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Bohr's Response to EPR: Criticism and Defense

> If a specific question has meaning, it must be possible to find operations by which an answer may be given to it. It will be found in many cases that the operations cannot exist, and the question therefore has no meaning.
> —Bridgman, *The Logic of Modern Physics*

1. Introduction

In *Quantum Dialogue* (1999, 154), Mara Beller notes a significant shift of focus that Niels Bohr makes in responding to EPR.[1] EPR appeal to measurement as sometimes *sufficient* in the determination of physically real properties ("elements of reality"). In his response, Bohr, while paraphrasing the words of EPR, morphs them into appeal to measurement and experiment as *necessary* in determining the *meaning* ("unambiguous meaning") of the term 'physical reality' itself (Bohr, 696). That shift opens the door to a semantic thesis that Bohr lays out later. He asserts that the "conditions which define

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This paper developed from a presentation to the *Workshop in Memory of Mara Beller* held at California Institute of Technology, October 27–29, 2005. My thanks to the participants at that workshop for helpful comments and suggestions. I also want to call attention to M. Whitaker's analysis of the controversy over Bohr (Whitaker 2004), which has helped my own thinking. In particular Whitaker's insight that some defenders of Bohr actually concede incompleteness has proved to be a useful thread to follow. His "three positions" over incompleteness correspond to arguments discussed in my earlier treatment of Einstein (Fine 1996) that I follow up on here.

[1] I use the common abbreviation EPR to refer to A. Einstein, B. Podolsky and N. Rosen 1935. Bohr's response is Bohr 1935a and in referring to it I will omit the year tag using simply 'Bohr'.

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the possible types of predictions regarding the future behavior of the system . . . constitute an inherent element of the description of any phenomenon to which the term 'physical reality' can be properly attached" (Bohr, 700). So (object level) conditions for future predictions are constitutive of what is physically real and (meta-level) descriptions that omit such conditions are not descriptions of something that it would be proper to call physically real. The conditions that Bohr discusses are a mix of applicable physical laws (especially conservation laws) and the "operations" (see above the Bridgman epigraph) involved in setting up particular measurements. This semantic doctrine lies at the heart of Bohr's complementarity. As Whitaker (2004) notes, among Bohr's peers it seemed not only natural to associate complementarity with logical positivism (see Bridgman again) but also honorable to do so. It has become less honorable. Still, it is no less obvious that Bohr's doctrine incorporates a positivist treatment of physical concepts and their significance, dressed in the Copenhagen style. Some recent commentators have resisted this conclusion and offered interesting reconstructions of Bohr's doctrines and procedures. That resistance is pointed primarily at work by Mara Beller and myself. Sadly, Mara is not here to deal with it. I'll try, beginning, as Mara would insist, by situating the issues in historical context.

2. *The Background in 1935*

Although he was initially enthusiastic about the "quantum mechanics" that emerged from 1926 to 1930, Albert Einstein soon became its foremost critic. His dissatisfaction is often portrayed as a last ditch longing for determinism or causality ("God does not throw dice"), as against the essentially probabilistic character of quantum physics. To be sure, although Einstein was a master at statistical physics, he was certainly troubled by a science where probability occurs fundamentally, describing the chance elements in his 1917 treatment of quantum gases as a "weakness" in the theory. Nevertheless his problem with the quantum theory was not about determinism alone, nor even primarily about determinism at all. In a 1930s letter to his old friend and translator, Maurice Solovine, Einstein expresses his concerns this way: "I am working with my young people on an extremely interesting theory with which I hope to defeat modern proponents of probability-mysticism and their aversion to the notion of reality in the domain of physics" (Solovine 1987, 91). This is a typical linkage in Einstein's thought. In almost every context in which Einstein expresses reservations about quantum indeterminism, he couples them with reservations about the irrealism of the theory; that is, giving up the ideal of treating individual events, or what he referred to as real states of affairs.

As usually understood, the quantum theory does not treat real states of affairs at all, not even probabilistically. Typically, it does not tell us whether an electron is likely (even) to be here or there, spinning up or down. Quantum theory only gives the probability for finding the electron here, or finding it spinning up, if one actually measures it for that particular property. This is the irrealism that Einstein found so disturbing. That there could be laws, even probabilistic laws, for finding things if one looks, but no laws of any sort for how things are independently of whether one looks, was mysticism, a "mindless" form of empiricism. Einstein responded with a characteristic, epistemological program. He first set out to establish the limitations of the concepts used in the quantum domain. Then he explored the possibility of transcending those limitations with a positive theory. He began by challenging the uncertainty formulas. He accepted that they limit the simultaneous, precise measurement of conjugate quantities (like position and linear momentum) but he questioned the ontological reading in which they limit what is simultaneously real. He went on to examine the rationale offered, especially by Bohr, both for the statistical character of the quantum theory and for its irrealism. Beginning with his Como lecture of 1927 and continuing throughout his writing up to 1935, Bohr had postulated an uncontrollable physical exchange, on the order of Planck's constant h, between measured object and measuring apparatus in every act of measurement. He maintained that this uncontrollable "influence," as he frequently called it prior to 1935 (see Beller 1999, 157), made a statistical treatment necessary and also prevented states of affairs being defined independently of the measurement.

In a series of thought experiments Einstein developed the concept of indirect measurement as a challenge to Bohr's postulate of uncontrollable disturbance. This culminated in the 1935 EPR paper, co-authored with his research assistants Boris Podolsky and Nathan Rosen, and composed by Podolsky. That paper involved the idea that Schrödinger dubbed "entanglement" (*Verschränkung*). Entanglement occurs when, after quantum systems interact, quantities of the different systems become linked stochastically. Einstein exploited this linkage to demonstrate the possibility of non-disturbing measurements for spatially separated systems. Coupled with relativity-inspired assumptions about local interactions, EPR argued that one must attribute determinate values to certain unmeasured quantities in circumstances where the usual interpretation of quantum states withholds them. Thus the theory's state descriptions are "incomplete." In fact the argument in EPR is quite garbled. As Einstein immediately complained to Schrödinger, Podolsky's text "smothered the central thing" (Fine 1996, 35). Einstein had at least three different version of the EPR argument, which are slowly being acknowledged in the literature (Fine 1996, 70–72). Bohr responded initially to what he could make of the original text and, later, he ignored Einstein's clarifications, even those made shortly afterward in an article (Einstein 1936) that Bohr certainly read since he refers to it elsewhere (Bohr 1949). But what was Bohr's response?

In a close reading of Bohr's 1935 reply to EPR, Mara Beller and I examined this question (Beller and Fine 1994), and in chapter 7 of her *Quantum Dialogue* (1999) Beller deepened that examination. She argues persuasively that after EPR "a basic change occurred in Bohr's notion of disturbance, reality, acausality and the indispensability of classical concepts" (Beller 1999, 166) and also in his treatment of objectivity (Beller 1999, 159). The specific conclusions I want to call attention to are these. (1) Bohr's response is a watershed marking a retreat from his pre-1935 program of tracing basic conceptual features of the quantum theory to robust physical interactions. (2) Bohr's response makes heavy use of a physical analogy between the EPR situation and a 2-particle double slit experiment; but that analogy fails to model EPR adequately. (3) In the end Bohr relies on recognizably positivist (specifically, verificationist) semantic doctrines to try to avoid the difficulty posed by EPR. Sparked by Don Howard's constructive re-appraisal of Bohr (Howard 1979) and by Beller's critique, there has been a small revival of interest in Bohr recently among philosophers of physics. Some of that, focused on EPR, challenges these conclusions by offering a different reconstruction of Bohr's response. Item (3), the charge of positivism, seems to be a special sticking point. The strategy of the challenges, then, is to defend an account of Bohr's reply to EPR that does not make essential use of positivist doctrines in order to avoid EPR incompleteness.

3. Summary of the EPR Argument

Writing to Paul Ehrenfest in 1932 Einstein describes an arrangement for indirectly measuring an electron **m** by using correlations (established via Compton scattering) with a photon, which can be measured directly.

> Thus without an experiment on ***m*** it is possible to predict freely, at will, *either* the momentum *or* the position of ***m*** with, in principle, arbitrary precision. This is the reason why I feel compelled to ascribe objective reality to *both*. I grant, however, that it is not logically necessary. (Einstein 1932, my translation)

In EPR this logical gap is overcome by adding to quantum theory, in its standard (Copenhagen) version, several assumptions concerning local causality. The most salient of these is the so-called *criterion of reality*, which functions as a sufficient condition for what EPR call "elements of reality."

> If, without in any way disturbing a system, we can predict with certainty (i.e., 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, 777)

But EPR contains two other "locality" assumptions, more or less buried in Podolsky's text. They relate to the central example there of two spatially separated systems in what I shall call the EPR *state*.[2] One assumption (*separate reality*) is simply that while the systems are apart they each have some real physical state, a state which would be described in a complete theory. This is presupposed in the other assumption (*no real change at a distance*) which posits that

> [N]o real change can take place in the second system in consequence of anything done to the first system. (EPR, 779)

The incompleteness argument then proceeds on the basis of strict correlations (entanglement), which obtain in the special EPR state between the positions of the two systems and also between their momenta. So if the position of system 1 were measured directly, then we could use the correlations to predict the position of system 2. Similarly for momentum. Thus we could measure the second system indirectly by measuring the first system directly. EPR argue that in these circumstances *locality* requires that the variables of the second system (here *position* and *momentum*) that can be so-measured indirectly must be real; i.e., that we must attribute a real position and a real momentum to the unmeasured system. Since no quantum state can be determinate simultaneously for both position and momentum, the conclusion

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[2] Formally the EPR state is this:

$$\Psi(x_1, x_2) = \int_{-\infty}^{\infty} dp \exp[(2\pi i/h)(x_1 - x_2 + x_0)p]$$

is that the quantum description of the unmeasured system is incomplete.

EPR go on to agree with conventional quantum wisdom that no simultaneous measurement of position and momentum can be made on either system, but note that their argument does not suppose anything to the contrary. Indeed they notice that one could get around their conclusion, semantically, by making "the reality of P and Q [on the unmeasured system] depend on the process of measurement carried out on the first system." But they urge, "No reasonable definition of reality could be expected to permit this" (EPR, 780). Clearly the heart of the EPR case for incompleteness concerns how entanglement in the EPR state meshes with the *locality* assumptions to entail the reality of position and momentum on the second system. If there is an adequate response to EPR it must be focused on these elements: the EPR state, entanglement in that state, and *locality*.

Concerning the EPR state there is certainly ample room for worry. As Dickson (2002a) emphasizes, it is a momentary state only (its support has measure zero in configuration space) and not one that could be arrived at by letting systems interact and evolve according to ordinary Schrödinger dynamics. (In these respects it is not like the singlet state used to illustrate Bell entanglement.) Nevertheless in the C*-algebraic formalism it can be replicated as an honest "state"; namely, as a positive linear functional on the Weyl algebra for two degrees of freedom (Halvorson 2000). Thus we can treat the EPR state as though it were a simultaneous eigenstate of relative position Q_1–Q_2 with eigenvalue x_0, and total momentum P_1+P_2 with eigenvalue 0. (Of course, as continuous operators, these do not have proper eigenstates.) Thus, if one of the systems has a determinate position, so does the other, and similarly for momentum. In short they are entangled as EPR require. That leaves the mesh with *locality* as the only promising place to dig in for a response.

What EPR require is a *locality* principle that accomplishes this: when added to the entanglement that occurs in the EPR state, *locality* entails that the second system has determinate values for position and momentum. It is not difficult to find such a principle. Consider first the conditional assertion

C(S_1, S_2). In the EPR state if S_1 were measured on the first system so as to yield a determinate value, then S_2 on the second system would also have a determinate value.

Clearly this conditional assertion, which signals strict correlations, holds for the position pairs and also for the momentum pairs in the EPR state. (EPR use state reduction to support $C(S_1, S_2)$, but other arguments would suffice.) Then *locality* can be taken to be the requirement that

> If $C(S_1, S_2)$ holds then S_2 has a determinate value ("an element of reality").

This requirement blends the *criterion of reality* and *separate reality* assumptions, which pose sufficient conditions for the reality of values, with *no real change at a distance* which is a necessary condition for the reality of values.

The rationale for *locality* would go like this. Suppose the composite system is in the EPR state. If there were a successful measurement of either position or momentum on the first system (only one of them is measured, not both together or in tandem) then $C(S_1, S_2)$ would imply that there is a value for the second system at the conclusion of the measurement. Then the *criterion of reality* guarantees that this would be a real value (an element of reality). If that value on the second system were not already there before measuring the first system, but only became real afterward (or simultaneously), then there would have been real change in the second system. (This assumes that the second system had some reality to change, on our *separate reality* assumption.) But "no real change can take place in the second system in consequence of anything done to the first system" (*no real change at a distance*). It follows that we must count the value for the second system at the conclusion of measurement as real all along; that is, independently of whether the first system were measured at all. Thus $C(S_1, S_2)$ implies the existence of a determinate value for the relevant observable (S_2) on the second system. The upshot is that *locality* allows the inference from strict correlations among spatially distant systems to the presence of determinate values for the correlated observables. No actual measurements need occur for that inference to obtain. We do not need to measure position or momentum on either system. According to *locality*, entanglement and spatial separation are sufficient.

It is interesting to see what happens if we omit the *criterion of reality*. For while criticism of the *criterion* is the main focus of Bohr's response, the *criterion* (along with the language of "elements of reality") never occurs in Einstein's own accounts of the EPR "paradox" (his term). The only use of the *criterion* is to certify that the value that would be inferred for the unmeasured system is "real." Without the *criterion* we would still be able to infer a definite value for the second system. But it would be moot whether acquiring that value constitutes a "real change" in the system, i.e., one to which the *no real change at a distance* principle would apply. Concretely, the question is whether position and momentum are to be thought of as properties of a system whose change can not be immediately affected at a distance. It is precisely here that Bohr appears to fall back on positivistic analysis. For he not only regards position and momentum to be defined by operations associated with measurement (operationalism), he insists that where no setup for measurement is in place, it is meaningless to talk about position or momentum at all (strong verificationism). In his language, the properties are not "defined." Some commentators like to describe this as a "relational" or "contextual" view of the relevant concepts. But where the relation or context requires a measurement setup, the view is just strong verificationism by another name.

4. Halvorson and Clifton

In their reconsideration of Bohr's reply to EPR, Hans Halvorson and Rob Clifton[3] (hereafter HC) take issue with the charge of positivism and set out to show "that Bohr's defense of the completeness of quantum mechanics does not depend in any way on questionable philosophical doctrines." They aim to provide a "formal reconstruction" of Bohr's reply which they claim "is dictated by the dual requirements that any description of experimental data must be *classical* and *objective*" (HC 2004, 370). Note the "dictated," which corresponds to the rhetoric of inevitability that Beller points to in Bohr. Unfortunately, HC do not see the need to provide an argument for why their claim about Bohr's reply does not itself involve questionable philosophical doctrines. For example, they define classical and objective, along with certain other philosophical notions, purely in terms of their formal model and never take a close look at whether these concepts, persuasively defined, relate in any sensible way to Bohr's usage, or to ours.[4]

[3] I only came across the paper discussed here shortly after Rob Clifton died. He had never mentioned it to me. No doubt my discussion would have drawn a characteristically lively and penetrating response from Rob, a loss for us all—over this and much more.

[4] Their definition of classicality requires that observables commute on the EPR state in order to be real. Thus simultaneous position and momentum are ruled out by

According to HC (here they say they are following Howard 1979) Bohr would replace EPR *locality* with a contextualized version, thus breaking the EPR argument for incompleteness. Contexualized locality is just the conditional antecedent $C(S_1, S_2)$ of *locality*:

> if S_1 were measured on the first system so as to yield a determinate value, then S_2 on the second system would also have a determinate value.

In their formal reconstruction by way of C*-algebras, HC prove a theorem (Theorem 2, HC 2004, 388–89) which they interpret as showing that in the EPR state (as they reconstruct it) contextualized locality can be justified for correlated positions and momenta. For that purpose they impose certain conditions on the EPR state and on subalgebras of the Weyl algebra of the EPR pair, notably, classicality and objectivity as just mentioned. Their theorem shows that contextualized locality is consistent with these conditions. Consistency, however, is weaker than what they need, so to get the inferences sanctioned by contextualized locality they impose a further condition for maximizing "objective" descriptions. The point is to show that we can obtain the usual EPR correlations in a no-collapse version of quantum mechanics, to which they enroll Bohr. Thus if we measure position on the first system, where the composite is in the EPR state, then according to HC we would be "warranted in attributing" (2004, 391) position to the distant unmeasured system (and not warranted in attributing momentum to it, due to "classicality"). This warrant, they emphasize, does not depend on the collapse or "reduction" of the EPR state, which is what EPR invoke for the same purpose. However, their move from a consistency result to a "warrant" is a little fast, and not examined at all by HC except in so far as they back off claiming that the warranted assertion is true. Still, one would suppose that there are lots of things consistent with certain information that, true or false, one is not warranted in asserting. Perhaps HC hold a coherence view of justification (one can't tell from their scant remarks) but even on such a view, consistency may be necessary for justification but it is hardly

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definition. What they call "objectivity" is a unitary invariance condition preserving certain features of a measurement context. But what does this symmetry have to do with the subject/object split (the focus of Bohr's pre-1935 treatment of objectivity) or with intersubjective communicability (Bohr's Kantian, post-1935 version of objectivity)?

sufficient. Thus the “reconstruction” offered by HC does not go very far by way of supporting even contextualized locality.[5]

Even if it did, HC would only get to the preliminary stage in the EPR argument, to assigning correlated values among distant systems. But HC carry their analysis no further. That is, they do not provide an analysis showing where EPR go wrong in using these correlations to demonstrate incompleteness, nor (as they apparently intended) do they actually justify (or even discuss) Bohr’s defense of the completeness of the quantum mechanical description. Instead, using the metaphor of contextuality, they simply assert that “in certain contexts we are warranted in attributing certain elements of reality to distant (unmeasured) systems,” an assertion not really supported by their results but still one agreed to by all parties. They then attribute to Bohr the additional claim “that if we attempt to make context independent attributions of reality to these distant systems, then we will come into conflict with the experimental record” (HC 2004, 391). Although this last claim resonates with the well-known experimental violations of the Bell inequalities, HC provide no source for it in Bohr’s reply to EPR in 1935. I can not find it there, indeed, I can not find it in any of Bohr’s writings. That is just as well since, for example, we know from the empirical equivalence between quantum mechanics and Bohmian mechanics (where at least position is context independent) that the claim is false.

If HC had gone further they would have had to face the issue of disturbance. They pose it themselves as the challenge of “giving a coherent account of *how* a measurement on one system can influence what is real for some spacelike separated system” (2004, 373). Does the position (momentum) we are warranted in ascribing to the unmeasured system count as a new feature of that system, as a change in it? If so, does that require an explanation? If so, how do we account for it? These are the crucial, philosophically difficult issues that EPR put on the table for Bohr. HC pay them little attention. Their theorem only assures us that it would be consistent with the EPR state and certain symmetry conditions to ascribe such a value, and that if we seek a certain maximal descriptive extension we would include it. It is difficult to see how this counts as an explanation, even a variety of structural explanation.

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[5] HC conjecture a further result, a uniqueness theorem that might strengthen their claim to a warrant. If there were only one way of extending our information base consistent with certain conditions, then we may be warranted in so extending it; maybe, if you are a coherentist, depending on the conditions and other circumstances.

HC's treatment of Bohr has another curious feature. Their defense of the quantum correlations in a no-collapse version of quantum mechanics is supposed to provide a justification for ascribing, say, position to the unmeasured system in the EPR state, which is not an eigenstate of position for that system. Indeed HC take pains to emphasize that their Bohr does not subscribe to the eigenvalue-eigenstate link. But the whole issue over completeness is precisely whether the description by means of the quantum state alone tells the whole story for subsystems like those in EPR. So what HC show (bracketing problems over justification raised above) is that there is a rational basis (the consistency of a larger story) for thinking that those state descriptions do *not* tell the whole story; indeed that they are incomplete. Adding maximality is necessary for getting at the whole story. Curiously, the import of their consistency theorem, as they interpret it, is a vindication of the EPR conclusion concerning incompleteness, although not of the EPR argument for this conclusion. Perhaps one could reconcile this circumstance with the aim HC express of defending Bohr over completeness by suggesting that their Bohr has a different account of completeness, one suitable for a no-collapse (or modal) interpretation. This may well be a plausible line to take (although they do not take it; see 2004, 374). But then it will turn out that EPR were right about incompleteness of the quantum state descriptions, perhaps without Bohr necessarily being wrong about the completeness (in some other sense) of quantum mechanics itself. Perhaps so. But HC do not tell this story on Bohr's behalf, despite their claim to be defending him on completeness.

If we concede incompleteness in the EPR sense, the issue that remains between EPR and Bohr will center on *locality* as opposed to measurement-context-dependence. Here is where we might hope to locate a defense of Bohr's position without "depending in any way on questionable philosophical doctrines." But, as we have seen, HC offer no defense at all, only the claim, put in Bohr's mouth, that context independence will "conflict with the experimental record," a claim that is false.[6] In the EPR situation to deny context independence is to deny that it is meaningful to

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[6] Some readers might think that the Bell-Kochen-Specker theorem is relevant here, since that result is sometimes interpreted as demonstrating the need for contextualism. But HC make no mention of the theorem and, anyway, the claim that it "demonstrates" contextualism is, to put it mildly, controversial. The assumptions of that theorem turn out to be deeply at variance with the inner product structure of the Hilbert space, even contradicting its dimensionality (Malley 2006).

ascribe (or weaker, to deny that one is warranted in ascribing), say, position to the second system if one is not set up to measure (or maybe if one has not already measured) the position of the first system. While HC defend the converse (that one *is* warranted if one *has* measured) on a basis they obviously regard as philosophically unquestionable, it is important to see that this defense has nothing to do with issues that divide EPR and Bohr. EPR have a defense of *locality* that involves three assumptions that look reasonable enough, although they could certainly be questioned. In HC's hands, Bohr has no defense for his measurement dependence. In his own reply to EPR, Bohr offers no defense either. As I have emphasized (section 1), in the only relevant passage Bohr (p. 700) asserts a semantic doctrine according to which, as he would put it later, the properties that can be ascribed to a system depend on the whole experimental arrangement. That, however, is not a defense of measurement-dependence, it is a restatement of it. Since measurement-dependence itself is precisely what strong versions of verifiability (or operationalism) amount to in the EPR situation, it seems transparent that Bohr falls back on positivist, semantic doctrines. It may be that Bohr has good reasons, ones that are not philosophically questionable, for adopting positivism in response to EPR. If so he does not share them with us and HC fail to uncover them as well. Just here Beller recognizes "two voices" in Bohr's response, one is the operational voice we have just explored. Another is a physical voice which we will explore in considering Michael Dickson's work below.

5. *Dickson & Modality*

In a series of articles Michael Dickson (2002a, 2002b, 2004) offers several analyses of EPR and Bohr's response, coupled with a critique of Beller (1999) and Beller and Fine (1994). Increasingly those articles move away from Bohr and his critics to focus constructively on analyzing the concept of a reference frame in the context of the quantum theory. Here I concentrate on the roots of that analysis in the treatment of Bohr.

Unlike HC, who see no room to associate "questionable philosophical doctrines" with Bohr, Dickson recognizes passages where Bohr seems to identify the physical meaning of an observable like position with the means of measuring it; that is, to adopt a strong form of operationalism. Dickson suggests that these passages can be read differently. He suggests we take

Bohr to maintain, for instance, that a well-defined frame of reference is part of the very concept of position. So the meaning of a physical property would not relate to full acts of measurement but would only involve the association of a physical frame of reference. The suggestion is that position and other physical variables are to be understood as relational properties; it is never just position but always position-*with-respect-to* a frame of reference. Since measurement presupposes a frame of reference, this restriction on the meaning of physical variables (and the meaningfulness of cognate assertions) is a sort of halfway house to operationalism. Dickson offers no general rationale for this restriction, nor any basis for judging its scope. Does it apply across the board to all determinables, quantum and classical? What would lead one to treat determinable properties as essentially relational? Unlike the lesson from special relativity about the relativity of simultaneity, frame relativism does not seem to be taught by the physics of moving bodies. Perhaps it is derived from a general semantics of properties or perhaps from a metaphysics of relations. Whatever its pedigree, it remains to be seen whether frame relativism will help Bohr in his response to EPR.

The general connection of frame relativism with quantum uncertainty, though, is straightforward. Reference to position carries implicit reference to a physical space frame of reference. Necessarily, when we measure position we introduce an uncontrollable exchange of momentum (or energy) with the associated space frame. (Note again what Beller calls the rhetoric of inevitability.) This "quantum of interaction" breaks momentum conservation (or some other relevant principle) and, as a consequence, the space frame becomes unsuitable as a reference frame for momentum. Thus the position measurement makes momentum ill defined. This is a canonical Bohr-disturbance argument that, presumably, transfers suitably to other pairs of conjugate variables. (How it applies to energy and time may be a bit problematic.) The argument would underwrite the uncertainty relations understood as a restriction on simultaneous measurement: if measuring A makes B ill defined then we can not measure both A and B together. Thus we could never know the values of both variables by direct measurement on both together. But does such an analysis of the consequences of measurement entail that conjugate variables are never simultaneously well defined? Recall that this was Einstein's question. Surely the verificationist principle that if you cannot measure them both together then they do not exist simultaneously (or are not well defined) is questionable. If frame relativism can help here, the only possibility that I see would be to argue, for instance, that no frame suitable for position can exist simultaneously with determinate momentum. In order to avoid verificationism, that conclusion could not be based on outcomes of real or hypothetical measurements. I cannot imagine how it would go. So I suggest that Dickson's introduction of frame relativism can at best underwrite quantum uncertainty as an epistemological restriction. (Since Dickson mostly hedges about realism, he might well agree.)

Frame relativism seems important to Dickson because he understands the EPR argument to involve both position and momentum measurements, one actual and the other hypothetical. Thus he maintains that to make their argument for incompleteness valid, EPR require a strong principle of locality ("non-disturbance"). Where the subscripts refer to system 1 or system 2 in the EPR situation, Dickson's strong principle is this.

> *Strong non-disturbance*. Suppose S_2 has a determinate value. Then if T_1, incompatible with S_1, were measured, S_2 would still have a determinate value.

But, according to Dickson, Bohr subscribes only to a weak principle.

> *Weak non-disturbance*. Suppose S_2 has a determinate value. Then even if S_1 had not been measured, S_2 would still have a determinate value.

(Dickson does not say, in either case, whether the before and after values of S_2 are the same; presumably they are.)

Dickson's reason for rejecting the strong principle is that the S_1, S_2 correlations between the systems break down if one measurement (of S_1) is followed by an incompatible one (of T_1). Thus we are unable to infer the S_2 value following a T_1 measurement. So we reject the stability of the S_2 value. Dickson's rational flows from the canonical Bohr-disturbance argument; namely, that the T_1 measurement effectively destroys the frame of reference for the S_2 value, so following that measurement it is no longer meaningful to refer to S_2. This line of response emphasizes that frames of reference will need to be actual physical objects, for otherwise it would seem meaningful to refer S_2 to the frame of reference that had been in place prior to the T_1 measurement. Moreover, at that time the global frame for the S-observables (extending from the place of one system to the place of the other) would presumably mesh with a local frame for S_2 in the vicinity of the second system. Why are we supposed to assume that the T_1 measurement on the first system renders the local S_2 frame ineffective? That certainly has the air of action-at-a-distance.

In any case, according to Dickson, Bohr's reply to EPR goes like this. If we measure, say, momentum on system 1 then, to be sure, we can infer momentum on the unmeasured system. But if we now were to measure position on system 1, the conditions for the definiteness of momentum on the unmeasured system would not be satisfied. *Strong non-disturbance* thus fails. So we do not get simultaneous position and momentum on the unmeasured system, and so no incompleteness. This supposes that we require a measurement on one system in order to infer determinate values on the other. But EPR impose no such requirement which, as Dickson notes, would amount to a form of operationalism. Rather, in Dickson's story it is Bohr to whom this operationalist supposition seems so natural that he attributes it unquestioningly to EPR. But EPR are clear, for example, that their *criterion* for "elements of reality" is a sufficient and not a necessary condition. As emphasized, EPR only need the entangled correlations themselves for the inference to determinate values, not any measurements—not even hypothetical ones. The *locality* condition takes their place.

But this only pushes the question back as to whether the rationale for *locality* involves *strong non-disturbance*. That case, recall, looks at momentum and position separately. The situation that Dickson has Bohr responding to, where one type of measurement is followed by another, never needs to occur. Instead we would infer momentum on the second system following a momentum measurement on the first system. We then ask whether that constitutes a change in the second system. According to the *no real change at a distance* principle it can not. So the second system must have had a determinate momentum all along. Similarly (and separately) for position. Recall that *no real change at a distance* just asserts that no real change can take place in the second system in consequence of anything done to the first system. Although that may appear to be a version of *strong non-disturbance*, its application here does not involve stability across a second type of measurement on the first system. Indeed, this application of the no-disturbance principle only involves what Dickson calls *weak non-disturbance*. That is, all we need suppose is that the momentum inferred on the second system does not require the momentum measurement on the first system. If momentum is there, it is there regardless of the measurement on the first system. (Similarly for position!)

In trying to salvage Bohr, Dickson adopts a risky strategy. He looks at a modal principle that, he contends, EPR need to use. Then one blunts the EPR argument by moving to a weaker modal principle. This is risky because, as an examination of the EPR paper quickly shows, the details of their argument for how we get *simultaneous P* and *Q* on the unmeasured system are missing. EPR do not say that first we do one kind of measurement then another. EPR simply say, as in Einstein's letter to Ehrenfest, that since we can measure either *P* or *Q* on the first system without disturbing the second system we have their "simultaneous reality" (EPR, 780). So, like all interpreters of EPR, Dickson teases out an argument on their behalf, arriving at a simple, valid argument that uses his strong modal principle. Other commentators construct different arguments for EPR, not always simple or valid. The construction we have followed here is one of Einstein's own and it involves only the weak form of non-disturbance.

Could it be that both Einstein and Bohr endorse the weak form? Suppose we infer the value of momentum on the second system by measuring momentum on the first system. On the conventional view, which EPR employ, this would result from the reduction of the composite EPR state to yield an eigenstate for the momentum of the second system. But *weak non-disturbance* says that the momentum of the second system would be definite (it would be the eigenvalue associated with the reduced eigenstate) even without such a measurement and reduction. Here Dickson runs into the same problem as HC. For if the unmeasured system has a determinate momentum in the original EPR state, unreduced, then that state description is not complete. Thus charity suggests that *weak non-disturbance* is much too strong for Bohr since, coupled with entanglement, it implies incompleteness. So it appears that Bohr should reject the weak form. There might seem to be an alternative principle that Bohr could accept, an even weaker one that undoes the inference to a determinate, measurement-free value. In line with Dickson's association of reference frames with meaningful ascriptions of values, one might suggest that even in the absence of the first measurement it would still have been *meaningful* to ascribe a value to the unmeasured system. Perhaps we need only the reference frame and not the complete first measurement. Unfortunately for Bohr, however, this would allow us to make the question meaningful as to whether the unmeasured system had some value (e.g., momentum) beforehand. If yes, then we get incompleteness. If no, then it seems that the measurement at one place will have produced the value at a distant place. That is the sort of nonlocality that, as we are about to see, Bohr seems to reject and to which Dickson certainly does not want to commit him.

Dickson notes that *weak non-disturbance* runs counter to operationalism. For if *weak non-disturbance* holds, then we countenance situations in which we can meaningfully ascribe physical properties without requiring a measurement for them. But if *weak non-disturbance* fails, then in some situations, at least, measurement will be required for property ascription. So in the EPR case the failure of *weak non-disturbance* allows that for the second system to have a determinate momentum might well depend on having made a momentum measurement on the first system. This would require more than having in place a suitable frame of reference for momentum; it would involve an actual momentum measurement. Thus Dickson's frame-relative strategy for separating Bohr from operationalism does not seem adequate if Bohr must reject *weak non-disturbance*.

Can he; that is, can Bohr make sense out of the situation where momentum for one system only arises in the context of a momentum measurement made on a distant system? The EPR paper was received for publication by *Physical Review* on March 25, 1935. (It was sent to be published on the 26th!) In a manuscript dated on almost the same day, on March 21, 1935, Bohr discussed a double slit experiment with electrons.

> 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 . . . it is completely incomprehensible that in its later course [the electron] should let itself be influenced by this hole down there being open or shut. (Bohr 1935b)

Not only the dates but also Bohr's language of non-disturbance here is eerily reminiscent of similar language in EPR. The point he is making is that influences at a distance are "irrational" and "completely incomprehensible." In view of this it is difficult to see how Bohr could accommodate the failure of *weak non-disturbance*. It is true that in later writings (after EPR) Bohr cautions against using the language of disturbance:

> The unaccustomed features of the situation with which we are confronted in quantum theory necessitate the greatest caution as regard all questions of terminology. Speaking, as it is often done, of disturbing a phenomenon by observation, or even of creating

> physical attributes to objects by measuring processes is liable to be confusing, since all such sentences imply a departure from conventions of basic language which even though it can be practical for the sake of brevity, can never be unambiguous. (Bohr 1939, 320)

Still, the question is whether there is an unambiguous reading of Bohr's own remarks that would allow him to reject the elements that go into EPR's *locality*. So far, at least, Dickson's reading of Bohr in terms of frame relativism does not seem rich enough to support such a reading.

That is because Dickson takes EPR to argue only that simultaneous position and momentum are *possible* for the unmeasured system, based on the possibility of *sequential* position and momentum measurements. Like Whitaker (2004), I see EPR (at least in one version) as arguing that these conjugate elements of reality are simultaneously *actual* for that system, based on entanglement in the EPR state and the possibility of *separate* position and momentum measurements. The issue, however, is not about which reading of EPR is better. I think both Dickson and HC miss something crucial, which is that EPR have in place the machinery to make an argument for incompleteness that works for each variable singly (either position or momentum alone) without reference to the other. As Einstein commented to Schrödinger (right after he read EPR as published), simultaneous values for conjugate variables are not necessary for the incompleteness claim (Fine 1996, 38). But simultaneous values based on the possibility of sequential measurements are what Dickson and the measurement-contextualism of HC set out to block.

6. Dickson & Bohr's Model

Corresponding to the two voices that Beller finds in Bohr, Dickson also has two approaches to EPR. We have seen the first, which deals in abstract modal principles and forms of argument. There is a second line of investigation that complements it. Here Dickson explores a physical model for EPR that Bohr discusses briefly. In line with his frequent use of a double slit experiment in analyzing the novel conceptual features of the quantum theory, Bohr suggests a model of the EPR situation using a two-particle double slit experiment, each particle with given initial momentum passing through its own slit. Bohr assumes that total momentum is conserved. Relative position of the particles will be fixed by the distance between the slits. It is not clear exactly what function this model has in Bohr's reply to EPR. He appears to be trying to pin down an error in the EPR argument, or its assumptions, something they missed that depends on taking into account an uncontrollable exchange of momentum or an uncontrollable displacement involved in a critical measurement—his standard key to resolving conceptual disputes in the quantum domain. However Beller and Fine (1994) and Beller (1999) think Bohr's model fails to do the job and that the failure was instrumental in moving him further along the road to positivism. Dickson disagrees and uses the model to defend Bohr's reply to EPR and also to deepen the understanding of how reference frames enter crucially in Bohr's analysis of physical concepts.

Dickson notes an error in our treatment of EPR. We suggest, incorrectly, that the EPR state, which is an eigenstate of relative position (Q_1–Q_2) and total momentum (P_1+P_2), is also compatible with either one of Q_1 or P_1 (not both) being simultaneously determinate. We then fault Bohr's model for not satisfying this condition. (The mistake was mine in confusing being determinate with being determinable by measurement.) But of course the EPR state is not also an eigenstate of either Q_1 or P_1 and Dickson is right to point out that our criticism of Bohr's model on this account is mistaken. We do, however, find other problems with the model. We point out, correctly, that in the model the measurement of position Q_1 for the first particle must be made at the very moment that the pair passes through the two slit screen if we are to use that result to infer the value of Q_2 from our knowledge of the distance between the slits, since after passage Shrödinger evolution makes (Q_1–Q_2) indefinite. Thus Bohr's account, which describes the measurement being made "subsequent" to passage through the slits, is quite misleading. (Bohr, 699) This feature of the model amounts to requiring that the preparation of the two-particle system in the right state must coincide with the Q_1 measurement. Again Bohr's description, which refers to "two free particles" each passing the slits "independently of the other," is not quite right since the Q_1 measurement would be useless unless both particles were, at the exact moment of preparation, passing through their respective slits.

Dickson does not regard this as a problem, since the EPR state is only a momentary state and so, he maintains, it can be a state for the composite system only at the moment of preparation. I am not sure this last point is correct in general. Presumably one could obtain the EPR state by state reduction. The interaction that produces the reduction is what is usually considered the preparation and it may well be a process that takes some time. So in principle there could be a gap between preparation and measurements made in the resulting state. Not so in Bohr's model. This would not necessarily be a problem, except that it has an unfortunate consequence, given Bohr's arrangement. For, as we have seen, the initial composite state in Bohr's model must be one in which, for a moment, each particle is located precisely at its slit. This is stronger than requiring, as the EPR state does, that the particles are a certain fixed distance apart. (As Dickson notes, relativity theory aside, distance is frame independent.) For how is the preparation of that initial state going to result in placing each particle exactly at its slit? To accomplish this we have to locate the slits. So placing the particles at their respective slits requires that the locations of the slits themselves be determinate, not just the distance between them. It requires a space frame connected to the preparation with respect to which each slit is located and, therefore, with respect to which initially each particle has a definite location as well. (This might be the laboratory frame with respect to which the location of the screen containing the slits is well defined. But who knows?) Thus in Bohr's model the initial state must allow for determinate positions for both particles. But in order to go proxy for the EPR state, Bohr's state must not assign probability *one* for these particular positions. Thus in Bohr's state the quantum state description is incomplete, perhaps in the manner of a modal interpretation. Instead of avoiding incompleteness, Bohr's model seems actually to imply it.

There is a further difficulty in Bohr's model, associated with the measurement of momentum. As Bohr's treatments of the standard two slit experiment always emphasizes, and as he repeats in replying to EPR, a momentum measurement requires that the screen containing the two slits be free to move. So at the moment of preparation, when the two particles will have to be exactly at the slits in the screen, the screen must be absolutely still (to get the particle locations right) but also free to move. Perhaps it is within the license of a thought experiment to suppose we can get the particles exactly in place without jiggling the screen even a tiny bit. But if we could do that, then why would the momentum transfer from the particles as they transit through the slits erase the *initial* positions at the slit? For it is that erasure that Bohr (and Dickson) count on to prevent simultaneous position and momentum. To be sure, after the complete measurement of P_1 the screen will have moved ("uncontrollable displacement . . . inevitable"; Bohr, 700) so that its position afterward may no longer be well defined relative to the initial space frame. But the measured momentum P_1 and its inferred twin P_2 both refer to that initial moment when the particles are at the slits with well-defined positions. Thus in Bohr's setup it looks as if that situation allows for (indeed requires) simultaneous *P* and *Q*, which it is supposed to forbid. The only alternative is to maintain that if we choose to measure momentum on the first particle, that measurement must somehow make the already well-defined position of both the local and the distant second particle become indeterminate. But that is influence-at-a-distance, not explained by frame dependence or other ordinary physical interactions, and there is no reason to think that Bohr would wish to sanction it.

Dickson is right to emphasize that the EPR state is a queer duck and so models of it will inherit queer features too. Because of this, in pressing these difficulties connected with state preparation and the initial state for the model sketched by Bohr, perhaps we are pressing too hard. But if we are not allowed to press Bohr's model over the physical consequences implicit in the experimental arrangement that he proposes, then parity would seem to require that he (and Dickson) should not be allowed to profit from using the model that way either. Indeed Bohr makes it clear in a footnote (Bohr, 699) that the "obvious impossibility" of carrying out the measurements he proposes "does clearly not affect the theoretical argument" (here he has conservation of momentum in mind). If we accept that attitude then the difficulties with Bohr's model show that it fails to provide physical support for his treatment of physical concepts, even granting his assumption of uncontrollable, inevitable disturbances. It is that failure, we think, that required Bohr to fall back on verificationist doctrines, which he does increasingly after 1935, doctrines that simply make simultaneous *P* and *Q* meaningless.

7. *Where Are We?*

We began by asking what was Bohr's reply to EPR. We learn several important things about that from Halvorson and Clifton. First of all, building on work of Halvorson, they show how to use the C*-algebra formalism to construct a plausible version of the two-particle EPR scenario for continuous operators corresponding to position and momentum observables. That formalism lends itself to treating quantum measurements without invoking collapse and HC develop a route to justify the correlations used in the EPR argument on that basis. Thus the standard collapse machinery used by EPR is in fact unnecessary and the same is true for the eigenvalue-eigenstate link, which is built into the use EPR have in mind for their *criterion*. Unfortunately HC do not seem to recognize the anomaly involved in offering a justification for ascribing these correlated values. For if there is no collapse, then the values apply in situations where uncollapsed wave functions do not themselves warrant those ascriptions. That is precisely the anomaly that EPR call attention to when they conclude "that the quantum-mechanical description of physical reality given by wave functions is not complete" (EPR, 780). There is an irony here. Notice that the strategy employed by HC is identical to the strategy of EPR. In both cases one begins with a mathematical representation of a quantum system to which one adds certain formal constraints that go beyond quantum theory. EPR add their locality conditions and HC their classicality, symmetry, and maximality conditions. The incompleteness conclusion then follows. The integrity of the conclusion clearly depends on the validity of the added constraints: do they beg the question? are they consistent with quantum phenomena? and so on. EPR locality is challenged by the Bell inequality experiments and that certainly weakens the force of the EPR argument. Ironically, HC now move to their rescue by providing different constrains that ground the same conclusion, perhaps constraints that may turn out to be less of a challenge. But what of Bohr? In the end HC think they need to block simultaneous attributions of position and momentum. To accomplish that they simply fall back on the doctrine, which they suggest is Bohr's, that such attributions are context dependent. Where the context is provided by measurement, that fall-back is a form of positivism: attributions of values to physical properties are not proper (warranted? meaningful? true?—you pick) unless a measurement is specified. No measurement, no (warranted? meaningful? true?) values. The virtue of this response, as EPR note, is that if you define "reality" so that it depends on measurement, the EPR conclusion about simultaneous P and Q becomes unavailable. It would be difficult, however, to credit that response with many other virtues, in particular with being philosophically unquestionable.

The core of Michael Dickson's handling of Bohr contains a related doctrine. Dickson rejects the idea that measurement is necessary for property ascription. What Bohr requires, according to Dickson, is not measurement as such but only an appropriate frame of reference; that is, the sort of thing that would serve as a reference object for a measurement result. Since Dickson sometimes suggests that the frame of reference is part of the meaning of property ascription, for him the doctrine seems to be this: no frame, no meaningful attribution of values. (In reading Dickson I am sometimes not clear whether his concern is with what is meaningful to ascribe or with what one is warranted in ascribing. Since the former seems closer to Bohr I adopt that reading.) Dickson shows that this idea of frame dependence fits well with what Bohr says when he talks about what can be "defined" in the context of a given observation, where he invokes the idea of an "uncontrollable exchange." Without pretending to an adequate analysis of these notions, Dickson nevertheless shows nicely how Bohr uses them as a way of rendering frames unavailable in certain contexts of measurement, blocking, for example, attributions of momentum when position is being observed. The problem for Dickson is to show that this analysis is adequate not just for the "uncontrollable exchanges" that occur in direct measurements, but also for the indirect, measurements-at-a-distance of EPR.

Here Dickson's analysis runs into a problem. In order to block what he takes to be the EPR path to simultaneous position and momentum, Dickson wants Bohr to subscribe only to a weak principle of non-disturbance. (He thinks EPR require a stronger principle.) According to the weak principle, if locating one system here enables one to infer (meaningfully) where the distant system is located, then that distant location would apply even in the absence of the observation made here. An advantage of attaching Bohr to the weak principle is that it detaches him from the positivist "no measurement, no value" doctrine. The cost, however, is that it commits him to ascribing values to the unmeasured system in the unreduced EPR state. That state may allow for such values, as it does in modal interpretations, but it does not by itself describe them. Like HC, Dickson's interpretation of Bohr appears to endorse quantum incompleteness. To avoid it Bohr will have to reject the weak principle (as well as the stronger one). But that will close the gap between Bohr and positivism that Dickson had been prying open—Hobson's choice.

Part of Dickson's defense of Bohr is to defend the thought experiment that Bohr proposed as providing a reasonably adequate physical model of the EPR situation. This is an important test for Dickson's reading, where it is not attention to measurement *per se* but only to its effect on frames of reference that is needed for understanding Bohr and his rejection of the EPR conclusions. It turns out, however, that the conditions for Bohr's two-particle, double-slit experiment are highly peculiar. If they are to allow an initial measurement of position, then both particles will need to have initial determinate locations. This already leads to incompleteness of the state description. Worse yet, making a momentum measurement in these circumstances will allow simultaneous attributions of both position and momentum. The only way out seems to be this. Concede that the system must be prepared differently depending on whether one intends to measure position or momentum. As Beller put it, "changing the measurement from Q_1 to P_1 on the first particle demands a change in the mechanical arrangement" (Beller 1999, 150). This would block the simultaneous P, Q problem but not the inference to incompleteness. The cost would be that one no longer has a plausible model of EPR at all. Thus Dickson's frame-relativism is of no avail to Bohr here because Bohr's model itself is simply inadequate as a representation of EPR.

What should we conclude, then, about Bohr's reply to EPR? All parties agree that Bohr's way out does not involve accepting quantum nonlocality if that amounts to there being causal influences between spacelike separated events. Indeed all parties agree that, as Beller emphasized, Bohr's response placed the focus on *meanings*. Bohr begins with semantics, not physics. Both HC and Dickson, moreover, seem to agree that it all hinges on Bohr's treatment of the conditions for associating determinate values to determinables, like position and momentum. HC promote context relativism: the propositions that are available as truth bearers depend on a measurement context. No measurement context, no truth warrants. Dickson promotes frame relativism: no frame in place, no meaningful property ascriptions. These relativisms could be used to block (warranted) simultaneous attributions of position and momentum, provided a cogent argument could be made that the presupposed physical conditions (for measurements or frames, respectively) are not simultaneously realizable. This is the murky territory of "uncontrollable" exchanges and complementarity which, as we have seen, gives off more than a whiff of positivism. Even so, neither of these semantic relativisms actually blocks quantum incompleteness. Indeed, in different ways, both HC and Dickson make arguments that commit them (i.e., Bohr) to the incompleteness of the quantum state descriptions. In view of this perhaps we should just admit that Bohr has no adequate reply. That is what Mara and I concluded and, for now at least, I see no reason to change.

To put it back in context: the Copenhagen defense of quantum irrealism was based on inevitable and uncontrollable measurement disturbances (or "exchanges"). Einstein introduced the idea of non-disturbing, indirect measurement to challenge that defense. So far there does not seem to be an adequate response to the challenge.

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