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Peer Review

Review of: "Questioning the Reasonableness of the Quantum Nonlocality Debate"

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The aim of the present paper is to expose fallacies and inconsistencies occurring in the quantum nonlocality debate. By the time I prepared my report, a new version appeared; even the title has changed, becoming somewhat less provocative, and also reports of two other referees have become available. Therefore, I am preparing a new version of my report. A fairly unusual form of peer reviewing a paper. While I do agree with the author on many things, I also agree with most of the criticism the other two referees have put forward.

I do agree with the author that the arguments and proofs, correct or incorrect, presented so far are not sufficient to decide definitely whether nonlocality is present in the quantum world or not. Rejection of the applicability of counterfactual reasoning in quantum physics and/or rejection of the existence of hidden variables does not prove locality; only it leaves open the possibility that locality is valid. I find it an important point that in this case, the observed correlations, especially the perfect ones, remain unexplained. Therefore, accepting locality means rejecting the need for an explanation for the correlations (or hoping that such an explanation will eventually be found). It is also true that it is empirically consistent to postulate that operational quantum mechanics is local (no-signalling).

Eq.(1) indicates that what is proven by the EPR argument is that realism follows from locality alone, and I find this misleading, a gross oversimplification. There are other points in the argument, too, like the existence of perfect correlations.

I do admit that before reading the present paper, I had thought that counterfactual considerations are required to derive Bell inequalities. This paper, and even more a previous one from the author (Found. Phys. 51:84, 2021), convinced me that I was wrong. I note that in the case of Bell inequalities with

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measurements of several outcomes, more parties, and more measurement settings per party, the number of effective hidden variables may be huge, but I do not think it leads to any fundamental problem. I do agree with all the conclusions the author draws (both in the present paper and his other ones that I have read) from the violation of the Bell inequality derived supposing the existence of nonconspiratory local hidden variables.

The same inequalities can be derived without supposing the existence of hidden variables, but using counterfactual reasoning and assuming that the measurement outcome of one party does not depend on the measurement setting of the other party in each round of the experiment. This is regarded as the condition of locality, a statement that is counterfactual itself. When the same expression can be derived in more than one different way, using different assumptions and/or different considerations, if the expression is not obeyed either by a theory or by experimental results, conclusions can be drawn from each derivation separately. The author dismisses the conclusions that may be drawn from the counterfactual way of derivation of the Bell inequality in his papers, including the present one, regarding them as irrelevant, misleading, and inconsistent, that can and should be ignored. I am not at all convinced. The author claims that when the Bell inequality is derived this way, not only is counterfactual reasoning used, but counterfactual definiteness is also supposed, in which case, according to the author, results of unperformed measurements are treated as physically real. In an earlier paper (Int. J. Quantum Inf. 19(03):2150018, 2021), and also in the present paper, the author shows a sort of counterfactual derivation of the Bell inequality, doing exactly that. However, in my opinion, this is not a correct way to do it. I doubt it would convince anyone about the relevance of this sort of reasoning. Therefore, I will sketch two other counterfactual proofs, which do not treat unmeasured quantities as real.

The first one is about the CHSH inequality. I have not seen exactly this line of reasoning in the literature, but I am sure it has already been invented by others. It is actually the same as Stepp's argument given in the author's other previous paper (Found. Phys. 52:98, 2022) in Appendix B, taken somewhat further. Let us consider a long enough set of measurements with random choices of settings by both parties in each round. After all results are available, let us calculate the averages of the products of the outcomes for each pair of settings, and calculate their combination appearing in the inequality. Now let us take three imaginary sets of measurements. In those sets, one party, or the other party, or both parties choose the other measurement setting than they actually did in each round. Under the circumstances, one can reasonably suppose that each one of the three imaginary experiments would give the same number for the relevant average value as the experiment that has actually been performed, within statistical errors. If we formally take the average of those four approximately equal numbers while imposing the usual locality requirement, and rearrange the order of the terms, we get the bound in the usual way.

The other proof is not about the CHSH inequality, but about the angular dependence of the correlation of the outcomes of measurements of light polarization (or electron spins) on maximally entangled pairs. The argument I briefly summarize here can be found in the book by Nick Herbert, entitled Quantum reality beyond the new physics. Let the measurement settings be characterizable by some spatial directions. Let the systems and the binary quantities measured be such that the correlations of the outcomes depend only on the relative angle between the measurement directions of the parties, and at zero degrees let there be full correlation. Therefore, if the parties measure in the same direction, they always get identical results. Let one of the parties turn his/her measuring device by some angle. Then sometimes they will register different outcomes, say in d% of the rounds. Because of the locality requirement, the reason for the discrepancy between the outcomes will be that the party who changed the measurement direction, and only that party, will register the other outcome in those rounds. The same is true if the other party changes the measurement direction. Now let both parties turn their devices by the same angle in opposite directions. The relative angle will be two times the original one. It is trivial that the discrepancy between their outcomes now cannot be more than 2d% (it can be smaller if there is an overlap between the rounds when they got the opposite result). That is, the discrepancy at a certain angle cannot be more than two times the discrepancy at one half of the angle. Actually, this means that the angular dependence of the correlation at zero degrees cannot be differentiable, while quantum mechanics predicts differentiable functions for both light polarization and electron spin measurements.

Here unmeasured values have not been treated as real; only very plausibly looking assumptions are made about them. When the inequality is violated, conclusions can be drawn concerning those assumptions about the unmeasured measurement values, or on the applicability of the approach. I regard it mostly a matter of taste if someone regards those conclusions as interesting, or even relevant. I personally regard the potential consequences as even more intriguing than the non-existence of nonconspiratory local hidden variables. One possibility, which may look like the only one at first and superficial sight, is the spooky action at a distance. After all, taking the Copenhagen interpretation literally, this is exactly what happens. Another conclusion can be the inadequacy of counterfactual reasoning in a quantum mechanical context, taking most seriously that unperformed experiments have no results. However, it is also true that the rejection of counterfactual reasoning alone does not explain the correlations observed, especially the perfect ones. Nevertheless, in my opinion, the possible inadequacy of counterfactual reasoning in quantum physics is interesting and intriguing. It is not that easy to accept that it may be inappropriate. Counterfactual reasoning is a very integral part of human thinking. Our rational decisions are mostly based on counterfactual reasoning. In classical physics, counterfactual definiteness, whichever definition one accepts, is valid. The results of unperformed measurements can in principle be determined if the properties of the system and the details of the potential measurement are known; therefore, one can speak meaningfully about them. These results are even falsifiable because one may perform that measurement after all, and if everything is done carefully enough, the result will be the expected one.

An argument in an earlier paper by the author (Int. J. Quantum Inf. 19(03):2150018, 2021) seems to contradict this statement. The argument is based on Michel Feldmann's local realistic model. Actually, the model may be realized with a purely classical system. The objects to be measured may be pairs of antiparallel coins sent to the parties to be measured, whose spatial orientation follows some probability distribution. The measurement is to look at the coin from some direction to get either heads (+1) or tails (-1). The probability distribution for the orientation of the coins depends on the measurement settings. Although the setup is completely classical, the correlations mimic those one gets when measuring the spin projections of electron pairs in singlet states; therefore, even the maximum violation of the CHSH inequality can be achieved with an appropriate measurement setting. It is easy to see what goes wrong in the proof of the inequality, and that is not counterfactual definiteness. Simply, the measurement result of one party now does depend on the measurement setting of the other one. Let us choose the version of the model when the probability distribution of the orientation of coins depends on the measurement setting of party A. Then if A chooses her setting 1, the source will prepare a pair of coins in some orientation (according to a probability distribution), while if she chooses setting 2, the orientation of the coins will be another, different one (according to a different distribution). Obviously, the outcome of B will depend on the orientation of his coin, therefore on the setting of A. Therefore, the condition that is supposed to be the locality requirement fails now; possibly it should be refined. Actually, this model cannot be realized such that the decisions about the measurement settings are made only when the particles are already on their way.

In my opinion, several of the issues the author raises are relevant and important, but I also agree with many of the objections of the other two referees, and I do not agree with many of the arguments written about the counterfactual derivation of the Bell inequality.

Declarations

Potential competing interests: No potential competing interests to declare.

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