Appendix C: Point-by-Point Refutal of Gill's Latest Critique of my Disproof of Bell's Theorem

In the version 8 of his critique of my disproof, [**C**1], Richard D. Gill has changed the title of his earlier preprint and repeated his old arguments in somewhat different language, apart from adding some new *argumentum ad hominem* against me and against the editorial boards of the journals *Royal Society Open Science* and *IEEE Access* in which two of my three papers are recently published [**C**2][**C**3]. In addition, Gill has published a sequel to [**C**1] on the arXiv, [**C**4], in response to my latest paper [**C**5]. But, unfortunately, both of his critiques, [**C**1] and [**C**4], contain numerous mathematical and conceptual mistakes, similar to those in his earlier critiques. The trouble with [**C**1] begins already with its new title, which reads: "Does geometric algebra provide a loophole to Bell's theorem?" But my disproof of Bell's theorem has nothing whatsoever to do with any loopholes. Thus, his new title reveals a lack of understanding of what my argument against Bell's theorem is all about. The contents of his critique reveal even more serious mistakes, and they multiply in his latest preprint [**C**4]. In what follows, I refute Gill's arguments in [**C**4] point-by-point, by first reproducing his own words and then giving my responses, in order to expose the elementary nature of his mistakes.

My Response to What Gill Calls "The Heart of the Matter":

Gill writes:

This paper is a sequel to my paper [**C**1] which analyses a whole sequence of papers by Joy Christian, including the publications [**C**6], [**C**2], [**C**3]. Christian's "oeuvre" was recently extended with a pedagogical paper [**C**5], and it is useful to similarly extend my own work.

My response:

There is no "extension" of Gill's "work" in his latest critique. As we will soon see, it contains only repetitions of his previous mistakes and adds some new mistakes. But it does give an opportunity to expose his mistakes more clearly.

Gill writes:

Equations (24) and (25) of [**C**5] talk of two sets of bivectors $\mathbf{L}(\mathbf{a},\lambda)$ with the scalar $\lambda=+1$ and $\lambda=-1$ but these equations do not fix the relation between the two sets of bivectors. However that becomes fixed by equations (26) and (27). We learn that $\mathbf{L}(\mathbf{a},\lambda)=\lambda I\mathbf{a}$ where I is the trivector or pseudo-scalar $\mathbf{e}_1\wedge\mathbf{e}_2\wedge\mathbf{e}_3$, the context is Geometric Algebra, the main reference being the standard work Doran and Lasenby (2003) [**C**7]. The symbols $\mathbf{a}$ and $\mathbf{b}$ stand for ordinary real 3D vectors. I commutes with everything and $I^2=-1$. It follows that

$$\mathbf{L}(\mathbf{a},\lambda)\mathbf{L}(\mathbf{b},\lambda)\;=\;\lambda^2 I^2\mathbf{ab}\;=\;-\,\mathbf{ab}$$

which does not depend on λ at all.

My response:

Equations (24) and (25) of [**C**5] do not talk of two sets of bivectors. They talk of only *one* set of spin bivectors $\mathbf{L}(\mathbf{n},\lambda)$, with the choice of handedness $\lambda=+1$ or $\lambda=-1$ of the 3-sphere, *with respect to the detector bivectors* $\mathbf{D}(\mathbf{n})$, where all vectors $\mathbf{n}$ are unit vectors. While the equation Gill has written is mathematically correct, its RHS is not independent of this *relative* handedness λ. It would be a meaningless equation if its LHS "depended" on λ but its RHS did not.

Gill writes:

In fact, from geometric algebra we know that

$$-\,\mathbf{ab}\;=\;-\,\mathbf{a}\cdot\mathbf{b}-I(\mathbf{a}\times\mathbf{b})\;=\;-\,\mathbf{a}\cdot\mathbf{b}-\mathbf{L}(\mathbf{a}\times\mathbf{b},+1).$$

So equation (25) is correct when $\lambda=+1$ but seems to be wrong when $\lambda=-1$.

My response:

What Gill has missed here, and has missed it for the past eight years, is the fact that the handedness λ of the spin bivector $\mathbf{L}(\mathbf{n},\lambda)$ is meaningful only with respect to the handedness of the detector bivector $\mathbf{D}(\mathbf{n})$, and vice versa. In fact, what we know from Geometric Algebra is not what Gill claims, but the following:

$$-\,\mathbf{a}\mathbf{b} \;=\; -\,\mathbf{a}\cdot\mathbf{b}-\mathbf{a}\wedge\mathbf{b} \;=\; -\,\mathbf{a}\cdot\mathbf{b}-\mathbf{L}(\mathbf{a}\times\mathbf{b},\,\lambda), \tag{C1}$$

where $\wedge$ represents the anti-symmetric outer product in GA. So equation (25) is correct for both $\lambda=+1$ and $\lambda=-1$. Once again, the handedness λ of $\mathbf{L}(\mathbf{n},\lambda)$ is meaningful *only* with respect to the handedness of $\mathbf{D}(\mathbf{n})$, and vice versa. Gill's mistake here is to assume $\mathbf{a}\wedge\mathbf{b}=I(\mathbf{a}\times\mathbf{b})$ instead of $\mathbf{a}\wedge\mathbf{b}=J(\mathbf{a}\times\mathbf{b})$, with $J=\lambda I$ being the volume form on the 3-sphere of handedness λ. For further discussion, see Eq. (B12) of Appendix B above and Eq. (98) of Ref. [**C**3].

Gill writes:

Ah, but we now see where the handedness interpretation of λ could come in. Perhaps the author has both a right-handed and a left-handed cross product. Introduce two cross-products, $\times(\lambda)$ where $\lambda=\pm1$, by the rules

$$\mathbf{a}\times(+1)\,\mathbf{b} \;=\; \mathbf{a}\times\mathbf{b}, \qquad \mathbf{a}\times(-1)\,\mathbf{b} \;=\; \mathbf{b}\times\mathbf{a}.$$

My response:

The mistake made here by Gill is quite elementary. In vector algebra, the cross product between the vectors $\mathbf{a}$ and $\mathbf{b}$ is the same in both right-handed and left-handed coordinate frames. In the right-handed frame the three vectors, $\mathbf{a}$, $\mathbf{b}$, and $\mathbf{a}\times\mathbf{b}$, respect the right-hand rule and in the left-handed frame they respect the left-hand rule. But the rule for calculating the cross product remains the same. Therefore, the sign change in $\mathbf{a}\times\mathbf{b}$ introduced by Gill is illegitimate.

Since Gill has exhibited much difficulty for the past eight years with this elementary concept from vector algebra, let me elaborate on it here further. Given the basis vectors $\mathbf{e}_1$, $\mathbf{e}_2$, and $\mathbf{e}_3$, in vector algebra the components and direction of the cross product vector $\mathbf{c}=\mathbf{a}\times\mathbf{b}$ are calculated using the rules of calculating a determinant as follows:

$$\mathbf{c} = \begin{vmatrix} \mathbf{e}_1 & \mathbf{e}_2 & \mathbf{e}_3 \\ a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 \end{vmatrix} = \mathbf{e}_1(a_2b_3-a_3b_2)+\mathbf{e}_2(a_3b_1-a_1b_3)+\mathbf{e}_3(a_1b_2-a_2b_1), \tag{C2}$$

where $\mathbf{a}=a_1\mathbf{e}_1+a_2\mathbf{e}_2+a_3\mathbf{e}_3$ and $\mathbf{b}=b_1\mathbf{e}_1+b_2\mathbf{e}_2+b_3\mathbf{e}_3$. Note that handedness does not enter this prescription at all. It is designed in such a way that the three vectors, $\mathbf{a}$, $\mathbf{b}$, and $\mathbf{a}\times\mathbf{b}$, in the given order, have the same handedness as the coordinate frame in which they are evaluated, regardless of the handedness of that frame [**C**8]. In the left-handed coordinate frame the three vectors satisfy the left-hand rule, but the above prescription gives the same answer for $\mathbf{c}$.

This is not some quirk of vector algebra. There are very good reasons why the definition of a cross product has been devised in this manner. Vectors are "geometric objects" [**C**9]. They are so called because they exist independently of coordinate systems or reference frames. In particular, our vectors $\mathbf{a}$ and $\mathbf{b}$ and their cross product vector $\mathbf{c}=\mathbf{a}\times\mathbf{b}$ are all geometric objects. They all exist independently of coordinate systems or reference frames. Consequently, they remain independent of the handedness of a coordinate system or reference frame chosen to express them in practice.

Gill writes:

Now equation (25), corrected, makes sense and is consistent with what follows:

$$\mathbf{L}(\mathbf{a},\lambda)\mathbf{L}(\mathbf{b},\lambda) \;=\; -\,\mathbf{a}\cdot\mathbf{b}-\mathbf{L}(\mathbf{a}\times(\lambda)\,\mathbf{b},\lambda).$$

My response:

Gill has not "corrected" my equation (25) as he claims, but replaced it with his own incorrect equation, thereby introducing an illegitimate sign change in the definition of the cross product, just as he has done for the past eight years. The above equation harbors the same mistake he had made eight years ago in the second unnumbered equation of his critique [**C**1]. As I have explained above [cf. Eq. (36)], Gill has inserted an additional λ in my equation, *by hand*, in the middle of that equation. But, as we just saw, it does not belong there. I have explained this in the paragraph that includes Eq. (36) in my rebuttal in [**C**10]. But this mistake in [**C**1] remains uncorrected even after eight years.

To appreciate how nonsensical his version of my equation is, let us evaluate the bivector $\mathbf{L}(\mathbf{a}\times(\lambda)\,\mathbf{b},\lambda)$ appearing on the RHS of his equation above:

$$\mathbf{L}(\mathbf{a}\times(\lambda)\,\mathbf{b},\lambda) = \lambda I(\mathbf{a}\times(\lambda)\,\mathbf{b}) = \lambda^2 I(\mathbf{a}\times\mathbf{b}) = I(\mathbf{a}\times\mathbf{b}) = \mathbf{D}(\mathbf{a}\times\mathbf{b}). \tag{C3}$$

Thus, Gill's version of my equation is actually this:

$$\mathbf{L}(\mathbf{a},\lambda)\mathbf{L}(\mathbf{b},\lambda) \;=\; -\,\mathbf{a}\cdot\mathbf{b} - \mathbf{D}(\mathbf{a}\times\mathbf{b}). \tag{C4}$$

So, in Gill's version of my equation, for $\lambda = -1$ the bivectors $\mathbf{L}(\mathbf{a},\lambda)$ and $\mathbf{L}(\mathbf{b},\lambda)$ appearing on its LHS are left-handed whereas the bivector $\mathbf{D}(\mathbf{a}\times\mathbf{b})$ appearing on its RHS is right-handed. Thus, Gill's equation does not define the bivector subalgebra consistently in *any* coordinate frame. Far from restoring consistency, Gill has introduced *inconsistency.*

To see that Eq. (C4) above is the same as the second equation in the earlier versions of Gill's critique [**C**1], we use Eq. (32) of my paper [**C**5] (which Gill agrees with) to recognize that $\mathbf{D}(\mathbf{a}\times\mathbf{b}) = \lambda\,\mathbf{L}(\mathbf{a}\times\mathbf{b},\,\lambda)$, and rewrite (C4) as

$$\mathbf{L}(\mathbf{a},\lambda)\mathbf{L}(\mathbf{b},\lambda) \;=\; -\,\mathbf{a}\cdot\mathbf{b} - \lambda\,\mathbf{L}(\mathbf{a}\times\mathbf{b},\lambda). \tag{C5}$$

Note that this equation is not the same as my Eq. (25). There is an extra λ on the RHS that does not belong there.

<u>Gill writes</u>:

Having restored consistency to the definitions we can now quickly check formulas (30) and (31), taking account of (32), which give us

$$\mathbf{L}(\mathbf{a},\lambda) = \lambda I\mathbf{a}, \quad \mathbf{D}(\mathbf{a}) = \lambda\lambda I\mathbf{a} = I\mathbf{a}.$$

<u>My response</u>:

As noted, Gill has not "restored consistency" but introduced *inconsistency.* While the above two equations are correct, they are just my equation (32). They are not related to anything Gill has claimed in his previous comments.

<u>Gill writes</u>:

From the central expressions in (30) and (31) (the ones with limits as $\mathbf{s}_1$ converges to $\mathbf{a}$ and $\mathbf{s}_2$ converges to $\mathbf{b}$) we find that

$$\mathcal{A}(\mathbf{a},\lambda) = -\mathbf{D}(\mathbf{a})\mathbf{L}(\mathbf{a},\lambda) = -\lambda I^2\mathbf{a}^2 = \lambda,$$

$$\mathcal{B}(\mathbf{b},\lambda) = +\mathbf{D}(\mathbf{b})\mathbf{L}(\mathbf{b},\lambda) = \lambda I^2\mathbf{b}^2 = -\lambda,$$

exactly as the right hand sides of (30) and (31) proclaim.

<u>My response</u>:

These equations are correct only for $\mathbf{s}_1 \neq \mathbf{s}_2$. They ignore the conservation of zero spin angular momentum, which amounts to setting $\mathbf{s}_1 = \mathbf{s}_2$. In other word, the above equations hold in general only if the conservation of zero spin angular momentum is violated, or, equivalently, the Möbius-like twists in the Hopf bundle of S^3 are ignored. That is to say, the equations hold if the entire argument of my paper is missed and one stoops back to the flat geometry of $\mathrm{I\!R}^3$.

<u>Gill writes</u>:

This is consistent with (45)–(50). The final evaluation of (50) in (55)–(62) must be incorrect, the correlation must come out as -1.

My response:

It is easy to verify that the final evaluation of (50) in (55)–(62) is correct, and the correlation comes out as $-\mathbf{a}\cdot\mathbf{b}$.

Gill writes:

The author does not take account of the fact that surely, his cross-product too should also consistently follow the left- or right-handedness of the coordinate frame chosen by λ.

My response:

I have exposed Gill's mistake in this claim in my response above. It stems from his failure to understand how cross products are calculated in vector algebra. They do not change sign between left- and right-handed coordinate frames.

Gill writes:

The interested reader may search for the mistake themselves, it is hidden in the derivation (34)–(40).

My response:

It appears that either Gill has not been able to carry out the derivation (34)–(40) himself (even though it is quite straightforward), or has not been able to find any mistake in it. In fact, no invisible mistake exists in the derivation.

Gill writes:

In the computer code in Section IV, the error in the evaluation of the correlation is "fixed" by the line

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if(lambda==1) {q=A B;} else {q=B A;}
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My response:

There is no error in the evaluation of the correlation to be "fixed." The computer code correctly reflects what has been evaluated analytically. The errors are made by Gill himself for the past eight years, despite my patient teachings.

Gill writes:

At the request of Joy Christian, I mention that he states that he has refuted all criticism of his works in the papers [**C**10], [**C**11], [**C**12].

My response:

I have also addressed various criticisms, and answered many questions in detail, in the Appendix B of Ref. [**C**3].

This completes my response to [**C**4]. But let me also reproduce here the summary of my responses to Gill's earlier criticisms. In the unpublished versions of [**C**1] with a different title, Gill had attempted to criticize an earlier version of my local-realistic model presented in [**C**13]. His critique, however, contains some very surprising mathematical and conceptual mistakes. For example, the abstract of the first version of his preprint refers to the quantity $-\mathbf{a}\cdot\mathbf{b}-\mathbf{a}\times\mathbf{b}$ as a "bivector." And even after my detailed explanations of the difference between a cross product and a wedge product, and the difference between a bivector and a multivector within geometric algebra, all subsequent versions of his preprint continue the mistake of referring to the multivector $-\mathbf{a}\cdot\mathbf{b}-\mathbf{a}\wedge\mathbf{b}$ as a bivector, leading to more serious mistakes later on in his critique. I have systematically corrected these mistakes in my responses [**C**10] and [**C**11].

One of the surprising oversights in Gill's critique is the distinction between the detector bivectors $\mathbf{D}(\mathbf{a})$ and $\mathbf{D}(\mathbf{b})$ and the spin bivectors $-\mathbf{L}(\mathbf{s},\,\lambda)$ and $+\mathbf{L}(\mathbf{s},\,\lambda)$ considered in [**C**13], together with the reciprocal relation between them,

$$\mathbf{L}(\mathbf{n},\,\lambda)\,=\,\lambda\,\mathbf{D}(\mathbf{n})\;\Longleftrightarrow\;\mathbf{D}(\mathbf{n})\,=\,\lambda\,\mathbf{L}(\mathbf{n},\,\lambda)\,, \tag{C6}$$

with λ being the uncontrollable hidden variable in the sense of Bell [**C**14]. In other words, the correct representation of EPR-Bohm experiment and the corresponding spin detection processes defined in Eqs. (58) and (59) are entirely missing in Gill's portrayal of my model. Consequently, what is described in the preprint [**C**1] is *not* my model at all.

Moreover, Eq. (4) of Gill's critique makes another serious mistake regarding the physics underlying the EPR-Bohm experiments. It inserts the equation $\mathscr{A}(\mathbf{a},\,\lambda)\mathscr{B}(\mathbf{b},\,\lambda) = (-\lambda)(+\lambda) = -1$ for all $\mathbf{a}$ and $\mathbf{b}$ even when $\mathbf{b} \neq \mathbf{a}$ by identifying $\mathscr{A}(\mathbf{a},\,\lambda)$ with $-\lambda$ and $\mathscr{B}(\mathbf{b},\,\lambda)$ with $+\lambda$, despite the fact that no such equation exists in my model. The insertion of this equation not only violates the conservation of spin angular momentum captured in Eqs. (69) and (70) of [**C**13], but also confuses the measurement results $\mathscr{A} = \pm 1$ and $\mathscr{B} = \pm 1$, which occur at remote stations, with the initial state $\lambda = \pm 1$, which originate at the central source in the overlap of the backward light cones of Alice and Bob. It is evident from Eqs. (69) and (70) that $\mathscr{A}\mathscr{B} = -1$ for $\mathbf{b} \neq \mathbf{a}$ can occur if and only if the said conservation law is violated.

In summary, Gill's critique in all versions of [**C**1] is a straw-man argument that ignores the fact that my approach to strong correlations is based on a *relative* orientation of a quaternionic 3-sphere, taken as Bell's local hidden variable. So much so, that Gill actually replaces one of my central equations with one of his own (thereby introducing a sign error), criticizes his own mistaken equation, and then declares that he has refuted my model. Indeed, in Eq. (2) of his critique an additional λ is inserted *by hand*, in the middle of that equation. As discussed above, I have explained this in the paragraph that includes Eq. (36) in my response [**C**10]. But this mistake in [**C**1] still remains uncorrected.

What is more, in the latest version of [**C**1] (version 8), Gill has added further elementary mistakes. For example, in the beginning of an argument he writes: "Take any unit bivector v. It satisfies $v^2 = 1$ hence $v^2 - 1 = (v-1)(v+1) = 0$." But any unit bivector squares to -1, not $+1$. Consequently, this mistake reduces Gill's entire argument to absurdity.

In another paper [**C**15] Gill criticizes my proposed experiment to test the relevance of Bell's theorem in a macroscopic setting [**C**6]. Unfortunately, this critique too contains surprisingly elementary mathematical and conceptual mistakes. For example, in the very equation of mine that this critique claims to be criticizing (namely, the standard definition of the bivector subalgebra [**C**7]), Gill forgets to sum over the bivector-index, arriving at a rather strange conclusion. What is more, the Bell-CHSH correlator is also calculated incorrectly in [**C**15], by summing over spin detections of physically incompatible experiments. What is astonishing is that nowhere in my paper [**C**6] such a correlator involving incompatible experiments is even considered. In his critique, the correlator is simply made up, attributed to me, and then criticized. I have explained these and further errors in Gill's critiques in my response [**C**11] and analysis [**C**16].

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