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Comment on "Stapp's Theorem Without Counterfactual Commitment" *

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Abstract

Micheal Dickson claims to have found a flaw in the 'could' version of my nonlocality theorem. His argument is examined, and the points of contention identified. The main error in his reasoning is that he takes the full content of the locality requirement to be the conjunction of certain consequences of the separate demands that there be no influence from A to B (with influence from B to A allowed), and no influence from B to A (with influence from A to B allowed). In my paper I emphasized that this conjunction is insufficient to do the job, and used, accordingly, a stronger locality condition, which comes from the demand that there be no influence from A to B and, at the same time, no influence from B to A. Thus Dickson's demonstration that my proof does not go through if one uses only the weaker locality condition is in complete accord with my own identical claim, and does not entail any flaw in my argument. Several other points are also discussed.

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In an article in this journal Michael Dickson [1993] has noted that a generalization of Bell's theorem described in several papers of mine [Stapp (1988), (1989), (1990), (1991)] is significantly stronger than those proved by other authors [Bell (1971), Clauser and Shimony (1978), Jarrett (1984)]. He claims, however, that my proof is not valid. Let us examine his argument.

An initial task in the formulation of locality is to specify what it means for something to be "influenced" by something else. I discussed this question in detail in Stapp (1988), and gave in Stapp (1990) a very weak sufficient condition:

> Suppose there is a set $(e_1, ..., e_m)$ of experimental parameters, a theory T, and a variable z (representing the possible outcomes of some measurement). If for some value of each of the e_j with $j \neq i$ there is *no value* $\overline{z}$ of z such that the theory T permit e_i to range over its entire range with z held fixed at $\overline{z}$, then it is said that the theory *mandates an influence* of e_i on z.

Dickson claims that this definition has a serious problem. He says:

> Suppose that T is a stochastic theory (in which conservation of probability holds) so that in T the conditional probability $p(z|e)$ is nontrivial. Suppose that in T, $z = \pm 1$, $e\epsilon\{b, b'\}$, and $p_r(z = +1|e = b) = \frac{1}{4}$ while $p_r(z = +1|e = b') = \frac{3}{4}$. By Stapp's account z does not 'depend' on e (in T), because for every value $\overline{z}$ of z, T permits e to be varied over its entire range while z retains its value $\overline{z}$. (It does so because both $z = +1$ and $z = -1$ have nonzero probability for every value of e). But clearly z *does* depend upon e, according to T.

This 'serious problem' with my definition is no problem at all. My criterion for the existence of an influence is purposely taken to be a very weak sufficient condition: it is neither designed to be, nor stated as, a necessary condition. It asserts only that if the stated condition holds then there must be an influence. It does not assert that there is no influence if this condition is not satisfied. However, Dickson's argument asserts that "By Stapp's account z does not 'depend' on e (in T), because ... ". That statement is incorrect because it uses the necessary condition, whereas my criterion is only a sufficient condition. Thus

Dickson's rejection of my criterion rests on a logical error: a failure distinguish necessary from sufficient.

Because of this supposed serious problem with my criterion Dickson adopts a substitute:

> Dependence: A theory T 'mandates an influence' of some physical quantity, e, on some physical quantity, z, just in case the conditional probability $p(z|e)$ in T is different for some different values of e, ...

For this replacement to be similar to mine it should be converted from a necessary and sufficient condition to a sufficient condition: 'just in case' should be replaced by 'if'. Even then it appears to be very different from mine. It mandates the existence of an influence under the condition that the conditional probability function $p(z|e)$ depends on e, whereas my sufficient condition for a theory T to mandate an influence makes no mention of the conditional probability function: my sufficient condition is that T permits there to be *no* possible outcome $\overline{z}$ of some measurements such that this outcome could occur no matter which value of the parameter e is selected.

Dickson claims, without proof, that he could easily construct a counterexample to my argument if he retained my definition. It is hard to imagine how he could do so, for my (exclusively) sufficient condition for an influence provides no logical basis for proving a contradictory nondependence. A logically correct way for him to invalidate my proof would be for him to exhibit this easy construction.

Dickson considers next the locality condition that I use in Stapp (1989):

> For all that pertains to the generation of a result of a measurement performed in R_A, the choice made by the experimenter in R_B can be considered not to exist, *and* [ditto with $A \leftrightarrow B$].

I immediately stated that this condition was to be used in conjunction with the following rule of inference:

> If a condition X can be satisfied *only* by having something depend nontrivially upon something that does not exist, then that condition cannot be satisfied.

Dickson was unable see how these two parts could be combined. The reason is this: he tried to combine the two parts by identifying X with *Locality* whereas an examination of the argument in Stapp (1989) shows that, in the present context, it should be identified not with Locality but, instead, with a *failure* of Locality. This mix-up of Locality with its converse led, not surprisingly, to total nonsense, which Dickson used to justify his rejection of my Locality condition.

I conclude that Dickson's arguments for rejecting my Influence and Locality conditions are both invalid: in each case his argument rests on mixing up the key condition with its opposite.

It is, nevertheless, interesting that Dickson, although starting from alternative formulations of my two key assumptions, was able to derive all but the final step in my proof: all of the steps leading up to this crucial final step were obtained by Dickson starting from his alternative starting point. This result tends to confirm these preliminary steps in my analysis, and places the focus of the discussion exactly where it belongs — on the crucial final step.

Each of the consequences of the locality condition that was deduced prior to this crucial final step was a consequence either of the requirement (1), that there be no influence from R_A to R_B (with influences from R_B to R_A allowed), or of the requirement (2), that there be no influence from R_B to R_A (with influences from R_A to R_B allowed). The 'flaw' that Dickson discovered in my proof is that the conjunction of these consequences of the two one-way conditions is not sufficient to justify the final step. I had already emphasized that very point. Thus Dickson's failure to be able to justify the final step of my argument without using some stronger consequence of locality is completely in line with my own prior claim.

Let me explain this point in more detail. Dickson and I both arrive at the same penultimate conclusion:

There exists a set of values $(x_1, x_2, y_{11}, y_{12}, y_{21}, y_{22})$ such that the following four conditions are all satisfied:

A1. If $M_1^A M_1^B$ were to be performed, then it could be that $r_A = x_1$ and $r_B = y_{11}$.

A2. If $M_1^A M_2^B$ were to be performed, then it could be that $r_A = x_1$ and $r_B = y_{12}$.

A3. If $M_2^A M_1^B$ were to be performed, then it could be that $r_A = x_2$ and $r_B = y_{21}$.

A4. If $M_2^A M_2^B$ were to be performed, then it could be that $r_A = x_2$ and $r_B = y_{22}$.

and

There exists a set of values $(y_1, y_2, x_{11}, x_{12}, x_{21}, x_{22})$ such that the following four conditions are all satisfied:

B1. If $M_1^A M_1^B$ were to be performed, then it could be that $r_A = x_{11}$ and $r_B = y_1$.

B2. If $M_1^A M_2^B$ were to be performed, then it could be that $r_A = x_{12}$ and $r_B = y_2$.

B3. If $M_2^A M_1^B$ were to be performed, then it could be that $r_A = x_{21}$ and $r_B = y_1$.

B4. If $M_2^A M_2^B$ were to be performed, then it could be that $r_A = x_{22}$ and $r_B = y_2$.

The conditions A1-4 arise from the demand that there be no influence from R_B to R_A (with influence from R_A to R_B allowed). The conditions B1-4 arise from the demand that there be no influence from R_A to R_B (with influence from R_B R_A allowed).

I noted in Stapp (1989) that " It is obvious that no conflict with quantum theory can arise from $A' + B'$ (essentially the simple conjunction of A1-4 with B1-4) since quantum theory is certainly compatible with each of the two parts of LOC taken by itself. This can be seen by noting that the predictions of quantum theory can be explained either by a model with collapse in R_A and R_B induced by the measurement in R_A (this model has influence from R_A to R_B , but no influence from R_B to R_A), or by a model with collapse in R_A and R_B induced by the measurement in R_B (this model has influence from R_B to R_A, but no influence from R_A to R_B). Each model satisfies one part of LOC but not the other. (Hence the predictions of quantum theory must be compatible with each of the two separate parts of LOC separately: A1-4 can be satisfied, and also B1-4 can be satisfied.) However, LOC imposes its two subconditions simultaneously: it demands that both conditions be satisfied together, or simultaneously. (i.e., LOC imposes on the predictions the more severe requirements that follow from the demand that there be no influence from R_A to R_B and conjunctively, i.e.,at the same time, no influence from R_B to R_A. This locality requirement is stronger than the conjunction of A1-4 and B1-4.)

I have added to the original text the remarks in the parenthesis, in order to make the argument easier to follow.

It is only by using these stronger consequences of the locality condition that I was able to derive the final conclusion of this argument, which is Property C: There exists a quartet (x_1, x_2, y_1, y_2) such that nature could:

(1) If M_1^A were to be performed, produce in R_A the result x_1 independently of which measurement is performed in R_B, and if M_2^A were to be performed, produce in R_A the result x_2 independently of which measurement is performed in R_B, and

(2) If M_1^B were to be performed, produce in R_B the result y_1 independently of which measurement is performed in R_A, and if M_2^B were to be performed, produce in R_B the result y_2 independently of which measurement is performed in R_A.

My argument for obtaining Property C goes as follows: Immediately after stessing the need to use the stronger locality requirement I say "Now our assumption UR (Unique Results) requires, for each of the four possible values of (i,j), that if (M_i^A, M_j^B) is (I should have said "were to be")performed then some single pair (i,j) must appear. If several conditions are imposed on this pair (i,j), then they are imposed upon a single pair (i,j): this pair cannot be two different pair. Thus if the two parts of LOC are imposed simultaneously, or together, then there must exist a pair of sextets, $(x_1, x_2, y_{11}, y_{12}, y_{21}, y_{22})$ and $(x_{11}, x_{12}, x_{21}, x_{22}, y_1, y_2)$ such that the follwing four conditions are all satisfied:

1. If $(i,j) = (1,1)$, then the appearing pair (x,y) *could* be (x_{11}, y_1) and (x_1, y_{11}).

2. If $(i,j) = (2,1)$, then the appearing pair (x,y) *could* be (x_{21}, y_1) and $(x_2, y_{2,1})$.

3. If $(i,j) = (1,2)$, then the appearing pair (x,y) *could* be (x_{12}, y_2) and (x_1, y_{12}).

4. If $(i,j) = (2,2)$, then the appearing pair (x,y) *could* be (x_{22}, y_2) and (x_2, y_{22}).

This condition is equivalent to property C."

Dickson examines the transition from the simple conjunction $A' + B'$ to Property C and concludes that " Inference from one to the other is a fallacy in the probability calculus". That observation is correct, and it is in complete accord with my emphatic assertion that one must, in order to derive Property C use consequences of locality that are stronger than the conjunction of A' and B'.

Dickson goes on to ask whether it helps to add my postulate of Unique Results to $A' + B'$ and finds that the answer is no! This is again in complete accord with my claim that one must use locality requirements that are stronger than $A' + B'$.

Dickson proceeds to consider various possible ways of obtaining Property C, always restricting the consequences of the locality condition to the conjunction of A1-4 and B1-4, and always fails. He concludes, finally, that: "The inference from $A' + B'$ to C is therefore a probablistic fallacy of the sort described above." That conclusion is completely correct, and completely in line with my own claim that $A' + B'$ is insufficient. However, that fact does not establish the existence of any flaw in my proof. For I never claimed that Property C followed from $A' + B'$. In fact, I emphatically denied it.

Dickson's paper is, nevertheless, not without value. For it shows, in effect, that any effort to discredit the 'could' version of my nonlocality theorem must discredit my strengthening of the locality condition, namely the step that takes the locality condition beyond $A' + B'$. This is because Dickson has effectively validated the rest of my argument, and has shown, independently of my own argument for the same conclusion, that $A' + B'$ is insufficient. This strengthening of the locality condition is described in great detail in Stapp (1991).

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