Reply to Gillis's "On the Analysis of Bell's 1964 Paper by Wiseman, Cavalcanti, and Rieffel"

Howard M. Wiseman

Centre for Quantum Dynamics, Griffith University, Brisbane, Queensland 4111, Australia

Eleanor G. Rieffel

QuAIL, NASA Ames Research Center, Moffett Field, CA 94035

Eric Cavalcanti

Centre for Quantum Dynamics, Griffith University, Brisbane, Queensland 4111, Australia

Abstract. We address Gillis' recent criticism of a series of papers (by different combinations of the present authors) on formulations of Bell's theorem. Those papers intended to address an unfortunate gap of communication between two broad camps in the quantum foundations community that we identify as "operationalists" and "realists". Here, we once again urge the readers to approach the question from an unbiased standpoint, and explain that Gillis' criticism draws too heavily on the philosophical inclinations of one side of that debate – the realist camp. As part of that explanation we discuss intuition versus proof, look again at Bell's formalizations of locality, and correct misstatments by Gillis of our views, and those of Bell and Einstein.

1. Introduction

Gillis's "On the Analysis of Bell's 1964 Paper by Wiseman, Cavalcanti, and Rieffel" [1] refers to three papers [2, 3, 4] that discuss opposing views of Bell's theorem and attempt at least a partial reconciliation.‡ Wiseman's original paper [2] carefully examined assumptions behind the two predominant views, which he termed the operationalist and realist views, and performed a detailed textual analysis of papers of Bell's, particularly his 1964 paper [5] to determine what was proved, and under what assumptions, at that time. His conclusion [2, 6] was that Bell proved two different theorems, one in 1964 and one in 1976, the first corresponding the view of Bell's theorem taken by the operationalist camp, the second to the view of the realist camp.

‡ Wiseman, the only author on all three of these papers, was asked to report on Ref. [1], but declined to take any part of the refereeing process because of a clear conflict of interest.

Essentially everyone agrees that the two theorems are mathematically correct. But there is huge disagreement as to whether the first is (a) of any interest, and (b) due to Bell. At the heart of the disagreement is the meaning of various localistic phrases in the writings of Bell's and others. In this response, we will discuss only analyses relevant to Gillis's arguments. For a more general discussion, please see [2] and the two subsequent papers [3, 4].

Wiseman's paper has achieved some of its aims toward better communication between the two camps. For example, many operationalists are now aware that there is a different and interesting Bell's theorem from the one they knew. There remains staunch opposition in some corners of the realist camp to recognizing any legitimacy to the operationalist view [7, 8, 1], though some of the most vocal critics have come part way along the path Wiseman laid out [8]. The issue that raises the most passion, and over which there is least agreement, is the content of Bell's 1964 paper.

Since this question is a purely historical matter, one might ask how much effort should be spent by the physics community in resolving it. We believe there are strong reasons to resolve this issue. One of the key claims by some of the realist critics, including Gillis, is that the corect notion of locality has been clear enough since the 1935 paper of Einstein, Podolsky and Rosen [9] that other notions are not worth considering. We believe that this view is not only historically incorrect, but also holds researchers back from understanding and contributing to ongoing research such as [10, 11, 3] that are elucidating the relation of various notions of causation to quantum mechanics. For this reason, addressing these claims has impact beyond the "merely" historical.

Here, we respond specifically to arguments contained in Gillis's "On the Analysis of Bell's 1964 Paper by Wiseman, Cavalcanti, and Rieffel" [1]. Because his paper is less an analysis of our papers than a strongly stated defense of the realist stance, we will at times, of necessity if briefly, review material that is already contained in our previous papers. In none of our papers [2, 4, 3] do we argue for either the realist or the operationalist camp – all three works recognize the value of both Bell's Theorems and aim to explain why different versions of Bell's theorem are favoured by each camp and to bridge communication gaps between the camps. Because Gillis's paper comes from the realist point of view, it may appear that in this paper we are taking the operationalist side. That is not the case. Rather, we are trying to explain what operationalists will find unconvincing about his arguments. Gillis engages surprisingly little with the arguments in our papers, focusing on only a few sections of [2], discussing only one passage in [4], and making no specific mention of arguments in [3] or its new formulation in terms of causation that provides a means to bridge the gap between the two camps. Thus, there is less to respond to in terms of specific disagreements with our arguments than might be expected.

In response to Gillis's paper, we discuss intuition versus proof, look again at Bell's formalizations of locality, and correct misstatments by Gillis of our views, and those of Bell and Einstein. Before getting to those deeper issues, we address a question of terminology.

2. Objection to WCR terminology

We object to the use of WCR for what Wiseman called the operationalist viewpoint on the content of the 1964 paper – it incorrectly gives us credit for a widely held view, one that Bell himself complained [12] was the reading "almost universally reported."§ To name it after us is highly misleading, and is not the "neutral terminology" that Gillis claims it is. By making it appear to be a disagreement with us, rather than with the views of a large community, he does not have to wrestle with why a large number of people hold that view and provide an alternative explanation to Wiseman's as to why they do. We will continue to refer to the operationalist view, but if Gillis and others would like "more neutral" terminology, we recommend they use AUR, for "almost universally reported," rather than WCR.

Gillis also uses WCR to refer to a personified entity that will "formalize," "insist," and "fail to distinguish." Such an entity is entirely hypothetical. Prior to this paper, the three of us had not published together, so any such statement would need at least two citations. While in some cases his use of WCR may reflect views all three of us share, in many cases he is referring to material in Wiseman's solo paper, and sometimes to material in a two-author paper, but often his hypothetical entity, as we will illustrate, espouses views that none of us hold.

3. Intuition versus mathematical formulations

One of the reasons we expect that readers who do not already hold Gillis's views will not find his arguments convincing is that he makes no distinction between Bell's intuitive statements and his more formal formulations and proofs. Wiseman's paper [2] was concerned with what Bell did, and did not, prove in the 1964 paper, not his intuitions, however interesting, at the time. Had Gillis made a clear distinction between Bell's intuitive views in 1964 and what he had succeeded in formalizing and proving by that time, we expect many of his disagreement with [2] and the subsequent papers [4, 3] would vanish.

Gillis quotes from Bell's 'prequel' paper (published in 1966 [13]) "... there are features which can reasonably be desired in a hidden variable scheme. The hidden variables should surely have some spacial significance and should evolve in time according to prescribed laws. These are prejudices, but it is just this possibility of interpolating some (preferably causal) space-time picture, between preparation of and measurements on states, that makes the quest for hidden variables interesting to the unsophisticated" as support for his claim that "prior to the 1964 paper Bell had already described a concept essentially equivalent to what he called 'local causality' in 1990." In doing so, he confuses early intuitions with later formalizations. Bell himself was clear on the distinction, seeing both the desirablity and the difficulty of formalizing intuitive notions and being rightly cautious about the potential for incorrect formalizations.

§ We will use blue for quotes from Bell, gold for quotes from Gilles, violet for quotes from our papers.

Wiseman has never disputed the plausibility of Bell having always had a heuristic localistic notion which he understood OQM (Orthodox Quantum Mechanics) to lack, but he only formalized that notion, as local causality, in 1976 [14]. What he offered in 1964 was a statement of locality that requires a little interpretation to be formalized, but by any plausible reading is quite different from local causality. By one reading, locality as Bell used it in 1964 could only be applied to deterministic theories, in which case it cannot even be applied to quantum theory, so cannot be used to make any claim about the locality or nonlocality of OQM. More charitably (which has always been Wiseman's approach) it could be applied to probabilistic theories but (unfortunately for Bell) it still does not imply that OQM is nonlocal.

As a side note, Gillis's hopes the vague intuitions Bell gives in the 'prequel' paper provide a notion of locality from which predetermination could be derived from predictability. But the quotes all discuss the potential localistic properties (which Bell thought would be desirable) of *hidden variable theories*. So this notion, even if had been carefully formulated, which it had not, would clearly not have enabled one to *derive* the existence of hidden variables from any quantum correlations. If this is what Bell meant by locality, it would make his suggestion that OQM is nonlocal not merely false, but foolish (to steal a phrase used by Bell [7]).

In suggesting that the notion of local causality was there all the time, Gillis fails to recognize the formidable intellectual effort, starting with Bell's 1971 paper [15], then the Clauser-Horne paper of 1974 [16], then Bell's 1976 paper [14] and the subsequent conversation about free will [17, 18], to develop the formal notion of local causality. Furthermore, the assurance with which Gillis asserts that this notion is the way to capture these intuitive notions runs counter to Bell's own thinking on this subject. Even in 1990, after thinking about these issues for decades, in his final essay on the subject, La Nouvelle Cuisine [19], a main theme is "the problem of formulating [cause and effect] sharply in contemporary physical theory." In this same essay, notice how tentative he is at the very point at which he introduced local causality even at this late date: "it is precisely in cleaning up intuitive ideas for mathematics that one is likely to throw out the baby with the bath water. So the next step should be viewed with the utmost suspicion: A theory will be said to be locally causal..." When interpreting Bell, it is critical to recognize both the value he placed on correct formalization of intuitions and his extreme caution when attempting to do so.

4. What Bell meant by 'locality'

Gillis make an interesting suggestion, on page 7, that what Wiseman has taken to be Bell's definition of locality,

> 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.

could instead be read as stating what Bell took to be a *consequence* of locality (whatever that may be). We admit that is a possible reading. However, his phrase "more precisely that" most naturally indicates a definition. If he had meant merely "a particular consequence of locality," as Gillis suggests, then Bell should have said "more particularly that." Whatever the case may be, it doesn't help move Gillis's general argument forward. Bell does not formalize any broader concept of locality that could play the role that Gillis wants it to play.

Gillis suggests a variety of vague statements to describe what he claims to be Bell's views of locality in 1964 such as "no action-at-a-distance," "continuous, subluminal propagation through space," and "No Superluminal Effects (NSE)" all of which he presumes mean roughly the same thing as local causality as Bell introduced it in 1976. A phrase like "no action-at-a-distance" means next to nothing in itself. It cannot be used to derive any conclusions until it has been formalized, and cannot be used to reach agreement on fundamental issues with those holding different intuitions about what these phrases mean.

With respect to Bell's assumptions and proofs in the 1964 paper, nothing in Gillis's paper is likely to dissuade readers of Ref. [2] from its key claims, briefly,

(i) There is no notion of locality set out in 1964, or earlier, by Bell, which allows a rigorous derivation of his result without assuming predetermination of outcomes (or something like it).

(ii) This was not simply because Bell could not be bothered to formalize something obvious. Rather, it was a major effort over several years and several authors, to get to local causality as Bell defined it in 1976.

(iii) Bell shows no appreciation in 1964 of the nuances of Einstein's earlier work. It is doubtful that Bell had even read the EPR paper [9], and he also misses the crucial dual assumptions in Einstein's 1949 scientific autobiography [20]; see also Ref. [4].

(iv) Bell is very clear about his assumptions in 1964, stating four times that his theorem assumes predetermination. He had an *informal* argument in favour of considering this assumption, but he knew that he did not have a *formal* notion of locality that would do the job without the assumption of predetermination.

We thank Gillis for his arguments (pp. 8–9) as to why Bell's statements about locality in the 1964 paper focus on the settings. While Gillis intends his arguments to explain why Bell phrased his paper in terms of settings rather than a more general notion of locality, these arguments instead provide support for the operationalist or AUR reading of Bell's 1964 paper. They explain why Bell may have initially thought about locality primarily in terms of the settings, and why he was able to formulate his notion of local causality only at a later date.

5. Correcting misstatments of our views, and those of Bell and Einstein

Gillis presents some material in a way that would suggest to many readers that the material is new to the discussion, when in fact it was already discussed in Wiseman's paper, often in greater depth. Examples include the factorizability condition, Jarrett and Shimony's notion of Jarrett completeness or outcome independence, and the relation between these concepts.

In this section, we attempt to correct Gillis's serious misstatements of our views, and those of Bell and Einstein.

5.1. Quoting Bell out of context

Gillis claims that Bell's 1964 paper [21] proved that 'NSE' ("No Superluminal Effects") is contradicted by 'QSP' ("Quantum Statistical Predictions"). Bell uses no term similar to NSE — he uses neither the term 'cause' nor the term 'effect' — which should raise warning bells about Gillis's analysis. Nevertheless, Gillis quotes Bell, towards the end of his (Gillis's) Sec. 2, to attempt to support his (Gillis's) interpretation of Bell:

> Moreover, the signal involved must propagate instantaneously, so that such a theory could not be Lorentz invariant.

However, in context, this quote actually supports the operationalist or AUR reading: that Bell made two assumptions in 1964, locality and predetermination. The context is, in fact, provided by Gillis at the beginning of his Sec. 1: The immediately preceding sentence is

> 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 measurement device can influence the reading of another instrument, however remote.

Thus, when Bell says "such a theory" he means a theory with predetermined measurement outcomes, not arbitrary theories.

In the next sentence, which Gillis also quotes, Bell raises the possibility that (to use Gillis's term) QSP might be false. Bell naturally did not raise the 'possibility' that determinism might be false because all of his readers would have known that. In 1964 the challenge was to convince readers that there was *any* point considering deterministic theories at all. This is why he begins in the first sentence with the "paradox of EPR", to motivate the idea of hidden variables. Note that he does not, in the abstract, say that the EPR argument implies determinism/causality from locality. He just says it was "advanced as an argument [for hidden variables]." He does not even commit to it being a correct argument, and in this he certainly reflects the prevailing mood at the time (see also Sec. 5.2 below).

As a second, related example, Gillis says that "Wiseman's interpretation of 'locality' as PI is inconsistent with Bell's statement at the beginning of the [1964] paper that

additional (i.e.., hidden) variables were needed to restore locality to quantum theory." Gillis is cherry picking here, and not quoting Bell properly. What Bell actually says is "These additional variables were to restore to the theory causality and locality [2]." By leaving out "causality" and introducing the word "needed", Gillis changes the meaning in an important way. Revealingly, in his concluding paragraph, Gillis quotes the final part of Bell's sentence, but puts ellipses in place of "causality and." In order to have any chance of convincing operationalists of his reading, Gillis would have to explicitly deal with the appearance of causality in these sentences.

5.2. EPR and Bohm and Aharonov

Gillis claims that the EPR argument was well understood as an "obvious clash between the principle of no superluminal action-at-a-distance (which was regarded as essential to relativity), and the perfect correlations between spacelike-separated measurement results that quantum theory predicted, but could not explain." This is not, however, how EPR presented the argument. They never mentioned relativity and they certainly do not formulate a "principle of no superluminal action-at-a-distance." This is crucial to understand: *No physicist had formulated any such principle*, applicable to probabilistic theories such as quantum mechanics, that Bell could plausibly have used in 1964 as the single assumption to derive his inequality (see Sec. 3.3 of Ref. [2]). Without reasonably precise definitions, it is impossible to prove anything, and Bell rightly believed that he had proved something [2].

Gillis quotes the 1957 paper by Bohm and Aharonov [22], which Bell was certainly aware of prior to 1964, as evidence that physicists, Bell included, well understood the EPR argument. But the Bohm and Aharonov passage is far from a clear explanation of the EPR paradox. It is even less formal than Bell's presentation. It is merely an intuitive argument, containing no definition of locality from which one could draw the conclusion that OQM is nonlocal. This is to be contrasted with the EPR paper itself, which is very formal, with a complex web of necessary and sufficient conditions, albeit still not completely unambiguous (see Ref. [23], and the appendix in Ref. [2]).

Gillis says "Given this very widespread understanding, it was entirely reasonable for Bell to proceed based on a brief, informal recapitulation of EPR." But Gillis presents no evidence that there was widespread understanding of any precise formulation of an EPR-style argument from perfect correlations to predetermined outcomes. As Wiseman and Rieffel note [4], the famous 1969 paper by Clauser *et al.* [24] is non-committal about the correctness of the EPR argument (our p. 97), and Bell in 1971 [15] says only that perfect correlation "strongly suggests" predetermination (our p. 88). This, and much other evidence we present, supports our contention that Bell did not have a formal argument in mind in this part of the paper. Nevertheless it was indeed "entirely reasonable " for him to give an informal argument because this part of the paper appears merely as a motivation for his assumption of predetermination, and Bell explicitly indicates that this is the role of the EPR argument in his 1971 paper [15].

It would be unfair to expect Bell's first paper to be flawless. He had simply not thought hard enough, or written slowly enough, in 1964, to be completely consistent. He had proven a brilliant new theorem and wanted to communicate that quickly. Gillis says the first paragraph of Bell's "Formulation" is 'strained' under Wiseman's interpretation. The strain is unavoidable because of the flaw in Bell's logic itself, as others who disagree with Wiseman's more general arguments have admitted [8]. The argument Bell gives is, at best, heuristic. At this point in time Bell had himself not sorted out the difference between his intuitive localistic notions and the notion of locality he actually uses in his theorem. But his *theorem* is not at all flawed because he is always clear in his 1964 paper, when stating his theorem, that he assumed predetermination.

5.3. Bell's logic when referencing Einstein

Rieffel and Wiseman [4] described Bell's citations of Einstein in his 1964 paper as interruptions of his sentences. Gillis seems to have entirely misunderstood our argument when he says "they cannot rewrite Bell's paper in order to eliminate portions that conflict with their interpretation of it." When we say that "In this, and every other, instance, the reference '[2]' could be omitted from the sentence, and it would actually improve the grammatical and scientific clarity of the sentence," we were not suggesting that the referencing of [2] is in "conflict with [our] interpretation of it." Rather, we were making the point that one cannot read Bell's paper as if the text of the footnote were present in the text anywhere the "[2]" appears. We were thus inviting the reader to consider why Bell used a footnote here rather than including this material in the main body of his text. To us, the answer is clear: the quotation of Einstein expresses a similar notion of locality to the more precise one which Bell introduces, and Bell was appealing to his authority to motivate it. If these ideas and phrases of Einstein's were absolutely crucial to the understanding of his (Bell's) text, it would have been an exceedingly odd choice on Bell's part to place them in a mere footnote.

For the benefit of the reader, the Einstein quote in the footnote in question reads:

> 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.

As discussed in [2], and in greater detail in Ref. [4], Einstein himself states that an extra assumption is needed to conclude that OQM is incomplete – he explicitly assumes that the conditional wavefunctions of the distant systems are part of such "real factual situations". Thus, even if Bell's 1964 notion of locality were to be equated with the Einstein quote, it would not necessarily be a property that quantum mechanics lacked, and it could certainly not be equated with local causality.

6. Conclusion

Wiseman's main aim in writing [2, 3, 4] was to facilitate communication between two interpretational camps, whom he called 'operationalists' and 'realists', by explaining carefully how and why they use the phrase "Bell's theorem" to mean different things: his 1964 theorem (assuming locality and determinism) and his 1976 theorem (assuming local causality), respectively. As Wiseman has emphasized many times, his papers address Bell's theorem(s), not Bell's intuitions. It is perfectly legitimate to try to understand Bell's intuitions, of course, but these intuitions should not be confused with his stated assumptions, and what he proved from those assumptions. It remains the hope of all three of us that through careful consideration of what Bell proved, and what notions were formalized, Gillis and others can come to a better understanding of the views of those in other camps, and also a deeper understanding of the substantial intellectual effort that went into Bell's formalization of intuitive notions of locality as local causality. By doing so, they can also come to appreciate, even contribute to, recent efforts [10, 25, 3] to make further advances in solving "the problem of formulating [cause and effect] sharply in contemporary physical theory."

Acknowledgements

EGC acknowledges funding from an Australian Research Council grant DE120100559, and the Foundational Questions Institute (FQXi) Large Grants program. HMW acknowledges support by the ARC Discovery Project DP140100648.

References

[1] Edward J. Gillis. On the analysis of Bell's 1964 paper by Wiseman, Cavalcanti, and Rieffel. *International Journal of Quantum Foundations*, 1:??, 2015.

[2] H M Wiseman. The two Bell's theorems of John Bell. *J. Phys. A*, 47(42):424001, 2014.

[3] H. M. Wiseman and E. G. Cavalcanti. *Causarum Investigatio* and the two Bell's theorems of John Bell. arXiv:1503.06413. To be published as a chapter in *Quantum (Un)speakables II*, 2015.

[4] Howard M. Wiseman and Eleanor G. Rieffel. Reply to Norsen's paper "are there really two different Bell's theorems?". *International Journal of Quantum Foundations*, 1:85–99, 2015.

[5] J. S. Bell. On the Einstein-Podolsky-Rosen paradox. *Physics*, 1:195, 1964. reprinted in Ref. [26].

[6] Howard M. Wiseman. Bell's theorem still reverberates. *Nature*, 510:467–469, 2014.

[7] Tim Maudlin. What Bell did. *J. Phys. A*, 47(42):424010, 2014.

[8] Travis Norsen. Are there really two different Bell's theorems? *International Journal of Quantum Foundations*, 1:65–84, 2015.

[9] A. Einstein, B. Podolsky, and N. Rosen. Can quantum-mechanical description of physical reality be considered complete? *Phys. Rev.*, 47(10):777, May 1935.

[10] Eric G. Cavalcanti and Raymond Lal. On modifications of Reichenbach's principle of common cause in light of Bell's theorem. *J. Phys. A: Math. Theor.*, 47(42):424018, 2014.

[11] Christopher J. Wood and Robert W. Spekkens. The lesson of causal discovery algorithms for

quantum correlations: Causal explanations of Bell-inequality violations require fine-tuning. arXiv:1208.4119v2, 2012.

[12] John S. Bell. Bertlmann's socks and the nature of reality. *Journal de Physique Colloques*, 42(C2):41–62, 1981. (Reproduced in Ref. [27].).

[13] John S. Bell. On the problem of hidden variables in quantum mechanics. *Rev. Mod. Phys.*, 38(3):447–452, Jul 1966. (Reproduced in Ref. [27].).

[14] J. S. Bell. The theory of local beables. *Epistemological Lett.*, 9:11–24, March 1976. (Reproduced in Ref. [27].).

[15] J. S. Bell. Introduction to the hidden-variable question. In *Foundations of Quantum Mechanics*, pages 171–81, New York, 1971. Academic. (Reproduced in Ref. [27].).

[16] John F. Clauser and Michael A. Horne. Experimental consequences of objective local theories. *Phys. Rev. D*, 10:526–35, 1974.

[17] A. Shimony, M. A. Horne, and J. F. Clauser. Comment on 'The theory of local beables'. *Epistemological Lett.*, 13:1–8, 1976. (Reproduced in Dialectica **39** (1985).).

[18] J. S. Bell. Free variables and local causality. *Epistemological Lett.*, 15:79–84, February 1977. (Reproduced in Ref. [27].).

[19] J. S. Bell. La nouvelle cuisine. In A. Sarmeljin and P Kroes, editors, *Between Science and Technology*, pages 97–115. Elsevier/North-Holland, 1990. (Reproduced in Ref. [27].).

[20] Albert Einstein. Autobiographical notes. In P. A. Schilpp, editor, *Albert Einstein: Philosopher-Scientist*, volume 7, 1949. Translation (the odd-numbered pages) by the editor.

[21] J. S. Bell. On the Einstein-Podolsy-Rosen paradox. *Physics*, 1:195, 1964. (Reproduced in Ref. [27].).

[22] D. Bohm and Y. Aharonov. Discussion of experimental proof for the paradox of einstein, rosen, and podolsky. *Phys. Rev.*, 108:1070–1076, 1957.

[23] Howard M. Wiseman. Quantum discord is Bohr's notion of non-mechanical disturbance introduced to counter the Einstein-Podolsky-Rosen argument. *Annals of Physics*, 338:361–374, 2013.

[24] John F. Clauser, Michael A. Horne, Abner Shimony, and Richard A. Holt. Proposed experiment to test local hidden-variable theories. *Phys. Rev. Lett.*, 23:880–884, 1969.

[25] Christopher J. Wood and Robert W. Spekkens. The lesson of causal discovery algorithms for quantum correlations: Causal explanations of Bell-inequality violations require fine-tuning. *New J. Phys.*, 17, 2015.

[26] J. S. Bell. *Speakable and Unspeakable in Quantum Mechanics*. Cambridge University Press, Cambridge, 1987.

[27] M. Bell, K. Gottfried, and M. Veltman, editors. *John S. Bell on the Foundations of Quantum Mechanics*. World Scientific, Singapore, 2001.