I. INTRODUCTION

In a recent paper [1], I characterized interpreters of Bell’s Theorem [2] as falling into two classes: (1) those who think that Bell’s result (and the associated experimental data) prove that non-locality is a necessary feature of any empirically viable theory and hence a feature of nature itself, and (2) those who think that the results prove merely that non-locality is a necessary feature of any empirically viable theory of hidden variables (HV – or any theory containing a counter-factual definiteness (CFD) property, or some other similar anti-orthodox or anti-Copenhagen character). [11] In that earlier paper I took it for granted that Bell’s derivation of the (original, Bell) inequality did use a HV/CFD assumption, but argued that a modified (rigorous) version of the EPR argument [3] establishes that the necessary HV/CFD principle follows from locality – and hence should not be thought of as a separate axiom behind Bell’s inequality, restricting the class of theories to which the inequality is applicable (and hence likewise restricting the class of theories which the theorem shows must be non-local in order to agree with experiment). In short, I argued that locality alone gives rise to the inequality (but in a two-step dance consisting of the modified EPR argument followed by the derivation of the Bell inequality), thus demonstrating the correctness of the interpreters in “class (1)” from above.

An anonymous referee for that earlier paper, however, questioned whether this sophisticated two-step argument was really needed, since the CHSH [4] inequality (unlike the original Bell inequality, he suggested) could be derived without assuming HV/CFD. That is, the referee claimed that the CHSH inequality follows (in one step, so to speak) from locality alone – and hence its empirical violation demonstrates already that nonlocality is a fact of nature, without further analysis or discussion. [2]

I was initially skeptical of this claim, operating under the assumption that the difference between the various (generalized-) Bell inequalities is (roughly) ease of empirical testing, and not anything about their logical “inputs”. After a rather (and, in retrospect, unfairly and unfortunately) dismissive response to the referee, a voice in the back of my head urged me to think about this matter further, which I proceeded to do.

The first puzzle raised by this further thinking was this: is it even true that the (original) Bell inequality has some HV/CFD assumption built into it? This, I think, is widely accepted as a truism and there’s no doubt that many of the later attempts to popularize, explain, and derive a Bell inequality do make a HV/CFD assumption explicitly. (Mermin’s various derivations using the idea of “instruction sets” [6] are probably the clearest examples here.) But where, exactly, does this assumption appear in Bell’s original derivation? The answer turned out to be more subtle than I expected. But finding, eventually, that the answer is (probably) “yes” put me on the path toward eventually identifying a similar (though not identical, and the difference is subtle and interesting) sort of HV/CFD assumption that appears also in the derivation of the CHSH inequality.

So I am left, in the end, (basically) agreeing with my initial sense, though also embarrassed at how superficial my prior basis for this conclusion had been. I am also left with a much deeper appreciation for why there has been such long and lingering disagreement between advocates of (1) and (2) from the first paragraph above. For the sense in which HV/CFD appears in the CHSH derivation is subtle and not obvious, and it is indeed still not crystal clear to me that this assumption is even really there! So the final conclusion in my mind is this: it’s good that the modified EPR argument (from locality to HV/CFD) exists to unequivocally settle the dispute in favor of (1).

But the real point of the current essay is the interesting journey, not so much the conclusion. So let us jump in with that.

II. BELL

Let’s get notation out of the way first. Throughout, we will consider the standard EPR-Bell setup, in which a central source emits oppositely-directed particles (electrons, say) in the spin-entangled singlet (total spin zero) state. (That, at least, is how orthodox QM describes the relevant state; other candidate theories might give a
more or less or differently detailed account of the particle pair’s state.) Note in particular that the state is not a spin eigenstate for either of the individual particles; i.e., orthodox QM (OQM) attributes no definite spin-component values to either particle individually prior to measurement.

After the particles fly apart to some large distance (so that subsequent spin-component measurements, which take some finite time, can nevertheless be made with spacelike separation), two experimenters (Alice and Bob) randomly choose spatial axes (\(\hat{a}\) and \(\hat{b}\) respectively) along which to measure the spins of their particles. The outcomes of these measurements are bivalent, and we’ll use units in which the possible outcomes are \(A = \pm 1\), \(B = \pm 1\).

Bell’s derivation of the inequality gets started as follows: consider a theory (not necessarily OQM) according to which a complete description of the state of the particle pair (on some hypersurface just prior to the measurements, say) is denoted \(\lambda\). Note that \(\lambda\) could be merely the QM wave function, or it could include more – or indeed less – structure attributed to the particles. The mere notation (\(\lambda\)) commits us, really, to nothing. I stress this point because there is an unfortunate tendency in the Bell literature to call \(\lambda\) “the hidden variable” in contexts in which there is no reason or need to commit to this (i.e., to commit to the claim that \(\lambda\) attributes additional structure to the pair’s state, compared with OQM’s wave function). This tendency has no doubt contributed to the confusion about whether, and how, some HV/CFD type assumption is present in the derivation of the inequalities. We will go beyond orthodoxy soon enough, but let’s not fool ourselves into thinking that the mere idea of a complete state description (or an arbitrary choice of symbol for it) commits us to anything anti-orthodox.

Following Bell, let us now assume that the outcomes are determined \(^{12}\) – i.e., that there exist functions

\[
A(\hat{a}, \hat{b}, \lambda) = \pm 1 \tag{1}
\]

and

\[
B(\hat{a}, \hat{b}, \lambda) = \pm 1 \tag{2}
\]

which give the outcomes of Alice’s and Bob’s experiments under the relevant conditions. Let us then require locality (i.e., each outcome is determined without influence from the distant apparatus orientation), so the relevant functions have the form

\[
A(\hat{a}, \lambda) = \pm 1 \tag{3}
\]

and

\[
B(\hat{b}, \lambda) = \pm 1. \tag{4}
\]

Now, clearly, we are no longer talking about OQM, and for (already) two distinct reasons. The first is determinism: Bell assumes that for a given \(\lambda\) and a given \(\hat{a}\) and \(\hat{b}\), unique outcomes are determined. But OQM is not a deterministic theory. Were we to insist on maintaining allegiance to orthodox quantum philosophy, we should talk only of probabilities for the various possible outcomes, e.g., \(P(A|\hat{a}, \hat{b}, \lambda)\). (As we will discuss later, the major advance of the CHSH inequality relative to the original Bell inequality is that it does away with this assumption of determinism; but more on that later.)

Bell also goes beyond orthodoxy in requiring locality. Orthodox QM simply does not respect Bell’s locality condition (elaborated in \(^{1}\)). OQM is not a local theory.

To summarize, Bell has us consider a deterministic local hidden variable (LHV) theory. “Deterministic” and “local” are obvious; the theory being considered is a “hidden variable” theory because the complete state description (\(\lambda\)) cannot merely be the quantum mechanical wave function (which simply does not attribute enough structure to the particles to uniquely and locally determine the outcomes of spin measurements!).

Continuing with the functions \(A(\hat{a}, \lambda)\) and \(B(\hat{b}, \lambda)\), Bell has us consider the expected value of the product of the outcomes of Alice’s and Bob’s measurements:

\[
E(\hat{a}, \hat{b}) = \int d\lambda \, \rho(\lambda) \, A(\hat{a}, \lambda) B(\hat{b}, \lambda) \tag{5}
\]

where \(\rho(\lambda)\) is the probability density for the “singlet state” pair preparation procedure to produce the state \(\lambda\).

Bell now imposes the perfect correlation requirement (a special case of the predictions of OQM): whenever Alice and Bob measure along the same axis, they will get opposite outcomes. Thus

\[
A(\hat{b}, \lambda) = -B(\hat{b}, \lambda) \tag{6}
\]

which allows us to rewrite the correlation function \(^{13}\) as

\[
E(\hat{a}, \hat{b}) = -\int d\lambda \, \rho(\lambda) \, A(\hat{a}, \lambda) A(\hat{b}, \lambda). \tag{7}
\]

This expression contains the first apparent seeds of an additional (third) anti-orthodox assumption in the derivation. Taken straight, the meaning of this expression is something like:

- the expected value for the product of
  
  (i) the outcome of Alice’s measurement along \(\hat{a}\), and

  (ii) the outcome of Alice’s measurement along \(\hat{b}\).

The important point here is not that the expression implies the existence of a particular value for the outcome of either measurement. That aspect is justified by the already-noted assumption of determinism. What’s new and crucial here is the (apparent) requirement that the values \(A(\hat{a}, \lambda)\) and \(A(\hat{b}, \lambda)\) are simultaneously meaningful, even though at most one of the measurements (along \(\hat{a}\) or \(\hat{b}\)) can actually be performed. The whole idea of
a meaningful correlation (or more precisely its expectation value) between the outcomes of two incompatible measurements, surely goes beyond the verificationist/positivist emphasis of the orthodox framework, in particular Bohr’s insistence that “the measuring instruments... serve to define the conditions under which the phenomena appear.”[2, pg 2] I find it helpful to think of this orthodox principle as asserting the “contextuality” of measurement outcomes: because (according to the orthodox view) the outcomes are not pre-encoded in the measured object, but only arise in the interaction of the object with the measuring apparatus, it is meaningless to refer to the outcomes of merely hypothetical measurements. “Unperformed measurements... serve to define the conditions under which the phenomena appear.”[2, pg 2] Or: “No elementary phenomena is a phenomenon until it is a registered (observed) phenomenon.”[3]

So in addition to the determinism and locality assumptions already noted, we have here a third assumption that might be called “non-contextuality” or “counter-factual meaningfulness” (CFM – the idea being that it is meaningful to talk about the outcomes of not-actually-performed measurements).

It is rather difficult, however, to separate this third assumption from the first-noted assumption (determinism). The contrast here would be a theory that is anti-orthodox in (say) the first two ways (it is deterministic, and it is local) but which is nevertheless orthodox in the third sense: according to this hypothetical theory, the outcomes of measurements are brought into being (deterministically and locally) by an interaction with the measurement apparatus, so while it is perfectly meaningful to speak of $A(\hat{a}, \lambda)$ (in a situation where a measurement along $\hat{a}$ is actually occurring) or of $A(\hat{b}, \lambda)$ (in a situation where a measurement along $\hat{b}$ is actually occurring), it would be impossible to speak meaningfully of the product $A(\hat{a}, \lambda)A(\hat{b}, \lambda)$ since the measurement apparatuses needed to make (respectively) the first and second factors meaningful, are incompatible. So there can be no situation in which the product is meaningful.

Actually, the last few paragraphs have been deliberately (but justifiably) misleading. This third anti-orthodox assumption (CFM or non-contextuality) is actually not implied by the mere writing of Equation [11] – a point that is clear if we just remember where Equation [11] came from. Recall that the apparently problematic expression came from the combination of the unproblematic definition of the correlation function – Equation [8] – with the perfect correlation condition – Equation [6]. Remembering this allows us to give meaning to the correlation as expressed in Equation [11] without making any CFM assumption, simply by reading the factors as

(i) $A(\hat{a}, \lambda) = \text{the value the theory predicts for } A \text{ when Alice measures along } \hat{a} (\text{with the pair in state } \lambda)$, and

(ii) $A(\hat{b}, \lambda) = \text{the opposite of the value the theory predicts for } B \text{ when Bob measures along } \hat{b} (\text{with the pair in state } \lambda)$.

And since the two relevant measurements here (Alice’s along $\hat{a}$ and Bob’s along $\hat{b}$) are perfectly compatible, we need not assume the meaningfulness of any measurement outcomes which are not (and indeed cannot be) actually performed.

By parsing it this way, we see that this third sort of anti-orthodoxy is not actually present in Equation [7]. But our reason for going into this is that this anti-orthodox sort of CFM is assumed in the next step in Bell’s derivation, and in a way that cannot be eliminated by any re-parsing of the problematic mathematical expressions. Let us see how this comes about by following through with Bell’s derivation. Consider the difference in correlation functions, expressed as in Equation [9], for two different pairs of angles:

$E(\hat{a}, \hat{b}) - E(\hat{a}, \hat{c}) = \int d\lambda \rho(\lambda) [A(\hat{a}, \lambda)A(\hat{c}, \lambda) - A(\hat{a}, \lambda)A(\hat{b}, \lambda)].$ (9)

So far so good. But now we insert unity, into the first term in the square brackets, in the form

$1 = A(\hat{b}, \lambda)A(\hat{b}, \lambda)$ (10)

(justified by the idea that $A(\hat{b}, \lambda) = \pm 1$ so that, either way, its square is one). This leaves us with

$E(\hat{a}, \hat{b}) - E(\hat{a}, \hat{c}) = \int d\lambda \rho(\lambda) [A(\hat{a}, \lambda)A(\hat{b}, \lambda)A(\hat{b}, \lambda)A(\hat{c}, \lambda)$

$- A(\hat{a}, \lambda)A(\hat{b}, \lambda)] = \int d\lambda \rho(\lambda) A(\hat{a}, \lambda)A(\hat{b}, \lambda) [A(\hat{b}, \lambda)A(\hat{c}, \lambda) - 1]$ (11)

From here it is straightforward to get Bell’s inequality. Taking absolute values on both sides, using the fact that $|A| \leq 1$, and identifying

$- \int d\lambda \rho(\lambda) A(\hat{b}, \lambda)A(\hat{c}, \lambda) = E(\hat{b}, \hat{c})$ (13)

gives Bell’s inequality:

$\left| E(\hat{a}, \hat{b}) - E(\hat{a}, \hat{c}) \right| \leq 1 + E(\hat{b}, \hat{c}).$ (14)

The reader has probably already noticed the step to which we wish to call attention. One of the terms in Equation [11] reads

$\int d\lambda \rho(\lambda) A(\hat{a}, \lambda)A(\hat{b}, \lambda)A(\hat{b}, \lambda)A(\hat{c}, \lambda)$ (15)

which, in words, evidently means
the expected value for the product of

(i) the outcome of Alice’s measurement along \( \hat{a} \),

(ii) the outcome of Alice’s measurement along \( \hat{b} \) (squared), and

(iii) the outcome of Alice’s measurement along \( \hat{c} \).

Now the same kind of analysis we gave before will go through, but this time without the escape hatch that saved us before – namely, the re-parsing of one of the two apparently-incompatible factors in terms of another, actually-compatible measurement (namely Bob’s). For even supposing we parse one of the group \{ (i), (ii), and (iii) \} in terms of a measurement made by Bob, we are here left inevitably with two incompatible measurement contexts (e.g., \( \hat{a} \) and \( \hat{b} \)) for Alice. And so, from the point of view of any theory that respects this third orthodox principle, such an expression as Equation (15) is simply meaningless – and therefore anything that comes after its appearance (such as Bell’s inequality) is equally meaningless. And hence Bell’s inequality simply would not apply to the kind of theory we raised the possibility of before: one which is deterministic and local, but which accepts the orthodox principle of contextuality (i.e., which denies CFM).

Summing up, it appears that Bell’s derivation of the inequality is premised on three distinct assumptions:

- determinism
- locality
- CFM (or non-contextuality)

any of which might, in principle, be rejected in the face of empirical data contradicting the inequality. 13

One point should be addressed here before finally moving on to consider the CHSH inequality. Someone skeptical of the previous discussion could raise the following objection: surely Alice needn’t actually set up a measurement apparatus oriented along \( \hat{b} \) in order to justify the statement that \( A(\hat{b}, \lambda)A(\hat{b}, \lambda) = 1 \). Even a merely imagined measurement along \( \hat{b} \) (even one which conflicts with an actually performed measurement along, say, \( \hat{a} \)) must obey \( A(\hat{b}, \lambda) = \pm 1 \), and hence \( A(\hat{b}, \lambda)^2 = 1 \). Can’t we then parse the allegedly-objectionable factors in Equation (15) in terms of such an imaginary measurement, thus removing the need for any CFM-type assumption?

The problem with (i.e., the answer to) this objection is that “imagination” provides far too much leeway. For example, couldn’t we imagine that \( A(\hat{b}, \lambda) = +1 \) ... and then imagine that \( A(\hat{b}, \lambda) = -1 \) ... so the product is \(-1\) rather than the required \(+1\)? Or, for that matter, couldn’t we imagine \( A(\hat{b}, \lambda) = 0 \) or \( 17\pi/\sqrt{2} \) or any other value whose square is not \(+1\)? Of course, the objector will want to say that the imaginations must be constrained by the (local deterministic) theory, which must (by prior assumptions) attribute either the value \(+1\) or the value \(-1\) to Alice’s (real or imaginary) measurement along \( \hat{b} \). But this just brings out the fundamental problem with the objection. Such a theory need only attribute this value to Alice’s measurement for an experimental context in which a measurement along \( \hat{b} \) actually occurs. There are no constraints whatever on what value (if any) such a theory must attribute to a measurement along the \( \hat{b} \) direction in an experimental context in which Alice is actually measuring along the \( \hat{a} \) direction. Indeed, in principle, for a contextual theory, there can be no such attributed value, for the very idea being considered (the outcome of a measurement that is incompatible with another actually performed measurement) is simply meaningless.

And so the original claim – that Bell’s derivation of the inequality requires not only locality and determinism, but also a CFM-type assumption – stands.

III. CHSH

The main difference between the Bell inequality and the CHSH inequality is that the latter does not require determinism. Instead of beginning with an assumption that the outcomes \( A \) and \( B \) are fixed once the experimental context \( (\hat{a}, \hat{b}) \) and particle-pair state \( (\lambda) \) are specified, CHSH require only that a theory specify the probabilities for the various possible outcomes. Thus, instead of functions \( A(\hat{a}, \lambda) \) and \( B(\hat{b}, \lambda) \), we begin with two probability functions:

\[
P(A|\hat{a}, \lambda) \quad (16)
\]

and

\[
P(B|\hat{b}, \lambda). \quad (17)
\]

Note that we have already imposed the locality condition, whereby the probability (assigned by the theory in question to the various possible outcomes) depends only on facts which are locally accessible to the experiment in question. Thus, for example, the probability for different outcomes \( A \) depends only on \( \hat{a} \) and \( \lambda \) – not on the distant setting \( \hat{b} \) or the distant outcome \( B \). (Note that it is only because the state description \( \lambda \) is assumed to be a complete description, that the non-dependence of the probability of \( A \) on the distant outcome \( B \) follows from local causality.)

For such a (local, but not necessarily deterministic) theory, the expectation value of the product of the two outcomes will be given by:

\[
E(\hat{a}, \hat{b}) = \int d\lambda \rho(\lambda) \sum_{A,B} A B P(A|\hat{a}, \lambda) P(B|\hat{b}, \lambda). \quad (18)
\]

Since \( A = \pm 1 \) and \( B = \pm 1 \), this can be simplified to

\[
E(\hat{a}, \hat{b}) = \int d\lambda \rho(\lambda) \bar{A}(\hat{a}, \lambda) \bar{B}(\hat{b}, \lambda) \quad (19)
\]
Rearranging and factoring \( \mathcal{A}(\hat{a}, \lambda) \) inside the integrand then which, in words, is evidently

\[
\mathcal{A}(\hat{a}, \lambda) = \sum_A A \, P(A|\hat{a}, \lambda) = P(A = +1|\hat{a}, \lambda) - P(A = -1|\hat{a}, \lambda)
\]

is the theory’s prediction for the average value of Alice’s experiment (with the apparatus set along \( \hat{a} \) and with particles in the state \( \lambda \)). (And similarly for \( \hat{B} \).

Thus parsing Equation (19): the expected value of the product of Alice’s and Bob’s experiments is given by a weighted average (over all the pair states that could be produced by the preparation procedure) of the product of

(i) the average value for Alice’s measurement, and

(ii) the average value for Bob’s measurement

where the averages here are averages over the possible outcomes allowed by the theory when the experimental context \( \hat{a} \) and \( \hat{b} \) is realized.

Continuing with the derivation, let us consider, as before, the difference in the correlation function for two different pairs of angles:

\[
E(\hat{a}, \hat{b}) - E(\hat{a}, \hat{b}') = \int d\lambda \, \rho(\lambda) \left[ \mathcal{A}(\hat{a}, \lambda) \mathcal{B}(\hat{b}, \lambda) - \mathcal{A}(\hat{a}, \lambda) \mathcal{B}(\hat{b}', \lambda) \right]
\]

where \( \hat{b} \) and \( \hat{b}' \) refer to two distinct settings of Bob’s apparatus (as will, likewise, \( \hat{a} \) and \( \hat{a}' \) refer shortly to two distinct settings of Alice’s apparatus). So far so good. But now, to continue with the derivation, we need to add zero inside the integrand in the clever form

\[
0 = \pm \mathcal{A}(\hat{a}, \lambda) \mathcal{A}(\hat{a}', \lambda) \mathcal{B}(\hat{b}, \lambda) \mathcal{B}(\hat{b}', \lambda) + \mathcal{A}(\hat{a}, \lambda) \mathcal{A}(\hat{a}', \lambda) \mathcal{B}(\hat{b}, \lambda) \mathcal{B}(\hat{b}', \lambda).
\]

Rearranging and factoring \( \mathcal{B} \) inside the integrand then gives

\[
E(\hat{a}, \hat{b}) - E(\hat{a}, \hat{b}') = \int d\lambda \, \rho(\lambda) \left[ \mathcal{A}(\hat{a}, \lambda) \mathcal{B}(\hat{b}, \lambda) \left( 1 \pm \mathcal{A}(\hat{a}', \lambda) \mathcal{B}(\hat{b}', \lambda) \right) \right.
\]

\[
- \mathcal{A}(\hat{a}, \lambda) \mathcal{B}(\hat{b}', \lambda) \left( 1 \pm \mathcal{A}(\hat{a}', \lambda) \mathcal{B}(\hat{b}, \lambda) \right) \right] \]

which is easily reduced, by taking absolute values on both sides, to

\[
|E(\hat{a}, \hat{b}) - E(\hat{a}, \hat{b}')] + |E(\hat{a}', \hat{b}) + E(\hat{a}', \hat{b}')| \leq 2
\]

which is the CHSH inequality.

The reader has probably already noticed the step to which we wish to draw attention. The two terms (which add to zero) that appear for the first time in Equation (25) have the form

\[
\int d\lambda \, \rho(\lambda) \, \mathcal{A}(\hat{a}, \lambda) \, \mathcal{A}(\hat{a}', \lambda) \, \mathcal{B}(\hat{b}, \lambda) \, \mathcal{B}(\hat{b}', \lambda)
\]

which, in words, is evidently

- the expected value for the product of
  - (i) the average value for the outcome of Alice’s measurement along \( \hat{a} \),
  - (ii) the average value for the outcome of Alice’s measurement along \( \hat{a}' \),
  - (iii) the average value for the outcome of Bob’s measurement along \( \hat{b} \), and
  - (iv) the average value for the outcome of Bob’s measurement along \( \hat{b}' \).

which, like the similar terms discussed in the previous section, would only seem to be meaningful for a theory of non-contextual hidden variables. That is, for a theory according to which the experimental outcomes are brought into existence by some kind of interaction between the measuring apparatus and the measured object, it would be meaningless to talk simultaneously about the outcomes of incompatible measurements (such as Alice’s measurements along both \( \hat{a} \) and \( \hat{a}' \)).

Of course, one might object, we are not here forced, by the algebra, to talk about specific outcomes for these pairs of incompatible measurements. Instead, we only need simultaneous talk about the averages of those pairs of incompatible measurements. This objection is correct as far as it goes: the assumption here is indeed weaker than the full “counter-factual definiteness” which is used in the derivation of the original Bell inequality. But there is still here, in the CHSH derivation, an assumption of the weaker condition we have called CFM: counter-factual meaningfulness. This parallels closely the discussion in the previous section, so we need not elaborate in great detail. Suffice it to point out that locality alone (which is the only explicit assumption noted so far in the derivation of the CHSH inequality) does not (in any obvious way) warrant a claim that the average value for a measurement by Alice along \( \hat{a}' \) should be the same under two scenarios: first, the measurement along \( \hat{a}' \) is actually performed, and second, the measurement along \( \hat{a}' \) is performed while the apparatus is oriented along \( \hat{a} \). Indeed, it is difficult to understand how a theory could yield up any determinate value for the average under the second set of conditions since those conditions are in principle unrealizable – they involve a contradiction, because the apparatus cannot be simultaneously aligned along both \( \hat{a} \) and \( \hat{a}' \).

Of course, there is no such problem in a theory of non-contextual hidden variables – i.e., a theory in which the outcomes of experiments (or the probabilities for various possible outcomes) are pre-encoded in the state of the object alone, with the role of the experimental apparatus being simply to reveal those pre-existing values (or to reveal one of the possible values according to the pre-existing probability distribution that is, so to speak, encoded in the state of the “measured” object). In such a theory, we can understand the problematic expressions such as Equation (25) as referring, not to (averages of) outcomes of actually-performed experiments, but to the hidden variables themselves – to those features of the
state of the object which determine what the averages will be if a measurement is performed (but with no implication that such a measurement need actually be performed in order to give meaning to the expression).

We thus conclude that, in addition to the assumption of locality (which nobody denies is present), the derivation of the CHSH inequality requires also a second assumption: not the “counter-factual definiteness” (CFD) that is needed to derive the Bell inequality, that is true, but a weaker condition of counter-factual meaningfulness (CFM, which can be roughly thought of as “CFD minus determinism”). CFM is the assumption that underwrites simultaneous talk about (the averages of) outcomes of incompatible experiments, and is practically equivalent to the assumption of non-contextuality (which is violated by both orthodox QM and contextual hidden variable theories such as Bohmian Mechanics, both of which are, however, non-local theories). Thus, the only familiar examples of theories satisfying the two conditions needed to derive the CHSH inequality would be local, non-contextual hidden variable theories. We therefore conclude that it is misleading (or flat wrong) to assert that the CHSH inequality is an example of a Bell inequality “without hidden variables.” It’s true that one need not explicitly assume hidden-variables in order to arrive at the inequality; but the CFM assumption one does need is, for all practical purposes, equivalent.

**IV. DISCUSSION**

We have identified three assumptions

1. determinism

2. locality

3. counter-factual meaningfulness (CFM)

which function as premises in the derivation of the Bell inequality (which requires all three premises) and the CHSH inequality (which requires only the second and third).

The status of the CHSH inequality relative to the Bell inequality has been obscured, in previous literature, by the packaging of our premises 1 and 3 into “counter-factual definiteness” (CFD) – the idea that there is some uniquely determined outcome to not-actually-performed measurements. This packaging has apparently led some people to the erroneous conclusion that, since the full CFD property is not assumed in the derivation of the CHSH inequality, it is based on no assumption other than locality. But this is false. One does still need the (weaker) CFM principle (and locality) to arrive at CHSH. And so, in principle a violation of the CHSH inequality could be blamed either on a failure of locality or a failure of counter-factual meaningfulness (i.e., a failure of non-contextuality).

In another nice paper that argues for a Bell-type inequality making “no mention...of ‘hidden variables’ or similar superstitions”, Asher Peres characterizes the logical status of the inequality as follows: “Let us assume that the outcome of an experiment performed on one of the systems is independent of the choice of the experiment performed on the other. Now, let us try to imagine the results of alternative measurements, which could have been performed on the same systems instead of the actual measurements. Then there is no way of contriving these hypothetical results so that they will satisfy all the quantum correlations with the results of the actual measurements.” Peres also discusses how the derivation “involves a comparison of the results of experiments which were actually performed, with those of hypothetical experiments which could have been performed but were not” and points out that “it is impossible to imagine the latter results in a way compatible with (a) the results of the actually performed experiments, (b) long range separability of results of individual measurements, and (c) [the empirical predictions of] quantum mechanics.”

What then should we infer from the fact that the inequalities are empirically violated? Peres tells us that “[i]t there are two possible attitudes in the face of these results. One is to say that it is illegitimate to speculate about unperformed experiments. In brief ‘Thou shalt not think.’... Alternatively, for those who cannot refrain from thinking, we can abandon the assumption that the results of measurements by A[a]lice are independent of what is being done by B[ob].” These statements nicely summarize the conclusions of the current essay. Unlike many other authors, Peres correctly characterizes the additional assumption (beyond Locality) needed to derive the CHSH inequality as weaker than “counter-factual definiteness” – it is, rather, the assumption that it is meaningful to speculate at all about unperformed experiments. It’s not just that one shouldn’t think of them as having definite outcomes; one cannot even think of them as having a well-defined average outcome. In short, following Peres, one “shalt not think” about the results of un-performed (and/or un-performable) experiments at all.

A theory which maintains allegiance to this orthodox principle (“Thou shalt not think” about un-performed experiments) could make predictions consistent with the inequalities (and hence, now, with experiment) even if it were local. Or so it might seem. But before spending any time trying to concoct such a theory (which, if found, would be an explicit counterexample refuting the claims of those – from “class (1)” mentioned in the introduction – who think that the empirical violations of Bell’s inequalities proves that nature is non-local) one would do well to remember the existence of the modified EPR argument (as detailed in ). For this (unfortunately neglected, if not completely forgotten) argument proves that the various other principles needed to arrive at a Bell-type inequality can all be derived from the assumption of locality – that locality requires a local, non-contextual hidden-variables theory (to explain the fact
of perfect anti-correlation when Alice and Bob measure along the same axis). So when we bring this modified EPR argument back in, we see that, after all, there is no choice but to blame the empirical violations of Bell-type inequalities on the failure of the locality assumption. We cannot, after all, blame the Bell-violating data on a failure of CFM or any other principle going beyond orthodoxy (other than locality).

So our real conclusion is this: it is nice that we don’t need to leave aside the modified EPR argument (as we have done throughout the current paper). Without that other argument (the first half of Bell’s own two-part argument for non-locality) we might accidently fool ourselves into thinking that the empirical data can be dealt with by rejecting something other than locality. But it can’t – a realization which leaves us in a better position to appreciate Bell’s statement that “For me then this is the real problem with quantum theory: the apparently essential conflict between any sharp formulation and fundamental relativity. That is to say, we have an apparent incompatibility, at the deepest level, between the two fundamental pillars of contemporary theory...” [pg 172]

V. CONFESSION

Aside from a few parenthetical qualifications snuck into the abstract and introduction, I have tried to present the arguments here as forcefully and univocally as possible. But I feel I should confess to being, myself, not at all convinced that these arguments are correct. The main thesis I’ve argued for is that certain mathematical expressions, which necessarily appear in the middle-stages of the derivation of generalized Bell inequalities, are simply meaningless from the point of view of contextual theories (i.e., theories rejecting CFM), since these expressions contain a kind of implicit reference to incompatible pairs of measurements.

But is there really a new and separate CFM assumption here? For example, in the original Bell derivation, once we allow the existence of functions such as $A(\hat{a}, \lambda)$, haven’t we already tacitly allowed that these functions are well-defined? That is, can’t we simply regard $A(\hat{a}, \lambda)$ and $A(\hat{a}', \lambda)$ as “just the same functions ... with different argument” (as Bell puts it in answering a related objection in his paper “Locality in quantum mechanics: reply to critics” [2])? That is, don’t the kind of hidden variables that are already on the table, based on the determinism and locality assumptions, support simultaneous talk of $A(\hat{a}, \lambda)$ and $A(\hat{a}', \lambda)$ – as simply the values that the theory yields for outcomes along two possible (but not necessarily actual, and by no means “actually simultaneous”) measurements?

And then can’t one argue in parallel for the average values that are used in the context of the CHSH derivation, thus concluding that, indeed, the CHSH inequality follows (in one step) from the locality assumption alone (just as the previously-mentioned anonymous referee claimed)?

The arguments presented in the earlier sections of this paper have the following basic structure: certain mathematical expressions appearing in the intermediate stages of the algebraic derivation of Bell-type inequalities, can be “translated back” into prose descriptions of certain correlation functions – i.e., expectation values for products of certain sets of measurement outcomes. See, for example, Equation (15) and the subsequent prose translation. This “back translation” is supposed to have been justified by the fact that it is merely doing, in reverse, what we did originally to write down a mathematical expression – Equation (15) – for “the expected value of the product of Alice’s measurement along $\hat{a}$ and Bob’s measurement along $\hat{b}$”. And then, according to our earlier argument, since the “back translated” prose statement is operationally meaningless (because it refers to the product of outcomes of incompatible measurements), so is the corresponding mathematical expression.

But who says we need to – or, indeed, are entitled to – make this “back translation”? By the time we get to the relevant stage in the algebraic derivation, there is no question about the meaningfulness of the individual factors in (for example) Equation (15). Each is simply the outcome that the theory in question predicts for the measurement in question. And if each of those is individually meaningful, how in the world can there be any problem in multiplying them together (and then averaging the result over the possible states $\lambda$ that might be, according to the theory, produced by the preparation procedure)? Sure, if we translate the math back into prose in a certain way, we get something that is operationally meaningless. But we could just as easily – and, I would argue, more faithfully – translate the mathematical expression into prose this way: Equation (15) represents

- the average (over possible $\lambda$s) of the product of
  
  \begin{enumerate}
  
  \item the value that the theory predicts for the outcome of a measurement by Alice along $\hat{a}$
  
  \item the value that the theory predicts for the outcome of a measurement by Alice along $\hat{b}$ (squared), and
  
  \item the value that the theory predicts for the outcome of a measurement by Alice along $\hat{c}$.
  
  \end{enumerate}

And this way there is no reference to the actual outcomes of actually-performed measurements that are incompatible, no tacit assumption of CFM. We are simply talking about what some theory says will happen in various circumstances, and mathematically manipulating values that are perfectly well defined. There seems to then be no problem – no mysterious third anti-orthodox assumption – whatsoever.

I think the fundamental point here is one that has already been emphasized, from a slightly different point of view, in [1]. Confusion arises when one forgets (in the context of analyzing Bell’s theorem) that one is not
talking directly about measurement outcomes, but about theories and their predictions. (This confusion is probably prevalent because of the influence of philosophical positivism on the quantum founding fathers and their followers.) In the context of the present paper, it seems that as long as one keeps this in mind – as long as one resists the temptation to require a direct operationalist interpretation for every intermediate stage in the algebraic derivation – it emerges that there is no distinct “CFM” assumption in the derivation of the Bell or CHSH inequalities. And hence it emerges that the CHSH inequality in particular follows from locality alone, such that its empirical violation can only be blamed on the non-local character of nature.

Nevertheless (a meta-confession) I am not 100% certain that my own objection to my own arguments is correct. So, at least for the time being, I am relieved that the approach taken in [1] exists – that is, the approach of using a modified EPR argument as an “end run” around the question of CFM. The associated two-part argument for nonlocality still seems (to me) to be the most straightforward, least subtle, and most airtight proof that locality (and, in this context, nothing else) has been empirically refuted.

Acknowledgements:

Thanks to the anonymous referee of the earlier paper for stimulating my thinking on this question (and, I hope, for forgiving my earlier dismissiveness).

[1] T. Norsen, Bell Locality and the Nonlocal Character of Nature, quant-ph/0601205
[2] J. S. Bell, Speakable and Unspeakable in Quantum Mechanics (Second Edition), Cambridge University Press, 2004
[3] A. Einstein, B. Podolsky, and N. Rosen, “Can Quantum Mechanical Description of Reality be Considered Complete?”, Phys. Rev. 47, 777 (1935)
[4] J.F. Clauser, M. A. Horne, A. Shimony, and R.A. Holt, “Proposed experiment to test local hidden-variable theories,” Phys. Rev. Lett., 23, 880 (1969)
[5] For a particularly relevant discussion of the CHSH inequality, see: B. Skyrms, “Counterfactual definiteness and Local Causation”, Philosophy of Science, 49 43-50 (1982)
[6] N.D. Mermin, “Is the moon there when nobody looks? Reality and the quantum theory”, Physics Today, April 1985, pages 38-47
[7] A. Peres, “Unperformed experiments have no results”, Am. J. Phys. 46(7), 745 (1978)
[8] J.A. Wheeler, “Law Without Law” in Wheeler and W.H. Zurek (eds.), Quantum Theory and Measurement, Princeton University Press, 1983
[9] Note that, in addition to the apparent CFM assumption to be discussed, there is an additional assumption that the order of the four multiplied factors in the questionable terms can be freely rearranged – i.e., there is an assumption that the (averages of) outcomes for measurements along different directions commute. This is perhaps dubious in a theory similar to orthodox QM, where the relevant quantities would be calculated as expectation values of a certain sequence of non-commuting operators which represent the individual observables in question. For some discussion of this point, see, for example: M. Clover, “Bell’s Theorem: A Critique”, quant-ph/0502016; M. Clover, “Bell’s Theorem: A New Derivation that Preserves Heisenberg and Local-