appears in *APHK*, Bohr (1958a); the parallel sentences in the original version, Bohr (1933), are somewhat different.

[13] For a different opinion see, for example, Folse (1989a).

[14] This opinion was expressed by Bohr in a discussion following von Neumann's presentation at a conference at Warsaw, May 30th – June 3rd, 1938. Bohr presented there 'The Causality Problem in Atomic Physics' (Bohr, 1939).

[15] According to A. Pais, George Uhlenbeck informed him that no physicist, active at the time and known to him, occupied himself with the EPR case, because "that was an issue that could safely be left to Bohr and Einstein" (Pais, 1991, 430).

[16] Aber ich wusste sehr gern was Du dazu meinst. Und ob Du wirklich meinst, der Einsteinfall – nennen wir ihn so – restlos nichts zu denken gibt, sondern ganz klar und einfach und selbstverständlich ist. (So meinten bisher alle, mit denen ich zum ersten Mal darüber sprach, weil sie ihr Kopenhagener Credo in unum sanctum gut gelernt hatten . . . Aber klare Auskunft, warum alles so klar und einfach ist, bekam ich noch nicht. (von Meyenn, *et al*., eds., 1978, Schrödinger to Pauli, June 1935)

The EPR Experiment:
A Prelude to Bohr's Reply to EPR

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1 Einstein, Podolsky, and Rosen's argument

Bohr's (1935) reply to Einstein, Podolsky, and Rosen's (EPR's) (1935) argument for the incompleteness of quantum theory is notoriously difficult to unravel. It is so difficult, in fact, that over 60 years later, there remains important work to be done understanding it. Work by Fine (1986), Beller and Fine (1994), and Beller (1999) goes a long way towards correcting earlier misunderstandings of Bohr's reply. This essay is intended as a contribution to the program of getting to the truth of the matter, both historically and philosophically. In a paper of this length, a full account of Bohr's reply is impossible, and so I shall focus on one issue where it seems further clarification is required, namely, Bohr's attempt to illustrate EPR's argument by means of a thought experiment. In addition, I shall attempt to clarify a few other points which, however minor, have apparently contributed to misunderstandings of Bohr's position. As the title of this paper suggests, an account of these few points does not consitute an account of Bohr's reply, but it is an important step in that direction.

I shall begin by raising several points about EPR's argument, and especially their example of particles correlated in position and momentum. Some of these points have not been sufficiently noticed in the literature.

Let us begin with a standard, but incorrect, story about EPR's argument. Two particles are emitted from a common source, with momenta p and $-p$, respectively. For simplicity, we assume that their masses are the same. Some time later, particle 1 encounters a measuring device, which can measure either its position, or its momentum. If we measure its momentum to be p, then we can immediately infer that the momentum of particle 2 is $-p$. If we measure

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[*] Thanks to audiences at Indiana University and HOPOS 2000 for comments on related talks. Thanks to Arthur Fine for alerting me to some secondary literature. Thanks to Michael Friedman and Scott Tanona for helpful discussions.

its position to be x, then (letting the source be at the origin) we can immediately infer the position of particle 2 to be $-x$. Now, if we assume that the measurement on particle 1 in no way influences the state of particle 2, then particle 2 must have had those properties all along, because it could not obtain them merely as a result of the measurment on particle 1. But quantum theory cannot represent particle 2 as having a definite position and momentum, and therefore quantum theory is incomplete.

EPR do not make this argument. If they had, Bohr's reply could have been quite short. The short reply is to note that in order to make the requisite predictions, one must know the precise position and momentum of the source. Consider, for example, that you have just measured the momentum of particle 1 to be p. If you do not know the momentum of the source, then in particular you do not know in which frame of reference to apply conservation of momentum. (Above we assumed that the source is at rest relative to us, and so we infered that particle 2 has momentum $-p$.) Similarly, consider that you have just measured the position of particle 2 to be x. If you do not know the location of the source, then you cannot say where particle 2 is. It is 'the same distance from the source' as particle 1, in the other direction, but how far is particle 1 from the source? Unless you know where the source is, you cannot answer this question.

But if you must know the precise position and momentum of the source in order to make the inferences, then the uncertainty principle will always get in the way of EPR's argument. Suppose, for example, that you know the precise momentum of the source. Then you measure the position of particle 1. The EPR criterion for physical reality says:

> If, without in any way disturbing a system, we can predict with certainty…the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity. (Einstein et al., 1935, p. 777)

But we *cannot* predict particle 2's position with certainty, because we do not (and under the circumstances, cannot) know where the source is.

Good thing, then, that the 'standard story' about EPR's argument is wrong. We can see immediately that something is wrong with it, because nowhere did that story mention quantum theory, and yet EPR are very concerned to present their argument in quantum-theoretic terms (as they should be). Indeed, the first part of their paper rehearses a number of facts about the formalism of quantum theory, presumably so that they can present their argument in a quantum-theoretic context (which is what they do).

EPR continue by considering a generic system of two particles and a pair of generic (but non-commuting) observables on particle 1, A and B. EPR do not then write down a generic version of the so-called 'EPR state'. Instead, they merely point out that as a result of measuring A on particle 1, particle 2 may be left in one state—call it $\psi_k(x_2)$, as they do—while as a result of measuring B on particle 1, particle 2 may be left in quite another state—call it $\varphi_r(x_2)$, as they do.

At this stage of the argument, EPR might have pointed out that ψ_k and φ_r are eigenfunctions of *some* observables. Hence we would be able to predict, with certainty, the values of two observables as a result of two different measurements (of A or B) on the first system. One would then have to go on to show that those observables need not commute.

Instead of continuing with this generic case, however, EPR turn to a specific example, using the position and momentum observables. Here they do add the idea that ψ_k and φ_r can be eigenfunctions of position and momentum, respectively. To establish this claim, they suppose that the total system prior to any measurements is in the state

$$\Psi_{\mathrm{EPR}}(x_1, x_2) = \int_{-\infty}^{\infty} e^{(2\pi i/h)(x_1 - x_2 + x_0)p} dp, \tag{1}$$

where x_0 is some constant. EPR then show that (1) is a state of perfect (anti-) correlation between the positions and momenta of the two particles: measuring the momentum of particle 1 (hence collapsing the wavefunction for the compound system!) leaves particle 2 in the relevant eigenstate of momentum, and likewise for position.

So why is the 'standard story' inconsistent with this account? We have already noted that the 'standard story' is not quantum-mechanical, but the more important point for us here is that EPR nowhere describe how the compound system is prepared, nor how it evolves in time. Indeed, the notion of time never enters their discussion. The state Ψ_{EPR}—let us call it the 'EPR state'—is a 'snapshot' of the compound system at a time. Moreover, EPR *could not* give us a dynamical description of the situation, because the EPR state cannot be preserved under Hamiltonian evolution (unless we introduce an infinite potential, a point that I will no longer bother to mention).

The reason is familiar, though not usually mentioned in this context. The support of Ψ_{EPR} has measure 0 in configuration space: $\Psi_{\mathrm{EPR}}(x_1, x_2)$ is zero except when $x_2 - x_1 = x_0$, and so it is a line in the (two-dimensional) configuration space for the compound system. Such a state necessarily spreads under the evolution induced by any Hamiltonian. (We are, of course, ignoring the fact that the EPR state is not in $L^2(R^2)$ in the first place. Neither EPR nor Bohr seem to have been concerned about this point.)

Finally, note that there is no Hamiltonian evolution that can take a generic state $\Phi(x_1, x_2)$ to the EPR state (no matter what Φ is). Only a 'collapse' of the wavefunction can produce the EPR state. Hence we must imagine the EPR state to exist at *and only at* the moment of preparation.

EPR's argument, then, is based on such a state. They point out that upon measuring the position of particle 1, we can predict with certainty the position of particle 2, and likewise for momentum. Of course, only one of the two measurements can be performed, which raises the question whether some modal fallacy has been committed. After all, their argument apparently takes the form:

1. Actually: position is measured for particle 1, and therefore (actually) particle 2 has a definite position.
2. Possibly: momentum is measured for particle 2, and therefore (possibly) particle 2 has a definite momentum
3. Therefore: particle 2 (possibly? actually?) has a definite position and a definite momentm.

In this form, the argument is clearly fallacious (no matter which modal version of the conclusion you choose). Of course, the notion of 'non-disturbance' is supposed to help patch up the argument: although the circumstances under which we can predict the value of particle 2's position are incompatible with the circumstances under which we can predict the value of particle 2's momentum, the difference between these circumstances is supposed to make no difference to particle 2.

Even with the help of some principle of non-disturbance, it is not clear, however, that EPR's argument works. Let us consider, first, a 'weak principle of non-disturbance':

> **Weak non-disturbance**: if momentum is measured on particle 1 and (therefore, by the criterion for physical reality) momentum is definite for particle 2, then: had we not measured momentum on particle 1, particle 2 would still have had a definite momentum (and likewise, substituting position for momentum).

This principle is, alas, not enough to get EPR's conclusion. They need:

> **Strong non-disturbance**: if momentum is measured on particle 1 and (therefore, by the criterion for physical reality) momentum is definite for particle 2, then: had we not measured momentum on particle 1 but instead measured its position, then particle 2 would still have had a definite momentum (and likewise, switching position and momentum).

The weak principle does not entail the strong principle because it might be impossible (without destroying essential features of the situation, in particular, our ability to infer properties of particle 2 from the results of measurements on particle 1) both to measure position on particle 1 and for momentum to be definite for particle 2. (In terms of the 'possible-worlds' semantics for counterfactuals: while the closest 'momentum is not measured'-worlds to the 'momentum is measured and is definite for particle 2'-worlds might all be 'momentum is definite for particle 2'-worlds, those closest worlds may not contain any 'position is measured'-worlds, so that the closest 'momentum is not measured but position is'-worlds to the 'momentum is measured and is definite for particle 2'-worlds need not be 'momentum is definite for particle 2'-worlds. Now say that sentence three times fast.)

Bohr is sometimes understood to deny the strong principle by asserting that the act of measuring position on particle 1 'disturbs' in some strange 'semantic' (and non-local) way the very possibility of particle 2's having a definite momentum. Such a response is (rightly) taken to be uninteresting philosophically. In a longer account of Bohr's reply, I would argue that while Bohr does deny the strong principle, he does so for more interesting reasons. Here, however, I shall only make a few suggestions in that direction. The next section contains several observations about EPR's argument and Bohr's reply. These remarks are intended to clear the air of some minor criticisms of Bohr's reply. In the subsequent section, I shall discuss Bohr's thought experiment and make some brief suggestions about how to understand Bohr's reply.

2 Some Clarifications

1. *EPR speak in terms of a 'contradiction'.* Without calling into question Fine's (1986) logical analysis of EPR's argument, we may note that they do speak of a 'contradiction' between their criterion of reality and the completeness of standard quantum theory. At the end of the first section of their paper, Einstein et al. (1935) state their conclusion thus: "We shall show, however, that this assumption [completeness], together with the criterion of reality given above, leads to a contradiction".

As Beller and Fine (1994) argue, Bohr had no problems with EPR's criterion for physical reality, nor with their account of completeness, together understood in a fairly conservative sense (perhaps, in modern terms, as no more than the eigenstate-eigenvalue link). Hence the idea that there might be a 'contradiction' between the criterion and completeness would surely have worried Bohr, and would understandably be the focus of his reply. No wonder Bohr's rhetoric focused on 'soundness', 'rationality', 'lack of contradiction' and 'consistency' (cf. (Beller and Fine, 1994, pp. 3-4)). While we may endorse much of what Beller and Fine (1994) assert to be at the heart of Bohr's general concerns about consistency, the simple explanation seems to be just that EPR do, at least at one point, state their conclusion in terms of a contradiction, a contradiction that was (for reasons that Beller and Fine explore) threatening to Bohr's own position.

2. *The EPR argument focuses on the example.* I mentioned above that EPR begin their discussion in the abstract and could have finished it there, but they do not, instead resorting to the example involving position and momentum. Bohr, too, focuses on the example. Indeed, he takes the example to constitute the argument, writing that "[b]y means of an interesting example, to which we shall return below, they [EPR] next proceed to show that …[the] formalism [of quantum mechanics] is incomplete" (Bohr, 1935, p. 696). Nobody involved in the debate seems to have thought that this focus on the example is unwarranted or misleading. The point is important for two reasons.

First, it lends greater importance to a proper understanding of Bohr's attempt to realize the example in a thought experiment. From a contemporary standpoint, one might be tempted to suppose that the real substance of the EPR argument, and of Bohr's reply, is (and was taken by them to be) in the more abstract discussions (for example, in the early part of EPR's paper and the mathematical footnote in Bohr's reply). While these more abstract discussions can provide important clues to understanding EPR and Bohr's reply, their mutual focus on the example of position and momentum suggests that we too focus on that example in order to understand what is going on.

Second, the focus on the example *is*, in the end, unwarranted and misleading. Indeed, from a contemporary standpoint, we can see that EPR chose a particularly unfortunate example to make their point. As I shall emphasize again below, the main problem is that position (unlike momentum) is not a conserved quantity, so that correlations in position will in general not be maintained under free (or for that matter, almost any other) evolution. Bohm's (1951) reworking of EPR's argument in terms of a new example (involving incompatible spin observables) fixes the problem (because spin is conserved), and it is unclear whether Bohr's reply could work in this case. (In any case, his thought experiment is mostly irrelevant to the Bohmian example.)

3. *The observables $X_1 - X_2$ and $P_1 + P_2$ can be determined simultaneously.* EPR presume that the total momentum ($P_1 + P_2$) and the distance between the particles ($X_1 - X_2$) can be known simultaneously. There is no obstacle in principle to obtaining such knowledge, since the obervables in question are compatible (mutually commuting). Indeed, the EPR state is a simultaneous eigenstate of both of these observables. (Again, we ignore the fact that plane waves and delta functions are not, strictly speaking, states, i.e., not in $L^2(R^2)$.)

But how might one actually prepare the EPR state, or more generally, how might one actually determine $X_1 - X_2$ and $P_1 + P_2$ simultaneously? That is, from a physical point of view, why do these operators commute? Note first—and this point is crucial to an understanding of Bohr's reply—that Bohr insisted that neither position nor momentum observables have any clear physical meaning outside of the specification of some frame of reference. Bohr is acutely aware of the role that reference frames play in relativity theory, and believes that their role in the quantum theory is even more significant—well-specified frames of reference are crucial to the very meaning of ‘spatial location’ and ‘momentum’. Bohr’s view seems to have been that only prior to the discovery of the quantum theory, and specifically the ‘essential exchange of momentum’ involved in any interaction, could one dispense with the insistence that reference frames are essentially involved in the very notion of ‘position’ and ‘momentum’. While a full analysis of Bohr’s position on this point (and most especially of his understanding of the term ‘essential exchange of momentum’) is out of the question here, it is worth noting that Bohr insisted upon the necessary role that well-defined reference frames play in the very definition of the notion of position. He writes:

> Wie von EINSTEIN betont, ist es ja eine für die ganze Relativitätstheorie grundlegende Annahme, daß jede Beobachtung schließlich auf ein Zusammentreffen von Gegenstand und Meßkörper in demselben Raum-Zeitpunkt beruht und insofern von dem Bezugssystem des Beobachters unabhängig definierbar ist. Nach det Entdeckung des Wirkungsquantums wissen wir aber, daß das klassische Ideal bei der Bescreibung atomarer Vorgänge nicht erreicht werden kann. Insbesondere fürht jeder Versuch einer raum-zeitlichen Einordnung der Individuen einen Bruch der Ursachenkette mit sich, indem er mit einem nicht zu vernachlässigenden Austausch von Impuls und Energie mit den zum Vergleich benutzten Maßstäben und Uhren verbunden ist, dem keine Rechnung getragen werden kann, wenn diese Meßmittel ihren Zweck erfüllen sollen.(Bohr, 1929, p. 485)[1]

Continuing this line of thought, in his reply to EPR (1935, p. 699), Bohr writes:

> To measure the position of one of the particles can mean nothing else than to

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[1] In (Bohr, 1934, pp. 97–98), the passage reads

> As Einstein has emphasized, the assumption that any observation ultimately depends upon the coincidence in space and time of the object and the means of observation and that, therefore, any observation is definable independently of the reference system of the observer is, indeed, fundamental for the whole theory of relativity. However, since the discovery of the quantum of action, we know that the classical ideal cannot be attained in the description of atomic phenomena. In particular, any attempt at an ordering in space-time leads to a break in the causal chain, since such an attempt is bound up with an essential exchange of momentum and energy between the individuals and measuring rods and clocks used for observation; and just this exchange cannot be taken into account if the measuring instruments are to fulfil their purpose.

As Michael Friedman pointed out to me, the translation does not perfectly match the original. For example, rather than “an essential exchange of momentum” one should probably say “a non-negligible [nicht zu vernachlässigenden] exchange”. These subtle differences are important for a full understanding of Bohr’s view and especially (perhaps) its development, but for our purposes here they are not crucial.

> establish a correlation between its behavior and some instrument rigidly fixed to the support which defines the space frame of reference.

Bohr is careful to discuss position (and momentum) in these terms, not speaking of 'the position [or momentum]' of a system, but its position relative to some other system. For example, at p. 697 of his reply he speaks not of the uncertainty of the position of a particle, but of 'the uncertainty Δq of the position of the particle relative to the diaphragm'. The fact that not only position, but also uncertainty in position, must be discussed relative to a physically defined reference frame indicates the extent to which, for Bohr, such reference frames are involved in the very meaning of 'position'.

These points are important, because failing to appreciate them fully, one can be too easily persuaded that passages such as the one above indicate Bohr's adherence to a rather strong form of operationalism. He might, in other words, be suggesting that physical properties are defined by the operations used to 'measure' them. But given the history of Bohr's insistence on the role of (physically specified) reference frames in quantum theory, we can just as well (and indeed, I would argue, more fruitfully) read the passage above and others like it as insisting that a well-defined frame of reference is crucially a part of the notion of position.

4. *The observables* $X_1 - X_2$, $P_1 + P_2$, X_1, *and* P_1 *are not mutually commuting.* It is easy to suppose that without losing our knowledge of $X_1 - X_2$ and $P_1 + P_2$, we may go on to determine either X_1 or P_1. (This mistake is all the easier if one conceives of the EPR experiment in terms of the 'standard story' that I outlined above.) The following passage, for example, seems to make this suggesstion:

> EPR consider a composite system in a state where, at least for a moment, both the relative position $X_1 - X_2$ and the total momentum $P_1 + P_2$ are co-measurable. Moreover, in EPR both of these quantities are simultaneously determinable with either the position or the momentum (not both) of particle 1. (Beller and Fine, 1994, p. 15)

However, X_1 fails to commute with $P_1 + P_2$ and P_1 fails to commute with $X_1 - X_2$. If the EPR situation allowed us to co-determine both $X_1 - X_2$ and $P_1 + P_2$ with either X_1 or P_1, then a great deal more than Bohr's reply would be in jeopardy. If we are to determine X_1, then we must give up our knowledge of $P_1 + P_2$, and if we are to determine P_1, we must give up our knowledge of $X_1 - X_2$.

As Beller (1999, ch. 6) explains, the early Bohr was very concerned to explain *why* it is not possible to observe simultaneous values for incompatible observables. I will suggest, below, that Bohr's reply to EPR continues this discussion, i.e., that he is, in part, attempting to explain why one cannot measure $X_1 - X_2$, $P_1 + P_2$, and either of X_1 or P_1 simultaneously, within the context of EPR's example. (Here, then, is one sense in which Bohr's reply involves themes and argumentative strategies that he had already used in other cases.)

3 Bohr's Thought Experiment

We are now in a position to assess the relevance of Bohr's proposed thought experiment to EPR's argument. Bohr's discussion begins with a rehearsal of two different sorts of experiment. In the first, there is a screen with a single slit, "rigidly fixed to a support which defines the space frame of reference" (1935, p. 697), and a particle is fired at the screen. We assume that the particle's initial momentum is well-defined. Bohr asks whether, after preparing the particle in a state of well-defined position by passing it through the slit (and thereby, according to de Broglie's relation, rendering its momentum uncertain), we cannot take into account the exchange of momentum between the particle and the apparatus, thereby 'repairing' the loss of initial certainty about the momentum. His answer is 'no', because the exchange of momentum "pass[es] into this common support" which, because it *defines* the space frame of reference, *must* be taken to be at rest, and so "we have thus voluntarily [by fixing the initial screen to the support and taking that support to define the spatial reference frame] cut ourselves off from any possibility of taking these reactions separately into account" (ibid.). (Recall Bohr's claim that "just this exchange cannot be taken into account if the measuring instruments are to fulfill their purpose", quoted above.)

If, on the other hand, we allow the initial screen to move freely relative to the support, then we can indeed measure the exchange of momentum between the particle and the screen, but in so doing, we necessarily lose whatever information we might previously have had about the location of the initial screen relative to the support, and therefore passing the particle through the slit is no longer a preparation of its position relative to the support:

> In fact, even if we knew the position of the diaphragm relative to the space frame [i.e., the 'support'] before the first measurmeent of its momentum, and even though its position after the last measurement [required to determine the exchange of momentum] can be accurately fixed, we lose, on account of the uncontrollable displacement of the diaphragm during each collision process with the test bodies, the knowledge of its position when the particle passed through the slit. (1935, p. 698)

Note that two measurements of the momentum of the screen are *required* (in addition to a prior measurement of the momentum of the incident particle) in order to apply conservation of momentum to the total system, by which we can determine the momentum of the incident particle after it has passed through the slit. Bohr claims that the second measurement of the momentum of the screen disturbs its position relative to the support in an ‘uncontrollable’ way, thereby preventing us from determining its position (relative to the support) at the moment that the particle passed through the slit.

My aim in making these observations is not to analyze Bohr’s claims in detail. Such an analysis would include a deeper discussion of Bohr’s notion of a ‘reference frame’, and his notion of ‘uncontrollable disturbance’, both of which are crucial to a complete understanding of Bohr’s reply. The aim here, rather, is only to remind the reader of the broad outlines of Bohr’s understanding of the uncertainty principle, and roughly how he defends that understanding by means of simple thought experiments. The main point is that Bohr believes that the ‘uncontrollable exchange’ of momentum and energy between a measured system and a measuring apparatus entails that those experimental situations that allow the determination of a particle’s position relative to a given reference frame forbid the determination of its (simultaneous) momentum relative to that frame, and similarly, those experimental situations that allow the determination of a particle’s momentum relative to a given frame—by means of an application of conservation laws—forbid the determination of its (simultaneous) position relative to that frame.

Let us turn, then, to Bohr’s realization of EPR’s particular case. He proposes a thought experiment to prepare the EPR state, and to perform the relevant measurements, as follows:

> The particular quantum-mechanical state of two free particles, for which they [EPR] give an explicit mathematical expression, may be reproduced, at least in principle, by a simple experimental arrangement, comprising a rigid diaphragm with two parallel slits, which are very narrow compared with their separation, and through each of which one particle with given initial momentum passes independently of the other. (Bohr, 1935, p. 699)

The arrangement as described thus far allows one to prepare the pair of particles in an eigenstate of $X_1 - X_2$, the eigenvalue being, of course, the distance between the slits (x_0 in EPR’s notation). In order to determine $P_1 + P_2$, Bohr proposes the following (a continuation of the quotation above):

> If the momentum of this diaphragm is measured accurately before as well as after the passing of the particles, we shall in fact know the sum of the components perpendicular to the slits of the momenta of the two escaping particles, as well as the difference of their initial positional coordinates in the same direction. (ibid.)

Thus, at this point in the description of the thought experiment, we have determined (or prepared) the values of $X_1 - X_2$ and $P_1 + P_2$ simultaneously.

The crucial question, now, is how one may go on to measure either X_1 or P_1, in order to determine either X_2 or P_2. Concerning the measurement of X_1, Bohr begins

> [T]o measure the position of one of the particles can mean nothing else than to establish a correlation between its behavior and some instrument rigidly fixed to the support which defines the space frame of reference. Under the experimental conditions described such a measurement will therefore also provide us with the knowledge of the location, otherwise completely unknown, of the diaphragm with repect to this space frame when the particles passed through the slits. Indeed, only in this way we obtain a basis for conclusions about the initial position of the other particle relative to the rest of the apparatus. (Bohr, 1935, p. 700)

Bohr has not yet arrived at his main point, but is here pointing out that, because the initial screen must be allowed to move freely with respect to the support (so that conservation of momentum can be applied to it plus the pair of particles), we do not know where it is relative to the support until we measure the position of one of the particles (relative to the support). After such a measurement, we can learn the position of the screen, because the particles are located where the slits in the screen are located. And once we know where the screen itself is in relation to the support, we can use our knowledge of X_1 to infer the location of the other particle, as Bohr says. Note, in particular, that Bohr nowhere supposes that the measurement of the position of the particle disturbs the screen.

Bohr continues:

> By allowing an essentially uncontrollable momentum to pass from the first particle into the mentioned support, however, we have by this procedure cut ourselves off from any future possibility of applying the law of conservation of momentum to the system consisting of the diaphragm and the two particles. (ibid.)

The consequence, as Bohr notes, is that in fact we *lose* the ability to predict the momentum of the second particle, *even if* we were (counterfactually, of course) to measure the momentum of the first particle. In the terms of the first section of this essay, Bohr has rejected 'strong non-disturbance', more or less for the reason suggested there: a measurement of X_1 necessarily destroys an essential feature of the compound system prior to measurement, that feature being the truth of the conditional: if we were to measure P_1, then we could predict (with certainty) P_2. (A more complete analysis of Bohr's position would require a longer discussion of the logic of counterfactuals, which we cannot pursue here.)

From this point of view, Beller and Fine's (1994) complaints against Bohr's thought experiment are not quite right. They make two complaints. First, they are unhappy with the fact that, in Bohr's arrangement, "we have no choice but to measure X_1 at the very moment of passage of the two particles through the first diaphragm" (Beller and Fine, 1994, p. 14). As I have already pointed out, however, there is really no choice. No quantum-mechanical state can evolve into the EPR state, and the EPR state cannot be preserved by any time evolution. Hence it can be the state of a system at, and only at, the moment of preparation. We can hardly fault Bohr for this situation.

Their second complaint arises from the first. They rightly point out that Bohr does not describe in any detail how the measurement of X_1 is to occur. Indeed, straightforwrd physical consideration of the situation seems to imply that any such measurement would involve a disturbance of the diaphragm with the two slits—either indirectly (for how could one interact with the particle without 'touching' the diaphragm?) or directly, by simply fixing the diaphragm to the support. Beller and Fine appear to opt for the latter. After apparently claiming (as I noted above) that EPR's case allows for the simultaneous determination of $X_1 - X_2$, $P_1 + P_2$ *and* either X_1 or P_1, they write:

> Bohr's double slit arrangement does not satisfy this requirement. In Bohr's example only one of $X_1 - X_2$ or $P_1 + P_2$ could be co-determined together with the variable [X_1 or P_1] one chooses to measure on particle 1. Indeed, we actually have to change the set-up of the two-slit diaphragm depending on whether we intend to measure position or momentum on particle 1. In the first case the two-slit diaphragm must be immovable; in the second case it must be moveable. (1994, p. 15)

Mainly because of this situation, Beller and Fine refer to Bohr's realization of EPR's argument a "flawed assimilation of EPR to a double slit experiment" (ibid., p. 16).

I suggest an alternative account. According to this account, Bohr completely ignores the fact—even if it follows from simple physical considerations—that a measurement of X_1 implies either a disturbance of the diaphragm or that it is fixed to the support. Instead, he is concerned to point out that a measurement of X_1 involves an uncontrollable exchange of momentum between particle 1 and the support that defines the space frame of reference, in *precisely* the same way that it does in the simpler cases discussed prior to EPR. Hence the momentum of particle 1 becomes undefined, and hence the total momentum (of the pair of particles) becomes undefined. Or to put the point in more Bohrian terms: conservation of momentum cannot be applied to the compound system, and therefore $P_1 + P_2$ is undefined, because in order for it to be defined, we must be able to apply conservation of momentum to the diaphragm plus the two particles.

At the very least, this account has the merit of following quite closely Bohr's account of the disturbance. He does not say that, in the measurement of X_1, momentum is exchanged between particle 1 and the diaphragm; nor does he ever suggest, in the EPR arrangement, that the diaphragm is fixed to the support. Rather, he says that "momentum [passes] from the first particle into the mentioned support" (Bohr, 1935, p. 700).

Similarly, in his account of what goes wrong when we measure P_1, he claims that such a measurement removes the possibility of determining the location of the diaphragm relative to the support. He could have two arguments in mind. First, along lines suggested by Beller and Fine, one might argue that any measurement of P_1 must involve a disturbance of (exchange of momentum with) the diaphragm, thereby disturbing its position relative to the support, because the measurement of P_1 must occur at the moment of preparation. Second, along the lines that are suggested here, one might argue that since the arrangement *requires* the diaphragm to move freely with respect to the support (lest we be unable to determine $P_1 + P_2$), the only way to determine the location of the diaphragm relative to the support would be to measure the position of one of the particles, relative to the support. But for reasons that were discussed prior to the case of EPR, measuring P_1 'cuts one off' from the possibility of determining particle 1's (and therefore the diaphragm's) position relative to the support.

4 Bohm's version of the argument

I finish with a brief comment regarding Bohm's (1951) alternate realization of the EPR state. The main point is that Bohm's realization does not involve position and momentum, but incompatible spin observables. There are two essential differences between this case and Bohr's (and EPR's). First, spin observables, while in a sense dependent on the specification of a spatial frame of reference (because we need to know *which* direction is, for example, the 'z' direction), are not bound up as closely with the very notion of a frame of reference. In particular, the sort of exchange that must occur between particle and apparatus in a measurement of spin does not seem to involve a disturbance of the very reference frame used to define the notion of 'direction of spin'. Second, spin is a conserved quantity (unlike position), so that the measurement of spin on one particle can be made long after the preparation of the particles.

It remains to be seen whether a Bohrian response of the EPR argument can be worked out in the case of spin. My suspicion is that the Bohrian response would at the least require significant revision. As far as I am aware, Bohr never reacted, publicly or privately, to Bohm's proposed thought experiment. (And, of course, it is more or less Bohm's version that was eventually performed.) However, the investigation of these questions must be preceded by a more complete account of Bohr's reply to EPR, to which the remarks here are at best a partial preface.

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Can Quantum-Mechanical Description of Physical Reality be Considered Complete?

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It is shown that a certain "criterion of physical reality" formulated in a recent article with the above title by A. Einstein, B. Podolsky and N. Rosen contains an essential ambiguity when it is applied to quantum phenomena. In this connection a viewpoint termed "complementarity" is explained from which quantum-mechanical description of physical phenomena would seem to fulfill, within its scope, all rational demands of completeness.

In a recent article[1] under the above title A. Einstein, B. Podolsky and N. Rosen have presented arguments which lead them to answer the question at issue in the negative. The trend of their argumentation, however, does not seem to me adequately to meet the actual situation with which we are faced in atomic physics. I shall therefore be glad to use this opportunity to explain in somewhat greater detail a general viewpoint, conveniently termed "complementarity," which I have indicated on various previous occasions,[2] and from which quantum mechanics within its scope would appear as a completely rational description of physical phenomena, such as we meet in atomic processes.

The extent to which an unambiguous meaning can be attributed to such an expression as "physical reality" cannot of course be deduced from *a priori* philosophical conceptions, but—as the authors of the article cited themselves emphasize—must be founded on a direct appeal to experiments and measurements. For this purpose they propose a "criterion of reality" formulated as follows: "If, without in any way disturbing a system, we can predict with certainty the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity." By means of an interesting example, to which we shall return below, they next proceed to show that in quantum mechanics, just as in classical mechanics, it is possible under suitable conditions to predict the value of any given variable pertaining to the description of a mechanical system from measurements performed entirely on other systems which previously have been in interaction with the system under investigation. According to their criterion the authors therefore want to ascribe an element of reality to each of the quantities represented by such variables. Since, moreover, it is a well-known feature of the present formalism of quantum mechanics that it is never possible, in the description of the state of a mechanical system, to attach definite values to both of two canonically conjugate variables, they consequently deem this formalism to be incomplete, and express the belief that a more satisfactory theory can be developed.

Such an argumentation, however, would hardly seem suited to affect the soundness of quantum-mechanical description, which is based on a coherent mathematical formalism covering automatically any procedure of measurement like that indicated.* The apparent contradiction in

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[1] A. Einstein, B. Podolsky and N. Rosen, Phys. Rev. **47**, 777 (1935).

[2] Cf. N. Bohr, *Atomic Theory and Description of Nature*, I (Cambridge, 1934).

* The deductions contained in the article cited may in this respect be considered as an immediate consequence of the transformation theorems of quantum mechanics, which perhaps more than any other feature of the formalism contribute to secure its mathematical completeness and its rational correspondence with classical mechanics. In fact, it is always possible in the description of a mechanical system, consisting of two partial systems (1) and (2), interacting or not, to replace any two pairs of canonically conjugate variables (q_1p_1), (q_2p_2) pertaining to systems (1) and (2), respectively, and satisfying the usual commutation rules

$$[q_1p_1]=[q_2p_2]=ih/2\pi,$$
$$[q_1q_2]=[p_1p_2]=[q_1p_2]=[q_2p_1]=0,$$

by two pairs of new conjugate variables (Q_1P_1), (Q_2P_2) related to the first variables by a simple orthogonal transformation, corresponding to a rotation of angle θ in the planes (q_1q_2), (p_1p_2)

$$q_1=Q_1\cos\theta-Q_2\sin\theta \qquad p_1=P_1\cos\theta-P_2\sin\theta$$
$$q_2=Q_1\sin\theta+Q_2\cos\theta \qquad p_2=P_1\sin\theta+P_2\cos\theta.$$

Since these variables will satisfy analogous commutation rules, in particular

$$[Q_1P_1]=ih/2\pi, \qquad [Q_1P_2]=0,$$

it follows that in the description of the state of the combined system definite numerical values may not be assigned to both Q_1 and P_1, but that we may clearly assign fact discloses only an essential inadequacy of the customary viewpoint of natural philosophy for a rational account of physical phenomena of the type with which we are concerned in quantum mechanics. Indeed the *finite interaction between object and measuring agencies* conditioned by the very existence of the quantum of action entails —because of the impossibility of controlling the reaction of the object on the measuring instruments if these are to serve their purpose—the necessity of a final renunciation of the classical ideal of causality and a radical revision of our attitude towards the problem of physical reality. In fact, as we shall see, a criterion of reality like that proposed by the named authors contains—however cautious its formulation may appear—an essential ambiguity when it is applied to the actual problems with which we are here concerned. In order to make the argument to this end as clear as possible, I shall first consider in some detail a few simple examples of measuring arrangements.

Let us begin with the simple case of a particle passing through a slit in a diaphragm, which may form part of some more or less complicated experimental arrangement. Even if the momentum of this particle is completely known before it impinges on the diaphragm, the diffraction by the slit of the plane wave giving the symbolic representation of its state will imply an uncertainty in the momentum of the particle, after it has passed the diaphragm, which is the greater the narrower the slit. Now the width of the slit, at any rate if it is still large compared with the wave-length, may be taken as the uncertainty Δq of the position of the particle relative to the diaphragm, in a direction perpendicular to the slit. Moreover, it is simply seen from de Broglie's relation between momentum and wave-length that the uncertainty Δp of the momentum of the particle in this direction is correlated to Δq by means of Heisenberg's general principle

$$\Delta p \Delta q \sim h,$$

which in the quantum-mechanical formalism is a direct consequence of the commutation relation for any pair of conjugate variables. Obviously the uncertainty Δp is inseparably connected with the possibility of an exchange of momentum between the particle and the diaphragm; and the question of principal interest for our discussion is now to what extent the momentum thus exchanged can be taken into account in the description of the phenomenon to be studied by the experimental arrangement concerned, of which the passing of the particle through the slit may be considered as the initial stage.

Let us first assume that, corresponding to usual experiments on the remarkable phenomena of electron diffraction, the diaphragm, like the other parts of the apparatus,—say a second diaphragm with several slits parallel to the first and a photographic plate,—is rigidly fixed to a support which defines the space frame of reference. Then the momentum exchanged between the particle and the diaphragm will, together with the reaction of the particle on the other bodies, pass into this common support, and we have thus voluntarily cut ourselves off from any possibility of taking these reactions separately into account in predictions regarding the final result of the experiment,—say the position of the spot produced by the particle on the photographic plate. The impossibility of a closer analysis of the reactions between the particle and the measuring instrument is indeed no peculiarity of the experimental procedure described, but is rather an essential property of any arrangement suited to the study of the phenomena of the type concerned, where we have to do with a feature of *individuality* completely foreign to classical physics. In fact, any possibility of taking into account the momentum exchanged between the particle and the separate parts of the apparatus would at once permit us to draw conclusions regarding the "course" of such phenomena,—say through what particular slit of the second diaphragm the particle passes on its way to the photographic plate—which would be quite incompatible with the fact that the probability of the particle reaching a given element of area on this plate is determined not by the presence of any particular slit, but by the positions of all the slits of the second diaphragm within reach

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such values to both Q_1 and P_2. In that case it further results from the expressions of these variables in terms of (q_1p_1) and (q_2p_2), namely

$$Q_1 = q_1 \cos\theta + q_2 \sin\theta, \qquad P_2 = -p_1 \sin\theta + p_2 \cos\theta,$$

that a subsequent measurement of either q_2 or p_2 will allow us to predict the value of q_1 or p_1 respectively.

of the associated wave diffracted from the slit of the first diaphragm.

By another experimental arrangement, where the first diaphragm is not rigidly connected with the other parts of the apparatus, it would at least in principle* be possible to measure its momentum with any desired accuracy before and after the passage of the particle, and thus to predict the momentum of the latter after it has passed through the slit. In fact, such measurements of momentum require only an unambiguous application of the classical law of conservation of momentum, applied for instance to a collision process between the diaphragm and some test body, the momentum of which is suitably controlled before and after the collision. It is true that such a control will essentially depend on an examination of the space-time course of some process to which the ideas of classical mechanics can be applied; if, however, all spatial dimensions and time intervals are taken sufficiently large, this involves clearly no limitation as regards the accurate control of the momentum of the test bodies, but only a renunciation as regards the accuracy of the control of their space-time coordination. This last circumstance is in fact quite analogous to the renunciation of the control of the momentum of the fixed diaphragm in the experimental arrangement discussed above, and depends in the last resort on the claim of a purely classical account of the measuring apparatus, which implies the necessity of allowing a latitude corresponding to the quantum-mechanical uncertainty relations in our description of their behavior.

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, this body can no longer be used as a measuring instrument for the same purpose as in the previous case, 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. In fact, even if we knew the position of the diaphragm relative to the space frame before the first measurement of its momentum, and even though its position after the last measurement can be accurately fixed, we lose, on account of the uncontrollable displacement of the diaphragm during each collision process with the test bodies, the knowledge of its position when the particle passed through the slit. The whole arrangement is therefore obviously unsuited to study the same kind of phenomena as in the previous case. In particular it may be shown that, if the momentum of the diaphragm is measured with an accuracy sufficient for allowing definite conclusions regarding the passage of the particle through some selected slit of the second diaphragm, then even the minimum uncertainty of the position of the first diaphragm compatible with such a knowledge will imply the total wiping out of any interference effect—regarding the zones of permitted impact of the particle on the photographic plate—to which the presence of more than one slit in the second diaphragm would give rise in case the positions of all apparatus are fixed relative to each other.

In an arrangement suited for measurements of the momentum of the first diaphragm, it is further clear that even if we have measured this momentum before the passage of the particle through the slit, we are after this passage still left with a *free choice* whether we wish to know the momentum of the particle or its initial position relative to the rest of the apparatus. In the first eventuality we need only to make a second determination of the momentum of the diaphragm, leaving unknown forever its exact position when the particle passed. In the second eventuality we need only to determine its position relative to the space frame with the inevitable loss of the knowledge of the momentum exchanged between the diaphragm and the particle. If the diaphragm is sufficiently massive in comparison with the particle, we may even arrange the procedure of measurements in such a way that the diaphragm after the first determination of its momentum will remain at rest in some unknown position relative to the

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* The obvious impossibility of actually carrying out, with the experimental technique at our disposal, such measuring procedures as are discussed here and in the following does clearly not affect the theoretical argument, since the procedures in question are essentially equivalent with atomic processes, like the Compton effect, where a corresponding application of the conservation theorem of momentum is well established.

other parts of the apparatus, and the subsequent fixation of this position may therefore simply consist in establishing a rigid connection between the diaphragm and the common support.

My main purpose in repeating these simple, and in substance well-known considerations, is to emphasize that in the phenomena concerned we are not dealing with an incomplete description characterized by the arbitrary picking out of different elements of physical reality at the cost of sacrifying other such elements, but with a rational discrimination between essentially different experimental arrangements and procedures which are suited either for an unambiguous use of the idea of space location, or for a legitimate application of the conservation theorem of momentum. Any remaining appearance of arbitrariness concerns merely our freedom of handling the measuring instruments, characteristic of the very idea of experiment. In fact, the renunciation in each experimental arrangement of the one or the other of two aspects of the description of physical phenomena,—the combination of which characterizes the method of classical physics, and which therefore in this sense may be considered as *complementary* to one another,—depends essentially on the impossibility, in the field of quantum theory, of accurately controlling the reaction of the object on the measuring instruments, i.e., the transfer of momentum in case of position measurements, and the displacement in case of momentum measurements. Just in this last respect any comparison between quantum mechanics and ordinary statistical mechanics,—however useful it may be for the formal presentation of the theory,—is essentially irrelevant. Indeed we have in each experimental arrangement suited for the study of proper quantum phenomena not merely to do with an ignorance of the value of certain physical quantities, but with the impossibility of defining these quantities in an unambiguous way.

The last remarks apply equally well to the special problem treated by Einstein, Podolsky and Rosen, which has been referred to above, and which does not actually involve any greater intricacies than the simple examples discussed above. The particular quantum-mechanical state of two free particles, for which they give an explicit mathematical expression, may be reproduced, at least in principle, by a simple experimental arrangement, comprising a rigid diaphragm with two parallel slits, which are very narrow compared with their separation, and through each of which one particle with given initial momentum passes independently of the other. If the momentum of this diaphragm is measured accurately before as well as after the passing of the particles, we shall in fact know the sum of the components perpendicular to the slits of the momenta of the two escaping particles, as well as the difference of their initial positional coordinates in the same direction; while of course the conjugate quantities, i.e., the difference of the components of their momenta, and the sum of their positional coordinates, are entirely unknown.* In this arrangement, it is therefore clear that a subsequent single measurement either of the position or of the momentum of one of the particles will automatically determine the position or momentum, respectively, of the other particle with any desired accuracy; at least if the wave-length corresponding to the free motion of each particle is sufficiently short compared with the width of the slits. As pointed out by the named authors, we are therefore faced at this stage with a completely free choice whether we want to determine the one or the other of the latter quantities by a process which does not directly interfere with the particle concerned.

Like the above simple case of the choice between the experimental procedures suited for the prediction of the position or the momentum of a single particle which has passed through a slit in a diaphragm, we are, in the "freedom of choice" offered by the last arrangement, just concerned with a *discrimination between different experimental procedures which allow of the unambiguous use of complementary classical concepts.* In fact to measure the position of one of the particles can mean nothing else than to establish a correlation between its behavior and some

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* As will be seen, this description, apart from a trivial normalizing factor, corresponds exactly to the transformation of variables described in the preceding footnote if $(q_1 p_1)$, $(q_2 p_2)$ represent the positional coordinates and components of momenta of the two particles and if $\theta = -\pi/4$. It may also be remarked that the wave function given by formula (9) of the article cited corresponds to the special choice of $P_2 = 0$ and the limiting case of two infinitely narrow slits.

instrument rigidly fixed to the support which defines the space frame of reference. Under the experimental conditions described such a measurement will therefore also provide us with the knowledge of the location, otherwise completely unknown, of the diaphragm with respect to this space frame when the particles passed through the slits. Indeed, only in this way we obtain a basis for conclusions about the initial position of the other particle relative to the rest of the apparatus. By allowing an essentially uncontrollable momentum to pass from the first particle into the mentioned support, however, we have by this procedure cut ourselves off from any future possibility of applying the law of conservation of momentum to the system consisting of the diaphragm and the two particles and therefore have lost our only basis for an unambiguous application of the idea of momentum in predictions regarding the behavior of the second particle. Conversely, if we choose to measure the momentum of one of the particles, we lose through the uncontrollable displacement inevitable in such a measurement any possibility of deducing from the behavior of this particle the position of the diaphragm relative to the rest of the apparatus, and have thus no basis whatever for predictions regarding the location of the other particle.

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

The experimental arrangements hitherto discussed present a special simplicity on account of the secondary role which the idea of time plays in the description of the phenomena in question. It is true that we have freely made use of such words as "before" and "after" implying time-relationships; but in each case allowance must be made for a certain inaccuracy, which is of no importance, however, so long as the time intervals concerned are sufficiently large compared with the proper periods entering in the closer analysis of the phenomenon under investigation. As soon as we attempt a more accurate time description of quantum phenomena, we meet with well-known new paradoxes, for the elucidation of which further features of the interaction between the objects and the measuring instruments must be taken into account. In fact, in such phenomena we have no longer to do with experimental arrangements consisting of apparatus essentially at rest relative to one another, but with arrangements containing moving parts,—like shutters before the slits of the diaphragms,—controlled by mechanisms serving as clocks. Besides the transfer of momentum, discussed above, between the object and the bodies defining the space frame, we shall therefore, in such arrangements, have to consider an eventual exchange of energy between the object and these clock-like mechanisms.

The decisive point as regards time measurements in quantum theory is now completely analogous to the argument concerning measurements of positions outlined above. Just as the transfer of momentum to the separate parts of the apparatus,—the knowledge of the relative positions of which is required for the description of the phenomenon,—has been seen to be entirely uncontrollable, so the exchange of energy between the object and the various bodies, whose relative motion must be known for the intended use of the apparatus, will defy any closer analysis. Indeed, it is *excluded in principle to control the energy which goes into the clocks without interfering essentially with their use as time indicators.* This use in fact entirely relies on the assumed possibility of accounting for the functioning of each clock as well as for its eventual comparison with other clocks on the basis of the methods of classical physics. In this account we must therefore obviously allow for a latitude in the energy balance, corresponding to the quantum-mechanical uncertainty relation for the conjugate time and energy variables. Just as in the question discussed above of the mutually exclusive character of any unambiguous use in quantum theory of the concepts of position and momentum, it is in the last resort this circumstance which entails the complementary relationship between any detailed time account of atomic phenomena on the one hand and the unclassical features of intrinsic stability of atoms, disclosed by the study of energy transfers in atomic reactions on the other hand.

This necessity of discriminating in each experimental arrangement between those parts of the physical system considered which are to be treated as measuring instruments and those which constitute the objects under investigation may indeed be said to form a *principal distinction between classical and quantum-mechanical description of physical phenomena.* It is true that the place within each measuring procedure where this discrimination is made is in both cases largely a matter of convenience. While, however, in classical physics the distinction between object and measuring agencies does not entail any difference in the character of the description of the phenomena concerned, its fundamental importance in quantum theory, as we have seen, has its root in the indispensable use of classical concepts in the interpretation of all proper measurements, even though the classical theories do not suffice in accounting for the new types of regularities with which we are concerned in atomic physics. In accordance with this situation there can be no question of any unambiguous interpretation of the symbols of quantum mechanics other than that embodied in the well-known rules which allow to predict the results to be obtained by a given experimental arrangement described in a totally classical way, and which have found their general expression through the transformation theorems, already referred to. By securing its proper correspondence with the classical theory, these theorems exclude in particular any imaginable inconsistency in the quantum-mechanical description, connected with a change of the place where the discrimination is made between object and measuring agencies. In fact it is an obvious consequence of the above argumentation that in each experimental arrangement and measuring procedure we have only a free choice of this place within a region where the quantum-mechanical description of the process concerned is effectively equivalent with the classical description.

Before concluding I should still like to emphasize the bearing of the great lesson derived from general relativity theory upon the question of physical reality in the field of quantum theory. In fact, notwithstanding all characteristic differences, the situations we are concerned with in these generalizations of classical theory present striking analogies which have often been noted. Especially, the singular position of measuring instruments in the account of quantum phenomena, just discussed, appears closely analogous to the well-known necessity in relativity theory of upholding an ordinary description of all measuring processes, including a sharp distinction between space and time coordinates, although the very essence of this theory is the establishment of new physical laws, in the comprehension of which we must renounce the customary separation of space and time ideas.*

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* Just this circumstance, together with the relativistic invariance of the uncertainty relations of quantum mechanics, ensures the compatibility between the argumentation outlined in the present article and all exigencies of relativity theory. This question will be treated in greater detail in a paper under preparation, where the writer will in particular discuss a very interesting paradox suggested by Einstein concerning the application of gravitation theory to energy measurements, and the solution of which offers an especially instructive illustration of the generality of the argument of complementarity. On the same occasion a more thorough discussion of space-time measurements in quantum theory will be given with all necessary mathematical developments and diagrams of experimental

The dependence on the reference system, in relativity theory, of all readings of scales and clocks may even be compared with the essentially uncontrollable exchange of momentum or energy between the objects of measurements and all instruments defining the space-time system of reference, which in quantum theory confronts us with the situation characterized by the notion of complementarity. In fact this new feature of natural philosophy means a radical revision of our attitude as regards physical reality, which may be paralleled with the fundamental modification of all ideas regarding the absolute character of physical phenomena, brought about by the general theory of relativity.

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arrangements, which had to be left out of this article, where the main stress is laid on the dialectic aspect of the question at issue.