Refutation of Scott Aaronson's Critique of my Disproof of Bell's Theorem

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In May 2012 Scott Aaronson launched an unprovoked shaming campaign against me for a fortnight on his personal blog, without reading a single line of my argument against Bell's theorem [1]. The campaign involved mockery, defamation, incitement, name-calling, cyber-bullying, cyber-mobbing, and various other forms of intimidation tactics and *ad hominem* attacks, rationalized by reiteration of some incorrect criticisms of my argument previously advanced by others. In this note I repudiate his scientifically incorrect criticisms of my local-realistic model for the singlet correlations, as it is presented in [2]. Previously I have refuted such incorrect criticisms in these five preprints: [3–7]. I begin with a brief summary of my previous responses that address related issues raised by others.

A. Two Critiques by Gill

In an unpublished preprint [8] Gill has attempted to criticize an earlier version of the local-realistic model I have presented in [2]. His critique, however, contains quite a few elementary 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 the critique. I have systematically corrected these mistakes in my responses [5] and [7].

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 [2], together with the reciprocal relation between them,

$$\mathbf{L}(\mathbf{n},\,\lambda)\,=\,\lambda\,\mathbf{D}(\mathbf{n})\;\Longleftrightarrow\;\mathbf{D}(\mathbf{n})\,=\,\lambda\,\mathbf{L}(\mathbf{n},\,\lambda)\,, \tag{1}$$

with λ being the uncontrollable hidden variable in the sense of Bell [9]. 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 [8] 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 [2], but also confuses the measurement results $\mathscr{A}=\pm1$ and $\mathscr{B}=\pm1$, which occur at remote stations, with the initial state $\lambda=\pm1$, 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 [2].

In summary, the critique in [8] is a straw-man argument that ignores the fact that my Clifford-algebraic 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, but it does not belong there. I have explained this in the paragraph that includes Eq. (36) in my response [5]. But this mistake in [8] remains uncorrected.

In a second paper [10] Gill criticizes my proposed experiment to test the relevance of Bell's theorem in a macroscopic setting [11]. 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 [12]), 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 [10], by summing over spin detections of physically incompatible experiments. I have explained these and further errors in my response [7] and analysis [13].

B. Three Critiques by Moldoveanu

The two unpublished critiques [14; 15] of my model are quite similar to the one by Gill discussed above [8]. They too are riddled with elementary mathematical and conceptual mistakes [4]. The argument in these preprints is also a straw-man argument that ignores the physical process underlying the EPR-Bohm experiments as well as the fact

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that my model is based on a *relative* orientation of the quaternionic 3-sphere, taken as a local hidden variable. As I explain below, Moldoveanu too replaces one of my central equations with one of his own (thereby introducing a sign error *by hand* in the same manner as Gill), criticizes his own mistaken equation, and then declares that he has refuted my model. I have previously addressed a large number of his claims in my response [4]. Unfortunately, despite my detailed refutations in [4], these same incorrect criticisms have recently appeared in his private blogpost [16]. These online comments are also at best a series of misunderstandings of the calculations presented in [2]. It is, however, instructive to go through these comments one-by-one to understand the mistakes in the blogpost [16], as follows.

Mistake #1: It is claimed in the blog [16] that my paper has nothing to do with Friedmann-Robertson-Walker spacetime. But this claim is mistaken for several reasons, as explained in the following.

To begin with, in the context of Bell's theorem the question of local causality is properly addressed only within an adequately relativistic picture of spacetime. See, for example, the lucid discussion by John S. Bell himself in his last paper on the subject [17]. In this paper Bell defines local causality within a given spacetime as follows:

> A theory will be said to be locally causal if the probabilities attached to values of local beables in a space-time region 1 are unaltered by specification of values of local beables in a space-like separated region 2, when what happens in the backward light cone of 1 is already sufficiently specified, for example by a full specification of all local beables in a space-time region 3 (figure 6.4).

Moreover, as is well known, a violation of the relativistic local causality can be separated into two conceptually distinct parts: (1) a signalling non-locality incompatible with special relativity, and (2) a no-signalling non-locality compatible with special relativity [18]. These two conceptually distinct parts are kinematically captured by Bell in his definitions $\mathscr{A}(\mathbf{a},\,\lambda)$ and $\mathscr{B}(\mathbf{b},\,\lambda)$ of local measurement functions for any given initial state λ of the system [9][2]. This separates relativistic local causality into independence of the parameter $\mathbf{a}$ from $\mathbf{b}$ (and vice versa) preserving signalling locality, and independence of the outcome $\mathscr{A}$ from $\mathscr{B}$ (and vice versa) preserving no-signalling locality, in any EPR-Bohm type experiment. This separation allows one to recognize that quantum mechanics preserves *parameter independence* (thus remaining compatible with special relativity) but violates *outcome independence* (cf. Ref. [18]). Thus, despite appearances, relativistic causality is implicit and essential in any discussion involving Bell-type measurement functions.

Now, by definition, the singlet correlations in any EPR-Bohm type experiment are computed among measurement events that occur *simultaneously*, at equal times. In practice, this amounts to averaging over "coincidence counts" of simultaneously occurring spin detections at spacelike distances, confined to a spacelike hypersurface within spacetime:

$$\mathcal{E}(\mathbf{a},\,\mathbf{b}) \,=\, \lim_{n\,\gg\,1}\left[\frac{1}{n}\sum_{k\,=\,1}^{n}\,\mathscr{A}(\mathbf{a},\,\lambda^k)\;\mathscr{B}(\mathbf{b},\,\lambda^k)\right] \equiv \frac{\Big[C_{++}(\mathbf{a},\,\mathbf{b})\,+\,C_{--}(\mathbf{a},\,\mathbf{b})\,-\,C_{+-}(\mathbf{a},\,\mathbf{b})\,-\,C_{-+}(\mathbf{a},\,\mathbf{b})\Big]}{\Big[C_{++}(\mathbf{a},\,\mathbf{b})\,+\,C_{--}(\mathbf{a},\,\mathbf{b})\,+\,C_{+-}(\mathbf{a},\,\mathbf{b})\,+\,C_{-+}(\mathbf{a},\,\mathbf{b})\Big]}, \tag{2}$$

where $C_{+-}(\mathbf{a},\,\mathbf{b})$ etc. represent the number of simultaneous occurrences of detections $+1$ along $\mathbf{a}$ and -1 along $\mathbf{b}$, etc., with all vectors being spacelike. This may give a false impression that spacetime is irrelevant for the question of local causality in this context and a three-dimensional hypersurface would be sufficient for the analysis of correlations. We must not forget, however, that the measurement outcomes $\mathscr{A}(\mathbf{a},\,\lambda)$ and $\mathscr{B}(\mathbf{b},\,\lambda)$ depend, not only on the spacelike vectors $\mathbf{a}$ and $\mathbf{b}$, but also on the initial state λ of the singlet system *which originates in the overlap of the backward light-cones of Alice and Bob*, as in Fig. 1 of [2]. And it is this initial state λ originating in the overlap that brings about the measurement outcomes $\mathscr{A}(\mathbf{a},\,\lambda)$ and $\mathscr{B}(\mathbf{b},\,\lambda)$ for any freely chosen spacelike vectors $\mathbf{a}$ and $\mathbf{b}$. Thus relativistic considerations are by no means irrelevant for the understanding of local causality in the context of Bell's theorem.

But what is missing from the relativistic considerations by Bell in [17] are the algebraic, geometrical and topological properties of the physical space within which we are confined to perform all our experiments. And that is where the spacelike hypersurface, S^3, of a Friedmann-Robertson-Walker spacetime enters our analysis in Ref. [2]. As explained in that paper, the geometry of the quaternionic 3-sphere is essential for the derivation of strong correlations, and that geometry is provided by the spacelike hypersurface of one of the three cosmological solutions of Einstein's field equations, as captured in Eq. (9) of [2]. Thus, Friedmann-Robertson-Walker spacetimes are just the right spacetimes for addressing the question of local causality. They are sufficiently Newtonian to host the strong correlations predicted by the singlet state, and sufficiently relativistic to address the question of no-signalling non-locality that appears to be occurring at a spacelike distance in the EPR-Bohm experiments, with the vital condition for local causality being that the initial state λ must originate in the overlap of the backward light cones of Alice and Bob. Suppose, however, we ignore Eq. (9) of [2] and start the analysis from Eq. (10) instead. But removing the Friedmann-Robertson-Walker spacetime from the analysis in this manner would make the entire analysis *ad hoc*, with no physical justification for S^3. Thus, the claim that my paper has nothing to do with Friedmann-Robertson-Walker spacetime is quite wrong.

Mistake #2: It is claimed in the blog [16] that in Eq. (49) of Ref. [2], namely, in the standard bivector subalgebra

$$\mathbf{L}(\mathbf{a},\,\lambda)\,\mathbf{L}(\mathbf{b},\,\lambda) \,=\, -\,\mathbf{a}\cdot\mathbf{b}\,-\,\mathbf{L}(\mathbf{a}\times\mathbf{b},\,\lambda)\,, \tag{3}$$

two different algebras are combined into the same equation. The claim is that the bivectors appearing in the above equation are not of the same kind, but a mixture of bivectors corresponding to two different algebraic representations.

But this claim is manifestly incorrect. Regardless of λ, all three bivectors $\mathbf{L}(\mathbf{a},\,\lambda)$, $\mathbf{L}(\mathbf{b},\,\lambda)$, and $\mathbf{L}(\mathbf{a}\times\mathbf{b},\,\lambda)$ in the above equation belong to the *same* algebraic representation of the standard bivector subalgebra (48). Thus, contrary to the claim, Eq. (49) does not describe two different multiplication rules but the same multiplication rule of the standard bivector subalgebra. The mistaken claim stems from a failure to understand what λ stands for within S^3. It represents an orientation of the spin bivectors $\mathbf{L}(\mathbf{n},\,\lambda)$ *relative* to the detector bivectors $\mathbf{D}(\mathbf{n})$, as defined in Eq. (60). The meaning of λ and the relationship between $\mathbf{L}(\mathbf{n},\,\lambda)$ and $\mathbf{D}(\mathbf{n})$ are clearly brought out between Eqs. (81) and (92). They show that the left-handed subalgebra can be easily transformed into a right-handed subalgebra by reversing the order of the bivectors in their product, as verified also in the numerical simulations with a GAViewer program [19–21].

Mistake #3: It is claimed in the blog [16] that matrix representation of the bivector subalgebra using Pauli matrices is equivalent to the usual bivector representation of the subalgebra under consideration. While there is some element of truth in this claim, the matrix representation of bivector subalgebra fails at the very first step, because a product of two Pauli matrices can at most be an identity matrix, not a scalar number. On the other hand, what are observed in the experiments, as results of the interactions between the spins $\mathbf{L}(\mathbf{n},\,\lambda)$ and the detectors $\mathbf{D}(\mathbf{n})$, are pure scalar numbers: $\mathscr{A}(\mathbf{a},\,\lambda)=\pm\,1$ and $\mathscr{B}(\mathbf{b},\,\lambda)=\pm\,1$. Thus a matrix representation is of no use in the present context.

Mistake #4: It is claimed in the blog [16] that in Eqs. (71) to (79) of this paper I am summing over two different representations of the bivector subalgebra in a single sum. But it is quite evident from these equations that what is being averaged over are the measurement results $\mathscr{A}(\mathbf{a},\,\lambda)=\pm\,1$ and $\mathscr{B}(\mathbf{b},\,\lambda)=\pm\,1$, which are limiting scalar points of a quaternionic 3-sphere as defined in the Eqs. (58) and (59). Consequently, from Eqs. (71) and (76) we have the following geometrical and statistical identity:

$$\lim_{n\,\gg\,1}\left[\frac{1}{n}\sum_{k\,=\,1}^{n}\,\mathscr{A}(\mathbf{a},\,\lambda^k)\;\mathscr{B}(\mathbf{b},\,\lambda^k)\right]=\lim_{n\,\gg\,1}\left[\frac{1}{n}\sum_{k\,=\,1}^{n}\,\mathbf{L}(\mathbf{a},\,\lambda^k)\,\mathbf{L}(\mathbf{b},\,\lambda^k)\right]. \tag{4}$$

Evidently, all bivectors $\mathbf{L}(\mathbf{a},\,\lambda)$ and $\mathbf{L}(\mathbf{b},\,\lambda)$ in this identity belong to the *same* algebraic representation of the bivector subalgebra. The mistaken claim results from the previous two mistakes in [16]. In fact, the steps from (71) to (76) are quite straightforward and have been carefully explained just below Eq. (79). The steps from (76) to (79) are also straightforward. They follow at once upon using the relation (60). While there is no room for a mistake in these latter three steps, they can be avoided by following Eqs. (91) to (93) instead, which provide an independent confirmation of the derivation from (71) to (79). Not surprisingly, both calculations give one and the same result (79). What is more, two programmers have independently confirmed the validity of the derivation from (71) to (79) in two event-by-event numerical simulations of the singlet correlations using a GAViewer program based on Geometric Algebra [19–21].

This raises a question: Where does the claim of a result different from (79) stem from? It stems from an attempt to insert an additional λ into the model, *by hand*, without a meaningful justification for it. To be sure, the critique tries to justify the additional λ, but without considering either what λ stands for in the model or the relation (60) between the spin bivectors $\mathbf{L}(\mathbf{n},\,\lambda)$ and the detector bivectors $\mathbf{D}(\mathbf{n})$ it represents. The actual λ in the model is not a convention but a hidden variable that originates from a central source. The additional λ, on the other hand, is neither originated at the source nor detected by the detectors. It is inserted *by hand*. Moreover, contrary to the rationale for inserting an additional λ, there is no such thing as "orientation independent" or "orientation-free" objects in Geometric Algebra as claimed in [16]. An orientation of a vector space such as $Cl_{3,0}$ is a *relative* concept (cf. the textbook definition of orientation in section 5 of Ref. [11]). If $\mathbf{B}_1$ and $\mathbf{B}_2$ are two bivectors related by the orientation λ as in $\mathbf{B}_1=\lambda\mathbf{B}_2$, then $\mathbf{B}_2=\lambda\mathbf{B}_1$ by arithmetic necessity, because $\lambda^2=1$. Thus the attempt of inserting an additional λ into my model based on a narrative of "orientation independent" versus "orientation dependent" objects is seriously mistaken. For neither the spin bivectors $\mathbf{L}(\mathbf{n},\,\lambda)$ nor the detector bivectors $\mathbf{D}(\mathbf{n})$ actually depend on λ. They are only *related* by it. Thus the inclusion of additional λ is a pure fiction that has nothing to do with the actual S^3 model presented in [2].

Mistake #5: It is claimed in the blog [16] that "correlations must be computed using actual experimental results" and "must not be made in a hypothetical space of 'beables'."

Yes, correlations must be computed using actual experimental results of $+1$ and -1, *but only to the extent that quantum mechanics is able to predict such actual measurement results.* After all, any local-realistic theory is obliged to reproduce only that which quantum mechanics is able to predict statistically and experimentalists are able to observe experimentally [22]. So, with that important correction to the claim, the correlations are indeed computed in the paper using actual experimental results of $+1$ and -1. Such actual experimental results are explicitly specified by the limiting scalar points $\mathscr{A}(\mathbf{a}\,,\,\lambda)=\pm1$ and $\mathscr{B}(\mathbf{b}\,,\,\lambda)\pm1$ of a quaternionic 3-sphere, which models the physical space in which we are confined to perform all our experiments. They correspond exactly to the measurement results considered by Bell in his paper (cf. Eq. (1) of Ref. (9) and Eqs. (58) and (59) of Ref. [2]). These $+1$ or -1 results are then averaged over in Eq. (71), which is *the* standard way of computing the correlations in the experimental context of Bell's theorem. No "hypothetical space of 'beable'" is involved in this paper, or anywhere else in my work.

Mistake #6: It is claimed in the blog [16] that "James Weatherall found a mathematically valid example very similar with [my] proposal but one which does not use quaternions/Clifford algebras." But this claim is not correct, as explained in the following paragraph.

C. A Critique by Weatherall

Despite its claim, Weatherall's critique [23] is not a critique of my local-realistic model at all but merely an exposition of the standard Bell's theorem [9; 17]. The critique begins by giving the wrong impression that the author is about to present and criticize my quaternionic 3-sphere model for the strong correlations. But, in fact, it does no such thing. The critique immediately switches to a different, non-Clifford-algebraic model based on an ordinary 2-sphere[1] instead of a quaternionic 3-sphere, and shows that his unphysical model does not reproduce the strong correlations. But it is quite well known for more than fifty years that any naïve attempt which ignores the correct algebra, geometry, and topology of the compactified physical space (S^3) cannot reproduce the strong correlations between the measurement results such as $\mathscr{A} = \pm 1$ and $\mathscr{B} = \pm 1$. What is more, in an unnumbered equation Weatherall makes the same mistake regarding the conservation law underlying any EPR-Bohm experiment that Gill has made in his critique discussed above [8]. He inserts the product $\mathscr{A}(\mathbf{a},\,\lambda)\mathscr{B}(\mathbf{b},\,\lambda) = (-\lambda)(+\lambda) = -1$ for all $\mathbf{a}$ and $\mathbf{b}$ even when $\mathbf{b} \neq \mathbf{a}$, despite the fact that no such equation appears in my model. This equation not only violates the conservation of spin angular momentum captured in Eqs. (69) and (70) of [2], but also confuses the measurement results $\mathscr{A} = \pm 1$ and $\mathscr{B} = \pm 1$, which are observed at remote detectors, with the initial state $\lambda = \pm 1$, which originates at the central source. Moreover, it is evident from Eqs. (69) and (70) of [2] that $\mathscr{A}\mathscr{B} = -1$ for $\mathbf{b} \neq \mathbf{a}$ can occur if and only if the conservation of spin angular momentum is violated. Notwithstanding these facts, Weatherall argues that, since his non-Clifford-algebraic model based on a non-combable[1] 2-sphere fails, my Clifford-algebraic model of the correlations must also fail, without even mentioning a 3-sphere or a quaternion, and without pointing to any mistake in my explicit and constructive model. In my response in [6] I have explained the above shortcomings of his critique in greater detail. More recently, I have also pointed out an oversight in a Bell-type unphysical argument on which Weatherall's critique depends [13].

D. A Shaming Campaign by Aaronson

In May 2012 Scott Aaronson launched an unprovoked shaming campaign against me for a fortnight on his personal blog, without reading a single line of my argument against Bell's theorem [1; 25]. His campaign involved mockery, defamation, incitement, name-calling, cyber-bullying, cyber-mobbing, and various other forms of intimidation tactics and *ad hominem* attacks, rationalized by reiteration of some incorrect criticisms of my argument previously advanced by others. The purpose of his shaming campaign was not just public humiliation and discrediting of my research, but, in his own words, also to starve me off by cutting off my financial and academic supports [1], thereby thwarting my ability to continue my work [1; 26]. When he eventually did read a one-page summary of my disproof of Bell's theorem [25], he made the same elementary mistakes in its evaluation that Gill, Moldoveanu, and Weatherall had made previously.

Just as the above trio, Aaronson also did not seem to have realized that my argument is based on the geometry of 3-sphere, taken as the physical space, even though that is rather obvious from the one-page summary of my argument he claimed to have read [25]. Moreover, quite independently of my S^3 model, his first mistake is to think that Bell's "theorem" is a theorem in the mathematical sense. But, in truth, it is a *physical* argument, based on the mathematical inequalities discovered by George Boole some 100 years before Bell. And, as a physical argument, Bell's theorem is a deeply flawed argument, as I have explained elsewhere [13]. Ironically, Bell's mistake in his theorem is the same as that made by von Neumann in his own no-go theorem against hidden variables. Bell has been credited for discovering von Neumann's mistake even though both Einstein and Grete Hermann had discovered it some 30 years before Bell, and even though Bell's contemporaries such as Siegel, Jauch, Piron, Kochen, and Specker had also discovered it independently of Bell. But Bell does have a more succinct and lucid explanation of von Neumann's mistake in section 3 of the first chapter of his book. At the end of his section 3 Bell writes something that is quite ironic in my view:

> Thus the formal proof of von Neumann does not justify his informal conclusion. ... It was not the objective measurable predictions of quantum mechanics which ruled out hidden variables. It was the arbitrary assumption of a particular (and impossible) relation between the results of incompatible measurements either of which might be made on a given occasion but only one of which can in fact be made.

As noted, that is precisely the mistake Bell himself has made in his own famous theorem [13]. Aaronson, on the other hand, not only naïvely mistakes Bell's "theorem" as a theorem in the mathematical sense, but also fails to recognize that – just as von Neumann's no-go theorem – Bell's theorem also harbors a breathtakingly obvious physical blunder.

When it comes to my S^3 model for the singlet correlations, what Aaronson does is misrepresent what I have actually presented, criticize his own misrepresentation, and then declares that he has refuted my model. In other words, his argument against my model is also a straw-man argument. More specifically, borrowing his mistake from Gill and Weatherall, Aaronson too 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 identification exists in my model. As we saw above, the insertion of this equation into my model *by hand* not only violates the conservation of spin angular momentum captured in Eqs. (69) and (70) of [2], but also confuses the measurement results $\mathscr{A} = \pm 1$ and $\mathscr{B} = \pm 1$,

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[1] It is well known in algebraic topology that, unlike on a 3-sphere, hair on a 2-sphere cannot be combed without creating a cowlick.

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. Moreover, from Eqs. (69) and (70) of [2] it is evident that $\mathscr{A}\mathscr{B} = -1$ for $\mathbf{b} \neq \mathbf{a}$ can occur if and only if the said conservation law is violated [2]. Thus, Aaronson's mistake is quite elementary.

E. Summary of the Failures of Criticisms

A common flaw in all of the critiques discussed above is the omission of considering the correct physical process in any EPR-Bohm type experiment. What is involved in such experiments are two prearranged detectors of Alice and Bob with the same orientation, stationed at remote locations, which I have represented as bivectors $\mathbf{D}(\mathbf{a})$ and $\mathbf{D}(\mathbf{b})$, and two randomly oriented spins generated from a central source, which I have represented as spin bivectors $-\mathbf{L}(\mathbf{s},\,\lambda)$ and $+\mathbf{L}(\mathbf{s},\,\lambda)$ (cf. Fig. 2 in Ref. [2]). What is observed are then simultaneous interactions between the detectors $\mathbf{D}(\mathbf{a})$ and $\mathbf{D}(\mathbf{b})$ and the spins $-\mathbf{L}(\mathbf{s},\,\lambda)$ and $+\mathbf{L}(\mathbf{s},\,\lambda)$, at spacelike distances. The Clifford-algebraic calculations are then relatively straightforward [2]. It is the failure to take into account these basic features of the EPR-Bohm type experiments within an appropriate Clifford-algebraic setting that has led the critics to their mistaken conclusions.

Now, as Moldoveanu claims on his blog, *Annals of Physics*, did remove my paper from its website after a month of its publication, within minutes of receiving a complaining email from Richard D. Gill. However, the journal did not notify me about the removal for over two months [27], and, despite my repeated requests, *has not provided any evidence of a mistake in the paper – even privately*. On the web page of the paper the journal states that "...the results [of my paper] are in obvious conflict with a proven scientific fact, *i.e.*, violation of local realism that has been demonstrated not only theoretically but experimentally in recent experiments. On this basis, the Editors decided to withdraw the paper."

This is quite an extraordinary statement, not the least because my paper went through seven months and two rounds of rigorous peer review, but no editor or reviewer were able to detect the alleged "obvious conflict." Moreover, since the framework for the EPR-Bohm correlations presented in the paper reproduces all quantum mechanical predictions and experimental results for the singlet state *exactly*, the journal's claim of "obvious conflict" is manifestly wrong.

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