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Title: Quantum Correlations are Weaved by the Spinors of the Euclidean Primitives

Article Type: Research

Author: Joy Christian

To: Prof. Derek Abbott, Associate Editor

Dear Prof. Abbott,

Replies to Reviews of MS # RSOS - 180090

Thank you for sending me the reviewer comments on my manuscript. Thank you also for giving me the opportunity to respond to the comments and resubmit a revised version of the manuscript.

Below please find my point-by-point responses to the comments by each of the five reviewers, together with brief notes about the changes in the manuscript I have made in the light of these comments.

Kind regards,

Joy Christian

Author's general comment for the editors and reviewers:

Before addressing the specific comments by the reviewers I would like to comment about the true significance of the framework presented in my manuscript, which seems to have been missed by some of the reviewers. The primary concern of my manuscript is not Bell's theorem as such but understanding the origins and strengths of all quantum correlations in terms of the algebraic, geometrical, and topological properties of the physical space in which we are confined to perform our experiments. To my mind, Bell's theorem is a distraction that prevents us from understanding the true origins of quantum correlations. The reviewers who have resisted my results have done so because of their prior commitment to Bell's theorem and their failure to abandon the flatland perspective within which most such discussions take place in favor of the 7-sphere perspective constructed in the manuscript. It is not surprising at all that from the flatland perspective many constructions presented in the manuscript may seem misplaced.

Author action: In response to the skepticism of some of the reviewers I have added a short appendix (Appendix A) explaining the wider significance of the geometry of 7-sphere and its relation to the issues discussed in the manuscript.

Author's specific replies to the reviewers' comments:

Reviewer # 1: The paper claims there is a flaw in the standard derivation of Bell's theorem, and that the author has found a local and realistic explanation for Bell inequality violation. These claims have been made many times by the author before, but are as false now as they always have been.

Author reply: It is difficult to respond to these comments because they do not engage with the actual contents of my manuscript, or with the actual contents of any of my previous works, before declaring them all to be false.

In the current manuscript I have presented a physically well-motivated theoretical framework as a constructive counterexample to Bell's theorem, with explicit local-realistic derivations of two specific special cases of quantum correlations predicted by quantum mechanics — namely, the rotationally invariant 2-particle quantum state which predicts the singlet or EPR-Bohm correlations and the rotationally non-invariant 4-particle quantum state which predicts the GHZ or GHSZ correlations. The novelty of these geometrical derivations by itself would be of sufficient scientific interest to the "quantum foundations" community, or to those working on the geometric algebra based framework for physics, independently of any Bell-type claim that such derivations are impossible.

Moreover, in the Subsection 4.2 of the manuscript I have presented a detailed criticism of Bell's original argument, exposing a subtle conceptual flaw in it, and then derived the Bell-CHSH inequality without assuming locality or compromising Einstein's notion of realism. Again, the novelty of these arguments by itself would be of sufficient interest to the community.

Unfortunately Reviewer # 1 has preferred not to engage at all with either the constructive counterexamples based on geometric algebra, or with the specific criticism of Bell's theorem presented in Subsection 4.2, in a serious manner. It is then difficult to know what is "false" about my criticism of Bell's theorem or the constructive counterexamples.

Author action: No action.

Reviewer # 1: Bell's theorem can be made precise in many ways. The most common way these days is to start from the condition of factorisation, that is, any single run of an experiment (of the type considered) is described by the same probability function satisfying

$$P(A,B|a,b,\lambda) = P(A|a,\lambda)\,P(B|b,\lambda) \tag{1}$$

together with the assumption that $P(\lambda)$ is specified by the preparation of the experiment, and so is unchanged when conditioned on free choices a and b. The fact that "Alice cannot align her detector along a and a' at the same time" (p.24) is irrelevant. Alice is free to chose a or a', and that is all that matters. From these assumption the inequalities follow, and from the violation of the inequalities by QM the theorem follows.

Author reply: I am afraid the last sentence of the above paragraph is simply false. Bell-CHSH inequalities do not follow from the condition of factorization alone. That is to say, the last sentence of the above paragraph does not follow from the assumptions set out in the earlier sentences. To derive Bell-CHSH inequalities with the absolute bound of 2 one must further assume a CHSH sum of four factorizable probabilities such as

$$P(A,B|a,b,\lambda) = P(A|a,\lambda)\,P(B|b,\lambda), \tag{2}$$

$$P(A,B|a,b',\lambda) = P(A|a,\lambda)\,P(B|b',\lambda), \tag{3}$$

$$P(A,B|a',b,\lambda) = P(A|a',\lambda)\,P(B|b,\lambda), \tag{4}$$

$$\text{and}\quad P(A,B|a',b',\lambda) = P(A|a',\lambda)\,P(B|b',\lambda), \tag{5}$$

where a and a', and b and b', are mutually exclusive measurement directions, freely selectable by Alice and Bob. Each of these four probabilities correspond to a physically realizable experiment. But, regardless of factorization, all four of the experiments, or any two of the four for that matter, cannot be realized simultaneously, because the free choices of the experimental directions such as a or a' and b or b' are not physically realizable simultaneously in any possible world, classical or quantum. On the other hand, it is impossible to derive the absolute bound of 2 without integrating over the following unphysical sum of all four factorized probabilities simultaneously:

$$P(A|a,\lambda)\,P(B|b,\lambda) + P(A|a,\lambda)\,P(B|b',\lambda) + P(A|a',\lambda)\,P(B|b,\lambda) - P(A|a',\lambda)\,P(B|b',\lambda). \tag{6}$$

It is therefore simply wrong to claim as the reviewer has done that "The fact that 'Alice cannot align her detector along a and a' at the same time' (p.24) is irrelevant. Alice is free to chose a or a', and that is all that matters."

And therein lies the conceptual flaw, brought out in the Subsection 4.2 of the manuscript in considerable detail, which any such probability-based argument cleverly hides, or unwittingly obfuscates. This flaw can be recognized at once if we refrain from framing such Bell-type arguments in terms of probabilities rather than in terms of measurement results such as $A = \pm 1$ and $B = \pm 1$ that are actually observed in the experiments. Probabilities allow one to hide, or obfuscate the unphysical element being smuggled-in in the derivation of the Bell-CHSH inequalities, which has been spelt out quite clearly in the Eq. (214) of the manuscript.

Author action: In the light of the reviewer's comments I have added the following new footnote in the manuscript (footnote # 6 on page 33): "In the derivation of the absolute bounds on the Bell-CHSH correlator such as those in Eq. (212) above one usually employs factorized probabilities of observing binary measurement results rather than the actual measurement results we have used in our derivation. But employing probabilities in that manner only manages to obfuscate the conceptual flaw in Bell's argument we intend to bring out here."

Reviewer # 1: These assumptions are what the community has agreed to call local causality or local realism. Thus there is no explanation for Bell inequality violation from a local realistic theory. The author's claim to the contrary must come from using the words "local realism" in a pointlessly contrarian way (which seems most likely given his flawed critique of Bell's theorem), or a mistake in his calculations, or both. I do not think it necessary to perform an autopsy.

Author reply: Once again Reviewer # 1 has preferred not to engage with my argument at all before reaching negative conclusions, stemming from prior beliefs. There is no justification for the claim that I must have used local realism "in a pointlessly contrarian way." In fact I have used nothing but Einstein's conceptions of locality and realism, as made mathematically precise by John Bell himself. To quote from my manuscript:

> "...the functions $\mathscr{A}(\mathbf{a}\,,\,\lambda^k)$ and $\mathscr{B}(\mathbf{b}\,,\,\lambda^k)$ define local, realistic, and deterministically determined measurement outcomes. Apart from the common cause λ^k originating in the overlap of the backward lightcones of $\mathscr{A}(\mathbf{a}\,,\,\lambda^k)$ and $\mathscr{B}(\mathbf{b}\,,\,\lambda^k)$, the event $\mathscr{A} = \pm 1$ depends only on a freely chosen measurement direction $\mathbf{a}$. And likewise, apart from the common cause λ^k, the event $\mathscr{B} = \pm 1$ depends only on a freely chosen measurement direction $\mathbf{b}$. In particular, the function $\mathscr{A}(\mathbf{a}\,,\,\lambda^k)$ does not depend on either $\mathbf{b}$ or $\mathscr{B}$, and the function $\mathscr{B}(\mathbf{b}\,,\,\lambda^k)$ does not depend on either $\mathbf{a}$ or $\mathscr{A}$."

This formulation of local realism is straight from Bell's own papers and book, written in Bell's own language. I very much doubt that the community has turned its back on Einstein's and Bell's formulations of the local-realism I have adapted in my manuscript.

In science, when we are faced with an incontrovertible evidence that goes against our prior beliefs, we are expected to change our prior beliefs. That is what distinguishes science from other human methods of inquiry. The comments by Reviewer # 1, on the other hand, amount to doing the opposite. They amount to rejecting the inconvenient evidence encountered simply to preserve the prior beliefs, and doing so without engaging with the evidence at all.

Author action: No action.

Reviewer # 2: This paper is quite interesting for showing violation of Bell's inequality from a geometric point of view. Especially, the author reproduced the Bell's theorem without assuming locality. The author derived the result by only assuming that two places can be observed at the same time. The original proof of the Bell's theorem only showed that requiring locality guarantees no violation of Bell's inequality, but it does not imply that all non-locality effects must have violation of Bell's inequality. Thus, the generalization of the proof does not have contradiction of any mathematical logic. The violation of the Bell's inequality should be independent of any particular time. Hence, the author interpreted that the violation of Bell's inequality means impossibility of measuring two places at same time so violation of the Bell's inequality is a nature result. From the above contribution, I suggest publication.

Author reply: I thank Reviewer # 2 for these kind words and for recommending my manuscript for publication.

Author action: No action.

Reviewer # 3: I was aware of the work of the author before reading this work and already knew that the idea is to use an elementary piece of Clifford algebra, viz the algebra Cl30 to question and even better, to disproof Bell's theorem, as in [9] of the present paper.

Author reply: I am flattered that Reviewer #3 was already aware of my earlier work based on geometric algebra.

In my view the true significance of Clifford algebra for physics in general and quantum correlations in particular is not in its simplicity but in how well it captures the algebraic, geometrical, and topological properties of the physical space in which we are confined to perform our experiments (especially those within the context of Bell's theorem).

Author action: No action.

Reviewer # 3: I already knew that this alleged disproof was questioned if not canceled by Richard D. Gill in 1203.1504 and Does geometric algebra provide a loophole to Bell's theorem available on Academia.edu. All criticisms that the scientific community can address to this approach are essentially contained in these essays.

Author reply: Reviewer # 3 cites two unpublished preprints by Richard D. Gill, both of which are riddled with elementary mathematical and conceptual mistakes. I have systematically exposed these mistakes in my replies to him in arXiv:1203.2529 and arXiv:1501.03393 which the reviewer does not mention. In essence Gill's criticism is based on a straw-man that has nothing to do with my Clifford-algebraic approach to quantum correlations. 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 [cf. Eq. (36) of my reply arXiv:1203.2529]. It is also worth noting that the second preprint of Richard D. Gill cited by the reviewer is not only unpublished and riddled with mistakes, but was also rejected by the arXiv moderators.

But more importantly, the unpublished preprints of Richard D. Gill cited by the reviewer are not about the current manuscript under consideration. There are a number of differences between my earlier work, which is about a quaternionic 3-sphere model of the singlet correlations, and the current work, which is about a comprehensive framework based on an octonion-like 7-sphere for understanding *all* quantum correlations in terms of the algebraic and geometrical properties of the 3-dimensional physical space. While both the earlier work and the current work are based on closely related Clifford-algebraic structures, the construction presented in the current work is based on an explicit multiplication table (Table 1) of the algebraic elements with a built-in orientation (or hidden) variable that makes it immune to the kind of sign mistake Gill was able to introduce in my earlier work and then claim it to be my mistake [see Eq. (36) of my reply arXiv:1203.2529]. No such misrepresentation of the current framework is possible.

It is also important to note that, contrary to the mistaken impression given by the title of one of the two unpublished preprints by Gill, neither my earlier work nor the current manuscript has anything to do with loopholes. Mine is an exact theoretical framework for understanding all quantum correlations, whereas the possibility of loopholes within the experimental context of Bell's theorem is essentially an experimental issue concerning mainly the singlet correlations.

Author action: No action.

Reviewer # 3: But Florin Moldoveanu in 1109.0535 and

https://physics.stackexchange.com/questions/7506/disproof-of-bell-s-theorem

are useful links to be convinced that Christian's work is not scientifically sound.

Author reply: It is unfortunate that Reviewer # 3 has relied on some non-peer-reviewed online comments concerning my earlier work rather than engaging with what is actually presented in the current manuscript under consideration. The unpublished commentaries contain numerous elementary mistakes that I have addressed in considerable detail in arXiv:1110.5876, which again the reviewer does not mention. What is more, neither the unpublished preprint nor the unpublished online comments cited by the reviewer have much to do with the explicit construction presented in the current manuscript under consideration, as I already explained above in the context of Gill's unpublished preprints.

Author action: No action.

Reviewer # 3: I observed the following extra problems: part of the paper relies on arguments that can be found in "On a surprising oversight by John S. Bell in the proof of his famous theorem 1704.02876" that suffer the criticisms given above.

Author reply: This claim by the reviewer is evidently false. My preprint of 2017 cited by the reviewer not only has literally nothing to do with my earlier constructive counterexample to Bell's theorem based on Clifford algebra, but that 2017 paper of mine did not even exist in 2011 and 2012 when my earlier work was being discussed online and in unpublished preprints mentioned above. This fact alone undermines the arguments put forward by Reviewer # 3 and demonstrates that the reviewer has not actually read any of the unpublished preprints they have cited to know that my 2017 preprint has nothing to do with what was being discussed online back in 2011 and 2012.

Author action: No action.

Reviewer # 3: Writing that this paper is a kind of verbal science fantasy is correct even if at first sight the level of technicalities involved here is rather fantastic. I was not courageous enough to enter most of the equations.

Author reply: I fail to see any scientific basis that can justify such a dismissive remark about the current manuscript under consideration, especially considering the fact that none of the preprints and online comments relied on by the reviewer has been published anywhere, let alone in a respectable, peer-reviewed journal of some standing.

But suppose, for the sake of argument, that the framework presented in my manuscript is a "verbal science fantasy." But don't all good ideas in physics begin as a verbal science fantasy? Wasn't relativity of simultaneity a verbal science fantasy until its consequences were worked out by a patent clerk in concrete mathematical terms and were later verified experimentally? Isn't superposition of states a verbal science fantasy until its consequences are worked out in concrete mathematical terms and verified experimentally? Isn't the hypothesis that gravity is not a force but a manifestation of the curvature of spacetime a verbal science fantasy until its consequences are worked out in concrete mathematical terms and verified experimentally? In the same spirit, the theorem presented in Subsection 3.2 of my manuscript can be viewed as a verbal science fantasy until its consequences are seen to have been worked out in the later sections in concrete mathematical terms in a manner compatible with the experiments. The quantum correlations in Eq. (186) predicted by the 4-particle GHZ state are quite non-trivial. The fact that they are reproduced within my framework, as explicitly demonstrated in Eq. (203), is surely a remarkable confirmatory evidence of the framework I have presented:

$$\mathcal{E}^{\rm GHZ}_{L.R.}(\mathbf{a},\,\mathbf{b},\,\mathbf{c},\,\mathbf{d}) \\ = \cos\theta_{\mathbf{a}}\,\cos\theta_{\mathbf{b}}\,\cos\theta_{\mathbf{c}}\,\cos\theta_{\mathbf{d}}\,-\,\sin\theta_{\mathbf{a}}\,\sin\theta_{\mathbf{b}}\,\sin\theta_{\mathbf{c}}\,\sin\theta_{\mathbf{d}}\,\cos\left(\,\phi_{\mathbf{a}}\,+\,\phi_{\mathbf{b}}\,-\,\phi_{\mathbf{c}}\,-\,\phi_{\mathbf{d}}\,\right). \tag{203}$$

Author action: Although the S^7 framework presented in the manuscript is primarily about a new understanding (or a new interpretation) of quantum correlations, an experimental test of the framework is possible in principle. I have added a new footnote — footnote # 4 on page 32 — concerning a proposed macroscopic experiment (published in the International Journal of Theoretical Physics), which, if realized, is capable of testing the framework in principle.

Reviewer # 3: What is the associative algebra of S7 the author is introducing, I am not aware of such a mathematical object? My knowledge restricts to Hopf fibrations and to published papers about the link between entanglements and S3 and S7 spheres such as https://arxiv.org/abs/quant-ph/0108137 by R. Mosseri and R. Dandoloff.

Author reply: There is nothing surprising about the possibility of an associative algebra associated with S^7.

As is well known, S^7 can be parallelized by non-associative octonions, just as S^3 can be parallelized by associative but non-commutating quaternions, with the corresponding algebras being two of the only four possible normed division algebras permitted by Hurwitz's theorem. But nothing prevents us from considering an un-parallelized S^7. In fact, that would usually be the case if one simply defines a 7-sphere by the coefficients of an 8-dimensional vector satisfying

$$Z_0^2 + Z_1^2 + Z_2^2 + Z_3^2 + Z_4^2 + Z_5^2 + Z_6^2 + Z_7^2 = 1. \tag{7}$$

If, however, we take the basis elements of the above vector to satisfy octonionic algebra, then the corresponding 7-sphere would be automatically parallelized. But, as noted, octonionic algebra is not necessarily the only algebra that can be associated with a 7-sphere. Moreover, since the seminal discovery by John Milnor in 1956 it is also known that the 7-sphere admits even exotic differential structures.

More specifically, the reviewer's question amounts to asking: Can there be a closed set of 8-dimensional unit vectors whose basis elements satisfy an associative algebra? But of course there can be. It is provided, for example, by the set of vectors defined in Eq. 39 of the manuscript. Note that the vector defined in Eq. 39 has *graded* basis elements forming an associative algebra corresponding to the multiplication table given in Table I of the manuscript. And since the vectors in Eq. 39 remain normalized to unity, their coefficients satisfy the above constraint required for any 7-sphere. Thus we have a 7-sphere constructed with an associative but non-commutative algebra. It is much like the octonionic algebra, but happens to be associative, because all Clifford algebras are associative algebras by definition.

In the manuscript there is a whole subsection (Subsection 2.4) devoted to demonstrating the associativity of the algebra associated with the 7-sphere constructed in the manuscript using the graded Clifford-algebraic basis, but the reviewer seems to have missed that subsection entirely.

Author action: In addition to the Subsection 2.4 mentioned above, I have added a remark about the associativity of the algebra used in the manuscript just below Eq. 62.

Reviewer # 3: In the abstract, the Lie group E8 is mentioned but this is never used except for immediately rejecting the non-associativity of the ongoing algebra. As a conclusion, I am not convinced that the scientific arguments of the author are different from previous seemingly incorrect work and the extra mathematics adds to the confusion. I recommend not to accept this paper.

Author reply: The first few lines of the abstract are the following:

> The exceptional Lie group E_8 plays a prominent role in both mathematics and theoretical physics. It is the largest symmetry group associated with the most general possible normed division algebra, namely, that of the non-associative real octonions, which — thanks to their non-associativity — form the only possible closed set of spinors (or rotors) that can parallelize the 7-sphere. By contrast, here we show how a similar 7-sphere also arises naturally from the algebraic interplay of the graded Euclidean primitives, such as points, lines, planes, and volumes, which characterize the three-dimensional conformal geometry of the ambient physical space, set within its eight-dimensional Clifford-algebraic representation. Remarkably, the resulting algebra remains associative, and allows us to understand the origins and strengths of all quantum correlations locally, in terms of the geometry of the compactified physical space, namely, that of a quaternionic 3-sphere, S^3, with S^7 being its algebraic representation space.

I am not quite sure what is wrong with mentioning the Lie group E_8, in the manner I have done, to introduce, motivate, and situate the framework presented in the manuscript within a wider context of current work being done in both mathematics and theoretical physics. Surely, the fact that the Lie group E_8 is mentioned in this manner but not explicitly used because it is not explicitly needed to derive the strong correlations is not a sufficient reason to reach the negative conclusion the reviewer has reached. Unfortunately the reviewer has admitted to not have engaged with the manuscript at all but instead has preferred to rely on non-peer-reviewed online commentaries and unpublished criticisms of my earlier work to form a negative opinion about the work presented in the current manuscript.

Author action: To assist clarity, I have added a couple of new remarks about E_8 in the last paragraph of the Subsection 2.5.

Reviewer # 4: The manuscript is well written and the author has become successful to present results at proper place. I recommend for publication.

Author reply: I thank Reviewer # 4 for these kind words and for recommending my manuscript for publication.

Author action: No action.

Reviewer # 5: In the second paragraph the author states: "It is, in fact, an argument that depends on a number of physical assumptions about what is and what is not possible within any locally causal theory, and these assumptions can be, and have been questioned before [9][10]."

The author's claims about disproving Bell's theorem were also questioned before and by now the nature of author's mistakes is completely understood.

Author reply: Yes, my earlier work challenging Bell's theorem has been questioned before, and by now the nature of critics' mistakes is also completely understood. It is also important to note that most of the criticisms of my earlier work have been in the form of non-peer-reviewed or unpublished online commentaries. I have extensively replied to such unpublished criticisms, in considerable detail, in at least the following three preprints: arXiv:quant-ph/0703244, arXiv:1110.5876, and arXiv:1203.2529. There are also two peer-reviewed and published criticisms of my earlier work, to which I have responded, again in considerable detail, in these two preprints: arXiv:1301.1653 and arXiv:1501.03393.

It is important to note, however, that while there are some overlapping conceptual elements between my earlier work and the work presented in the current manuscript (for example, both works use Clifford-algebraic concepts compatible with normed division algebras), the current work builds a 7-sphere framework from the first principles, whereas the earlier work was based on a 3-sphere framework meant for only the singlet and Hardy-type correlations.

Author action: In the light of the reviewer's comment I have added footnote 3 on the page 23 of the manuscript, which clarifies the relationship between my earlier work based a quaternionic 3-sphere and the current, more complete framework based on an octonion-like 7-sphere.

Reviewer # 5: There are two fundamental mistakes. First, the correlations have to be computed using actual experimental results of +1 and -1 and not like in Eqs. 94-99. (see https://arxiv.org/pdf/1212.4854v2.pdf).

Author reply: Yes, the correlations have to 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. So, with that important qualification, the correlations are indeed computed using actual experimental results of $+1$ and -1 within the local-realistic framework presented in the manuscript. Such actual experimental results are explicitly specified by the limiting scalar points, $\mathscr{A}(\mathbf{a}\,,\,\lambda^k)=\pm 1, \mathscr{B}(\mathbf{b}\,,\,\lambda^k)\pm 1$, etc., of the elements of the 7-sphere constructed in the manuscript. They correspond exactly to the measurement results considered by Bell in his seminal work [compare Eq. 1 of his 1964 paper and Eqs. (82) and (83) of my manuscript]. These $+1$ or -1 results are then averaged over in Eq. (94), which is *the* standard way of computing the expected value in the experimental context of Bell's theorem. Within 7-sphere, the result of this average then necessarily works out to give

$$\mathcal{E}_{L.R.}(\mathbf{a},\,\mathbf{b},\,\mathbf{c},\,\mathbf{d},\,\dots)=\lim_{m\,\rightarrow\,\infty}\left[\frac{1}{m}\sum_{k\,=\,1}^{m}\mathscr{A}(\mathbf{a}\,,\,\lambda^k)\,\mathscr{B}(\mathbf{b}\,,\,\lambda^k)\,\mathscr{C}(\mathbf{c}\,,\,\lambda^k)\,\mathscr{D}(\mathbf{d}\,,\,\lambda^k)\,\dots\right]$$
$$=-\,\cos\theta_{\mathbf{xy}}(\mathbf{a},\,\mathbf{b},\,\mathbf{c},\,\mathbf{d},\,\dots),\tag{234}$$

as proven explicitly by the derivation shown in Eqs. (95) to (99) in the manuscript. Admittedly, this result is difficult to appreciate if one is committed to the prior belief that such a result is impossible. And it is indeed impossible in the flatland of any description. But it is inevitable within the octonion-like S^7 framework considered in the manuscript.

What the reviewer seems to have missed are the equations and constructions that have been discussed in the manuscript prior to (and after) the computation of average in Eq. (94), the result (234) of which is proven by the derivation in Eqs. (95) to (99). Together with the conservation law discussed in Eqs. (112) to (114), they lead to the geometrical equivalence of the following two averages:

$$\mathcal{E}_{L.R.}(\mathbf{a},\,\mathbf{b},\,\mathbf{c},\,\mathbf{d},\,\dots)=\lim_{m\,\rightarrow\,\infty}\left[\frac{1}{m}\sum_{k\,=\,1}^{m}\mathscr{A}(\mathbf{a}\,,\,\lambda^k)\,\mathscr{B}(\mathbf{b}\,,\,\lambda^k)\,\mathscr{C}(\mathbf{c}\,,\,\lambda^k)\,\mathscr{D}(\mathbf{d}\,,\,\lambda^k)\,\dots\right]\tag{91}$$

and

$$\mathcal{E}_{L.R.}(\mathbf{a},\,\mathbf{b},\,\mathbf{c},\,\mathbf{d},\,\dots)$$
$$= \lim_{m\,\rightarrow\,\infty}\left[\frac{1}{m}\sum_{k\,=\,1}^{m}\mathbf{N}(\mathbf{a}_r,\,\mathbf{a}_d,\,0,\,\lambda^k)\,\mathbf{N}(\mathbf{b}_r,\,\mathbf{b}_d,\,0,\,\lambda^k)\,\mathbf{N}(\mathbf{c}_r,\,\mathbf{c}_d,\,0,\,\lambda^k)\,\mathbf{N}(\mathbf{d}_r,\,\mathbf{d}_d,\,0,\,\lambda^k)\,\dots\right]. \tag{92}$$

See also the derivation that leads to the identity (116) in the manuscript. So whoever the reviewer thinks has “understood" the non-existent “mistakes" has clearly misunderstood the above straightforward calculations within geometric algebra.

It is also worth noting that the preprint cited by Reviewer # 5 (which concerns my earlier work and not the current manuscript under consideration) is quite misleading. In it the author gives the impression that he is about to present and then criticize my earlier Clifford algebra based 3-sphere model for the singlet correlations. But in fact he does no such thing. He immediately switches to his own non-Clifford algebraic model and shows that his model does not reproduce the strong correlations. Therefore, he argues, that my Clifford-algebraic model must also be wrong. In any case, his 2012 claim has nothing to do with either my earlier work or the current work under consideration, because contrary to mine his model for the correlations is based on non-Clifford algebraic concepts. Moreover, in my reply to him (cf. arXiv:1301.1653) I have already elucidated the straw-man on which his mistaken argument is based.

Author action: In response to the reviewer’s skepticism I have added a proof of Eq. 192 in Appendix B.

Reviewer # 5: Second there is a cleverly hidden sign mistake which happens in the transition from Eq. 97 to Eq. 98.

Author reply: There is no sign mistake in the transition from Eq. 97 to Eq. 98, hidden or otherwise. In fact the transition is quite straightforward and very easy to understand. Let us begin with Eq. 97, which reads:

$$\mathcal{E}_{L.R.}(\mathbf{a},\,\mathbf{b},\,\mathbf{c},\,\mathbf{d},\,\dots)$$
$$= -\,\mathbf{x}_r\cdot\mathbf{y}_r\,-\,\mathbf{x}_d\cdot\mathbf{y}_d\,-\lim_{m\,\rightarrow\,\infty}\left[\frac{1}{m}\sum_{k\,=\,1}^{m}\mathbf{N}\left(\mathbf{x}_r\times\mathbf{y}_r+\mathbf{x}_d\times\mathbf{y}_d,\,\mathbf{x}_r\times\mathbf{y}_d+\mathbf{x}_d\times\mathbf{y}_r,\,0,\,\lambda^k\right)\right]. \tag{97}$$

Now we use Eq. 74, which relates the spinor $\mathbf{N}$ to the spinor $\mathbf{D}$, and in the present context takes the explicit form

$$\mathbf{N}\big(\mathbf{x}_r\times\mathbf{y}_r+\mathbf{x}_d\times\mathbf{y}_d,\,\mathbf{x}_r\times\mathbf{y}_d+\mathbf{x}_d\times\mathbf{y}_r,\,0,\,\lambda^k\big)$$
$$= \lambda^k\,\mathbf{D}\,(\mathbf{x}_r\times\mathbf{y}_r+\mathbf{x}_d\times\mathbf{y}_d,\,\mathbf{x}_r\times\mathbf{y}_d+\mathbf{x}_d\times\mathbf{y}_r,\,0)\,. \tag{74}$$

Next, since the coin $\lambda^k=\pm1$ is just a scalar number, substituting the above equation into Eq. 97 immediately gives

$$\mathcal{E}_{L.R.}(\mathbf{a},\,\mathbf{b},\,\mathbf{c},\,\mathbf{d},\,\dots)$$
$$= -\,\mathbf{x}_r\cdot\mathbf{y}_r\,-\,\mathbf{x}_d\cdot\mathbf{y}_d\,-\lim_{m\,\rightarrow\,\infty}\left[\frac{1}{m}\sum_{k\,=\,1}^{m}\lambda^k\right]\mathbf{D}\,(\mathbf{x}_r\times\mathbf{y}_r+\mathbf{x}_d\times\mathbf{y}_d,\,\mathbf{x}_r\times\mathbf{y}_d+\mathbf{x}_d\times\mathbf{y}_r,\,0)\,, \tag{98}$$

which is Eq. 98 in the manuscript. So, as we can see, the transition from Eq. 97 to Eq. 98 is quite straightforward, with no sign mistake in sight. Moreover, between Eqs. 100 and 103 I have used a very different method to arrive at Eq. 99 from Eq. 96, which again confirms the correctness of the transition from Eq. 97 to Eq. 98.

Author action: To make the transition from Eq. 97 to Eq. 98 clearer, I have added the following lines in the manuscript just below Eq. 99: "Here Eq. (98) follows from Eq. (97) by using Eq. (74), which now takes the form

$$\mathbf{N}\big(\mathbf{x}_r \times \mathbf{y}_r + \mathbf{x}_d \times \mathbf{y}_d,\, \mathbf{x}_r \times \mathbf{y}_d + \mathbf{x}_d \times \mathbf{y}_r,\, 0,\, \lambda^k\big) = \lambda^k\, \mathbf{D}\,(\mathbf{x}_r \times \mathbf{y}_r + \mathbf{x}_d \times \mathbf{y}_d,\, \mathbf{x}_r \times \mathbf{y}_d + \mathbf{x}_d \times \mathbf{y}_r,\, 0)\,, \tag{74}$$

together with $\lambda^k = \pm 1$."

Reviewer # 5: The objects in the sum in Eq. 97 are not of the same kind, but a mixture of objects corresponding to different algebra representations.

Author reply: Actually the objects in the sum in Eq. 97 are all of the same kind. They are *not* a mixture of objects corresponding to different algebraic representations. The objects being summed over in Eq. 97 are the pure spinors,

$$\mathbf{N}\left(\mathbf{x}_r \times \mathbf{y}_r + \mathbf{x}_d \times \mathbf{y}_d,\, \mathbf{x}_r \times \mathbf{y}_d + \mathbf{x}_d \times \mathbf{y}_r,\, 0,\, \lambda^k\right),$$

as we can see at once from looking at Eq. 97:

$$\mathcal{E}_{L.R.}(\mathbf{a},\, \mathbf{b},\, \mathbf{c},\, \mathbf{d},\, \dots) = -\,\mathbf{x}_r \cdot \mathbf{y}_r \,-\, \mathbf{x}_d \cdot \mathbf{y}_d \,-\, \lim_{m \to \infty}\left[\frac{1}{m}\sum_{k=1}^{m} \mathbf{N}\left(\mathbf{x}_r \times \mathbf{y}_r + \mathbf{x}_d \times \mathbf{y}_d,\, \mathbf{x}_r \times \mathbf{y}_d + \mathbf{x}_d \times \mathbf{y}_r,\, 0,\, \lambda^k\right)\right]. \tag{97}$$

These pure spinors, which represent the objectively existing systems in full compliance with Einstein's conception of realism, are defined in Eq. 64 of the manuscript as non-scalar parts of the non-pure spinors constituting the 7-sphere. Within the framework presented in the manuscript their geometric product satisfies the following algebraic relation:

$$\mathbf{N}(\mathbf{a}_r,\, \mathbf{a}_d,\, 0,\, \lambda^k)\, \mathbf{N}(\mathbf{b}_r,\, \mathbf{b}_d,\, 0,\, \lambda^k) = -\,\mathbf{a}_r \cdot \mathbf{b}_r \,-\, \mathbf{a}_d \cdot \mathbf{b}_d \,-\, \mathbf{N}\left(\mathbf{a}_r \times \mathbf{b}_r + \mathbf{a}_d \times \mathbf{b}_d,\, \mathbf{a}_r \times \mathbf{b}_d + \mathbf{a}_d \times \mathbf{b}_r,\, 0,\, \lambda^k\right). \tag{80}$$

This algebraic relation shows unambiguously that each one of the spinors $\mathbf{N}$ appearing in the sum in Eq. 97 belongs to the same algebraic representation of the 7-sphere, for all runs of the experiment represented by the index k.

To be sure, each spinor $\mathbf{N}$ representing the system is related to the spinor $\mathbf{D}$ representing the detector via Eq. 74,

$$\mathbf{N}\big(\mathbf{x}_r \times \mathbf{y}_r + \mathbf{x}_d \times \mathbf{y}_d,\, \mathbf{x}_r \times \mathbf{y}_d + \mathbf{x}_d \times \mathbf{y}_r,\, 0,\, \lambda^k\big) = \lambda^k\, \mathbf{D}\,(\mathbf{x}_r \times \mathbf{y}_r + \mathbf{x}_d \times \mathbf{y}_d,\, \mathbf{x}_r \times \mathbf{y}_d + \mathbf{x}_d \times \mathbf{y}_r,\, 0)\,, \tag{74}$$

and, for a given k, $\mathbf{D}$ *can* belong to an algebraic representation that is different in orientation (or handedness) from the one that $\mathbf{N}$ belongs to. But $\mathbf{D}$'s do not appear in the sum in Eq. 97 at all. Only $\mathbf{N}$'s appear in the sum in Eq. 97.

Author action: No action.

Reviewer # 5: Hence Eq. 99 is mathematically incorrect.

Author reply: Eq. 99 is mathematically correct. In the previous two replies I have demonstrated that, contrary to the reviewer's claims, (1) there is no "cleverly hidden sign mistake" in the transition from Eq. 97 to Eq. 98, and (2) the objects in the sum in Eq. 97 are *of the same kind* and not a mixture of objects corresponding to different algebraic representations. Moreover, in the manuscript, between Eqs. 100 and 103, I have used a very different method to again arrive at Eq. 99 from Eq. 96, which is confirmatory evidence of the correctness of the transition from Eq. 97 to Eq. 98.

But, despite these explicit demonstrations, suppose, for the sake of argument, that there is some invisible mistake in the derivation of the correlations in Eq. 99. If so, then that would be quite an extraordinary mistake to have allowed me to reproduce exactly, not only the correlations predicted in Eq. 108 for the rotationally invariant 2-particle singlet state, but also the highly non-trivial correlations predicted in Eq. 186 for the rotationally non-invariant 4-particle GHZ state. Such an extraordinary mistake would surely deserve to be called a law of nature or principle of physics.

Author action: No action.

Reviewer # 5: This manuscript adds a new element, a software code intended to recover the quantum correlations. This is supposed to be an answer to a "Randy Quantum Challenge" (https://arxiv.org/abs/1207.5294) which came about due to the author's claims of "disproof" of Bell's theorem.

Author reply: Neither the software code presented in my manuscript nor any actual Bell-type experiment based on coincidence counts has anything to do with the contents of the preprint cited by the Reviewer # 5. The code presented in that preprint is fictitious, with no counterpart in any known physics, either theoretical or experimental.

More importantly, nothing like Clifford algebra, quaternions, octonions, 3-sphere, or 7-sphere is even mentioned in the preprint cited by the reviewer, let alone implemented in a code using the GAViewer, as done in my manuscript.

The software code presented in my manuscript is not in response to any challenge. It simply provides an illuminating pedagogical tool, in addition to verifying the analytical results presented in the manuscript, such as the 2-particle and 4-particles correlations computed in Eqs. 125 and 203, respectively.

Author action: No action.

Reviewer # 5: However the code is another twist on the first fundamental mistake because it does not count correlations using the actual experimental outcomes.

Author reply: On the contrary, the code presented in the manuscript computes correlations among the actual experimental outcomes such as $A = +1$ or -1, $B = +1$ or -1, etc., in perfect harmony with the correlations analytically derived in the manuscript among such binary outcomes within the 7-sphere framework. In any case, the analytical results presented in the manuscript stand on their own and do not require the code for their validity. On the other hand, the event-by-event simulations of the strong correlations presented in the manuscript do provide added support to the analytical results presented therein, for they are both pedagogically and statistically illuminating.

Author action: No action.