Did Bohr understand EPR?

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Abstract: In 1935, Einstein, Podolsky and Rosen (EPR) famously published a paper arguing for the incompleteness of quantum mechanics, using the example of two spatially separated but entangled particles. In his almost equally famous reply, Niels Bohr argued against EPR by providing a careful analysis of quantum measurements from the point of view of complementarity. Perhaps oddly, this analysis focuses on the example of a *single* particle passing through a slit. In this paper I argue that the disanalogy between the two examples is only apparent, and does not constitute an obstacle in trying to understand Bohr's views on complementarity.

Key words: Niels Bohr; Einstein, Podolsky and Rosen; entanglement; complementarity

1. Introduction

> We need to return to Bohr's own words, filtered through no preconceived philosophical dogmas. We need to apply the critical tools of the historian in order to establish what those words were and how they changed over time. We need to assume, at least provisionally, that Bohr's words make sense. And we need to apply the synthetic tools of the philosopher in order to reconstruct from Bohr's words a coherent philosophy of physics.[1]

Bohr's philosophy of physics has attracted a great deal of both admiration and detraction from many sides. A case in point is his reply to the paper

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[1]Howard (1994).

by Einstein, Podolsky and Rosen of 1935 arguing for the incompleteness of quantum mechanics,[2] which is one of the most cited of Bohr's writings on the foundations of quantum mechanics, because it contains a particularly careful analysis of quantum mechanical measurements from the point of view of complementarity. The present paper intends to give a fresh look at some crucial aspects of Bohr's reply to EPR, in the spirit of Don Howard's remarks above — which are as actual now as when they were first made in the wake of the centennial of Bohr's birth.

The argument by Einstein, Podolsky and Rosen was ultimately an outgrowth of the discussions between Einstein and Bohr on the photon-box thought experiment, which took place at the sixth Solvay conference in October 1930.[3] As described in Bohr's account of the discussions, the original photon-box thought experiment was an attempt by Einstein to undermine the energy-time uncertainty relations, as follows. Take a box containing both monochromatic radiation and a clock regulating the automatic opening of a shutter. The clock can be used to measure the time of emission of a photon, and weighing the box before and after the emission can be used to measure the energy of the photon via relativistic mass-energy equivalence — both seemingly with arbitrary accuracy. According to Bohr, he and Einstein eventually worked out that the weighing of the box interfered with the operation of the clock via gravitational red-shift, thus confirming the validity of the uncertainty relations.[4]

At the latest after this episode, Einstein appears to have switched from trying to "beat" the uncertainty relations to accepting them and trying to use them to derive paradoxical consequences of quantum mechanics. The further specific transformations of the photon-box thought experiment are well described in the literature.[5] Suffice it to say that by mid-1931 Ehrenfest was describing to Bohr how Einstein understood the photon-box as an apparatus that by way of mutually exclusive operations *on the box* allowed one to predict *either* the time of arrival of the emitted photon at some observation point *or* the energy of the emitted photon, and such that the choice of the operation to be performed could be made *well after the*

[2] Bohr (1935); Einstein, Podolsky and Rosen (1935).

[3] See for instance Jammer (1974), Sect. 6.2, Fine (1986), Ch. 3, Howard (1985, 1990) and Held (1998), Ch. 3.

[4] See Bohr (1949).

[5] See the references cited in footnote 3.

photon had been emitted. As Ehrefest put it ("however, I am not able to formulate it in such a way as to be sure he would be happy with my formulation"), the point of interest for Einstein was

> to realise that the projectile [the emitted photon], which is already flying around isolated "by itself", must be ready to fulfil very different ""noncommuting"" prophecies, "without even knowing" which of these prophecies one will make (and verify).[6]

The final form of the thought experiment was given in the EPR paper: one takes two particles in a simultaneous eigenstate of the two commuting quantities $P_2 + P_1$ and $Q_2 - Q_1$ (sum of momenta and difference of positions). Assuming that the corresponding eigenvalues are, e.g., 0 and x_0, quantum mechanics predicts that if a measurement of P_1 yields the outcome p, a subsequent measurement of P_2 will yield with probability 1 the outcome $-p$, and that if a measurement of Q_1 yields the outcome x, a subsequent measurement of Q_2 will yield with probability 1 the outcome $x + x_0$. If the two systems are no longer interacting, we can assume we can carry out a measurement of either momentum or position on particle 1 without interacting with particle 2.[7]

The EPR argument is a direct argument for the incompleteness of quantum mechanics, in the sense that EPR give an argument for the existence of certain "elements of reality" that are not present in the quantum mechanical description. In order to do this, EPR need a (sufficient) criterion for determining when such elements of reality are present: "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".[8] Applying the criterion of reality to the thought experiment, EPR argue that at least one of the position and momentum of particle 2 is an element of reality not present in the quantum mechanical description. We need not worry about the detailed logic of the argument — which has been the subject of debate — since what is of interest to us is

[6] Ehrenfest to Bohr, 9 July [1931], AHQP-EHR17 (in German) [the idiosyncratic use of quotation marks is in the original].

[7] One can of course imagine that x_0 is very large, but EPR themselves do not explicitly use this.

[8] Einstein Podolsky and Rosen (1935), p. 777, whole passage emphasised in the original.

Bohr's own understanding of the argument in his reply, which we shall discuss in the next section.[9]

As mentioned, Bohr's reply to EPR is an important source for Bohr's views on complementarity, a much-debated issue being whether it marks a turning point in Bohr's views. Mara Beller and Arthur Fine in particular have made a powerful case for a shift in Bohr's understanding of complementarity in his reply to EPR, and the ensuing debate has focused mainly on whether Bohr thereby came to espouse a positivist view.[10] The discussion below will not be directed towards this particular question, but will address a related point.

Until 1935, the understanding of complementarity appears to have been grounded in the idea of an uncontrollable physical exchange (of the order of the quantum of action), and EPR's focus on two spatially separated particles appears to undermine this idea, since there can be no physical exchange between the measuring apparatus and the distant particle. Oddly, however, Bohr's reply to EPR seems to minimise any conceptual differences between the case of measurements on a single particle and the EPR case. Indeed, Bohr spends a large part of his reply to EPR discussing the example of one particle going through a single slit — essentially the same as the example he had famously discussed with Einstein at the 1927 Solvay conference[11] — and he prefaces his subsequent analysis of the EPR example with the words: "The last remarks apply equally well to the special problem treated by Einstein, Podolsky and Rosen, which has been referred to above, and which does not actually involve any greater intricacies than the simple examples discussed above"[12] This is odd

[9]For a nice discussion of the logic of the EPR paper, see Fine (2013). It is also well-known that Einstein was dissatisfied with the presentation in the published paper (which was written by Podolsky) and preferred to present the case for incompleteness as an indirect argument, as follows. Once we have ceased to interact with particle 2, our measurements cannot affect its real state. The measurements on particle 1 can only yield (perhaps incomplete) information on the real state of particle 2. By performing different measurements on particle 1, we obtain different quantum mechanical state descriptions for particle 2. But we cannot obtain different complete descriptions of the same real state. Thus the quantum mechanical state descriptions obtained through the different measurements on particle 1 must be incomplete. See Howard (1985) and Fine (1986), Ch. 3, for details.

[10]See Beller and Fine (1994), Beller (1999), Ch. 7, and for the ensuing debate, e.g., Whitaker (2004), Fine (2007), and references therein.

[11]See again Bohr (1949).

[12]Bohr (1935), p. 699.

precisely because in the case of the single particle Bohr's arguments appear to be grounded in very physical intuitions, e.g., the idea that in a measurement of the position of the particle an uncontrollable amount of momentum passes from the diaphragm used for the measurement into the rigid support defining the laboratory frame.[13] In the EPR case, instead, with its threat of "spooky action at a distance", the physical grounding of Bohr's arguments seems rather less immediate.

In the following Section 2, which is the core of the paper, I shall try to spell out more clearly the analogy between the single-particle case and the EPR case, arguing that Bohr is *correct* in taking EPR to involve no "greater intricacies". More precisely, I shall argue that Bohr understands both the single-particle case and the EPR case as composed of a first stage in which the "uncontrollable" physical exchange takes place, and a second stage involving *no further interaction* with the system of interest. Thus, while Bohr is (rightly or wrongly) treating "spookiness" as unproblematic, he sees it as a feature that is already present in his treatment of the single-particle case.

Section 3 provides some additional support for this proposed reading of Bohr's reply in the form of a remarkable letter by Pauli to Schrödinger from July 1935.

Finally, returning to the "big picture", I shall conclude in Section 4 by briefly suggesting that it was not specifically the separation of the particles in the EPR paper that prompted a shift in Bohr's view of complementarity in the mid-1930s — at least assuming that the proposed reading corresponds to Bohr's understanding of measurements already prior to 1935.

2. Bohr's argument and the analogy with EPR

In the introduction to his 1935 reply, Bohr gives a sketch of the EPR argument, hinting at where he will apply his criticism. After making a few preliminary remarks and introducing EPR's criterion of reality, Bohr states

[13] I believe the phrasing of this "lab frame argument" in Bohr's reply to EPR is somewhat misleading; for discussion, see Bacciagaluppi and Crull (2015a). For an alternative, more literal reading see Dickson (2004).

that

> [b]y means of an interesting example, to which we shall return below, [EPR] proceed to show that in quantum mechanics, just as in classical mechanics, it is possible under suitable conditions to predict the value of any given variable pertaining to the description of a mechanical system from measurements performed entirely on other systems which previously have been in interaction with the system under investigation.[14]

Application of the criterion of reality to predictions of canonically conjugate quantities then leads EPR to conclude that quantum mechanics is incomplete. Bohr's criticism, he tells us, will be that the EPR criterion of reality "contains ... an essential ambiguity when it is applied to the actual problems with which we are here concerned".[15]

One can easily think of a classical example in which it is indeed possible to predict values of canonically conjugate quantities of one system from suitable measurements on a second system. Assume we have two classical systems, say with equal masses and known (centre-of-mass) positions and momenta, and assume the initial common centre-of-mass position and the total momentum are both zero. Assume the two systems collide, say elastically, but we do not know their shapes and sizes, so that we cannot calculate their respective positions and momenta after the collision. Nevertheless, since the total momentum is conserved and the common centre of mass remains at rest, we know that after the collision $x_2 = -x_1$ and $p_2 = -p_1$. Measuring position or momentum on one of the two systems now allows us to "predict with certainty" the value of the same quantity on the other system. Classically, of course, we are not merely predicting the results of further measurements. We know that both systems have definite values of position and of momentum, and that these values for the two systems have become correlated through the interaction. What we are doing is simply inferring what they are. But we *could* also infer the independent existence of the predicted values by applying EPR's criterion of reality. In the classical case, Bohr would arguably not object to this move. In the quantum mechanical case, by contrast, he objects precisely to the application of the criterion of reality.

[14] Bohr (1935), p. 696.
[15] Bohr (1935), p. 697.

In order to understand Bohr's reply, I thus suggest, we need to look for how the analogy between the classical and quantum cases breaks down in a way that makes the EPR criterion "ambiguous" and blocks its application to the EPR example. My contention is that Bohr's treatment of the single-particle case serves *precisely this end*, and not merely that of illustrating how the "general viewpoint"[16] of complementarity works in a familiar case.

In order to see this, the crucial insight one needs is that Bohr thinks of such experimental procedures, both classically and quantum mechanically and in both the single-particle and the EPR case, as involving *two stages*. The system of interest is not manipulated directly, instead it interacts in a first stage with some auxiliary system. It is the auxiliary system that is then manipulated, and in this second stage one no longer "mechanically disturbs" the system of interest. Such an auxiliary system might be the nearby particle in the EPR case or the diaphragm in the single-particle case: it turns out that the analysis is exactly the same.

By way of example, we shall now discuss Bohr's own example of a particle passing through a movable diaphragm. We shall first look at it classically, and then try to identify where the analogy breaks down in the passage to quantum mechanics.[17]

Assume we know the initial momentum of the particle and of the diaphragm. The particle is our system of interest S, and the diaphragm is our auxiliary system M. When the particle passes through the slit, it collides with the edges of the slit and exchanges momentum with the diaphragm. By measuring the position of the diaphragm, we can predict also the result of a further measurement of the particle's position (at least immediately after its passage through the diaphragm). And if we measure the momentum of the diaphragm, we can predict also the result of a further measurement of the particle's momentum.

Note that the interaction between S and M has not left S undisturbed. Indeed, a collision will have disturbed the momentum of S. But we need not worry about this, because the purpose of the measurement is not to

[16]Bohr (1935), p. 696.

[17]Without going into details, today's quantum measurement theory agrees with such a two-stage analysis of measurements, generalising it to the case of so-called positive-operator-valued measures (POVMs). See, e.g., the textbooks by Busch, Lahti and Mittelstaedt (1996) and Busch, Grabowski and Lahti (1997).

extract information about the initial state of S (note that the initial momentum of the particle is in fact known), but to make predictions about the final values of position or momentum of S. (We might prefer to call such procedures "state preparations" rather than "measurements", but the terminology is inessential.) What is important is that once we have measured the momentum (or the position) of the diaphragm, we are able to *reconstruct* what has happened during the interaction between S and M with regard to the exchanged momentum (or the relative spatial co-ordination) of the two systems. As Bohr puts it: "the question of principal interest for our discussion is now to what extent the momentum thus exchanged can be taken into account in the description of the phenomenon to be studied by the experimental arrangement concerned".[18] Since the particle and the diaphragm have ceased to interact and we subsequently interact only with the diaphragm, there is no further "mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure".[19] Thus, if we can reconstruct the relevant aspects of the original interaction, we can indeed reliably predict the result of a further measurement of momentum or position on the particle.

Classically, there is no problem with this analysis, and since the particle always has a position and a momentum, we are simply inferring what their values are. Quantum mechanically, we cannot presuppose that the particle has definite values of position and momentum simultaneously, but EPR argue that by applying the criterion of reality we can nevertheless infer it has. Bohr's move in order to block this final inference, as I see it, is precisely to emphasise that, in order to make the prediction, we not only need no mechanical disturbance of the system of interest when we perform the measurement on the auxiliary system, but it is crucial that we be able to reconstruct what has happened during the previous interaction between the two systems: in Bohr's words we need to be able to "control ... the reaction of the object on the measuring instruments if these are to serve their purpose".[20] And this is precisely where the analogy between classical and quantum mechanics breaks down.

Indeed, according to Bohr, in order to use the diaphragm to predict the momentum of the particle, one has to measure the momentum of the

[18]Bohr (1935), p. 697.
[19]Bohr (1935), p. 700.
[20]Bohr (1935), p. 697.

diaphragm itself, but then one renounces the applicability of the space-time picture, and cuts oneself off from the possibility of reconstructing the relative spatial co-ordination of particle and diaphragm. And in order to use the diaphragm to predict the position of the particle, one has to measure the position of the diaphragm, but then one renounces the applicability of the law of conservation of momentum, and cuts oneself off from the possibility of reconstructing the exchange of momentum between the particle and the diaphragm.[21] Thus, we are able to reconstruct the salient aspects of the interaction *only if* we choose to use the auxiliary system as a measuring apparatus for the corresponding quantity.

Thus, the sense in which EPR's criterion of reality is ambiguous for Bohr is that while "[o]f course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation",[22] it is *our choice* of using the auxiliary system as a measuring device for one particular quantity that enables us in the first place to reconstruct the aspects of the original interaction that are needed for predicting the value of that quantity. In Bohr's words, "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*".[23] If "without disturbing the system" should mean "without disturbing the conditions enabling predictions on the system", then the conclusion of the EPR criterion would follow, but the premise does not apply. If it should mean "without mechanically disturbing the system", then the premise would apply, but at least according to Bohr the conclusion does not follow. For Bohr, the fact that in the quantum case a disturbance of the conditions enabling predictions on the system does take place suggests the need for "a radical revision of our attitude towards the problem of physical reality".[24]

One might wish to scrutinise further the last step in Bohr's reasoning, but this is now quite a separate point from the one I wish to make. What I think should be clear from the above way of presenting Bohr's argument, is that the clash between EPR's reasoning based on the criterion of reality and Bohr's strategy for blocking it *is played out in full already in the case*

[21] I find Howard's (1994) analysis of Bohr's doctrine of classical concepts particularly helpful in understanding this aspect of Bohr's reasoning. The details of how Bohr understands "cutting oneself off", however, are inessential for the purposes of this paper. See also Bacciagaluppi and Crull (2015a).

[22] Bohr (1935), p. 700.

[23] Bohr (1935), p. 700, emphasis in the original.

[24] Bohr (1935), p. 697.

of the particle and the diaphragm. Indeed, the lack of direct interaction with the system of interest, after the original interaction between the system and the auxiliary system has ceased, is an integral part of how Bohr conceives of a quantum state preparation. True, in the EPR example not only is there no direct interaction with the distant particle, there *cannot* be any because the two particles are now spatially separated. But from the point of view I am here attributing to Bohr, the fact that the lack of interaction is guaranteed by the spatial separation is a neat but inessential feature of the EPR example.

Bohr does go on to discuss explicitly the EPR state, and suggests a thought experiment for preparing it:

> The particular quantum-mechanical state of two free particles, for which [EPR] give an explicit mathematical expression, may be reproduced, at least in principle, by a simple experimental arrangement, comprising a rigid diaphragm with two parallel slits, which are very narrow compared with their separation, and through each of which one particle with given initial momentum passes independently of the other. If the momentum of this diaphragm is measured accurately before as well as after the passing of the particles, we shall in fact know the sum of the components perpendicular to the slits of the momenta of the two escaping particles, as well as the difference of their initial positional coordinates in the same direction; while of course the conjugate quantities, i.e., the difference of the components of their momenta, and the sum of their positional coordinates, are entirely unknown.[25]

But this example is entirely analogous to the single-particle one, with the role of the auxiliary system played now by the second particle instead of the (single-slit) diaphragm. The only difference between the two cases is that in the single-particle case there is a direct interaction between system of interest and auxiliary system, while in the two-particle case the interaction is mediated by the (two-slit) diaphragm: the particles exchange momentum and get correlated in position via their separate interactions with the diaphragm. The diaphragm itself plays no further role in the analysis.

[25] Bohr (1935), p. 699. Note that the state in Bohr's thought experiment is only approximately equal to the EPR state (which is anyway improper, i.e., not mathematically representable by an element of the Hilbert space), due to the finite width of the two slits in the diaphragm.

Should the point need reinforcing, think again of the classical case: there is no need for the two systems S and M to have interacted directly. Indeed, we could let two balls pass through two slits in a macroscopic screen, so that they both collide with the screen but do not interact directly with each other. Knowing the initial momentum of the balls and the screen, and measuring the momentum of the screen after the passage of the balls, we then know the total momentum of the two balls, as well as the difference in their positions. By measuring the momentum of one of the balls, we can then reconstruct the (mediated) exchange of momentum between the two balls. And by measuring the position of one of the two balls, we can determine (by determining the position of the screen) the position of the other ball at the time the balls passed through the screen. Since we are not interfering with the other ball, either directly or indirectly, these are reliable procedures for predicting results of measurements of the momentum or (immediately after passage) the position of the other ball. Again, the only difference between this and the case of a single system lies in the details of the initial interaction between the system of interest and the auxiliary system (whether it is direct or indirect). In both cases, we have the same absence of interaction with the system of interest when we perform the measurement on the auxiliary system.

3. Pauli on Bohr's reply

As additional support for the above reading of Bohr's discussion of the single particle, I wish to quote from a letter from Pauli to Schrödinger of 9 July 1935,[26] in which Pauli comments on Bohr's not yet published reply to EPR:

> One lets a particle with a given momentum in the z-direction pass through an opening L found in a screen, which as a whole is free to move in the x-direction.[27] [Figure omitted.] Furthermore, the

[26]Schrödinger's correspondence from the summer of 1935, not only with Pauli (and famously with Einstein) but also with various other physicists, is a rich source of insights into the EPR debate. It will be included in translation in Bacciagaluppi and Crull (2015a).

[27]Given that knowledge of the particle's momentum in the x-direction is what is crucial in the argument, I assume Pauli here means the particle's momentum lies *wholly* in the z-direction. That is, a plane wave is approaching the screen perpendicularly, and has zero momentum parallel to it.

> momentum p_x of the screen in the x-direction is known *before* the particle has passed through L. *After* the particle has passed through L, I now still have the *free choice* — both times *without* disturbing the particle mechanically — *either* to measure once again p_x on the screen: then I can with certainty predict the magnitude and direction of the particle's momentum *after* its passage through L — *or* after the passage of the particle through L, I can measure the *position* x of the screen S; then I can also predict the position of the particle, at least an "arbitrarily short" time after the position measurement on S, as this will then coincide with that of S.[28]

After describing also the two-slit case, Pauli (addressing a point Schrödinger had raised about the notion of "state") emphasises again both the experimenter's freedom of choice and the lack of disturbance of the system, and makes it clear that he at least thinks they are general features of quantum state preparations:

> *Thus far Bohr.*
>
> Now, whether one should describe "pure case" as a *state*? ... A pure case of A is an overall situation in which the results of particular measurements on A (a maximal set) are predictable with certainty. I have nothing against calling this the "state" — but even then it *is* the case that changing the state of A — i.e., that which is predictable of A — lies within the *free choice* of the experimenter even without directly disturbing A itself — i.e., even *after* isolating A. ... In my opinion *there is in fact no problem here* — and one knows the fact in question even without the Einstein example.[29]

Note that Schrödinger did not have to wait to be told by Pauli.[30] Already on 14 June, more than three weeks earlier, Schrödinger wrote to Edward Teller about state preparations in very similar if somewhat more colourful terms:

> According to quantum mechanics, the preparation of a system, whereby it is brought into a certain given state, does not merely consist in material treatment of the system with tools of all kinds, but, rather, what happens afterwards depends on what one does with

[28]Pauli (1985), p. 419.
[29]Pauli (1985), p. 420.
[30]See also footnote 34 in the next section.

> the tools — whether one burns them, melts them down, tramples on them or preserves them in a museum — but in particular whether one pays attention to the signs of wear on the tools, and which ones.[31]

Unlike Pauli, Schrödinger does think "there is in fact a problem here", as he clearly expresses earlier in the same letter: "This assumption arises from the standpoint of the savage, who believes that he can harm his enemy by piercing the enemy's image with a needle".

4. Conclusion

As Rosenfeld informs us, when the EPR paper was published in 1935, "[t]his onslaught came down upon us as a bolt from the blue", and "as soon as Bohr had heard my report of Einstein's argument, everything else was abandoned".[32] What was it that seemingly took Bohr by surprise in the EPR paper? Prima facie, there are two obvious (not mutually exclusive) candidates. The first one is the criterion of reality, which allowed EPR to formulate a direct argument for the incompleteness of quantum mechanics. And, indeed, the explicit thrust of Bohr's reply is directed at undermining EPR's criterion of reality. The second one is the separation of the two particles in the EPR example, which, as mentioned in Section1, could be thought of as undermining the previous grounding of complementarity in the idea of an uncontrollable physical exhange. But this should not have taken Bohr by surprise, since we have already mentioned in Section 1 that Bohr had received a fairly detailed report of Einstein's ideas from Ehrenfest in July 1931, and there were other intimations of what was to come.[33]

In this connection, it would be interesting to see if our analysis of Section 2 (assuming it is correct) corresponded to Bohr's understanding of the particle-and-slit experiment already before 1935. If so, Bohr would have already understood perfectly well that manipulations on one system affect

[31] Von Meyenn (2011), Vol. 2, p. 533.

[32] Rosenfeld (1967).

[33] For the lead-up to the EPR paper, from the photon-box thought experiment of 1930 onwards, see in particular Jammer (1974), Sect. 6.2, Howard (1990) and Held (1998), Ch. 3. Note that Held (1998), p. 99, suggests explicitly that the elements of the EPR argument were all known to Bohr previously to 1935, with the notable exception of the criterion of reality.

predictions on another system that no longer interacts with the first, and the conceptual import of the separation of the two particles in the EPR example would have been no novelty for him. Something of the kind seems in fact to be implied by Pauli's comments in his letter to Schrödinger quoted above in Section 3.

In order to do this, we would have to trace the origins of the essential aspects underpinning the analogy with the EPR example, namely: (a) the two-stage structure of a quantum measurement, in which first the system of interest interacts with an auxiliary system and then a measurement is performed on the latter; (b) the freedom to choose which measurement to perform on the auxiliary system; and — crucially — (c) the fact that the manipulation of the auxiliary system involves no longer any interaction with the system of interest.

This is not entirely straighforward, because explicit emphasis on these aspects is much easier to find in Einstein and physicists connected to him than in Bohr and his circle.[34] Some precedents and parallels can be found, however.

While aspect (a) is at least implicitly present in most discussions, it is quite explicit in systematic treatments of measurements such as the treatment of measurements in von Neumann's book[35] and, perhaps more relevantly to Bohr, in Pauli's famous handbook article.[36] Pauli's treatment of measurements of the "second kind" (in which the system is not left in an eigenstate of the measured observable[37]) is especially interesting, both because Pauli uses a very general description of measurement (corresponding to POVMs, in modern terminology), and because his discussion involves reconstructing from the reading of the measuring apparatus what has happened during its interaction with the system, and is thus closest to Bohr's 1935 discussion (though without explicitly mentioning lack of disturbance).

[34] Recall for instance the letter by Ehrenfest to Bohr quoted in Section 1. Another very explicit source, containing yet another early variant of the EPR thought experiment, is a letter by Schrödinger to Sommerfeld of 11 December 1931, in von Meyenn (2011), Vol. 1, pp. 489-490. The above letter by Pauli to Schrödinger is a remarkably explicit source from the Bohr circle, but not a very early one (July 1935).

[35] Von Neumann (1932), Ch. VI.

[36] Pauli (1933), Sect. 9.

[37] Pauli (1933), pp. 98-99 of the 1990 edition.

Also aspect (b) is clearly present in Bohr's own emphasis, in his 1927 discussions with Einstein about the two-slit experiment, on the experimenter's freedom of choice in measuring either the path of a particle or the interference at the screen — by either measuring the momentum of the two-slit diaphragm or bolting it to the lab frame.[38] It is perhaps present also in Bohr's comments on the Heisenberg microscope in the Como lecture.

Aspect (c) is clearly the most elusive of the three. Weizsäcker comes close to it in his own analysis of the Heisenberg microscope, in which the scattered photon is observed either in the image plane of the microscope (yielding a measurement of the position of the electron) or in the focal plane of the microscope (yielding a measurement of the momentum of the elctron).[39] However, when in 1967 Weizsäcker's attention was attracted to the "delayed-choice" aspect of his analysis by Max Jammer, Weizsäcker did not recall having noticed the analogy with EPR in 1935.[40]

There is one author, however, who did use explicitly and in print the delayed-choice aspect of the Heisenberg microscope before Bohr's reply to EPR (in fact two months before the publication of the EPR paper). This was Grete Hermann in the essay containing her argument for the causal completeness of quantum mechanics.[41] Hermann argues that quantum mechanics drives a wedge between causality and predictability: that while causal notions can no longer be used in *predicting* results of observations, in each observational context one can give a *retrospective* causal analysis of

[38]Note that Bohr's account is retrospective; see Bohr (1949). Note also that in the 1927 discussion there is no suggestion yet that the choice could be made *after* the particle has passed through the slits.

[39]Weizsäcker (1931).

[40]See Jammer (1974), pp. 178-180, and Weizsäcker (1985), Ch. 11 (Sect. 9.3.4 β of the 2006 edition). Jammer (1974), p. 97, also points out that the Heisenberg microscope and Bohr's particle-and-slit experiment are variants of each other. Indeed, also in the microscope example one has two systems whose momenta are known before they interact: the electron's position is smeared out over the object plane, so its momentum in that plane is sharply defined (at least approximately, because of the finite dimensions of the microscope), and the wavelength of the photon is known. Thus, like in Bohr's example, immediately after the collision the sum of the momenta (in the object plane) is known and the difference of positions is zero.

[41]Hermann's essay provides a comprehensive philosophical analysis of quantum mechanics from a very specific neo-Kantian point of view; see Hermann (1935). For a well-known recounting of Hermann's extensive discussions with Heisenberg and Weizsäcker, see Heisenberg (1969), Ch. 10.

the measurement.[42] Her main example is precisely the γ-ray microscope, for which she argues that, both in the case in which the photon is observed in the image plane of the microscope and in the case it is observed in the focal plane, one can trace the cause for where the photon is actually observed.[43] In fact, apart from the explicit emphasis on causation, Hermann's analysis closely matches Bohr's, in which, depending on the free choice of the observer, one is able to reconstruct only one or another aspect of the original interaction between system of interest and auxiliary system, leading to different kinds of predictions on the system.

If Bohr thought of quantum measurements already before 1935 in terms closely analogous to what would become the EPR example, this may have implications for the understanding of Bohr's views on complementarity, specifically for the way they may have changed as a result of the EPR paper in 1935. The analysis of Section 2 should, however, have established that Bohr's understanding of quantum measurements was strictly analogous to the EPR example at least in 1935. The apparent disanalogy is thus not a problem in understanding Bohr's reply to EPR and the discussion of complementarity contained in it.

Appendix

I collect in this appendix some further material on the precedents and parallels of Bohr's understanding of quantum measurements as discussed above.

Ad (a). Both von Neumann and Pauli are interested in giving an account of how the apparatus can be included in the quantum mechanical description of a measurement, which is "crucial for a consistent analysis of the concept of measurement" — as Pauli puts it.[44] Von Neumann's main result is to prove the existence of a Hamiltonian that will take an initial state $\sum_n c_n\varphi_n\xi$ of system and apparatus into a state $\sum_n c_n\varphi_n\xi_n$, so that applying the projection postulate to the system or to the apparatus will produce the same results.[45] Pauli is actually more explicit and more

[42]Hermann (1935), Sect. 12.
[43]Hermann (1935), Sect. 10.
[44]Pauli (1933), p. 92 in the. Page references are to the 1990 edition.
[45]Pauli (1933), Sect. VI.3. Immediately before that, von Neumann proves his

general than von Neumann in his treatment. He first models in detail a Stern-Gerlach experiment,[46] showing both that measuring the centre of mass of the atom after it has passed through the magnetic field yields the expected probability distribution for the internal states of energy of the atom, and that the atom subsequently behaves (with regard to any further measurements) as a statistical mixture of eigenstates of its internal energy.

As mentioned, Pauli's treatment of measurements of the "second kind" is especially interesting, both because Pauli uses a more general description of measurement, and because his discussion is close to Bohr's 1935 discussion. In Pauli's general treatment, given an initial state u_n of the system, the final state of system and apparatus will be

$$\sum_k v_k^{(n)} U_k \ , \tag{1}$$

where the U_k form an orthonormal basis (and some of the unnormalised $v_k^{(n)}$ could be the zero vector). For a general initial state $\sum_n c_n u_n$, the final state will thus be

$$\sum_k \psi_k U_k := \sum_{n,k} c_n v_k^{(n)} U_k \ , \tag{2}$$

and after the reading of the apparatus the state of the system becomes $\psi_k = \sum_n c_n v_k^{(n)}$ (up to normalisation).[47] Pauli then considers the special case in which for different n one gets disjoint sets of U_k in Eq. 1, i.e., the case in which for each k the summation in Eq.2 is over only one value of n. In this case, by observing the result k, the final state ψ_k of the system can be associated (possibly many-to-one) to a unique eigenstate u_n of the measured observable. In Pauli's specific example, one measures the energy of the system through collisions (which in general will change the energy of a system). Knowing the initial energy of the incoming particle, and measuring its energy after the collision, one can reconstruct for each k the exchange of energy between the atom and the particle. If each of the

"insolubility theorem" showing that ignorance of the microscopic state of the apparatus cannot explain the statistical results of a quantum measurement. Note that a slightly weakened form of this result also follows from the lack of disturbance of the system of interest through manipulation of the auxiliary system, since the details of the manipulation of the latter can have no effect on the final state of the former; see Bacciagaluppi (2013).

[46] Pauli (1933), pp. 91-92.

[47] Note that at this level of generality, this is a description of a general POVM-measurement.

observed energy differences for the particle corresponds to a unique energy difference $E_n - E_m$ in the atom, then each measurement result k can be associated to a unique component u_n in the initial state (as well as to the final state u_m).

Note that in the mid-1930s also Heisenberg repeatedly discussed the movability of the "cut" between system and apparatus, i.e., the possibility of treating quantum mechanically the interaction between system and apparatus. Heisenberg's most complete (and only mathematical) discussion of the "cut" argument is in fact contained in his own manuscript reply to EPR,[48] a little-known and posthumously published paper written during the summer of 1935 at Pauli's instigation, to which we shall return below.

Ad (b). Also as mentioned, the freedom of choice in manipulating the auxiliary system has an obvious precedent in Bohr's own treatment of the two-slit experiment in the informal discussions with Einstein at the fifth Solvay conference of 1927.[49] Einstein had suggested the possibility of controlling the momentum exchange between the particle and the two-slit diaphragm, so as to obtain which-path information. But Bohr realised that if we did control the momentum exchange, then the position of the diaphragm would become indeterminate to the extent that the interference pattern would be wiped out. As he puts it in 1949, "we are presented with a choice of *either* tracing the path of a particle *or* observing interference effects",[50] with the corresponding experimental arrangements given by a movable diaphragm or by the diaphragm bolted to the rigid support, respectively.[51]

Indeed, in his reply to EPR Bohr refers explicitly to the two-slit experiment to bolster the idea that if we fix the diaphragm to the support we cut ourselves off from the possibility of controlling the momentum exchange, and that if we measure the momentum of the diaphragm we cut ourselves off from the possibility of knowing its position. In both cases the argument is that momentum information for the diaphragm is connected with subsequent path information for the particle, while position information for the diaphragm is connected to the subsequent presence of

[48]Heisenberg (1985). His mathematical treatment of the interaction between system and apparatus in this manuscript is, however, faulty, as discussed in Bacciagaluppi and Crull (2009).

[49]Cf. Bohr (1949).

[50]Bohr (1949), p. 217.

[51]Bohr (1949), pp. 219-220.

interference — but path information and interference are incompatible.[52] Thus the freedom of choice in the reply to EPR is directly connected to that in the discussion of the two-slit experiment.[53]

As one knows from the general theory of measurement, the freedom of choice in performing different measurements on the auxiliary system allows one to steer the system into some element of one or another family of generally non-orthogonal states.[54] But it is particularly striking in the case of a maximally entangled state, where the different families of states into which the system can be steered are orthogonal, and thus eigenstates of different self-adjoint operators. Both the EPR state (though improperly) and Bohr's example (though approximately) are maximally entangled states.

In this regard, the obvious precedent (or close cousin) of Bohr's freedom of choice is Heisenberg's γ-ray microscope. The γ-ray microscope was originally introduced by Heisenberg in his uncertainty paper as a thought experiment for a position measurement on an electron. A photon of known γ-wavelength collides with an electron that is smeared out over the object plane of a microscope. Observing the photon (as normally done in a microscope) in the image plane of the microscope then allows one to measure the position of the electron. According to Heisenberg, however, due to Compton recoil, the momentum of the electron becomes uncertain. Famously, Bohr criticised Heisenberg's purely particulate analysis of the Compton scattering (which would have allowed one to reconstruct also the exchange of momentum),[55] and in his Como lecture gave an alternative discussion of the uncertainty arising in the Heisenberg microscope.

Like in Bohr's example, however, one can use the thought experiment not only for measurements of position (with the corresponding uncertainty in momentum), but also for measurements of momentum (with the

[52] Bohr (1935), pp. 697-698 and bottom of p. 698, respectively.

[53] Despite this connection, I believe Heisenberg overstates his case when he writes to Pauli on 2 July 1935 that: "The essential point of Bohr's reply is something like the following: it can be shown that the Einstein thought experiment is identical in principle to the repeatedly discussed screen with two slits. The separation between the slits fixes $x_1 - x_2$ of the light quanta, and the momentum measurement on the screen determines $p_{x_1} + p_{x_2}$. One then applies the usual arguments and shows that the simultaneous existence of interference and conservation laws leads to the Einstein paradoxes"; see Pauli (1985), p. 408.

[54] Indeed, EPR remark as such on p. 779 of their paper.

[55] Cf. Beller (1999), Chs. 4 and 6.

corresponding uncertainty in position). This may have been part and parcel of Bohr's discussion in the Como lecture, where he immediately goes on to discuss precisely how uncertainty in the position of an electron results from measurements of momentum (using the Doppler effect) on a photon.[56] It is explicit in the more rigorous (field-theoretical) analysis of the microscope that Heisenberg set Weizsäcker as a task in 1931.[57] Weizsäcker crucially notes that the microscope can be used to measure *either* the position of the electron *or* its momentum, depending, respectively, on whether one observes the photon (after the interaction) in the *image* plane of the microscope (this is the standard way in which a microscope is used) or in its *focal* plane. Thus, the Heisenberg microscope has exactly the same structure as Bohr's example of the particle and diaphragm.

As a matter of fact, Heisenberg used the γ-ray microscope in his own manuscript reply to EPR.[58] The crux of his argument was that (in our terminology from above) if one places the cut so as to include both system of interest and auxiliary system on the quantum-mechanical side, then one can use the auxiliary system to perform incompatible measurements on the system of interest. But then (repeating an argument Heisenberg had used in a letter to Einstein of 10 June 1927), if hidden variables existed that could explain the result of one of these measurements, they would destroy the interference needed to explain the result of the other measurement. (That Heisenberg was implicitly referring to the γ-ray microscope here is made explicit in a letter he wrote on 29 September 1935 to Bohr, who had found this passage in the manuscript rather unclear.)

Ad (c). The last and crucial aspect of the analogy with EPR is the lack of mechanical disturbance of the system of interest through the manipulation of the auxiliary system. While it is a feature of all the examples above (and other related ones, such as von Neumann's discussion of a chain of measurements — in which obviously each successive auxiliary system

[56]Note, however, that the Doppler measurement of momentum was a separate example in Heisenberg's uncertainty paper, coming several pages after the introduction of the γ-ray microscope, and also in the Como lecture Bohr may have just been juxtaposing two examples, instead of been describing two alternative ways of manipulating the same photon in the microscope example.

[57]Weizsäcker (1931).

[58]See Bacciagaluppi and Crull (2009) for an analysis of Heisenberg's arguments, and Crull and Bacciagaluppi (2011) for a translation of the manuscript, with a brief introduction and full references to the relevant correspondence.

interacts only with the immediately preceding one), it is not remarked upon explicitly by any of the above. Only in 1935 have we seen Pauli being very explicit about it in his letter to Schrödinger, and Grete Hermann discussing it in print.[59] In Hermann's words:

> How both conceptions [the wave picture and the particle picture] are consistent with one another depends on the type of measurement: if the light is absorbed in the image plane of the observed object, then one is to work in the wave picture with the conception of a spherical wave propagating from one point, and correspondingly to ascribe a sharp position but a smeared exchange of momentum to the corpuscularily interpreted collision between electron and light quantum. If one carries out the observation in the focal plane of the microscope, then one has to deal with a parallel beam of rays, and accordingly to work in the corpuscle picture with a precisely determined exchange of momentum but an unsharp position. The one observational context that the physicist enters through observation of the photographic plate therefore determines which features of both pictures are used.[60]

(In the further case in which no photographic plate is placed in either plane, Hermann explicitly states that one obtains a linear combination of product wave functions, and that the process is not *anschaulich*, the photon and the electron each lacking individual states.) What is more, one can check (indirectly) that the respective causal analyses are correct, by performing subsequent measurements on the *electron*, the results of which reflect the corresponding sharp position or sharp momentum at the time of the collision. As Filk puts it,[61] Hermann in her discussion included all the same elements as EPR, but drew the opposite conclusion about the completeness of quantum mechanics.

The similarities between Hermann and Bohr are striking, and she in fact portrays herself in later sections of her essay as presenting Bohr's own

[59] Hermann (1935). In his reply to EPR, Heisenberg refers to this essay as expressing positions close to his. For a rigorous analysis of Hermann's arguments and views, see, e.g., Soler (2009). For a wider discussion, see Bacciagaluppi and Crull (2015b) (which includes also a translation of Hermann (1935) and a reprint of Soler (2009)), in particular the chapter by Filk (2015) on Weizsäcker's and Hermann's treatments of the Heisenberg microscope.

[60] Hermann (1935), Sect. 12.

[61] Filk (2015).

doctrine of complementarity.[62]

Acknowledgements: This paper is based on joint work with Elise Crull, who also made detailed comments on a first draft; all translations are by Elise and/or myself. My very warm thanks go to Finn Aaserud for encouragement to submit this contribution to the present volume. Related material was presented at the Philosophy of Physics Seminar, University of Aberdeen, and at the XVII Seven Pines Symposium, Stillwater, Minnesota, and I am grateful to the respective audiences for helpful comments and suggestions. I also wish to thank Arthur Fine and very especially Olivier Darrigol and the editors for comments on the previous version of this paper that have greatly improved it.

References

Bacciagaluppi, Guido (2013). "Insolubility from No-Signalling." *International Journal of Theoretical Physics*, Online First 21 November 2013, `http://link.springer.com/article/10.1007%2Fs10773-013-1821-y`, `http://philsci-archive.pitt.edu/9802/` .

Bacciagaluppi, Guido, and Elise Crull (2009). "Heisenberg (and Schrödinger, and Pauli) on Hidden Variables." *Studies in History and Philosophy of Modern Physics* 40, 374-382, `http://philsci-archive.pitt.edu/archive/00004759/` .

Bacciagaluppi, Guido, and Elise Crull (2015a). *"The Einstein Paradox": The Debate on Nonlocality and Incompleteness in 1935.* Cambridge: Cambridge University Press. In preparation, expected publication 2015.

Bacciagaluppi, Guido, and Elise Crull, eds. (2015b). *Grete Hermann: Between Physics and Philosophy.* Berlin: Springer. In preparation, expected publication 2015.

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

Beller, Mara, and Arthur Fine (1994). "Bohr's Response to EPR." 1-31 in Faye and Folse (1994).

Bohr, Niels (1935). "Can Quantum-mechanical Description of Physical Reality be Considered Complete?" *Physical Review* 48, 696-702.

[62] Hermann (1935), Sects. 13-15.

Bohr, Niels (1949). “Discussion with Einstein on Epistemological Problems in Atomic Physics.” 200-241 in P. A. Schilpp, ed., *Albert Einstein: Philosopher-Scientist*, The Library of Living Philosophers, Vol. VII. La Salle: Open Court.

Busch, Paul, Marian Grabowski, and Pekka Lahti (1997). *Operational Quantum Physics*. Berlin: Springer, 2nd, corrected printing.

Busch, Paul, Pekka Lahti, and Peter Mittelstaedt (1996). *The Quantum Theory of Measurement*. Berlin: Springer, 2nd edition.

Crull, Elise, and Guido Bacciagaluppi (2011). “Translation of W. Heisenberg:‘Ist eine deterministische Ergänzung der Quantenmechanik möglich?’.” `http://philsci-archive.pitt.edu/8590/` .

Dickson, Michael (2004). “Quantum Reference Frames in the Context of EPR.” *Philosophy of Science* 71, 655-668.

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

Faye, Jan, and Henry Folse, eds. (1994). *Niels Bohr and Contemporary Philosophy*, Boston Studies in the Philosophy of Science, Vol. 153. Berlin: Springer.

Filk, Thomas (2015). “Carl Friedrich von Weizsäcker’s ‘Ortsbestimmung eines Elektrons’ and its influence on Grete Hermann.” In Bacciagaluppi and Crull (2015b).

Fine, Arthur (1986). *The Shaky Game: Einstein, Realism and the Quantum Theory*. Chicago: University of Chicago Press.

Fine, Arthur (2007). “Bohr’s Response to EPR: Criticism and Defense.” *Iyyun: The Jerusalem Philosophical Quarterly* 56, 1-26.

Fine, Arthur (2013). “The Einstein-Podolsky-Rosen Argument in Quantum Theory.” In E. N. Zalta, ed., *The Stanford Encyclopedia of Philosophy* (Winter 2013 Edition). http://plato.stanford.edu/archives/win2013/entries/qt-epr/.

Heisenberg, Werner (1969). *Der Teil und das Ganze*. München: Piper. Transl. by A. J. Pomerans as *Physics and Beyond*. New York: Harper and Row, 1971.

Heisenberg, Werner (1985). “Ist eine deterministische Ergänzung der Quantenmechanik möglich?” 407-418 in Pauli (1985). Transl. in Crull and Bacciagaluppi (2011).

Held, Carsten (1998). *Die Bohr-Einstein-Debatte: Quantenmechanik und Physikalische Wirklichkeit*. Paderborn: Schöningh.

Hermann, Grete (1935). "Die naturphilosophischen Grundlagen der Quantenmechanik." *Abhandlungen der Fries'schen Schule* 6, 75-152. Transl. in Bacciagaluppi and Crull (2015b).

Howard, Don (1985). "Einstein on Locality and Separability." *Studies in History and Philosophy of Science* 16, 171-201.

Howard, Don (1990). "'Nicht sein kann was nicht sein darf', or the Prehistory of EPR, 1909-1935: Einstein's Early Worries about the Quantum Mechanics of Composite Systems", 61-111 in A. I. Miller, ed., *Sixty-two Years of Uncertainty*. New York: Plenum Press.

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

Jammer, Max (1974). *The Philosophy of Quantum Mechanics*. New York: Wiley.

Meyenn, Kurt von, ed. (2011). *Eine Entdeckung von ganz außerordentlicher Tragweite: Schrödingers Briefwechsel zur Wellenmechanik und zum Katzenparadoxon*, 2 vols. Heidelberg: Springer.

Neumann, Johann von (1932). *Mathematische Grundlagen der Quantenmechanik*. Berlin: Springer (2nd edition 1996). Transl. by R. T. Beyer as *Mathematical Foundations of Quantum Mechanics*. Princeton: Princeton University Press, 1955.

Pauli, Wolfgang (1933). "Die allgemeinen Prinzipien der Wellenmechanik." In H. Geiger and K. Scheel, eds., *Handbuch der Physik*, 2nd ed., Vol. XXIV, part 1. Re-edited and annotated by N. Straumann. Berlin: Springer, 1990.

Pauli, Wolfgang (1985). *Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u. a., Band/Vol. II: 1930-1939*, ed. by K. v. Meyenn, A. Hermann and V. F. Weisskopf. Berlin: Springer.

Rosenfeld, Léon (1967). 114-136 in S. Rozental, ed., *Niels Bohr: His Life and Work as Seen by his Freinds and Colleagues*. Amsterdam: North-Holland.

Soler, Léna (2009). "The Convergence of Transcendental Philosophy and Quantum Physics: Grete Henry-Hermann's 1935 Pioneering Proposal", 329-344 in M. Bitbol, P. Kerszberg and J. Petitot, eds., *Constituting Objectivity: Transcendental Perspectives on Modern Physics*, The Western Ontario Series in Philosophy of Science, Vol. 74. Berlin: Springer. Repr. in Bacciagaluppi and Crull (2015b).

Weizsäcker, Carl Friedrlich von (1931). "Ortsbestimmung eines Elektrons durch ein Mikroskop." *Zeitschrift für Physik* 70, 114-130.

Weizsäcker, Carl Friedrich von (1985). *Der Aufbau der Physik*. München: Hanser. Transl. by H. Biritz and edited, revised and enlarged by T. Görnitz and H. Lyre as *The Structure of Physics*. Dordrecht: Springer, 2006.

Whitaker, Andrew (2004). "The EPR Paper and Bohr's Response: A Re-Assessment." *Foundations of Physics* 34, 1305-1340.

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