

Test of local realism via entangled $\Lambda\bar{\Lambda}$ system

Corresponding Author: Professor G.R. Liao

This manuscript has been previously reviewed at another journal. This document only contains information relating to versions considered at Nature Communications. Any mention or content from previous journals has been redacted.

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors present results of a new test of Bell's inequality (BI) using spin-entangled $\Lambda\bar{\Lambda}$ pairs produced in the BES III experiment, through the measurement of the momentum correlation between the decay products of Λ and $\bar{\Lambda}$. To my knowledge, this is the first experiment directly confirming Bell's inequality violation :

- involving the system of massive particles,
- ensuring the space-like separation of the measurements (decays),
- providing a conclusive statistical significance

at the same time. This test of quantum mechanics in a massive particle system adds another steady step towards verifying the universality of quantum mechanics.

While I firmly believe the achievement warrants a publication in the journal, it seems the novelty and the value of the study is not correctly articulated. There are also a few outstanding issues remained to be addressed from the last review in [journal name redacted]. My recommendation on the publication is therefore conditional, and I am happy to continue the discussion here. Below is the remaining questions to the authors' with the enumeration inherited from the letter. Many thanks for answering and implementing the comments.

(2) Now that this is not the first experiment achieving the BI test in the mass particle system, the motivation of this study needs to be clarified and carefully contextualized along the other previous studies, particularly in terms of the difference from the ones using massive particles. Should this result be considered as one more example besides the other experiments (using Cooper pairs, ions, $K^0\bar{K}^0$, $B^0\bar{B}^0$, protons etc.), or this has distinctive novelties over them?

(3) So then is it more sensible to align the title to a test of local realism? Now the title and the rest indicate different tests.

(4) I do not understand the argument here at all. Why replacing the correlation term into the "measurement of the correlation" would mark the efficiency loophole as resolved? (What is the "measurement of the correlation" at the first place? What is different from just "the correlation"?) I do not think the efficiency loophole is something that can be avoided by calibrating and correcting the detector since that is how exactly the loophole works i.e. the undetected events can have unreasonably high correlation compared with the detected ones via some unknown interactions or unknown physical mechanisms. I would agree with the statement that the efficiency loophole is such a conspiracy so accepting it as a loophole does not degrade the generality/value of the studies, but I do not think that is called a closure of the efficiency loophole.

Aside the validity of the statement, there seems no discussion to support the statement in the cited Ref 22 (Ref 19 in the original manuscript). Is this a correct citation? I think it is inappropriate to cite a reference on a non-trivial statement while the ref does not say anything about it. If there is no valid reference provided to the efficiency loophole discussion, I suggest to extend the discussion in the main body or supplementary material since it is not as trivial as being able to be settled by half of a paragraph.

(8) Apology I did not mean to suggest to include the test 3 in the main body, but rather asked to clarify the context and the relationship between the other tests. Do I understand correctly that the test 2 is supposed to be the improved version of the test 1 in terms of the loopholes? If so, would it be sufficient to just discuss the test 2? The relevance of the test 3 is not

discussed anywhere, which I think this needs to be contextualized.

I also have a few questions and comments that I hope to resolve during this round of review:

- L96-100: I do not understand the discussion about the free-will loophole here. How this addresses the arguments in Ref. 37/40 and why it is considered to be solved? Also does this solution apply for the other experiments in the past as well?

- L104-: The definition of n_1, n_2, n_1', n_2' is unclear to me. Are there two groups of events (sample A and B) and n_1 is the proton direction in sample A, n_1' the proton direction in sample B, n_2 the anti-proton direction in sample A, and n_2' the anti-proton direction in sample B? How come there are four groups of events in the end? Also if these vectors are the direction of the decay particles, how does one choose like $\theta_{12} = \theta_1'2 = \theta_1'2' = \theta_{12}'/3$? Are events not satisfying the condition discarded?

- Eq. 10: Should $\langle n_i n_j \rangle$ be $\langle n_i n_j' \rangle$? Also I believe the second line is not part of the definition.

L230-238: I do not understand this toy MC procedure to compute the $Q_{\max}$. Isn't it simply taking an average of $(n_i \times n_j')$ over the all signal events? The statistical significance derivation is also questionable since the quoted p-value should be something like $P(a \text{ } Q_{\max}=1 \text{ hypothesis achieves } Q_{\max} > \sqrt{2} \text{ by fluctuation})$.

- Fig. 4 does not seem to be the figure described in the caption and main body ($Q_{\max}$ with different N_{toyMC}). Also is the "Extended Data Fig. 4" mentioned in L234 should be "Fig. 4"?

- The signal extraction (amplitude analysis) and background subtraction rely on the knowledge of quantum mechanics. How this penalty is accounted in the context of a test of LHVTs? Naively this should affect the upper bound of the inequality.

Though not part of the peer-review, I feel the manuscript needs more effort to improve the clarity and formality. I particularly struggled with:

- the too long and colloquial general introduction (citing a Nobel prize sounds unusual as a journal paper)
- the novelty and context of the studies are not clearly described
- lack of a big picture outline in the analysis section
- L187-L238: the enumeration starts without context.

Reviewer #2

(Remarks to the Author)

The paper currently under review is a revised version of a previously submitted manuscript to [journal name redacted], which I have reviewed. After considering the authors' responses to my earlier report and the corresponding changes made in the revised version, I decided that I cannot recommend this paper for publication in Nature Communications in the present form. The reasoning for my conclusion is detailed below.

1) The authors failed to fully implement the recommendations addressed in my previous report, in particular regarding the modifications required at point 3). They have just moved the analysis on the Bell inequality violation obtained by means of the " $Q_{\max}$ " method, see Eq.(11) (which was previously reported in the appendix), to the main body of the paper, but leaving the analysis based on the CH method of Bell inequality violation and related discussions in the same place. Now at page 8, the authors summarize three methods used to evaluate the Bell inequality. This generates a confusion, since it seems that they have done three different measurements of Bell inequality, while all of them are equivalent, with the difference that the method of $Q_{\max}$ is optimized and gives the largest significance for the Bell violation (10 sigma), as indeed verified by the provided results on $Q_{\max}$ below Eq.(11) at page 8.

As I already mentioned, the analysis based on their CH method is not the best one because it depends on the choice of basis, while the basis-independent inequality tested via the $Q_{\max}$ in Eq.(11) (which is based on the CHSH and Horodecki test), provides the optimized condition for the Bell violation in bipartite qubit systems, which is by now the standard method. In particular, I suggested to drop altogether the inequality in (previous) Eqs. (5) and (6), and the basis-dependent discussion and figures, by replacing all of them with (previous) Eq.(15) in appendix and to provide only this test of Bell inequality violation. None of these last suggestions have been implemented in the new version of the manuscript.

Moreover, the main claim that their method to test Bell inequality violation based on the CH inequality is better than the other method ($Q_{\max}$) is not true. All the other methods analyzed here follow from the CH criterion and inherit all the advantages, therefore, there is no need to provide unnecessary information based on less efficient analyses of Bell inequality tests.

2) In the meantime a new published work on the Bell inequality violation in charmonium decays appeared (Phys.Rev.D 110 (2024) 5, 053008), that is using the method based on helicity amplitudes to reconstruct the density matrix and evaluate the $Q_{\max}$. This work can provide a new guideline for testing the Bell inequality in the present experimental analysis. In this respect, it would be very useful to also provide the values of the helicity amplitudes with associated errors, that can allow independent evaluations of $Q_{\max}$ by following this approach.

Moreover, the fact that the $Q_{\max}$ is close to its maximum value is purely due to kinematic, see reference above for more details. Then, to evaluate the violation of Bell's inequality at the experimental level for the charmonium decay into Λb

Lambda(bar) system, the key information required is basically the assessment of the errors associated with the corresponding helicity amplitudes. These errors can be directly translated into the $Q_{\max}$ error, which eventually determines the strength of significance to rule out the null hypothesis for the Bell inequality violation.

3) I noticed that the new references have been added in the revised version, but they are cited collectively without clarifying their relevance to the specific context. For instance, the fact that Ref.[35] was the first to show the violation of Bell inequality at high energies and in the presence of weak and strong interaction, has not been addressed.

In conclusion, the revisions in this new version are very minimal and have not significantly altered the previous version, leaving the most important changes I recommended in my earlier report unaddressed. Therefore, I am unable to recommend this paper for publication in the present form.

Reviewer #3

(Remarks to the Author)

Report on the paper "THE VERIFICATION OF QUANTUM NON-LOCAL CORRELATION IN A ANTI- Λ SYSTEM"

[previous journal mention redacted]

I have reviewed the new version of the work and find that all criticisms were addressed and the corresponding modifications to the text were implemented. The changes improved the quality and the added discussion resulted in a clearer presentation of the results. Important references that were missing were also included. Some sentences that overstated the findings of the work, that presented it as the first report of this nature using measurements with massive particles, were removed. I therefore recommend publication of this work in Nature Communications.

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

I appreciate the authors to have addressed the major comments to the previous manuscript, and am happy to recommend the acceptance.

However, the new manuscripts still contain incorrect or misleading descriptions (mainly in the introductory paragraphs) as pointed as below. These should be fixed before or during the proof reading stage. Also, I hope a more careful review and scrutinization would have been made inside the collaboration before the journal submission. It is like the process is outsourced to the peer reviewers, which made us either to sign-off an incomplete paper, or to devote a considerable amount of time to check in detail. Particularly, there were quite a few false claims and questionable citations initially that I had to go through the references to validate, which should be rather the responsibility of the co-authors.

Alas, congratulation to the successful results achieved in the paper.

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- There seems a significant confusion throughout the introduction between the "guide axes" (arbitrary axes chosen by the analyzer, such as the unit vectors denoted by a, b, c, d in Eq.(4)) and the actual proton/anti-proton orientations, both represented by the same symbols ($n, n', n_1, n_2, \theta_{pp}$ etc.) e.g.

- (A) n_1, n_2, θ_{pp} in Eq (1)(3): guide axes

- (B) n_1, n_2, θ_{pp} in Eq (2): proton (anti-proton) momentum in Λ (Λ bar)'s rest frame

- (C) n_a, n'_b, n_c, n'_d in Eq (4): proton (anti-proton) momentum in Λ (Λ bar)'s rest frame projected onto the guide axes n_a, n_b, n_c, n_d (defined in Ref 43).

- (D) $n_1, n_2, n'_1, n'_2, \theta$ in Eq.(7)-(9): guide axes (this is why you are allowed to fix θ to certain values in L142/L148 otherwise it does not make sense).

(A) is explained wrongly in the text; (C) is explained incompletely; (D) the definition in L122-123 is extremely vague. In case you are unsure, you may compactify the description and cite Ref 25 more. In fact, the discussion in L69-L149 is more likely the scope of Ref 25.

- L84: I think the value of α is determined by measurement but not from theoretical prediction (at least the quoted value is from the measurement).

- L117-118: Likewise, not sure this claim is valid.

- L98: I am not sure Ref. 15 really addresses the argument described in L93-L97. The claim of the loophole is "the decays cannot be guaranteed to be completely random without the active control of measurements" at the first place. I do not think the three sentences in Ref 15 sufficiently resolves the concern.

- L100: “ the sample survived from the free-will choice is a fair sampling of detected events”: I think this is the description to address the fair-sampling loophole, which has nothing to do with the freedom-of-choice loophole.
- L104-116: “A refined...” Since there is no description on how/where this refined, it rather sounds like it addressed the freedom-of-choice loophole by the context from the previous paragraph, which is misleading. Also this part is no longer connected to other parts of the main body of text. It looks this can go to Appendix with the test result entirely.
- L119: “a new LHVT”→“a new inequality”?
- L119: “namely the CH inequality” → “a CH-type inequality” (the CH inequality exists from before)
- L124: I wonder if you really split the events into four groups; you may just calculate P with different guide axes using the same events?
- Eq.(7), L135: You post-select the space-like events in the analysis (Eq.(10)) so the penalty factor beta should not exist in the inequality anymore.

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REVIEWER COMMENTS

Reviewer #1 (Remarks to the Author):

The authors present results of a new test of Bell's inequality (BI) using spin-entangled $\Lambda\bar{\Lambda}$ pairs produced in the BES III experiment, through the measurement of the momentum correlation between the decay products of Λ and $\bar{\Lambda}$. To my knowledge, this is the first experiment directly confirming Bell's inequality violation :

- involving the system of massive particles,
 - ensuring the space-like separation of the measurements (decays),
 - providing a conclusive statistical significance
- at the same time. This test of quantum mechanics in a massive particle system adds another steady step towards verifying the universality of quantum mechanics.

1. While I firmly believe the achievement warrants a publication in the journal, it seems the novelty and the value of the study is not correctly articulated. There are also a few outstanding issues remained to be addressed from the last review in [**journal name redacted**]. My recommendation on the publication is therefore conditional, and I am happy to continue the discussion here. Below is the remaining questions to the authors' with the enumeration inherited from the letter. Many thanks for answering and implementing the comments.

[Answer: Thank you for your insightful comments and for recognizing the significance of our work in testing Bell's inequality. We are committed to addressing the concerns and questions you have raised to ensure that the manuscript accurately reflects the novelty and value of the study.](#)

2. Now that this is not the first experiment achieving the BI test in the mass particle system, the motivation of this study needs to be clarified and carefully contextualized along the other previous studies, particularly in terms of the difference from the ones using massive particles. Should this result be considered as one more example besides the other experiments (using Cooper pairs, ions, K⁰ $\bar{K}^0$, B⁰ $\bar{B}^0$, protons etc.), or this has distinctive novelties over them?

[Answer: Thank you for your further inquiries into our research. Response to this comment, we add a paragraph to summarize the features of this experiment \(see line 56-65\). Although this is not the first experiment to achieve Bell inequality testing in a massive particle system, our study possesses some unique features and novelty.](#)

[\(1\) In this work, the interaction mechanisms involved not only include the strong interaction but also encompass the weak interaction.](#)

[\(2\) The observational methods also differ, as this research applies the CH inequality to measurements in the field of high-energy physics for the first time.](#)

[\(3\) Additionally, this is the first time Bell's inequality has been measured in the hyperon system.](#)

[\(4\) This is the first experiment directly confirming Bell's inequality violation involving the system of massive particles, with significant exclusion of LHVT.](#)

3. So then is it more sensible to align the title to a test of local realism? Now the title and the rest indicate different tests.

Answer: Thank you for your suggestion, the title has been changed to “ Test of local realism via entangled $\Lambda\bar{\Lambda}$ System ”

4. I do not understand the argument here at all. Why replacing the correlation term into the “measurement of the correlation” would mark the efficiency loophole as resolved? (What is the “measurement of the correlation” at the first place? What is different from just “the correlation”?) I do not think the efficiency loophole is something that can be avoid by calibrating and correcting the detector since that is how exactly the loophole works i.e. the undetected events can have unreasonably high correlation compared with the detected ones via some unknown interactions or unknown physical mechanisms. I would agree with the statement that the efficiency loophole is such a conspiracy so accepting it as a loophole does not degrade the generality/value of the studies, but I do not think that is called a closure of the efficiency loophole.

Answer: We fully agree with your comments. We realize that is inaccurate by stating “measurement of correlation can address the efficiency loophole”. We revise the draft by removing the sentence “Note that this loophole can be closed by improving detector efficiency or by using the Clauser-Horne inequality, which will be discussed below, as it relaxes the requirements of the experimental conditions [11].”, and adding some discussions in draft as line 50-54 :

5. Aside the validity of the statement, there seems no discussion to support the statement in the cited Ref 22 (Ref 19 in the original manuscript). Is this a correct citation? I think it is inappropriate to cite a reference on a non-trivial statement while the ref does not say anything about it. If there is no valid reference provided to the efficiency loophole discussion, I suggest to extend the discussion in the main body or supplementary material since it is not as trivial as being able to settled by half of a paragraph.

Answer: Sorry for the confusion and thanks for the suggestion, we remove this sentence and old reference 22. Our experiment does not close the detection loophole, since having a fair sampling of the recorded events is assumed. However, it is a widely accepted fundamental assumption in collider physics, as suggested in Ref. Phys. Rev. D 110, 053008 (2024). We use the CH inequality, which upper bound is independent of the data statistics. If we observed a violation in the detected subsample, then it implies a violation holds for the full data set. This is different from the measurement of event coincidence in the optical experiments.

6. Apology I did not mean to suggest to include the test 3 in the main body, but rather asked to clarify the context and the relationship between the other tests. Do I understand correctly that the test 2 is supposed to be the improved version of the test 1 in terms of the loopholes? If so, would it be sufficient to just discuss the test 2? The relevance of the test 3 is not discussed anywhere, which I think this needs to be contextualized.

Answer: Thanks for your suggestion. To avoid confusion about reporting three tests, we only report the CH test in the main draft. Other two tests are move to the appendix part. As for the relationship between them, we provide some discussion in main body of text (see Line 116-119). The CHSH inequality is the improved version of original Bell inequality, but these two schemes are dependent on the QM quantity α_Λ , and this bring about a new loophole. So, we put these two tests to the appendix as a cross check. In main draft, we only present the test of CH inequality, which is independent on the quantity α_Λ .

7. I also have a few questions and comments that I hope to resolve during this round of review:

- L96-100: I do not understand the discussion about the free-will loophole here. How this addresses the arguments in Ref. 37/40 and why it is considered to be solved? Also does this solution apply for the other experiments in the past as well?

Answer: Thank you for pointing out this issue. The free-will loophole is not resolved in high energy experiment, and it is still an open question. We revise this paragraph and provide a discussion in a weak tone, see line 93-102..

- L104-: The definition of n_1 , n_2 , n'_1 , n'_2 is unclear to me. Are there two groups of events (sample A and B) and n_1 is the proton direction in sample A, n'_1 the proton direction in sample B, n_2 the anti-proton direction in sample A, and n'_2 the anti-proton direction in sample B? How come there are four groups of events in the end? Also if these vectors are the direction of the decay particles, how does one choose like $\theta_{12} = \theta_{1/2} = \theta_{1/2'} = \theta_{12'}/3$? Are events not satisfying the condition discarded?

Answer: Yes, you are right. We rewrite these sentences: Here $\vec{n}_i$ or $\vec{n}'_i$ vectors are the optical generalization of polarizer settings, and $\vec{n}_1$ and $\vec{n}'_1$ indicate two directions to measure the momenta correlation of the protons with antiprotons, while $\vec{n}_2$ and $\vec{n}'_2$ are that for the anti-protons. As for the relation $\theta_{12} = \theta_{1/2} = \theta_{1/2'} = \theta_{12'}/3$, it is easy to understand in terms of optical settings.

- Eq. 10: Should $\langle n_i n_j \rangle$ be $\langle n_i n'_j \rangle$? Also I believe the second line is not part of the definition.

Answer: Yes, you are right. This is a typo. The first line is definition, but the second line is not. We add symbol $\equiv$ to the first line.

L230-238: I do not understand this toy MC procedure to compute the $Q_{\max}$. Isn't it simply taking an average of $(n_i \times n'_j)$ over the all signal events? The statistical significance derivation is also questionable since the quoted p-value should be something like $P(a \text{ } Q_{\max}=1 \text{ hypothesis achieves } Q_{\max} > \sqrt{2} \text{ by fluctuation})$.

Answer: Yes, you are right. We add some sentences to explain this process in more detail in the revised manuscript of supplementary materials.

- Fig. 4 does not seem to be the figure described in the caption and main body ($Q_{\max}$ with different N_{toyMC}). Also is the "Extended Data Fig. 4" mentioned in L234 should be "Fig. 4"?

Answer: It should be Fig.4, and this part will be moved to the appendix.

- The signal extraction (amplitude analysis) and background subtraction rely on the knowledge of quantum mechanics. How this penalty is accounted in the context of a test of LHVTs? Naively this should affect the upper bound of the inequality.

Answer: In the generation of the background events, quantum correlations have been taken into account with the amplitude model, and the uncertainty of the background affects the measurement precision, but do not change the upper bound.

Additionally, we have considered the systematic errors caused by this factor to estimate its impact.

Though not part of the peer-review, I feel the manuscript needs more effort to improve the clarity and formality. I particularly struggled with:

- the too long and colloquial general introduction (citing a Nobel prize sounds unusual as a journal paper)
- the novelty and context of the studies are not clearly described
- lack of a big picture outline in the analysis section
- L187-L238: the enumeration starts without context.

Answer: Thanks for the suggestion, we agree all of them, and they are revised in the updated version.

REVIEWER COMMENTS

Reviewer #2 (Remarks to the Author):

The paper currently under review is a revised version of a previously submitted manuscript to [journal name redacted], which I have reviewed. After considering the authors' responses to my earlier report and the corresponding changes made in the revised version, I decided that I cannot recommend this paper for publication in Nature Communications in the present form. The reasoning for my conclusion is detailed below.

1) The authors failed to fully implement the recommendations addressed in my previous report, in particular regarding the modifications required at point 3). They have just moved the analysis on the Bell inequality violation obtained by means of the "Q_max" method, see Eq.(11) (which was previously reported in the appendix), to the main body of the paper, but leaving the analysis based on the CH method of Bell inequality violation and related discussions in the same place. Now at page 8, the authors summarize three methods used to evaluate the Bell inequality. This generates a confusion, since it seems that they have done three different measurements of Bell inequality, while all of them are equivalent, with the difference that the method of Q_max is optimized and gives the largest significance for the Bell violation (10 sigma), as indeed verified by the provided results on Q_max below Eq.(11) at page 8.

Answer: Although the third method has a high level of significance, due to its dependence on α_Λ , which is measured in quantum mechanics scenario; we have decided to place it in the appendix. On the contrary, we will discuss the main content of our research, namely the second methods, in detail in the main text.

As I already mentioned, the analysis based on their CH method is not the best one because it depends on the choice of basis, while the basis-independent inequality tested via the Q_max in Eq.(11) (which is based on the CHSH and Horodecki test), provides the optimized condition for the Bell violation in bipartite qubit systems, which is by now the standard method. In particular, I suggested to drop altogether the inequality in (previous) Eqs. (5) and (6), and the basis-dependent discussion and figures, by replacing all of them with (previous) Eq.(15) in appendix and to provide only this test of Bell inequality violation. None of these last suggestions have been implemented in the new version of the manuscript.

Moreover, the main claim that their method to test Bell inequality violation based on the CH inequality is better than the other method (Q_max) is not true. All the other methods analyzed here follow from the CH criterion and inherit all the advantages, therefore, there is no need to provide unnecessary information based on less efficient analyses of Bell inequality tests.

Answer: We don't think so. The test of CHSH inequality depends on the Λ decay parameters, which is determined in quantum mechanics scenario. This implies that this test brings about a new loophole. While the test of CH inequality is independent on the

parameter α_Λ . We think this is better than CHSH inequality in the concerned process of LHVT test.

2) In the meantime a new published work on the Bell inequality violation in charmonium decays appeared (Phys.Rev.D 110 (2024) 5, 053008), that is using the method based on helicity amplitudes to reconstruct the density matrix and evaluate the Q_{max} . This work can provide a new guideline for testing the Bell inequality in the present experimental analysis. In this respect, it would be very useful to also provide the values of the helicity amplitudes with associated errors, that can allow independent evaluations of Q_{max} by following this approach.

Moreover, the fact that the Q_{max} is close to its maximum value is purely due to kinematic, see reference above for more details. Then, to evaluate the violation of Bell's inequality at the experimental level for the charmonium decay into $\Lambda\bar{\Lambda}$ system, the key information required is basically the assessment of the errors associated with the corresponding helicity amplitudes. These errors can be directly translated into the Q_{max} error, which eventually determines the strength of significance to rule out the null hypothesis for the Bell inequality violation.

Answer: We have noted that this method already has a certain research foundation. In this work, we have also employed the Q_{max} method for our research. We plan to use this method for a more in-depth analysis of the BESIII data in the future.

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3) I noticed that the new references have been added in the revised version, but they are cited collectively without clarifying their relevance to the specific context. For instance, the fact that Ref.[35] was the first to show the violation of Bell inequality at high energies and in the presence of weak and strong interaction, has not been addressed.

Answer: In addition to Ref.[35], Ref.[25] has also conducted related research. In our paper, we have cited these references as supplementary material.

In conclusion, the revisions in this new version are very minimal and have not significantly altered the previous version, leaving the most important changes I recommended in my earlier report unaddressed. Therefore, I am unable to recommend this paper for publication in the present form.

REVIEWER COMMENTS

Reviewer #3 (Remarks to the Author):

Report on the paper “THE VERIFICATION OF QUANTUM NON-LOCAL CORRELATION IN Λ ANTI- Λ SYSTEM”

[previous journal mention redacted]

I have reviewed the new version of the work and find that all criticisms were addressed and the corresponding modifications to the text were implemented. The changes improved the quality and the added discussion resulted in a clearer presentation of the results. Important references that were missing were also included. Some sentences that overstated the findings of the work, that presented it as the first report of this nature using measurements with massive particles, were removed. I therefore recommend publication of this work in Nature Communications.

Answer: We appreciate your affirmation of the new version of our work. We have made modifications based on previous criticisms and suggestions, and we believe these improvements have enhanced the quality and clarity of our presentation. We will continue to strive to ensure that our research results are accurately and clearly presented. Thanks.

REVIEWERS' COMMENTS

Reviewer #1 (Remarks to the Author):

I appreciate the authors to have addressed the major comments to the previous manuscript, and am happy to recommend the acceptance.

However, the new manuscripts still contain incorrect or misleading descriptions (mainly in the introductory paragraphs) as pointed as below. These should be fixed before or during the proof reading stage. Also, I hope a more careful review and scrutinization would have been made inside the collaboration before the journal submission. It is like the process is outsourced to the peer reviewers, which made us either to sign-off an incomplete paper, or to devote a considerable amount of time to check in detail. Particularly, there were quite a few false claims and questionable citations initially that I had to go through the references to validate, which should be rather the responsibility of the co-authors.

Alas, congratulation to the successful results achieved in the paper.

Our response: Thank you very much for your patience and the time you dedicated to reviewing our paper. Your comments and critiques are really important us.

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Referee wrote:

- There seems a significant confusion throughout the introduction between the “guide axes” (arbitrary axes chosen by the analyzer, such as the unit vectors denoted by a,b,c,d in Eq.(4)) and the actual proton/anti-proton orientations, both represented by the same symbols (n , n' , n_1 , n'_2 , $\theta_{p\bar{p}}$ etc.) e.g.
- (A) n_1 , n_2 , $\theta_{p\bar{p}}$ in Eq (1)(3): guide axes
- (B) n_1 , n_2 , $\theta_{p\bar{p}}$ in Eq (2): proton (anti-proton) momentum in $\Lambda(\bar{\Lambda})$'s rest frame
- (C) n_a , n'_b , n_c , n'_d in Eq (4): proton (anti-proton) momentum in $\Lambda(\bar{\Lambda})$'s rest frame projected onto the guide axes n_a , n_b , n_c , n_d (defined in Ref 43).
- (D) n_1 , n_2 , n'_1 , n'_2 , θ in Eq.(7)-(9): guide axes (this is why you are allowed to fix θ to certain values in L142/L148 otherwise it does not make sense).

(A) is explained wrongly in the text; (C) is explained incompletely; (D) the definition in L122-123 is extremely vague. In case you are unsure, you may compactify the description and cite Ref 25 more. In fact, the discussion in L69-L149 is more likely the scope of Ref 25.

Our response: Thank you very much for pointing out the unclear explanation in our manuscript. We have updated the manuscript based on your suggestions.

For A and B. We cite formalism Eqs. (1), (2) and (3) from Ref. [38], and follow the definitions of n_1 , n_2 and $\theta_{p\bar{p}}$. To response this comment, we modified the sentence as: (Line82-86)

“where the operator $\sigma \cdot \vec{n}_1(\sigma \cdot \vec{n}_2)$ represents the measurement of $\Lambda(\bar{\Lambda})$ spin projection along the guide axis $n_1(n_2)$, which coincides with the proton (antiproton) momentum direction in the

$\Lambda(\bar{\Lambda})$ rest frame. This direction is obtained by boosting the proton(antiproton) momentum to the $\Lambda(\bar{\Lambda})$ center-of-mass system. Here θ_{pp} is the opening angle between $\vec{n}_1$ and $\vec{n}_2$ with their reference frames superimposed.”

For C. We add the definition of momenta projections (moved to supplementary material in this version) as: (Line26-27)

“The guide directions are denoted by a, b, c and d , and momentum projections are defined as $\langle n_a n_b' \rangle = \langle n \cdot a \cdot n' \cdot d \rangle$, for example.”

For D. A better understanding of $n_1(n_2)$ and $n'_1(n'_2)$ in Eq.(4) in revised manuscript is via comparison of EPR test in optical experiments. To maintain consistency throughout the paper, we have changed these letters to n_a, n_b, n_c and n_d , and we modify the sentence as: (Line112-116)

“Here unit vectors $\vec{n}_{a,c}$ and $\vec{n}'_{b,d}$ denote the directions of chosen guide axes used to detect proton and antiproton, respectively. This can be viewed as a generalization of polarizer settings in optical experiments. $P(\vec{n}, \vec{n}') = \frac{1}{4}(1 + \alpha_\Lambda^2 \vec{n} \cdot \vec{n}')$ represents the probability to detect a event of proton and antiproton at direction $\vec{n}$ and $\vec{n}'$ respectively, coinciding with a $\eta_c \rightarrow \Lambda(p\pi^-)\bar{\Lambda}(\bar{p}\pi^+)$ decay. While $P(\vec{n}_c) = P(\vec{n}_b') = \frac{1}{2}$ indicates the probability to detect proton or antiproton alone.”

- L84: I think the value of alpha is determined by measurement but not from theoretical prediction (at least the quoted value is from the measurement).

Our response: Thanks, it is indeed cited from the measurement and sorry for the confusion. This sentence has been modified as: (Line88)

“where $\alpha = \alpha_\Lambda = 0.750 \pm 0.009 \pm 0.004$ is the asymmetry parameter of the $\Lambda \rightarrow p\pi^-$ decay,”

- L117-118: Likewise, not sure this claim is valid.

Our response: Similarly, this sentence is changed to be: (Line108)

“It should be noted that in previous scheme the $\Lambda(\bar{\Lambda})$ decay parameter $\alpha_\Lambda(\alpha_{\bar{\Lambda}})$ is introduced in the test of Bell inequality, which will bring about a new loophole.”

- L98: I am not sure Ref. 15 really addresses the argument described in L93-L97. The claim of the loophole is “the decays cannot be guaranteed to be completely random without the active control of measurements” at the first place. I do not think the three sentences in Ref 15 sufficiently resolves the concern.

Our response: Yes, you are right. We agree with you suggestion. We have revised the sentence as follows. (Line101-105)

“Although Ref.[15] suggests that measurements of Λ and $\bar{\Lambda}$ decays in the detector could serve as an ideal random generator, the freedom-of-choice loophole remains a significant challenge in high-energy experiments.”

- L100: “the sample survived from the free-will choice is a fair sampling of detected events”: I think this is the description to address the fair-sampling loophole, which has nothing to do with the freedom-of-choice loophole.

Our response: To clarify this confusion, we revise sentence as: (Line101-105)

“Nevertheless, we could introduce a weak assumption that the sample survived from the free-will choice should have the same distribution as the detected sample.”

- L104-116: “A refined...” Since there is no description on how/where this refined, it rather sounds like it addressed the freedom-of-choice loophole by the context from the previous paragraph, which is misleading. Also this part is no longer connected to other parts of the main body of text. It looks this can go to Appendix with the test result entirely.

Our response: Thanks for your suggestion. We move this part to appendix.

- L119: “a new LHVT”→“a new inequality”?

Our response: Thanks, it has been modified. (Line110)

- L119: “namely the CH inequality” → “a CH-type inequality” (the CH inequality exists from before)

Our response: Thanks, it has been modified. (Line110)

- L124: I wonder if you really split the events into four groups; you may just calculate P with different guide axes using the same events?

Our response: No, we test the reduced CH equality as given by Eq.(6) in the revised manuscript. The old description is try to help reader to understand Eq(4) [old Eq (7)] in a simple pictures. To respond comment, we change these sentences as, (Line114-116)

“ $P(\vec{n}, \vec{n}') = \frac{1}{4}(1 + \alpha_A^2 \vec{n} \cdot \vec{n}')$ represents the probability to detect a event of proton and antiproton at direction $\vec{n}$ and $\vec{n}'$ respectively, coinciding with a $\eta_c \rightarrow \Lambda(p\pi^-)\bar{\Lambda}(\bar{p}\pi^+)$ decay. While $P(\vec{n}_c) = P(\vec{n}_b) = \frac{1}{2}$ indicates the probability to detect proton or antiproton alone.”

- Eq.(7), L135: You post-select the space-like events in the analysis (Eq.(10)) so the penalty factor beta should not exist in the inequality anymore.

Our response: The space-like condition is necessary to close the locality loophole in the Bell inequality tests. And Eq.4 (old Eq.7) is the consequence of space-like requirement. Actually, this factor reduces the LHVT bound, and so increase the sensitivity to reject LHVT. It serves as a compensation factor to account for the low efficiency of space-like events