Are there really two different Bell's theorems?

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This is a polemical response to Howard Wiseman's recent paper, "The two Bell's theorems of John Bell". Wiseman argues that, in 1964, Bell established a conflict between the quantum mechanical predictions and the joint assumptions of determinism and (what is now usually known as) "parameter independence". Only later, in 1976, did Bell, according to Wiseman, first establish a conflict between the quantum mechanical predictions and locality alone (in the specific form that Bell would sometimes call "local causality"). Thus, according to Wiseman, the long-standing disagreements about what, exactly, Bell's theorem does and does not prove can be understood largely as miscommunications resulting from the fact that there are really two quite distinct "Bell's theorems". My goal here is to lay out what Wiseman briefly describes as an "alternate reading" of Bell's 1964 paper, according to which (quoting Wiseman here) "the first paragraph of Bell's 'Formulation' section [should be seen] as an essential part of his 1964 theorem, the first part of a two-part argument." I will argue in particular that this "alternate reading" is the correct way to understand Bell's 1964 paper and that Wiseman's reading is rather blatantly inconsistent with the available evidence.

I. INTRODUCTION

My goal here will be to summarize and record my side of a debate that has erupted in response to Howard Wiseman's recent paper on "The two Bell's theorems of John Bell." [1] The debate concerns the question (much in the air as we celebrate the 50th anniversary of Bell's 1964 theorem) of what, exactly, Bell did and didn't prove in 1964. [2] My view, which seems to match that of Bell himself as well as several other contributors to this forum (including for example Tim Maudlin and Jean Bricmont), is that already in 1964 Bell demonstrated the need for non-locality in any theory able to reproduce the standard quantum predictions. ("Non-locality" here means a violation of a generalized prohibition on faster-than-light causal influences.) Whereas the opposing view (which is probably a majority view among normal physicists who have not studied Bell's work carefully, and is especially prominent among those physicists Wiseman describes as "operationalists") is that Bell only established a conflict between the empirical predictions of quantum theory and the joint assumptions of locality and determinism.

Those adopting the opposing view tend to retain allegiance to locality (which, they suggest, is after all a requirement of relativity) and insist that the upshot of Bell's work is that determinism must be abandoned. That is, they regard Bell's theorem as fundamentally a no-hidden-variables proof and hence a vindication of some standard (orthodox/Copenhagen/operationalist) interpretation of quantum mechanics. This is in contrast to Bell's own view, according to which the conflict with experiment cannot be blamed on determinism or any other departure from orthodoxy, but instead establishes that, however strongly motivated it might be by relativity theory, locality is false.

Wiseman's view, expressed in his recent paper, is neither of the above. Instead, his view is that both sides are right – because they are talking about different things. In particular, according to Wiseman, the theorem in Bell's 1964 paper shows exactly what the operationalists claim: reproducing the quantum mechanical predictions requires abandoning *either* locality *or* deterministic hidden variables. Whereas, again according to Wiseman, Bell would *later* (in 1976) prove a second theorem establishing a conflict between the quantum mechanical predictions and a unitary notion of "local causality" that captures the prohibition on faster-than-light causal influences for general (stochastic, i.e., not necessarily deterministic) theories. So, according to Wiseman, Bell, Maudlin, Bricmont, myself, and others are correct to say that "Bell's theorem proves non-locality" – if by "non-locality" we mean a failure of Bell's "local causality" condition – while simultaneously those taking the opposing view are right to say that "Bell's theorem proves only that deterministic hidden-variable theories have to be non-local". We're both right; we just mean different things by "Bell's theorem". It was simple miscommunication all along.

But I simply don't think that's *right.* The truth, I think, is that the people taking the opposing view have simply missed, or misunderstood, the role of the EPR argument in Bell's 1964 paper.[1] And Wiseman, in constructing an interpretation of that paper according to which the opposing view is correct (even if only in regard to that paper) essentially repeats this common misunderstanding.

There is a lot going on in Wiseman's paper and hence a lot that a full analysis of it would need to go into. Providing such an analysis is not my goal here. Instead I will focus exclusively on the historical question of what, exactly, Bell wrote and meant and did in his 1964 paper. It will turn out that much of what is under dispute here

[1] Or, equivalently, they have refused, on some kind of anti-realist philosophical grounds, to even entertain as meaningful the issues that Einstein *et al.* – and Bell – raised.

hinges on exactly what Bell meant by the word "locality" and, in particular, on whether his several comments about "locality" should be understood as attempts to provide a generalized definition of this term (Wiseman's view), or instead (my view) merely as indications of a narrower implication of locality in the context of a particular type of theory. Fittingly, the dispute also involves a disagreement about how to understand Bell's intentions with regard to his repeated citation of a certain passage from Einstein's "Autobiographical Notes". [3]

In the following section I review Wiseman's reading of the 1964 paper and then present, in the subsequent section, an overview of my own reading. A final section then elaborates on the several problems I see in Wiseman's interpretation and summarizes the issues as I see them. There is a lot of quoting from Bell, Wiseman, and Einstein. So to make reading this essay as easy as possible I have color-coded the quotations from these three sources: Bell (1964) in blue, Wiseman (2014) in red, and Einstein (1949) in green. Quotations from other sources are cited in the usual way.

II. WISEMAN'S READING OF BELL'S 1964 PAPER

Wiseman describes Bell's 1964 theorem as showing "that there are quantum predictions incompatible with any theory satisfying locality and determinism" and emphasizes that "Bell's 1964 theorem suggests that Bell experiments leave us with a choice: accept that physical phenomena violate determinism, or accept that they violate locality." As Wiseman acknowledges, this reading puts him in the "almost universal" category of misunderstanding that Bell himself would later call attention to:

> "My own first paper on this subject ... starts with a summary of the EPR argument *from locality to* deterministic hidden variables. But the commentators have almost universally reported that it begins with deterministic hidden variables." [4]

Nevertheless, Wiseman insists that his reading of the 1964 paper (contrary to Bell's own later reading) is correct. How does he justify this reading?

Wiseman begins by quoting Bell's several statements about the meaning of "locality" in the 1964 paper. The first relevant passage occurs in "1 Introduction" (which Wiseman and I agree is really more like an abstract):

> "The paradox of Einstein, Podolsky, and Rosen was advanced as an argument that quantum mechanics could not be a complete theory but should be supplemented by additional variables. These additional variables were to restore to the theory causality and locality[2]. In this note that idea will be formulated mathematically and shown to be incompatible with the statistical predictions of quantum mechanics. It is the requirement of locality, or more precisely that the result of a measurement on one system be unaffected by operations on a distant system with which it has interacted in the past, that creates the essential difficulty."

Bell's footnote references the following excerpt from Einstein's "Autobiographical Notes" in the 1949 Schilpp volume: "But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation of the system S_2 is independent of what is done with the system S_1, which is spatially separated from the former."

Wiseman proceeds to assume "that the 'real factual situation' of a system is what is probed by measuring it" and to assert that "the notions of being 'independent of what is done with' or 'unaffected by operations on' a system clearly refer to the action of an agent (say Alice) on her system, and mean that Alice's action has no statistical effect." These suppositions lead him to formalize Bell's (i.e., Einstein's) definition of locality as:

$$P_\theta(B|a,b,c,\lambda) = P_\theta(B|b,c,\lambda) \tag{1}$$

which is the same condition that is usually referred to as "parameter independence" (PI) in the more recent Bell literature.[2]

Wiseman also quotes Bell's summary statement, from the Conclusion of his paper:

> "In a theory in which parameters are added to quantum mechanics to determine the results of individual measurements, without changing the statistical predictions, there must be a mechanism whereby the setting of one measuring device can influence the reading of another instrument, however remote."

[2] The "θ" subscripts in the formula refer to the particular candidate theory assigning the probabilities in question. Note also that the formula remains somewhat vague until one specifies exactly what each symbol – and in particular the notoriously controversial λ – is meant to capture. Wiseman is not explicit about this, but seems to follow Bell in understanding the λ as denoting a complete specification of (the non-controllable aspects of) the physical state of the particle pair at some appropriate time prior to any measurement. But this is also slightly puzzling since Wiseman also seems to think that "[f]or the operationalist, locality [i.e., PI] is a natural assumption..." In my opinion, any genuine "operationalist" would simply balk at a notion of locality that required (to use Bell's later characterization) comparing probabilities conditioned on "a full specification of local beables in a [certain] space-time region". [5] Thus I think to some extent Wiseman conflates PI with a (distinct, and genuinely operationally meaningful) "no signaling" condition. See Ref. [6] for some further discussion. But, having noted it here, I will ignore this side issue in the remainder of this paper.

Wiseman, however, italicizes the phrase "the setting of one measuring device can influence the reading of another instrument, however remote" and explains that he regards this as constituting a *formulation* of "(the negation of) locality". Wiseman writes:

> "As the above quote shows, Bell definitely means locality specifically as the absence of any influence of the *setting* a on the remote measurement device. This confirms my above reading of his definition of locality in [equation (1) above]. In fact, this reading is confirmed in two more places in the paper."

The two other places cited by Wiseman include the first paragraph of Bell's "2 Formulation" where Bell writes:

> "Now we make the hypothesis[2], and it seems one at least worth considering, that if the two measurements are made at places remote from one another the orientation of one magnet does not influence the result obtained with the other."

(Note that Bell again here cites the same passage from Einstein quoted earlier.) The other passage that Wiseman regards as confirming his interpretation of what Bell means by "locality" comes later in Bell's section 2:

> "The vital assumption[2] is that the result B for particle 2 does not depend on the setting a of the magnet for particle 1, nor A on b."

(Note that Bell here cites Einstein for a third time.)

Having thus laid out his evidence for interpreting Bell as having meant, by "locality" in 1964, our Equation (1) above, Wiseman turns his attention to Bell's recapitulation, in "2 Formulation", of the EPR argument. Wiseman quotes Bell's one-sentence summary of the argument

> "Since we can predict in advance the result of measuring any chosen component of $\vec{\sigma}_2$ by previously measuring the same component of $\vec{\sigma}_1$, it follows that the result of any such measurement must actually be predetermined."

and then remarks:

> "Here Bell has made a mistake. His conclusion (predetermined results) does not follow from his premises (predictability, and [PI]). This is simple to see from the following counter-example. Orthodox quantum mechanics (OQM) is a theory in which the setting a of one device does not statistically influence the result B obtained with the other:
>
> $$P_\theta(B|a,b,c,\lambda) = P_\theta(B|b,c,\lambda)$$
>
> Here, if c were to correspond to preparation of a mixed quantum state ρ_c, the variable λ would allow for a pure-state decomposition; in purely operational QM, only c would appear. But in OQM it is of course not true that the results of spin measurements are predetermined for a singlet state as Bell is considering."

Wiseman is here pointing out that OQM (which respects PI and is hence "local" in the sense Wiseman attributes here to Bell) predicts the usual kind of perfect correlations in the EPR setup, but fails to attribute outcome-determining local hidden-variables to the individual particles in the EPR pair. Wiseman, that is, regards orthodox quantum mechanics as a counter-example to the EPR argument (as supposedly recapitulated here by Bell).

And that is basically that. Wiseman closes by asserting that "Bell's EPR paragraph forms no part of his 1964 theorem" and remarking as follows on his accusation that Bell made a mistake in thinking that the EPR argument ("*from locality to* deterministic hidden variables") was valid:

> "I would classify Bell's mistake in this paragraph as a peccadillo, having no impact on the main result in his paper. It would have been an easy mistake for Bell to have made, if he had the idea that EPR had already proven determinism from some sort of locality assumption, and did not think hard about whether it was the same as the locality assumption he was about to use in his own theorem. Indeed the paper could be made completely sound by replacing 'it follows' in the above ('Since we can predict...') quote by 'the obvious explanation is', or 'EPR's premises imply'. Although Bell believed that he was reproducing EPR's argument, EPR's premises (which are never stated by Bell) are *not* equivalent to locality (as defined here by Bell), and they *do* justify the conclusion of pre-determined outcomes..."

(For the proof of this last claim – that EPR's premises, including a notion of locality distinct from the one Wiseman attributes here to Bell – we are sent to an Appendix in Wiseman's paper.)

III. MY OWN READING

Let me here give an overview of my own reading of Bell's 1964 paper, and then come back (in the next section) to explain exactly what I find implausible about Wiseman's interpretation.

I would begin with something Wiseman seems to barely notice: the title of Bell's paper. This, I think, already makes it quite obvious that Bell intends his novel result to be understood as being built "On the Einstein-Podolsky-Rosen paradox". That is, I think, Bell takes himself (quite correctly) to be adding a crucial second step to what had been previously established by Einstein *et al.* This foundational role of the EPR argument in Bell's work is made quite clear in the first section, "1 Introduction", which I quote here in full:

> "The paradox of Einstein, Podolsky, and Rosen was advanced as an argument that quantum mechanics could not be a complete theory but should be supplemented by additional variables. These additional variables were to restore to the theory causality and locality[2]. In this note that idea will be formulated mathematically and shown to be incompatible with the statistical predictions of quantum mechanics. It is the requirement of locality, or more precisely that the result of a measurement on one system be unaffected by operations on a distant system with which it has interacted in the past, that creates the essential difficulty. There have been attempts to show that even without such a separability or locality requirement no 'hidden variable' interpretation of quantum mechanics is possible. These attempts have been examined elsewhere and found wanting. Moreover, a hidden variable interpretation of elementary quantum theory has been explicitly constructed. That particular interpretation has indeed a grossly non-local structure. This is characteristic, according to the result to be proved here, of any such theory which reproduces exactly the quantum mechanical predictions."

The second part of the paragraph here tells us something about Bell's motivation for undertaking the reported work (namely, he wanted to see if *any* deterministic completion of quantum theory would *have* to have the "grossly non-local structure" displayed by the de Broglie - Bohm pilot-wave theory).

But let us focus here on the first part, which (like Wiseman) I read as essentially an abstract of the paper. It begins by noting that, according to the earlier EPR argument, a hidden-variable type theory could "restore to the theory [i.e., quantum mechanics] causality and locality". This clearly implies that, according to Bell, Einstein *et al.* had previously established that ordinary quantum mechanics *violates* both "causality and locality". The violation of causality (which, like Wiseman, I understand here to simply mean "determinism") is uncontroversial and unremarkable. But it is important to appreciate that already here Bell is claiming (and/or endorsing Einstein's previous claim) that ordinary quantum mechanics violates "locality". This is certainly consistent with what we know of Einstein's criticisms of quantum mechanics (to be elaborated further in the following section). In particular, it is consistent with the passage from Einstein that Bell specifically chose to cite here (and then two subsequent times) as, evidently, capturing his (Bell's) own understanding of this concept:

> "But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation fo the system S_2 is independent of what is done with the system S_1 which is spatially separated from the former."

In my opinion, what Bell means by "locality" has thus *already* been made rather clear.

But what about the immediately-following sentences of Bell's "1 Introduction"? Here Bell writes: "In this note that idea will be formulated mathematically and shown to be incompatible with the statistical predictions of quantum mechanics. It is the requirement of locality, or more precisely that the result of a measurement on one system be unaffected by operations on a distant system with which it has interacted in the past, that creates the essential difficulty." First off, what is "that idea" which will be formualted mathematically? I read Bell here as referring, with "that idea", back to the *conjunction* of "causality and locality" – i.e., the two features that were to be restored by the introduction of additional variables. This is, after all, precisely what he does later formulate mathematically in the first equation appearing in his paper. According to this equation, which I reproduce here, the outcomes are mathematically determined by locally-accessible variables:

$$A(a,\lambda) = \pm 1, \; B(b,\lambda) = \pm 1. \tag{2}$$

I thus interpret the statement about locality from the abstract (namely, "the result of a measurement on one system [should] be unaffected by operations on a distant system with which it has interacted in the past") not as an attempt to give a general formulation or definition of locality (he has already done this by quoting Einstein!), but instead as a description of the specific implication of locality (to deterministic hidden variable theories) that he will use later in the body of his paper. This seems perfectly natural since the statement appears in (what amounts to) an abstract of the paper, i.e., in a summary of the novel result the paper will announce. (Note in particular that the statement in question comes just after the future-tensed statement about what "will be formulated mathematically". Whereas the first two sentences of the abstract refer to the earlier work of EPR – and Einstein's earlier formulation of locality – in the past tense.)

It is also natural to interpret, in this same way, Bell's statement from later in his paper just after he writes what I have transcribed in equation (2) above:

> "The vital assumption is that the result B for particle 2 does not depend on the setting a, of the magnet for particle 1, nor A on b."

Here, that is, he is not telling us what "locality" (in the most general sense) means, but instead calling our attention to a particular feature of the deterministic model he's just written down: namely, it is a *local* deterministic hidden-variable theory. And similarly for his summarizing sentence in "6 Conclusion":

> "In a theory in which parameters are added to quantum mechanics to determine the results of individual measurements, without changing the statistical predictions, there must be a mechanism whereby the setting of one measuring device can influence the reading of another instrument, however remote."

Note in particular that the violation of locality (namely, the existence of "a mechanism whereby the setting of one measuring device can influence the reading of another instrument, however remote") is described here as applying specifically to deterministic hidden variable theories (i.e., theories "in which parameters are added to quantum mechanics to determine the results of individual measurements".)

These three statements – that Wiseman interprets as attempts to *define* "locality" – thus instead seem to me to be clearly only attempts to describe the specific implication of locality that Bell uses in the context of the deterministic hidden-variable type theory that, he argues (citing EPR), is required to restore locality to QM.

Thus, I think the overall structure of Bell's paper is as follows: first he cites EPR as having previously established that locality (in Einstein's sense) requires positing deterministic hidden variables (in order to explain the predicted perfect correlations); then, in the main body of the paper, he lays out his new proof that this kind of local deterministic theory runs afoul of the quantum predictions when more general correlations are considered. It is crucial here that Bell takes EPR to have *previously established* the need for deterministic hidden variables in order to restore locality. Bell is effectively (and, in retrospect, somewhat naively and unfortunately) taking for granted that his readers understand that this has already been established, and is thus (quite reasonably) focusing his expositional attention on the novel result, building on the foundation laid by EPR, that he has established. If we drop this overall context (i.e., ignore the foundational role of EPR and assume that Bell is starting from scratch) we are likely to misinterpret much of what he says, about "locality" in particular.

There is, however, one statement about "locality" in Bell's paper which is, from my point of view, somewhat problematic. This occurs in the first pargraph of "2 Formulation" where Bell is recapitulating the EPR argument (that he would later characterize as an argument "*from locality to* deterministic hidden variables"):

> "With the example advocated by Bohm and Aharanov, the EPR argument is the following. Consider a pair of spin one-half particles formed somehow in the singlet spin state and moving freely in opposite directions. Measurements can be made, say by Stern-Gerlach magnets, on selected components of the spins σ_1 and σ_2. If measurement of the component $\sigma_1 \cdot a$, where a is some unit vector, yields the value $+1$ then, according to quantum mechanics, measurement of $\sigma_2 \cdot a$ must yield the value -1 and vice versa. Now we make the hypothesis[2], and it seems one at least worth considering, that if the two measurements are made at places remote from one another the orientation of one magnet does not influence the result obtained with the other. Since we can predict in advance the result of measuring any chosen component of σ_2, by previously measuring the same component of σ_1, it follows that the result of any such measurement must actually be predetermined."

This is the only point in the paper where Bell is actually attempting to recapitulate the logic of the EPR argument and hence explain exactly why and how predetermination really "follows" from locality and perfect correlations. So it is here that we would most want and expect to see an explicit *general formulation* of locality (rather than just some statement about one of locality's implications in the specific context of deterministic theories). And, unfortunately, Bell disappoints us. What he says here about locality ("if the two measurements are made at places remote from one another the orientation of one magnet does not influence the result obtained with the other") certainly falls short of a general formulation (along the lines that he would later give, in 1976 and 1990). It should be clear, for example, from the involvement of "magnets" that he is only here talking about some kind of implication of locality in the specific EPR-Bohm setup (with spin 1/2 particles whose spins are measured using Stern-Gerlach magnets).

But even leaving that disappointing specificity aside, what Bell says here seems problematic in another way as well: what does it mean to say that some distant intervention "does not influence the result obtained" by a nearby measurement? As the following five decades of Bell literature eloquently illustrate, it is notoriously difficult and controversial to precisely capture the idea of causal influence in the context of general (not necessarily deterministic) theories.[3] So it is simply not clear how to translate Bell's words here (about locality) into a sharp

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[3] Indeed, it is not even really clear how one should relate ordinary quantum mechanics (with realistically-interpreted and collapsing wave functions) to what Bell writes here in words. In ordinary QM, the orientation of the distant magnet certainly does influence the "real factual situation" (on which much more later) of the nearby particle; and then the "real factual situation" of that nearby particle certainly does "influence the result obtained" in the nearby measurement. But, because each of these influences involves some randomness, it turns out that the nearby measurement outcome is statistically independent of the distant setting (i.e., it turns out that PI is respected). So, has the distant magnet setting influenced the nearby result? It is simply not clear. (Thanks to D.V. Tausk for some helpful discussion of this point.)

mathematical statement in terms of which the EPR argument might be rigorously rehearsed.

So, and especially taking into account the five decades of controversy that have followed, it must be admitted that Bell's recapitulation of the EPR argument in this paragraph leaves something to be desired. And given the increasing attention that Bell gave to this very point in his subsequent writings (for example, by later providing more fully general mathematical formulations of the idea of "locality" and by stressing more explicitly the precise arguments by which ordinary QM can be seen to violate locality and by which deterministic hidden variables can be seen to be genuinely required if the perfect correlations are to be explained locally) it seems that Bell himself would agree that this important aspect of his 1964 paper could and should have been strengthened.

But let us not lose sight of the big picture here. The EPR argument – and the EPR-ish argument given by Einstein in the passages surrounding the sentence cited *three times* by Bell, including in the very sentence we have just been scrutinizing – were, in the context of Bell's 1964 paper, "prior work". Bell was (rightly or wrongly) taking that prior work as given, taking its results as already established. And so even in the important first paragraph of "2 Formulation" we should not understand him as attempting to present a fully rigorous and detailed version of the argument ("*from locality to* deterministic hidden variables"). Instead, I think, we should understand him as giving a quick overview of this earlier argument, the fuller version of which he invites his readers to find in the paper of Einstein which he explicitly references.

In summary, I think that in 1964 Bell was taking for granted that Einstein *et al.* had previously established that determinism was required in order to provide a local account of the perfect (EPR) correlations. The main new result Bell presented in 1964 was that this particular method of attempting to restore locality to quantum mechanics could not succeed, since local deterministic (hidden variable) theories could not reproduce the QM predictions for a wider class of possible experiments. But to summarize the significance of Bell's 1964 paper by saying that he demonstrated a conflict between the QM predictions and the joint assumptions of "locality" and "determinism" is to simply ignore the crucial foundational role played by the earlier work of Einstein *et al.* It is clear that, for Bell, the significance of his new result was to show that locality simply cannot be maintained if the quantum predictions are correct: "It is the requirement of locality ... that creates the essential difficulty."

IV. DISCUSSION

So, whose reading of Bell's 1964 paper is correct? One important piece of evidence in support of my reading is simply that it agrees with what Bell himself later says about what he had been up to in 1964. Of course, such testimony is only reliable to the extent that there is independent evidence that Bell was an honest reporter about his own earlier work. But here there is literally universal agreement, among those who knew him and worked with him, that Bell was an almost uniquely humble, honest, and forthright person who took extreme care to get details right and to always err on the side of crediting others rather than himself. I would also submit, as relevant evidence, Bell's 1977 remarks on "Free variables and local causality", which include the following open confession of an earlier mistake (having nothing directly to do with what's at issue here): "Here I must concede at once that the hypothesis becomes quite inadequate when weakened in this way. The theorem no longer follows. I was mistaken." [7] Clearly Bell had no difficulty admitting mistakes when he made them.

Wiseman's interpretation, which requires one to believe that Bell made a mistake in 1964 and then engaged in a decades-long terminological cover-up maneuver, simply does not seem plausible given what we know about Bell.[4] But there are many other and more direct reasons to reject Wiseman's interpretation.

First and foremost, Wiseman's reading requires us to understand Bell to have meant, by "locality", the condition that would later become known as "parameter independence". This, I submit, is completely and utterly implausible. To begin with, nothing like my Equation (1) above appears anywhere in Bell's paper. Nor does Bell say, in words, anything that can plausibly be rendered as "parameter independence" (which, note, is a statement about *probabilities*). *All* of the statements that Bell makes about locality in 1964 are statements about "the reading" of an instrument or "the result" of an experiment. That is, they are statements that can only really be directly translated into mathematics in the context of specifically deterministic theories. I think that if one really wanted to attempt to capture, in a mathematical expression, what Bell says in words, it would look like this:

$$A(a, b, \lambda) = A(a, \lambda). \tag{3}$$

In arriving instead at his mathematical translation, our Equation (1) above, of Bell's various words, Wiseman is

[4] Wiseman denies that he is "accusing Bell or his followers of intellectual dishonesty". But this is somewhat difficult to reconcile with his description of what must have happened subsequently (under the assumption, of course, that Wiseman is right about what Bell meant by "locality" in 1964): "once Bell had explicitly defined [local causality, in 1976], he wished all previous localistic notions he had used, in particular the notion of locality as per [equation (1) above, PI], to be forgotten. Moreover, after a few years he became convinced that it was the notion of [local causality] that he had in mind all along. [For example], Bell implies in 1981 that both he and Einstein were always using the notion of [local causality], which Bell characterises later in this 1981 paper in the same way [he had described it] in 1976. As argued [previously] there is only one plausible reading of 'locality' in Bell's 1964 paper, and it is not [local causality]."

thus clearly engaging in some pretty creative interpretation.

And note that Wiseman's creative interpretation extends to Einstein as well. Recall that Bell makes very clear, by citing Einstein three different times, that his *general* notion of locality – as opposed to the specific implication of it that he applies to deterministic theories – was the notion that Bell understood Einstein to have in mind when he (Einstein) wrote:

> "But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation of the system S_2 is independent of what is done with the system S_1, which is spatially separated from the former."

Thus, Wiseman's interpretation of Bell's 1964 paper requires not only that Bell meant PI, but that Einstein (in 1949) had meant PI. And this is in my opinion implausible to the point of outright absurdity. As noted earlier, Wiseman takes Einstein's "real factual situation" of some system to denote "what is probed by measuring it" and takes Einstein's idea of a system being "independent" of distant operations as meaning that the distant "action has no statistical effect". But this weird, operationalist reading of Einstein is just completely and totally at odds with what we know of Einstein's thinking generally during the period in question and, more directly, completely and totally at odds with the entire drift of the discussion from which Einstein's sentence is taken.

This is worth elaborating and concretizing. The several-pages-long discussion of quantum incompleteness from Einstein's "Autobiographical Notes" begins as follows:

> "Physics is an attempt conceptually to grasp reality as it is thought independently of its being observed. In this sense one speaks of 'physical reality.' In pre-quantum physics there was no doubt as to how this was to be understood. In Newton's theory reality was determined by a material point in space and time; in Maxwell's theory, by the field in space and time. In quantum mechanics it is not so easily seen. If one asks: does a ψ-function of the quantum theory represent a real factual situation in the same sense in which this is the case of a material system of points or of an electromagnetic field, one hesitates to reply with a simple 'yes' or 'no'; why? What the ψ-function (at a definite time) asserts, is this: What is the probability for finding a definite physical magnitude q (or p) in a definitely given interval, if I measure it at time t? The probability is here to be viewed as an empirically determinable, and therfore certainly as a 'real' quantity which I may determine if I create the same ψ-function very often and perform a q-measurement each time. But what about the single measured value of q? Did the respective individual system have this q-value even before the measurement?"

The first sentence already makes perfectly clear that Einstein was not using phrases like "real factual situation" in the operationalist sense that Wiseman's interpretation suggests. And similarly, Einstein's focus on "the single measured value" and "the individual system" make it clear that he is not merely interested in the "statistical" type of effect that Wiseman describes.

It is worth continuing with Einstein's discussion. Picking up where the previous quote left off:

> "To this question there is no definite answer within the framework of the [existing] theory, since the measurement is a process which implies a finite disturbance of the system from the outside; it would therefore be thinkable that the system obtains a definite numerical value for q (or p) the measured numerical value, only through the measurement itself. For the further discussion I shall assume two physicists, A and B, who represent a different conception with reference to the real situation as described by the ψ-function.
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> "A. The individual system (before the measurement) has a definite value of q (i.e., p) for all variables of the system, and more specifically, *that* value which is determined by a measurement of this variable. Proceeding from this conception, he will state: The ψ-function is no exhaustive description of the real situation of the system but an incomplete description; it expresses only what we know on the basis of former measurements concerning the system.
>
> "B. The individual system (brefore the measurement) has no definite value of q (i.e., p). The value of the measurement only arises in coorperation with the unique probability which is given to it in view of the ψ-function only through the act of measurement itself. Proceeding from this conception he will (or, at least, he may) state: the ψ-function is an exhaustive description of the real situation of the system."

Einstein thus sets up a dilemma between two different views one might take. According to the "A" view, the distant system already possesses definite, pre-determined values "for all variables". We may *find out* the value of one of these variables by making an appropriate sort of measurement on the entangled nearby system. But we do not influence or create those distant values. Of course, the existence of such pre-determined values requires us to say that the ψ-function fails to provide a complete description of the real physical state of the distant system.

On the other hand, according to the "B" view, the ψ-function can be claimed to provide a complete description of the real physical state of the distant system because definite pre-determined values are no part of that real physical state. But then, as Einstein goes on to explain, the quantum state ψ_2 of the distant system S_2 "depends upon *what kind* of measurement I undertake on S_1". Continuing:

> "Now it appears to me that one may speak of the real factual situation of the partial system S_2. Of this real factual situation, we know to begin with, before the measurement of S_1, even less than we know of a system described by the [original, pre-measurement] ψ-function. But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation of the system S_2 is independent of what is done with the system S_1, which is spatially separated from the former. According to the type of measurement which I make of S_1, I get, however, a very different ψ_2 for the second partial system $(\phi_2, \phi_2^1, ...)$. Now, however, the real situation of S_2 must be independent of what happens to S_1. For the same real situation of S_2 it is possible therefore to find, according to one's choice, different types of ψ-function. (One can escape from this conclusion only by either assuming that the measurement of S_1 ((telepathically)) changes the real situation of S_2 or by denying independent real situations as such to things which are spatially separated from each other. Both alternatives appear to me entirely unacceptable.)"

What Einstein describes as "unacceptable" is unacceptable precisely in the sense of violating the notion of locality that he has articulated previously (in the sentence partially quoted by Bell). So the upshot of Einstein's discussion – which Einstein goes on to state in the following paragraph – is that the "B" view described earlier is unacceptable (i.e., non-local). And that of course leaves only the "A" view, which, remember, involves attributing definite pre-measurement values "for all variables" associated with the distant system. Einstein's conclusion is, in short, that the only way to avoid an "unacceptable" kind of nonlocality is to posit local deterministic hidden variables.

I have quoted and summarized this passage from Einstein's "Autobiographical Notes" at such length because it allows several crucial points to be made about Bell's 1964 paper and Wiseman's interpretation thereof. I have already noted the complete implausibility of Wiseman's operationalistic reading of Einstein. Let us also now consider Wiseman's charge that Bell "made a mistake" when he (Bell) summarized Einstein *et al.* as follows:

> "Since we can predict in advance the result of measuring any chosen component of σ_2 by previously measuring the same component of σ_1, it follows that the result of any such measurement must actually be predetermined."

Of course, Wiseman's claim that this is a mistake is based on Wiseman's interpretation of the word "locality" in this passage as meaning PI. Wiseman is certainly correct that the conclusion of locally pre-determined values does not follow from PI. But surely both Bell and Einstein were sufficiently adept with mathematics and quantum mechanics to recognize that the *statistics* of Alice's distant measurement outcomes could not be affected by Bob's nearby choice of measurement. Wiseman's charge that both Bell and Einstein "made a mistake" is doubly implausible in that it requires both men not only to have made a really stupid mathematical blunder, but to have meant something quite different (by "locality") from what Einstein expresses (and Bell quotes).

Wiseman's suggestion that orthodox quantum mechanics is some kind of "counter-example" to Einstein's argument (which Bell means to be summarizing) also underscores the implausibly creative nature of Wiseman's interpretation. Einstein's entire several-page-long discussion (quoted above) is fundamentally *about* orthodox quantum mechanics and how it is, and isn't, possible to understand that theory vis-a-vis locality and completeness. The idea that Einstein somehow made an argument for locally pre-determined values, but without bothering to consider the concrete example of orthodox quantum mechanics, is simply preposterous. His whole argument is embedded in a discussion of orthodox quantum mechanics from the very beginning.

Here it is worth recalling also Wiseman's remark about Bell's recapitulation (in the first paragraph of "2 Formulation") of the EPR/Einstein argument:

> "Although Bell believed that he was reproducing EPR's argument, EPR's premises (which are never stated by Bell) are *not* equivalent to locality (as defined here by Bell), and they *do* justify the conclusion of pre-determined outcomes..."

That is, Wiseman actually concedes that there is a valid argument, of the type given by EPR/Einstein, "*from locality to* deterministic hidden variables". He just doesn't think that what Bell says in "2 Formulation" captures this argument – essentially because, as we have already discussed, Wiseman thinks that Bell uses "locality" to mean PI. But later in his paper, Wiseman considers (only to then dismiss it) the possibility that Bell might have meant, by "locality", the condition articulated by Einstein in the above-quoted passage where Einstein speaks of one measurement "telepathically" influencing the other. Wiseman writes:

> "Now although Bell seemed to indicate (twice) that this ["no telepathy" condition] was equivalent to his definition of locality, it is different in that it requires not that Bob's

> *result B* be independent of Alice's setting *a*, but rather that the 'real factual situation' of Bob's system be thus independent."

Indeed, as Wiseman proceeds to acknowledge, Einstein's "no telepathy" notion of locality "has the same force as local causality" and hence would support a valid inference to deterministic hidden variables.[5] But Wiseman dismisses this as irrelevant. As we have already seen, Wiseman is simply unwilling to believe Bell even when he (Bell) indicates repeatedly that what he means by "locality" is what Einstein articulates in the essay he cites. And the existence of a valid argument from Einstein's "no telepathy" version of locality to deterministic hidden variables is also supposedly irrelevant because, according to Wiseman, Einstein does not "use it there [i.e., in his "Autobiographical Notes"] to make the argument that Bell wants to make, from predictability to determinism." But, as is plain from the passages from Einstein's essay that I have quoted above, this is simply false. Einstein makes *precisely* the argument that "Bell wants to make" and in the paragraphs immediately surrounding the sentence that Bell repeatedly quotes as capturing his understanding of the notion of "locality" in so far as it is relevant to the EPR/Einstein argument.

To sum up, Wiseman's interpretation requires us to believe things about the views of both Einstein and Bell that are so completely at odds with what is known generally about these thinkers – and so completely at odds with what they explicitly wrote in the specific passages in question – that I don't think it can be taken at all seriously as capturing what Bell was actually doing in 1964. Wiseman writes, in a footnote, that "there is no evidence to support the suggestion (Norsen, pers. comm.) that Bell began with a general notion of locality, along the lines of local causality, and only narrowed it to this definition after he had established determinism via his EPR paragraph." I am truly at a loss to understand how Wiseman could say this, since he himself has reviewed the extensive and overwhelming evidence: Einstein's formulation of locality, which Bell repeatedly cites, is precisely a "general notion of locality, along the lines of local causality", quite distinct from PI, which allows a perfectly valid argument "*from locality to* deterministic hidden variables", an argument which Einstein presents in the paragraphs immediately surrounding the sentence that Bell repeatedly cites.

Wiseman, as far as I can tell, accepts all of this and yet still somehow believes that the first paragraph of Bell's "2 Formulation" is merely "a one-paragraph motivation for considering hidden variable theories." I think it is clear that it is more than this. It is the first part of Bell's overall two-part argument. It's just that Bell is taking the first part as earlier work, as a previously-established result that he need not rehearse in rigorous detail, but may simply refer to and briefly summarize.

This leaves, to my mind, only one question: given that in 1964 Bell presented his new result as the second part of a two-part argument for the overall conclusion of nonlocality (the first part of which was of course the earlier EPR/Einstein argument), which portion, exactly, of this two-part argument deserves to be called "Bell's theorem"?

Here I know from private communication that, when pressed in some of the ways I've tried to lay out above, Wiseman retreats in the direction of saying that, while perhaps Bell may indeed have had the full two-part argument in mind from the beginning, only the second part of it (the part that was novel in 1964) deserves the epithet "theorem". But this strikes me as a terminological shell-game. If a commentator wants to reserve the word "theorem" for demonstrations meeting some minimal threshold of rigor (and chooses to place the threshold somewhere between the level found in Einstein's 1949 discussion and what Bell did after the first paragraph of "2 Formulation" in his 1964 paper) I would have no objection, so long as the commentator articulates clearly that "Bell's theorem", when combined with the earlier "EPR/Einstein non-theorem" establishing the need for deterministic hidden variables, leads to the overall conclusion that the QM predictions are incompatible with locality... and that *this* is what Bell took himself to have established already in 1964. I would even have no objection if such a commentator raised questions about whether this incompatibility was really *established* in 1964, since (the commentator might plausibly argue) genuinely *establishing* such a conclusion requires that all parts of the argument leading to it meet the commentator's threshold for theoremhood. What I do object to, however, is the gross historical mischaracterization that is involved in Wiseman's almost complete dismissal of the role of the EPR/Einstein argument (or non-theorem or whatever one wants to call it) in Bell's 1964 paper. At the end of the day, and setting terminological games aside, Wiseman's account of what Bell *did* in 1964 is simply inaccurate in that it fails to capture an *essential* aspect of what Bell actually established (and took himself to have established).

The long-standing disagreements about what Bell did, therefore, cannot simply be understood as mere miscommunications, based on the existence of two quite distinct

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[5] Strictly speaking this inference requires the additional assumption "(which ... Einstein makes explicitly) that systems have real factual situations". Recall that "denying independent real situations as such to things which are spatially separated from each other" was one of the two things that Einstein jointly described as "entirely unacceptable". In my opinion, and probably that of Einstein, one must clearly accept that spatially-separated systems *have* their own "real situations" before one can even meaningfully ask whether locality is respected. The additional required assumption here would thus seem to be a logical precondition for discussing locality, rather than something one might coherently deny *instead* of locality. Note that this point is closely related to the important point that Bell would later express by insisting that the notion of locality must be formulated "in terms of local beables." [8]

"Bell's theorems". The disagreements are instead fundamentally based on the failure – of Wiseman's "operationalists" and also apparently Wiseman himself – to appreciate the foundational role of the EPR/Einstein argument ("*from locality to* deterministic hidden variables") in Bell's 1964 paper. Wiseman's paper may perhaps be doing some good in so far as his project involves making it more widely known that Bell did *eventually* establish a direct conflict between locality (alone) and the quantum predictions. But in so far as his strategy involves telling the "operationalists" that they were right all along, in how they understood Bell's 1964 paper, Wiseman is distorting the historical record, muddying the waters, and doing a great disservice to Bell on this 50th anniversary of his great achievement.

[1] H. Wiseman, "The two Bell's theorems of John Bell," *J. Phys. A: Math. Theor.* **47** (2014) 424001, freely available online at `http://iopscience.iop.org/1751-8121/47/42/424001/pdf/1751-8121_47_42_424001.pdf`

[2] J.S. Bell, "On the Einstein-Podolsky-Rosen paradox," *Physics* **1** (1964) 195-200; reprinted in J.S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, 2nd ed., Cambridge, 2004.

[3] A. Einstein, "Autobiographical Notes" in *Albert Einstein: Philosopher-Scientist*, P.A. Schilpp, ed. and trans., Harper & Row, 1949

[4] J.S. Bell, "Bertlmann's Socks and the Nature of Reality," 1981, reprinted in *Speakable and Unspeakable in Quantum Mechanics*, *op cit.*

[5] J.S. Bell, "La Nouvelle Cuisine", 1990, reprinted in *Speakable and Unspeakable in Quantum Mechanics*, *op cit.*

[6] T. Norsen, "Local causality and completeness: Bell vs. Jarrett" *Foundations of Physics* Vol. 39, Issue 3 (2009), 273

[7] J.S. Bell, "Free variables and local causality", 1977, reprinted in *Speakable and Unspeakable in Quantum Mechanics*, *op cit.*

[8] J.S. Bell, "The theory of local beables," 1976, reprinted in *Speakable and Unspeakable in Quantum Mechanics*, *op cit.*