Review of: "Further comments on ‘Is the moon there if nobody looks? Bell inequalities and physical reality’"

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Potential competing interests: No potential competing interests to declare.

The author criticizes and misinterprets again our contextual hidden variable model (CHVM) defined by the equation (3) in [1]. He agrees now, that it is not a special case of a standard local hidden variable model (LHVM), what he claimed in the previous papers [2-4], but he dismisses this model, because, according to him, it allows arbitrary non-locality, *ie direct causal effects of local measurement settings on distant measurement outcomes*.

This conclusion is false, because it is based on incorrect causal interpretation of conditional probabilities. Our model has enough flexibility to explain experimental data in Bell Tests including “nonlocal” imperfect correlations and various anomalies, but it does not mean that it is vacuous or non-local. Newton’s equations can give precise predictions, only if explicit assumptions are made on forces, masses and initial conditions. Similarly in our model a definite probabilistic predictions can be made, if specific assumptions on X_a, X_b, p(.,.) and p_{ab}(.,.) are made. It is well known, that a singlet state describing an ensemble of “entangled pairs” is rotationally invariant, thus by introducing correlated setting dependent variables L_a and L_b , describing distant instruments, one can more easily reproduce cos(theta) dependence of correlations predicted by quantum mechanics (QM) and consistent with experimental data. In this review, L_a denotes lambda_a, a set of all L_a is denoted L(a) etc. If instrument variables are not correctly incorporated into probabilistic model, then strongly correlated distant binary outcomes in response to local perfectly random inputs escape rational explanation.

In our model, outputs (X, Y) are functions of the corresponding local variables (L_1, La) and (L_2, L_b) respectively and they do not depend on the distant settings. Only models in which local outputs depend on the distant inputs are called non-local. Please note that “a” and “b” are simply the names indicating Alice’s and Bob’s instrument variables, in the articles [5-8] they were never replaced by (0,1) or (1,2), thus no misunderstanding was possible. If one wants to be pedantic one could call them L'_a or M_a etc. Similarly “1” and “2” denote only the corresponding distant physical systems.

The violation of statistical independence neither implies the existence of spooky influences nor lack of experimenters’ freedom of choice [9]. In our model, a statistical independence is violated in a particular way:

$$p(a, b|L_a, L_b)=1 \qquad (1)$$

The equation (1) means only, that if a “hidden event” {L_a , L_b } “happened” then a specific binary setting labels (inputs (a, b)) were randomly chosen, before this particular round of measurements has started [6,8,10]. These binary inputs are locally chosen using quantum random number generators (QRNG) or human choices, p(a, b)=p(a) p(b) and they do not have any causal influence on the outputs (X,Y).

A choice of specific inputs (a, b) is followed by a choice of corresponding instruments and specific experimental protocols, described by two sets of hidden variables L(a) and L(b).In each experimental trial, X is a function of (L_1,L_a) and Y is a function of (L_2, L_b).

Of course (L_a, L_b) are neither functions of (L(a), L(b)) nor of (a, b), this why the author completely misunderstood our model:

"An actual value L_a or L_b determines the value of the setting a and moreover whether it belongs to Alice's or to Bob's apparatus. The actually used setting, as well as the user of the setting, is a function of the instrument setting-dependent hidden variable."

Few paragraphs below the author repeats:

"Anyway, MK wants to allow the pairs L_a, L_b to be statistically dependent, where moreover Alice's "a" is a function of L_a and Bob's "b" is a function of L_b."

and he claims, that we replace the Cartesian products of R^p spaces by points:

"But one can always artificially make such spaces disjoint: for the instrument hidden variables, replace R^p with its Cartesian product with a one-point set containing the ordered pair "(party, setting)". Here, "party" is Alice or Bob, ie, which apparatus, and "setting" could be an angle, or just a binary setting choice. One can carry out a similar operation on the two sources."

Then the author eliminates L_1 and L_2 as superfluous, replaces our L_a by "Alice" , our L_b by "Bob" , defines a probability distribution on 6-tuples: *L'_a={−1, +1}×{("Alice", a)} and L'_b= {−1, +1}×{("Bob", b)}* and after adding couple of additional assumptions, which we precise below, arrives to the conclusion that :*the distribution of (A,B,X,Y) is arbitrary.*

One does not need 6-tuplets to arrive to a similar conclusion. If, one eliminates L(1) and L(2) as superfluous, assumes that $p(L_a, L_b)$ is arbitrary and $x=X_a(L_a)$ and $y=Y_b(L_b)$, then one immediately may conclude that $p_{ab}(x,y)$ is arbitrary. If one also assumes, as the author does, that $a=f(L_a)$and $b=g(L_b)$, then p(a,b) is also arbitrary.

This reasoning and conclusions are correct, but it has nothing to do with our model the author wants to criticize. In our model, using a more pedantic notation:

$$P(L_1,L_2,L'_a,L'_b)=P_{ab}(L'_a,L'_b)P(a)P(b)P(L_1, L_2) \quad (2)$$

and a probability P(X=x,Y=y|a,b) is obtained from (2) by summing over all lambdas satisfying the conditions $X_a(L_1, L_a)=x$ and $Y_b(L_b)=y$ and by dividing by an appropriate normalization constant.

The author noticed that, the normalization constants C_{xy} =P(Ax and By different from 0) were missing in several equations in the papers [5-8]. It did not affect the reasoning and conclusions of these papers because these equations were never used to make quantitative predictions. Nevertheless, if it is possible we will publish an erratum.

Our model is not vacuous and by specifying various factors in (2) one can make quantitative predictions and compare them with the experimental data in Bell Tests.

The violation of statistical independence neither implies spooky influences nor super-determinism and as we explained in [1, 10] it may be due to the setting dependent post selection of final no vanishing data [5-8] or to global space-time symmetries.

In section 2, the author refers to the construction of a probabilistic coupling [2-4] of the model we proposed in [5] to describe the raw data in some Bell Tests. We explained in [1,11,12] that this construction was completely irrelevant and ,contrary to what the authors claimed, it did not invalidate the soundness and conclusions of the papers they criticized. Their papers contained several misleading statements, which might sometimes be perceived as defamatory. We will not repeat them here.

This paper contains also a statement, which does not fit to a serious scientific article:

"It seems to this author that the complexity of MK's notation and reasoning has led the author, over the years, deeper and deeper into misunderstanding of his own results. Each successive paper partially quotes his earlier results, but also modifies them, for instance by omitting key conditions. The mistakes possibly come about."

This statement should be retracted and this preprint rewritten because the author's reasoning and conclusion:

"Kupczynski's latest model of a Bell experiment places no constraints whatsoever on the statistics of the observed results. It effectively assumes non-locality of the effects of measurement settings on measurement outcomes."

are incorrect and misleading.

Author is a prominent scientist, thus his article is well written and his reasoning and conclusions seem to be convincing. This is why, it is not surprising, that two reviewers rated this article 4 stars. For these qualities the article merits high rating. Unfortunately, neither they nor the author noticed that this article is not about our model. In our model free random inputs (a, b) are not functions of (L_a, L_b), (L_1, L_2) are not superfluous and the locally created outputs do not depend causally on random distant inputs and as we wrote in [6]:

"In our model, measurement outcomes are neither predetermined nor produced in an irreducibly random way. We explain why, contrary to the general belief, the introduction of setting-dependent parameters does not restrict experimenters' freedom of choice. Since the violation of Bell-type inequalities does not allow the conclusion that Nature is non-local and that quantum theory is complete, the Bohr–Einstein quantum debate may not be closed"

This is why; I can only rate this article 1 star.

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