Einstein's Critique of Quantum Theory: The Roots and Significance of EPR

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The matrix mechanics path to quantum theory was initiated by Heisenberg in the summer of 1925. Dirac received a prepublication copy of Heisenberg's paper and by November he had worked out his own beautiful generalization of the ideas. Independently, and almost at the same time, Schrödinger was developing the wave mechanics route, marked out in his "first communication" at the end of January 1926.[1]

In the early months of 1926 Einstein corrresponded with both Heisenberg and Schrödinger about their work on quantum phenomena. As spring began he summed up his own impressions of the quantum world in letters to Ehrenfest and Lorentz. Heisenberg came to Berlin that spring to talk with Einstein and to the colloquium there, and later, in July, Schrödinger came as well. Thus before the new ideas were a year old, Einstein had acquired first-hand knowledge of them from the originators themselves. Einstein, whose own work on quantum problems played an important role in both lines of development, reacted to the new theory with an uncharacteristic ambivalence.

In a letter to Ehrenfest (February 12, 1926), Einstein criticized the matrix mechanics, focusing on the fact that it is not relativistically invariant and wondering whether the commutation relations for position and linear momentum actually hold in all cases. Yet a few weeks later (March 7, 1926) he wrote to Hedi Born, "The

Unless a published source is cited, references to the Einstein correspondence and papers are to material on microfilm in the Einstein Archives, housed in the Rare Book Room of the Firestone Library at Princeton University. The correspondence is indexed alphabetically for the correspondents cited. The unpublished manuscripts are on reel number two, IA2–IA7. I am grateful to David Malament and Michael Ferguson for help with the German, and to Micky Forbes for assistance with its English. I am responsible, however, for the translations as given. I want to acknowledge the cooperation of the trustees of the Estate of Albert Einstein with regard to the use of these materials, and to thank them for their permission to reproduce portions here. Part of the research for this study was supported by National Science Foundation Grant # SOC 76-82113.

1. See respectively Heisenberg (1925), Dirac (1925), and Schrödinger (1926).

Heisenberg-Born concepts leave us all breathless, and have made a deep impression on all theoretically oriented people. Instead of dull resignation, there is now a singular tension in us sluggish people" (Born 1971, p. 88). Within the week, however, Einstein wrote to Lorentz (March 13, 1926) that despite a great deal of effort in studying the Heisenberg-Born theory his instincts still resisted their way of conceiving things. He went on, in this letter, to recommend Schrödinger's work as a promising development of de Broglie's ideas. Responding to an earlier request from Lorentz that he address the fifth Solvay Conference (to be held in Brussels in October 1927), Einstein claimed to have nothing new to say and recommended that Schrödinger be invited in his stead. Einstein's endorsement of wave mechanics, however, was short lived. In April he expressed enthusiasm for Schrödinger's work to Ehrenfest, but by January (1927) he remarked to him, "My heart does not warm toward Schrödingerei—it is uncausal and altogether too primitive." ("Mein Herz wird nicht warm bei der Schrödingerei—sie ist unkausal and überhaupt zu primitiv.")

I do not think this remark represents some special predilection for causality, rather it is probably Einstein's way of acknowledging to Ehrenfest that wave mechanics is no better, in terms of causality, than is matrix mechanics. In any case, Einstein wrote a critique of wave mechanics early in 1927. (It may be that this critique, which was never published, was made in preparation for the October Solvay Conference. For Einstein had finally yielded to Lorentz's strong personal request and agreed to give a talk on quantum statistics—although, in fact, the talk was never given.) In these notes he criticizes wave mechanics on three grounds: he thinks that because of superposition it will be difficult to recover the classical Hamilton-Jacobi equations even as an approximation; he is worried that the treatment of many-electron systems in configuration space involves correlations between the electrons that violate the principle of action-by-contact; and he thinks that one will have to renounce the treatment of individual systems and will find that the theory offers at best a descriptive completeness only in the sense of statistics.[2]

If we put this critique of Schrödinger's wave mechanics together with the reservations expressed to Ehrenfest over Heisenberg's

2. In note 26 of chapter 6 I point out that what I call here a critique of wave mechanics is actually an unpublished attempt at a hidden variables interpretation (the *Bestimmt* manuscript referred to in that note), together with Einstein's own critique of that attempt. Between the project attempted in the manuscript and his critique of it the three points mentioned in my text do emerge. My thanks to Paul Forman for catching my mistakes here.

matrix mechanics, we find that by the spring of 1927 Einstein had already arrived at the following lines of criticism of the newly emerging quantum theory: (1) the equations of the theory are not relativistically invariant; (2) it does not yield the classical behavior of macroscopic objects to a good approximation; (3) it leads to correlations among spatially separated objects that appear to violate action-by-contact principles; (4) it is an essentially statistical theory that seems incapable even of *describing* the behavior of individual systems; and (5) the scope of the commutation relations may not in fact be so broad as the theory supposes.

Einstein's disagreement with the quantum theory is well known, and it seems to be widely believed that this disagreement is a reaction to the uncertainty formulas of the theory and largely directed at the refutation of them. This is the image, for example, that emerges from Bohr's (1949) retelling of his "debates" with Einstein. It is also the dominant theme of Jammer's detailed story of the Bohr-Einstein dispute up to 1930 (Jammer 1974, pp. 120, 132, 136). It is therefore important to note that Einstein could only have known of the uncertainty formulas in April 1927, for that is when he received a prepublication copy of Heisenberg's (1927) fundamental paper. Thus the five lines of criticism assembled above constitute Einstein's reaction to the quantum theory prior even to the formulation of the uncertainty formulas. Only item (5) on that list is directly relevant to those formulas, and that item is more by way of a question than an objection. It appears, then, that Einstein's initial disagreements with the quantum theory did not have to do with the uncertainty relations but were broader in scope than those relations, and perhaps also more central. Moreover, I believe that these initial disagreements were the ones that lasted, as the subsequent story will show.

Einstein attended the fifth Solvay Conference in October 1927, although he did not give the address on quantum statistics that he had promised Lorentz. He did, however, make a few remarks in discussion on the last day, notes of which he enclosed in a letter to Lorentz on November 21, 1927. His remarks were not addressed to the uncertainty relations, but rather to the question of a statistical versus a complete individual interpretation of the theory. He argued that if the state function were interpreted as expressing probabilities for finding properties of an individual system, then the phenomenon of the collapse of the wave packet would represent a peculiar action-at-a distance. The collapse prevents a particle, whose wave function is continuously distributed over some region of space, from producing an effect at two places in the region at once. It thus represents a peculiar nonlocalized mechanism which, Einstein suggests, violates relativity. Moreover, he thought that the representation of many-particle systems in configuration space raises two problems: how to obtain the Einstein-Bose statistics, and how to formulate the idea of forces acting only over small spatial distances. These are problems, however, not of the theory itself but of the interpretation according to which the theory gives a complete statistical description of individual systems. The alternative is to interpret the state function as providing information only about the distribution of an ensemble of systems and not about features of the individual systems themselves.

If Bohr's (1949) account of the unrecorded discussions at that meeting is reliable, then these remarks of Einstein's led to an informal discussion over the possibilities for more complete descriptions of individual systems. In this context, according to Bohr, Einstein raised the issue as to whether it might be possible in the case of a double slit experiment to determine both where the particle lands on the detecting screen and through which slit it has passed. In the ensuing discussion Bohr was able to show, apparently, that the possibilities for controlling the transfer of momentum to the diaphragm as the particle passes through are constrained by the uncertainty formulas in such a way as to preclude the required determinations. In effect, Bohr argued that one measurement (at the slits) disturbs the subsequent behavior (i.e., where the particle lands).

There is no record of Einstein's response to these discussions. But Bohr does say that at their conclusion Einstein asked "whether we could really believe that the providential authorities took recourse to dice-playing [ob der liebe Gott würfelt]" (Bohr 1949, p. 218). Within a fortnight Einstein wrote, in a letter to Sommerfeld, "On 'Quantum Mechanics' I think that, with respect to ponderable matter, it contains roughly as much truth as the theory of light without quanta. It may be a correct theory of statistical laws, but an inadequate conception of individual elementary processes."[3] It appears, then, that Einstein left the conference convinced that the only viable interpretation for the quantum theory was the statistical one he had suggested, and not convinced that it was impossible to build conceptions of "individual elementary processes" into a better theory.

Except for the problem of macroscopic approximation, one can see in Einstein's discussions throughout this time expressions of the same concerns that he had accumulated by the spring of 1927. In

3. This is letter 53 of November 9, 1927, in Hermann (1968). I have used the translation by R. and R. Stuewer in a preprint translation of Hermann's collection.

the period between the fifth Solvay Conference and the sixth one (October 1930), I can find nothing in Einstein's published work nor in his correspondence (both published and unpublished) that represents any shift or alteration in these concerns. In correspondence with Schrödinger, however, he does indicate an important conclusion about the quantum theory that he had not acknowledged previously. In the letter of May 31, 1928, Einstein notes his agreement with Schrödinger's conclusion that the limitations on the applicability of the classical concepts that are embedded in the uncertainty formulas indicate the need to replace those concepts by new ones. Einstein wrote, "Your claim that the concepts p, q [momentum, position] will have to be given up, if they can only claim such 'shaky' meaning seems to be fully justified" (Przibam 1967, p. 31). Einstein readily assimilated this conclusion to his concern over the completeness of the theory so that he would later write (in 1936) that quantum theory "is an incomplete representation of real things, although it is the only one which can be built out of the fundamental concepts of force and material points (quantum corrections to classical mechanics)" (Einstein 1954, pp. 315–16).

During the sixth Solvay Conference, Einstein once again made his only contribution by way of the discussion. He continued, here, the informal discussion of the possibility for the description of individual systems, this time explicitly directed to test the limitation imposed by the energy-time uncertainty formula. Einstein proposed a simple thought experiment involving the time at which a photon escapes from a box and which seemed to get around this limitation. Bohr, after a sleepless night, was able to use Einstein's own gravitational redshift formula to show that the determination of the time of the energy change was in fact limited by Heisenberg's relation. Once again Bohr showed how the measurement of one parameter (here weighing the box-plus-clock) directly interfered with the determination of another (here the clockrate). According to Bohr (1949, p. 226), Einstein contributed effectively in helping to work out Bohr's argument against Einstein's own speculation.

That spirit accords well with Einstein's scientific character, and with the fact that nowhere after 1930 do we find Einstein questioning the general validity of the uncertainty formulas.[4] Indeed the following year he published an article, jointly with Tolman and

4. Jammer (1974), p. 136, interprets this as Einstein's turning from a search for the internal inconsistency of the quantum theory to a demonstration of its incompleteness. But this is wrong on both counts for, as we have seen, the issue of completeness was Einstein's concern from the beginning, whereas nowhere do I find him trying to show the inconsistency of the theory.

Podolsky (1931), in which he argued that certain apparent possibilities for determining the history of a particle with an accuracy greater than that allowed by the uncertainty formulas would, in fact, permit violations of those forumlas. Since these possibilities had been allowed by both Bohr and Heisenberg (and simply dismissed with the positivist disclaimer that they would have no predictive value), we find that here Einstein is more orthodox about the uncertainty formulas than are the orthodox themselves.

If Einstein came away from the discussions at the Solvay conferences convinced of the general validity of the uncertainty relations, he must also have seen that the key to Bohr's interpretation of the theory lay in the doctrine of disturbance. For the idea that Bohr had twice used to undermine Einstein's attempts to get at a detailed description of individual systems was the doctrine that certain simultaneous determinations were not possible because any one of them would inevitably disturb the physical situation so as to preclude the others. It was probably clear to Einstein after the 1930 conference that to defend his own statistical interpretation he must somehow neutralize the doctrine of disturbance to be able to demonstrate the existence of real physical attributes that are left unattended by the theory, except insofar as they have statistical significance.

Once he was settled with regard to the uncertainty formulas, Einstein's five original objections to the theory were reduced to four. Of these, two concerned external constraints imposed by other theories: how to reconcile the quantum theory with the requirements of relativity and how to achieve a satisfactory classical approximation from the quantum theory. From his own brilliant work on relativity, Einstein understood that such external constraints are guideposts for the construction of new physical concepts. He knew, therefore, that to develop the new concepts to replace the classical ones it would be necessary to attend to such constraints. He believed, moreover, that working from the relativistic framework was a likely starting point (see chapter 2 for a discussion of Einstein's ideas in this area). But if one were interested in interpreting the new theory, which already relied so heavily on the classical concepts themselves, it may well have seemed reasonable to bracket off the anomalies engendered by these external constraints, at least for a while. If Einstein thought in these ways, then he would have two central problems left. One concerned the question of distant correlations and action-by-contact in the theory, and the other was the central issue of statistics and the description of individual systems. Notice that these very same concerns were the ones expressed by Einstein in the 1927 Solvay

Conference. He was to make one more attempt to combine them so as to neutralize Bohr's doctrine of disturbance and to support his own statistical interpretation.

On March 25, 1935 the editors of *Physical Review* received a short manuscript coauthored by Albert Einstein, Boris Poldolsky, and Nathan Rosen. The paper was published in the May 15, 1935, issue with the awkward title "Can Quantum Mechanical Description of Physical Reality Be Considered Complete?" Most often referred to by slogans incorporating the acronym "EPR," this short (four-page) article is the source of a voluminous published commentary and is the touchstone for several attempts to interpret (or to reïnterpret) the formalism of quantum theory.[5]

The argument of the paper is concerned with two assertions:

(INC) *The quantum mechanical description of a system given by the state function is incomplete* (as they say, not "every element of physical reality has a counterpart in the theory").

(NSV) *Observables represented by noncommuting operators cannot have simultaneous reality* (i.e., cannot have simultaneously sharp values).

The argument develops in two parts. The first part demonstrates the validity of the disjunction[6]

(INC) v (NSV).

The second part shows the validity of the conditional

~(INC) → ~(NSV).

The authors then conclude from this that

(INC)

must hold.

We can represent the logical structure of the argument as follows: From PvQ and ~P → ~Q, infer P. And one can show the validity of the argument by reasoning that if ~P then, by the second premise, one can get ~Q; and then, by the first premise, one can get P. So it follows from the premises that if ~P, then P. Since clearly if P, then P, the conclusion P follows by a simple constructive dilemma. Of course this is not the only way to get the conclusion from the premises, but it is perhaps the most straightforward way. (The authors do not give the reasoning, they just draw the conclusion.) The point I want to emphasize is that even from the point

5. See Hooker (1972) and the extended discussion in Jammer (1974).

6. For compactness, below I use standard logical symbols: "PvQ" for "P or Q," "~P" for "it is not the case that P," and "P → Q" for "if P, then Q."

of view of elementary logic, the argument of the paper appears quite complex. The subarguments are even more so.

To establish the disjunction, (INC) v (NSV), the authors show that ~(NSV) → (INC). Thus they suppose that a pair of noncommuting observables of a system have simultaneous values and they note that no state of the system is simultaneously an eigenstate for both observables. Hence they conclude that the description given by the state function for such a system would be incomplete.

To establish the conditional, ~(INC) → ~(NSV), the authors assume the antecedent (i.e., that the theory is complete) and try to establish the existence of simultaneous values for position and linear momentum (in the same direction) in a certain interesting system. This is a system consisting of two particles that interact so as to preserve total linear momentum (in a certain direction) and then fly apart in opposite directions so as to preserve their relative positions (in the same direction). The authors argue from the interaction formalism of the theory that, at least theoretically, there are such correlated two-particle systems. They then introduce the following *criterion of reality:* "If, without in any way disturbing a system, we can predict with certainty (that is, with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity" (EPR 1935, p. 777). In the case of the hypothetical correlated system, we can predict, from a measurement of the position of one system, the position of the other, and similarly with regard to linear momentum. If the systems are allowed to separate far enough spatially, there can be no question of the measurement of one system disturbing the other. Hence the authors invoke the criterion of reality to conclude that for such a system at least one particle must have simultaneously definite position and momentum. Since this is the desired conclusion in this part of the argument, the inference is achieved.

I shall reserve comments on the general argument for awhile, but there are several features of this second stage that I should mention. One is to note that the assumption of completeness is never actually used here, the authors simply show (or try to) that a certain system has simultaneous position and momentum. Thus they establish the conditional

~(INC) → ~(NSV)

simply by deriving the consequent, ~(NSV). But if they had just stated this as their objective in this second part, then the conclusion (INC) would follow immediately from the disjunction

(INC) v (NSV)

of part one. So once again the form of argument seems strangely complex. Moreover, the actual text is not as clear as I have made out. For the authors digress (or so it seems) to point out that by choosing to measure either position or momentum on one particle of the pair, one can alter the postmeasurement state function for the other particle at will.[7] And they seem to think that this runs counter to the assumed completeness. Finally it is by no means clear how, even with the stated criterion of reality, the fact that one can assign either a definite position or a definite momentum to the unmeasured particle establishes that the particle has *both* properties at once. Surely, the argument in this second part is both tangled and flawed.

Despite these difficulties of style and logic, I think one can see here just that combination of the ideas of correlated system, action-by-contact, and descriptive incompleteness that Einstein required to provide the background for his statistical interpretation. Moreover, the criterion of reality is clearly aimed at Bohr's doctrine of disturbance. It did not miss its target for, as Rosenfeld recalls, "This onslaught came down on us as a bolt from the blue. Its effect on Bohr was remarkable" (Rozental 1967, p. 128). And Rosenfeld goes on to tell how he worked with Bohr "day after day, week after week" to formulate a response. The response was announced on June 29, 1935, in a letter to the editor of *Nature* and spelled out in a longer paper (six pages to EPR's four) published in *Physical Review* (Bohr 1935). The announcement focused on EPR's criterion of reality which, in a typical phrase, Bohr said "contains an essential ambiguity." It was then precisely the question of disturbance to which Bohr responded. For he argued that the phrase "without in any way disturbing a system" was the ambiguous culprit. There was, he admitted, no question ("of course") of a physical ("mechanical") disturbance of one system brought about by measuring its correlated twin, "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.*"

I want to point out two significant features of Bohr's response. The first is that what Bohr himself underlines (the italics are his) is virtually textbook neopositivism. For Bohr simply identifies the attribution of properties with the possible types of predictions of future behavior. (I think this point needs emphasizing, for many commentators seem inclined to suppose that Bohr's tendency to

7. The significance of this "digression" is discussed in chapter 4, section 2, and in connection with "bijective completeness" in chapter 5.

obscure language is a token of philosophical depth, whereas I find that, as here, where it really matters Bohr invariably lapses into positivist slogans and dogmas.) The second feature, and one which neither Bohr nor his commentators have acknowledged, is that Bohr's response to EPR marks a definite break from his previously stated view. For in earlier writings and in his response to Einstein at the Solvay conferences, Bohr had always argued that the disturbance created by a measurement of a particular variable caused a real change in the physical situation which altered the preconditions for applying complementary variables. But here Bohr switched from this doctrine of actual physical disturbance to what one might call a doctrine of semantic disturbance. In a way that Bohr does not account for on physical grounds, the arrangement to measure, say, the position of one particle in a pair simply precludes meaningful talk of the linear momentum of the unmeasured (and admittedly undisturbed) other particle. I think it is fair to conclude that the EPR paper did succeed in neutralizing Bohr's doctrine of disturbance. It forced Bohr to retreat to a merely semantic disturbance and thereby it removed an otherwise plausible and intuitive physical basis for Bohr's ideas.

If Bohr's response to EPR is the most famous, it was nevertheless not the first. An equally important response was written a few days earlier, on June 19, 1935, and it was by Einstein himself. On that day Einstein responded to a June 7 letter from Schrödinger. In that letter Schrödinger had reminded Einstein of their discussions in Berlin (presumably in the summer of 1926) about "the dogmatic quantum theory," and responded to the calculations of EPR.[8]

In the June 19 letter Einstein wrote about EPR as follows: "For reasons of language this [paper] was written by Podolsky after much discussion. Still, it did not come out as well as I had originally wanted; rather the essential thing was, so to speak, smothered by the formalism [gelehrsamkeit]."[9]

I think we should take in the message of these few words: Einstein did not write the paper, Podolsky did, and somehow the central point was obscured. No doubt Podolsky (of Russian origin) would have found it natural to leave the definite article out of the title. Moreover the logically opaque structure of the piece is uncharacteristic of Einstein's thought and writing. There are no earlier drafts of this article among Einstein's papers and no corre-

8. See chapter 5 for further details of this letter and of Einstein's reply.

9. "Diese ist aus Sprachgründen von Podolsky geschrieben nach vielen Diskussionen. Es ist aber doch nicht so gut herausgekommen, was ich eigentlich wollte; sondern die Hauptsache ist sozusagen durch Gelehrsamkeit verschüttet."

spondence or other evidence that I have been able to find which would settle the question as to whether Einstein saw a draft of the paper before it was published. Podolsky left Princeton for California at about the time of submission and it could well be that, authorized by Einstein, he actually composed it on his own.

In any case, Einstein goes on in the June 19 letter to sketch the essential features which were obscured. He first tries to convey the sense of the assertion of incompleteness by means of the following illustration. Consider a ball located in one of two closed boxes. An incomplete description of this "reality" might be, for example, "The probability is one-half that the ball is in the first box." A complete description would be, for example, "The ball *is* in the first box." Thus an incomplete description is a probabilistic assertion, with probability less than unity, *made in circumstances in which there is some further truth that could be told.* This seems like an elementary and intuitive idea for incompleteness, but how are we to know whether there is some further truth to be told? That is, of course, the problem of measurement disturbance: does the measured result, so to speak, arise with the measurement or, rather, does the measurement simply reflect what is already there?

Einstein addresses this issue in the letter by continuing with the illustration as follows. He acknowledges that one cannot sort things out without assuming something more, and he then proposes to assume a principle of separation (Trennungsprinzip): "the contents of the second box are independent of what happens to the first box."[10]

If one now assumes an obvious conservation law, that balls are neither created nor destroyed, then I can find out by looking in the first box whether or not the ball is in the second box. (If I find it in the first box, it is not in the second box. If I do not find it in the first box, it is in the second box.) If my theory only allows, in these circumstances, probabilistic assertions (with probability less than unity), then my theory is incomplete. Thus, given the conservation law, the principle of separation would imply the incompleteness of my theory.

Einstein continues in this letter to give a technical reformulation of the EPR argument. It is a little confusing because it introduces

10. Einstein puts it delightfully like this: The separation principle is needed in order to get past the Talmudists. For "the Talmudic philosopher sniffs at 'reality' as at a frightening creature of the naive mind." ("Der Talmudistische Philosoph . . . pfeift auf die 'Wirklichkeit' als auf einen Popanz der Naivität . . .") Although no reference is made, I would guess that Bohr should be counted here as a Talmudist. The obvious ones are the positivists.

a further refinement of the idea of completeness (this time in terms of state functions correlated to real states of affairs). But I think there is enough material contained, as it were, in Einstein's boxes to give at least one formulation of some of the essentials of EPR that were obscured by Podolsky's exposition.

Consider the system of two particles correlated via the conservation law for total linear momentum. Separation is the claim that whether a physical property holds for one of the particles does not depend on measurements (or other interactions) made on the other particle when the pair is widely separated in space. Completeness is the claim that if a certain physical property in fact holds for one particle at a given time, then the state function for the combined system at that time should yield probability one for finding that the property does hold (i.e., the subsystem consisting of the particle should have a state function which is an eigenstate for the property in question).

One can now copy Einstein's box argument as follows. Suppose the two particles (A and B) are far apart and I measure, say, particle A for linear momentum (in a certain direction). Using the conservation law I can infer the linear momentum of particle B from the result of this measurement on A. Thus after the A measurement, the B particle has a certain linear momentum. By separation, this real property of B must have held already at the time when I began my measurement on A (or just before, in the case of an instantaneous measurement). For otherwise I would have created the momentum at B by measuring A, in violation of separation. But at the initial moment of the A measurement, the state of the composite system does not yield probability one for finding *any* momentum value for B, for that state is a nontrivial superposition of products of "momentum eigenstates" for the A and B subsystems. Hence the description provided by the state function given by quantum theory is incomplete. Here, as in the illustration, the argument establishes the incompatibility of separation and completeness.

It is this incompatibility that I take to be the central conclusion, which got obscured in EPR. Many years later, in Schilpp (1949, p. 682) Einstein put it succinctly in these words:

> the paradox forces us to relinquish one of the following two assertions:
> (1) the description by means of the ψ-function is *complete*
> (2) the real states of spatially separated objects are independent of each other.

It is important to notice that the conclusion Einstein draws from EPR is not a categorical claim for the incompleteness of quantum theory. It is rather that the theory poses a dilemma between completeness and separation; both cannot be true. It is also important to notice that the argument I have drawn from Einstein's illustration does not depend in any way on simultaneous measurements or even attributions of position and momentum. The argument depends on the satisfaction of a single conservation law and the inferences drawn from that concerning the measurement of a single variable. This feature of the situation, I believe, is completely buried in the original paper and, because of that, Einstein's ideas concerning completeness and separation have become needlessly entangled with discussions of the uncertainty formulas and hidden variables. In his letter to Schrödinger of June 19, 1935, Einstein says that if the argument he gives applies to pairs of incompatible observables "ist mir *wurst*," which I would translate loosely as "I couldn't care less." The argument nowhere depends on that, nor do the basic ideas. I think that this feature shows that Einstein has successfully managed to use the correlations to get around Bohr's doctrine of disturbance. For even in the semantic version of that doctrine, measuring the momentum on A does not preclude assigning a somewhat earlier *momentum* to B, which is all the argument requires.

Einstein wanted to use the dilemma posed by EPR to show that if we maintain the ideas of action-by-contact embodied in the separation principle, then we must view quantum theory as providing no more than a statistical account of a realm of objects whose properties outstrip the descriptive apparatus of the theory. As we have seen, he felt that the concepts needed to describe these properties adequately would be other than the dynamical concepts of classical physics. Thus, although Einstein took the incompleteness to be a sign that something better was required, he never showed any interest in the hidden variables program for filling out the theory from within. Rather, he hoped that some unification of quantum theory and relativity would, so to speak, provide a completion from without. This path would address the external constraints of relativity and of classical dynamics together, if it could be successfully followed. Of course, Einstein did not succeed. And now recent arguments by Bell and others suggest that separation alone may be incompatible with the quantum theory, and perhaps even with certain experiments.[11] Should that be correct, then the dilemma of EPR could be resolved by abandoning separation. I do not believe that the Bell arguments are in fact strong enough to

11. See chapters 4 and 9 for discussions of Bell.

force the issue this way, but even if they are, the question of completeness would remain. For it is possible that *both* separation and completeness turn out to be false.

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Einstein's reservations about the fundamental character of quantum theory began with reflections about completeness, and these reflections were the home base to which Einstein's thinking about the theory always returned. It seems appropriate, therefore, to close by citing the earliest reference to the completeness issue that I have been able to find. It occurs in a letter of February 18, 1926, which says, "it seems likely to me that quantum mechanics can never make direct statements about the individual system, but rather it always gives only average values."[12]

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This comes from a letter written *to* Einstein *by* Heisenberg. Thus, one might say that the original idea and focus on incompleteness came from him.

12. "Denn es scheint mir an sich wahrscheinlich, dass d. Quantenmechanik nie directe Aussagen über d. Einzelprozess machen kann, sondern immer nur Mittelwerte . . . gibt."

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