nature portfolio

Peer Review File

Measuring topological invariants for higher-order exceptional points in quantum three-mode systems

Corresponding Author: Professor Shibiao Zheng

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 characterized, both theoretically and experimentally, the exceptional points of a three-level quantum system. The main result of the article is the experimental extraction of the winding number as an example of a topological invariant. Although high-order exceptional points have already been investigated in superconducting circuits, the paper shows important progress in exceptional-point engineering, especially concerning the experimental design and extraction of eigenenergies. However, there are a few aspects that should be addressed before publication, as detailed below.

In general, one could benefit from a more in-depth explanation of potential applications of the results, especially in the conclusion. The claim that the paper explores a fully quantum-mechanical model also needs to be amended since the effects of quantum jumps are neglected [see, e.g., Phys. Rev. A 100, 062131 (2019)]. Moreover, the authors should elaborate on the physical meaning of some of their findings. For example, using terms like ‘frozen evolution’ may lead to incorrect interpretations. Namely, the dissipation in their model tends to produce asymptotic ground state population. The freezing of the dynamics seems to be a consequence of discarding the ground state population and normalizing the populations in the single-excitation subspace.

Please find below other specific points that could help improve the paper:

1. In the theoretical description, the authors place the most dissipative system on the leftmost side of the chain, using indices in ascending order from left to right in the diagram of Fig. 1a. However, when discussing the experimental setup, the most dissipative system is Rr, the states of which are assigned to the rightmost position. I strongly suggest having a consistent qubit ordering throughout the main text/SM to avoid confusion.

2. Although the topology of the NH Hamiltonian is an important fingerprint of the system, could the authors discuss its dynamics in particular regions of the parameter space, such as the isofrequency region and Fermi-arcs depicted in Figs. 1b and 1c? Are there regions in the parameter space that are more favorable for specific tasks, such as reset, preservation, or stabilization of tripartite entangled states? See, e.g., Phys. Rev. Research 5, 033119 (2023) for related discussion.

3. In Eq. (2), the authors could highlight that the coefficients are obtained upon discarding the ground state population. Otherwise, this state is not normalized since the ground state population approaches unity in the asymptotic behavior. Broadly speaking, could the authors explain if such a decaying behavior limits the applicability of exceptional points in superconducting circuits since such systems are generally lossy?

4. The non-reciprocity is discussed in Fig. 3, but the motivation behind the choice of parameters could be clarified. With this choice, the asymmetric excitation transfer is somehow expected since Rb is weakly coupled to its environment and to the rest of the chain.

5. In Fig. 3b, the experimental data deviates drastically from the theoretical curves at long times. Is this purely a consequence of disregarding the ground state population, or could it also indicate that the single-excitation model breaks down in the asymptotic limit? Showing the data for the ground state population could help to clarify this point.

6. The values of \lambda_j (or at least their range) could be added to the captions of Figs. S4, S6, and S9.

Reviewer #2

(Remarks to the Author)
In this article, Han et al. consider three coupled superconducting cavities, one very nonlinear being approximated by a qubit, and two linear ones. Initializing the system in the subspace with one excitation, they analyze how the post-selected no-jump dynamics leads to the emergence of EPs of high-order, understood in terms of a non-Hermitian Hamiltonian. They extract the spectral features of the system, and they show that, encircling the EPs, a global phase is acquired, resulting in a nonzero winding number. Finally, they reconstruct the state along the dynamics, showing how the post-selection procedure allows building up quantum coherences in the form of entanglement.

The paper is interesting, but we find some important issues that we strongly suggest the authors to address below.
Upon their resolution, we believe the paper to meet the standard for Nature Communications.

Major remarks:
1) All through the paper, the referee call their model a "three qubit model." Although the NHH is identical to that of a three qubit model in the subspace considered, the model does not have the same input-output properties (the response of the system is different). In other words, it would be confusing to call a linear cavity with just one excitation a qubit. I would be more precise in specifying that the authors do not implement a three qubit model, but rather that they have a single-particle systems on three modes.
2) "The system under consideration corresponds to a Heisenberg qubit chain", the Eq. (1) and the caption of Fig. (1) are slightly misleading. This is a three mode system, and it should be called that. Obviously this can be seen as the unit cell of a larger Heisenberg chain, but I think this should be a marginal remark and not the way equations and figures are introduced. Also, the authors introduce several dissipators, and then say "For simplicity, we assume $\kappa 2 = \kappa 3 = 0$, and drop off the subscript of $\kappa 1$." At this point, I would simply the model and introduce it with the minimal amount of parameters without claiming it is a chain.
3) When discussing about the Winding number, and its generalization to EP3, I am slightly confused by the mathematical construction. Normally, winding numbers discriminate the topological properties of a system using quantities based on the change of the wave function along a closed path. Here, instead, the authors consider a winding number simply based on the difference of energies. If I were to consider a diabolical point of a non-interacting system rather than an exceptional one, I am unsure this quantity observes any difference. Could the author show how the invariant changes for a simple diabolical point?
4) In Fig 3, by non considering decay, it's hard to appreciate what is really happening before post selection. I would suggest the author to put a figure without postselection in the supplementary. Furthermore, in panel (b) the data are absolutely not confirming the predicted model at long time as the noise is too large. As such, I am rather skeptical that this plot allows claiming unidirectionality or tripartite entanglement (figure S12) through measurement reconstruction.

Minor remarks:
1) The authors write "The Hermiticity of the Hamiltonian ensures that the probability is conserved during the system's quantum state evolution." This is true, but also any CPTP map does that. Furthermore the authors say: "Under this disturbance, the system's dynamics could significantly deviate from the unitary evolution even when it does not exchange any energy with the environment. Mathematically, the state trajectory, associated with this conditional evolution, is also governed by a Schrödinger equation but with a non- Hermitian (NH) Hamiltonian." Again, this statement is true if one has in mind that what the authors are considering is a jumpless trajectory. I would advise to be more clear in both these statements to avoid any confusion. I would also introduce the topic of EPs in quantum systems in the presence of dissipation, both in their Liouvillian and post-selected description.
2) "More importantly, the observed topology is underlain by quantum-mechanically entangled eigenstates." This is unclear.
3) "We further observe a counter-intuitive NH phenomenon, where a photon is unidirectionally localized to the lossless resonator through a nonlocal dynamics."
This phenomenon has been observed and discussed in some previous paper: the fact of not emitting a particle leads to a location on the least emitting system. This is a general feature of post-selected systems, not tied to EPs. For instance, see for similar examples:
-- J. Opt. Soc. Am. B 10, 524 (1993).
-- Phys. Rev. Lett. 112, 170501 (2014)
-- Phys. Rev. X 6, 041052 (2016)
-- Phys. Rev. A 103, 052201 (2021)
4) "with the experimental pulse depicted in Supplemental Material." The pulse scheme shown in the supplementary Fig. 3 can be more detailed. I suggest to specify what all lines do, namely XY, Z, and readout.
5) The paragraph starting with "To reveal the nonclassicality of the eigenstates underling the eigenspectra, we perform quantum state tomography on the output state of the system." is rather unclear.
6) FIG. S2: is a micrograph, not a diagram.Furthermore, for completeness, add Q1 and Q2.
7) The fast oscillation in the Hamiltonian reconstruction of the data are fairly difficult to see. I would rather suggest to reduce the line-with to make them more visible, or simply shade the area corresponding to the oscillation amplitude and correspondingly specify it in the text.
8) Finally, we advise a revision of the language through the paper. For instance, "a huge number of experimental test", " "Hamiltonian" " (why use the "" symbol?), "interacts more or less with the environment" just in the first paragraph.

Reviewer #3

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The authors have addressed my comments satisfactorily. Overall, the revisions have improved the manuscript, making it suitable for publication in Nature Communications.

Reviewer #2

(Remarks to the Author)
I have read with interest the reply by the authors, and I can say that they have replied to the largest portion of my concerns. Although the discussion on non-reciprocity was not essential, its complete removal seems kind of drastic in my opinion, and slightly undermines the overall span of the message.
Again, as I previously wrote, the unidirectionality is not essential to the discussion of EPs, but my previous comment was more concerning perspectives and the data actually shown than the overall importance of the finding.

That being said, the previous point 3) raised by myself and Reviewer 3 remains, in my opinion, not completely discussed. What we mean is the following.
Consider the model
$$\hat{H} = \omega_x \hat{\sigma}_x + \omega_y \hat{\sigma}_y.$$
This model admits two real eigenenergies, and one could study the diabolical point at $\omega_x = \omega_y =0$. If I were to apply the winding number proposed by the authors, what result would I get? In other words, would the winding number be able to discriminate between an EP and a DP? This discussion is, in my opinion, essential, as the authors use the winding number as a proxy to quantify the degree of degeneracy of the EP using experimental measurements. If this was to be the case, the article would still be interesting, but it needs to be clarified in the article, especially in the view of possible future follow-ups using this paper as a guide.

Upon clarification of this point and demonstration that indeed the article measure features associated with higher-dengerated EPs, I believe this paper reaches the standard of Nature Communications.

Reviewer #3

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Version 2:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
The authors have replied brilliantly to my previous remark, and have provided convincing evidence that the phenomenon observed is indeed associated with an EP and not a DP. Given the overall quality of the paper, the improvements made by the authors, and having clarified some previous issues, I am now confident that this paper meets the criteria for publication in Nature Communications.

Reviewer #3

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Dear Editor,

First of all, we would like to sincerely thank the referees for their positive assessments and professional comments, which have greatly helped to improve the quality of the manuscript. Below are our responses to the issues raised by the referees, where the comments are reproduced in blue italic and our responses are given afterwards in black. We hope that each issue has been addressed to their satisfaction.

Thank you very much for your time and kind assistance.

Sincerely yours,

the authors

To Reviewer #1

The authors characterized, both theoretically and experimentally, the exceptional points of a three-level quantum system. The main result of the article is the experimental extraction of the winding number as an example of a topological invariant. Although high-order exceptional points have already been investigated in superconducting circuits, the paper shows important progress in exceptional-point engineering, especially concerning the experimental design and extraction of eigenenergies. However, there are a few aspects that should be addressed before publication, as detailed below.

Response: We are grateful to the referee for these professional and positive comments. Below we will address the issues raised by the referee point by point.

In general, one could benefit from a more in-depth explanation of potential applications of the results, especially in the conclusion. The claim that the paper explores a fully quantum-mechanical model also needs to be amended since the effects of quantum jumps are neglected [see, e.g., Phys. Rev. A 100, 062131 (2019)]. Moreover, the authors should elaborate on the physical meaning of some of their findings. For example, using terms like 'frozen evolution' may lead to incorrect interpretations. Namely, the dissipation in their model tends to produce asymptotic ground state population. The freezing of the dynamics seems to be a consequence of discarding the ground state population and normalizing the populations in the single-excitation subspace.

Response: We thank the referee for these important suggestions. To highlight the potential applications of the results, we have added the sentence at the end of the conclusion part "Besides fundamental interest, the demonstrated NH dynamics associated with higher order EPs may have applications in quantum technologies, such as sensitivity enhancement in quantum metrology [Nature (London) 548, 192] as well as fast and robust generation of quantum entanglement [Phys. Rev. Lett. 131, 100202]."

To make the presentation more accurate, following the first sentence in the third paragraph, we have added the sentence "These EPs result from continuous and nonunitary evolutions in the single-excitation subspace without quantum jumps [Phys. Rev. A 100, 062131 (2019)] ."

The description of the frozen evolution appears in the section of the non-reciprocity. As the referees pointed out, this section is irrelevant to the characterization of the topological features of higher-order EPs, and the corresponding experimental results are not in well agreement with the theoretical predictions. For these reasons, we have removed this section from the text.

Please find below other specific points that could help improve the paper:
1. In the theoretical description, the authors place the most dissipative system on the leftmost side of the chain, using indices in ascending order from left to right in the diagram of Fig. 1a. However, when discussing the experimental setup, the most dissipative system is Rr, the states of which are assigned to the rightmost position. I strongly suggest having a consistent qubit ordering throughout the main text/SM to avoid confusion.

Response: We thank the referee for this suggestion. Following the suggestion, we have arranged R_r on the leftmost side in the sketch of the experimental system.

2. Although the topology of the NH Hamiltonian is an important fingerprint of the system, could the authors discuss its dynamics in particular regions of the parameter space, such as the isofrequency region and Fermi-arcs depicted in Figs. 1b and 1c? Are there regions in the parameter space that are more favorable for specific tasks, such as reset, preservation, or stabilization of tripartite entangled states? See, e.g., Phys. Rev. Research 5, 033119 (2023) for related discussion.

Response: We thank the referee for this professional, insightful, and helpful suggestion. Following the suggestion, before "In addition to the real Fermi arc" we have added the sentences: "In the isofrequency region, the real parts of the three complex eigenenergies vanish, so that the state evolution of the system is determined by the imaginary parts of these eigenenergies, each of which corresponds to a gaining or losing rate depending upon its sign. After a long-time evolution, only the eigenvector with the largest imaginary part can survive for the no-jump case. Since such an eigenvector is essentially a tripartite entangled state, the dynamics in the isofrequency region provides a way for robust generation and conditional stabilization of tripartite entanglement [Phys. Rev. Research 5, 033119]. Such an entanglement generation and stabilization process is illustrated by the simulations presented in Sec. S2 of the Supplemental Material."

Meanwhile, we have added a section in the Supplemental Material (S2), entitled "Simulation of conditional dynamics in the isofrequency region". In this section, we present the evolutions of fidelities and pairwise concurrences, as well as the probability evolution for the no-jump trajectory in this region.

3. In Eq. (2), the authors could highlight that the coefficients are obtained upon discarding the ground state population. Otherwise, this state is not normalized since the ground state population approaches unity in the asymptotic behavior. Broadly speaking, could the authors explain if such a decaying behavior limits the applicability of exceptional points in superconducting circuits since such systems are generally lossy?

Response: We thank the referee for this comment. To clarify the conditional characteristic of the state evolution of the system, before Eq. (2), we have changed "The system ends in the state" to "The output state of the system, associated with the no-jump trajectory, is given by". To highlight the unique feature of the present system, before "The joint tripartite output state is read out", we have added the sentence "As the excitation is conserved by the NH Hamiltonian while the quantum jump breaks down this conservation, the no-jump state trajectory can be postselected by discarding the outcome with null excitation." We note that for the no-jump case the system does not make evolution at the EP, where all the eigenstates pick up a trivial common factor during the evolution. This implies that the NH Hamiltonian dynamics at the EP cannot be observed in experiment. However, measurement of the winding number associated with the EP does not require observation of the dynamics at the EP. This topological invariant was extracted from the eigenenergies measured along a loop encircling the EP.

4. The non-reciprocity is discussed in Fig. 3, but the motivation behind the choice of parameters could be clarified. With this choice, the asymmetric excitation transfer is somehow expected since Rb is weakly coupled to its environment and to the rest of the chain.

Response: We appreciate this professional comment. After carefully reading the comment from the referee regarding the non-reciprocity, we agree that the asymmetric excitation transfer is not very novel and the experimental data do not well confirm the theoretical prediction due to the thermal noises. Furthermore, we realize that the non-reciprocal phenomenon is irrelevant to the main result of the manuscript, namely the measurement of the topological invariant associated to higher-order EPs.

As such, we have removed the part of non-reciprocity, including the second, third, and fourth paragraphs from the bottom on pages 4-5 **in the original version, and Fig. 3 in the original version,** which does not affect the importance, novelty, and integrity of the work in any way.

Meanwhile, **we have moved Fig. S3 from the Supplemental Material to the main text (Fig. 2),** which would help the readers better understand the experimental system. **Accordingly, Fig. 2 in the original version is renumbered as Fig. 3 in the present version**.

5. In Fig. 3b, the experimental data deviates drastically from the theoretical curves at long times. Is this purely a consequence of disregarding the ground state population, or could it also indicate that the single-excitation model breaks down in the asymptotic limit? Showing the data for the ground state population could help to clarify this point.

Response: We thank the referee for this comment. The drastic deviations of the experimental data in Fig. 3b from the theoretical results are due to the following reasons. On one hand, due to the dissipation of R_r, the photon initially stored in R_r decays quickly. On the other hand, the thermal excitation becomes severe with the increasing of the time. Consequently, the measured oscillation signals are seriously contaminated by the thermal noises after a long-time evolution.

As interpreted above, we have removed the part of non-reciprocity, including the second, third, and fourth paragraphs from the bottom on pages 4-5 in the original version, and Fig. 3 in the original version, which does not affect the importance, novelty, and integrity of the work in any way.

6. The values of \lambda_j (or at least their range) could be added to the captions of Figs. S4, S6, and S9.

Response: We thank the referee for this suggestion, following which we have given the values λ_j in the captions of Figs. S4, S6, and S10 .

To Reviewer #2

In this article, Han et al. consider three coupled superconducting cavities, one very nonlinear being approximated by a qubit, and two linear ones. Initializing the system in the subspace with one excitation, they analyze how the post-selected no-jump dynamics leads to the emergence of EPs of high-order, understood in terms of a non-Hermitian Hamiltonian. They extract the spectral features of the system, and they show that, encircling the EPs, a global phase is acquired, resulting in a nonzero winding number. Finally, they reconstruct the state along the dynamics, showing how the post-selection procedure allows building up quantum coherences in the form of entanglement.

The paper is interesting, but we find some important issues that we strongly suggest the authors to address below.

Upon their resolution, we believe the paper to meet the standard for Nature Communications.

Response: We are grateful to the referees for these professional and positive comments. Below we will address the issues raised by the referee point by point.

Major remarks:

1) All through the paper, the referee call their model a "three qubit model." Although the NHH is identical to that of a three qubit model in the subspace considered, the model does not have the same input-output properties (the response of the system is different). In other words, it would be confusing to call a linear cavity with just one excitation a qubit. I would be more precise in specifying that the authors do not implement a three qubit model, but rather that they have a single-particle systems on three modes.

Response: We appreciate this comment. To make the presentation more accurate, we have changed "three-qubit model" to "three-mode system" throughout the manuscript. Meanwhile, we have changed the sentences "In the subspace spanned by the zero- and one-photon states, each of these resonators serves as a photonic qubit, and the R_b-Q-R_r system is equivalent to a three-qubit model. The system dynamics, associated with no-jump trajectory, is described by the NH Hamiltonian (1) with $N = 3$." to "When the R_r-Q-R_b system initially has one excitation, its dynamics, associated with no-jump trajectory, is described by the NH Hamiltonian (1) with $N = 3$, where R_r, Q, and R_b correspond to three modes sharing a single excitation."

2) "The system under consideration corresponds to a Heisenberg qubit chain", the Eq. (1) and the caption of Fig. (1) are slightly misleading. This is a three mode system, and it should be called that. Obviously this can be seen as the unit cell of a larger Heisenberg chain, but I think this should be a marginal remark and not the way equations and figures are introduced. Also, the authors introduce several dissipators, and then say "For simplicity, we assume κ2 = κ3 = 0, and drop off the subscript of κ1." At this point, I would simply the model and introduce it with the minimal amount of parameters without claiming it is a chain.

Response: We thank the referee for these comments. To make the presentation more accurate, we have rephrased the fourth paragraph on page 2 in the original version as follows: The system under consideration corresponds to a multi-mode system with competing coherent nearest-neighboring interactions and incoherent dissipation, as shown in Fig. 1a. Under the competition, the system evolution is a weighted mixture of infinitely many trajectories, among which of special interest is the one without quantum jump. This trajectory is governed by the NH Hermiltonian

$$H_{NH} = -\frac{1}{2} i \sum_{j=1}^{N} \kappa_j a_j^\dagger a_j + \sum_{j=1}^{N-1} \lambda_j \left(a_j^\dagger a_{j+1} + \mathrm{H.c.}\right),$$

where $a_j^\dagger$ and a_j denote the creation and annihilation operators of the jth mode with a decaying rate κ_j, and λ_j represents the coupling coefficient between the jth and (j+1)th modes, and H.c. is the Hermitian conjugation. The excitation number of the total system is conserved along the no-jump trajectory, as the associated operator, $N_e = \sum_{j=1}^{N} a_j^\dagger a_j$, commutes with H_{NH}. Hereafter, we will consider the system behaviors restricted in the single-excitation subspace $\{|1_1 0_2 \ldots 0_N\rangle, \ldots, |0_1 \ldots 0_{N-1} 1_N\rangle\}$, where $|0_j\rangle$ and $|1_j\rangle$ respectively denote the ground and first excited states for the jth mode with j ranging from 1 to N.

In the caption of Fig. 1, we have changed "Sketch of the NH qubit-spin chain and Spectral structure. (a) Theoretical model. The system involves N qubits (Q_j), each serving as a pseudo-spin." to "Sketch of the NH multimode system and the spectral structure. (a) Theoretical model. The system involves N modes (R_j), arranged in a linear array." Meanwhile, we have changed "Q" to "R" in Fig. 1a.

3) When discussing about the Winding number, and its generalization to EP3, I am slightly confused by the mathematical construction. Normally, winding numbers discriminate the topological properties of a system using quantities based on the change of the wave function along a closed path. Here, instead, the authors consider a winding number simply based on the difference of energies. If I were to consider a diabolical point of a non-interacting system rather than an exceptional one, I am unsure this quantity observes any difference. Could the author show how the invariant changes for a simple diabolical point?

Response: We thank the referee for these comments. For a Hermitian quantum system, the topological feature is quantified by the integral of the Berry connection, defined in terms of the wave function, along a closed loop. As the eigenenergies of a Hermitian system are always real, the winding number of diabolical point cannot be defined in terms of the eigenenergies. In distinct contrast, the topology for a NH system can be manifested either by the wave function or by the complex eigenenergies. To clarify this point, in the second paragraph on page 1 of the revised manuscript, we have changed the sentence "The topology of an NH system can be quantified by some topological invariants, e.g., the winding number [8–12]" to "The topological invariant of an NH system can be quantified in terms of the eigenvectors or the complex eigenenergies [8-12]. This is in distinct contrast with the Hermitian case, where the topology cannot be defined by the eigenenergies, which are always real. "

4) In Fig 3, by non considering decay, it's hard to appreciate what is really happening before post selection. I would suggest the author to put a figure without postselection in the supplementary. Furthermore, in panel (b) the data are absolutely not confirming the predicted model at long time as the noise is too large. As such, I am rather skeptical that this plot allows claiming unidirectionality or tripartite entanglement (figure S12) through measurement reconstruction.

Response: After carefully reading the comments of the referees regarding the non-reciprocity, we agree that the asymmetric excitation transfer is not very novel and the experimental data do not well confirm the theoretical prediction due to the thermal noises. Furthermore, we realize that the non-reciprocal phenomenon is irrelevant to the main result of the manuscript, namely the measurement of the topological invariant associated to higher-order EPs.

As such, we have removed the part of non-reciprocity, including the second, third, and fourth paragraphs from the bottom on pages 4-5 in the original version, and Fig. 3 in the original version, which does not affect the importance, novelty, and integrity of the work in any way.

Meanwhile, **we have moved Fig. S3 from the Supplemental Material to the main text (Fig. 2),** which would help the readers better understand the experimental system. **Accordingly, Fig. 2 in the the original version is renumbered as Fig. 3 in the present version**.

Minor remarks:

1) The authors write "The Hermiticity of the Hamiltonian ensures that the probability is conserved during the system's quantum state evolution." This is true, but also any CPTP map does that. Furthermore the authors say: "Under this disturbance, the system's dynamics could *significantly deviate from the unitary evolution even when it does not exchange any energy with the environment. Mathematically, the state trajectory, associated with this conditional evolution, is also governed by a Schrödinger equation but with a non- Hermitian (NH) Hamiltonian." Again, this statement is true if one has in mind that what the authors are considering is a jumpless trajectory. I would advise to be more clear in both these statements to avoid any confusion. I would also introduce the topic of EPs in quantum systems in the presence of dissipation, both in their Liouvillian and post-selected description.*

Response: We thank the referee for these comments. To be more accurate, we have changed the sentence "The Hermiticity of the Hamiltonian ensures that the probability is conserved during the system's quantum state evolution" to "The Hermiticity of the Hamiltonian ensures that the system evolves unitarily". In addition, we have changed the sentence "Under this disturbance, the system's dynamics could significantly deviate from the unitary evolution even when it does not exchange any energy with the environment" to "Under this disturbance, the system dynamics could significantly deviate from the unitary evolution even when it does not make any quantum jump".

2) "More importantly, the observed topology is underlain by quantum-mechanically entangled eigenstates." This is unclear.

Response: We have changed this sentence to "The system eigenenergies are extracted from the output states of the three-mode system in the single-excitation subspace, measured for different evolution times."

3) "We further observe a counter-intuitive NH phenomenon, where a photon is unidirectionally localized to the lossless resonator through a nonlocal dynamics."

This phenomenon has been observed and discussed in some previous paper: the fact of not emitting a particle leads to a location on the least emitting system. This is a general feature of post-selected systems, not tied to EPs. For instance, see for similar examples:

-- J. Opt. Soc. Am. B 10, 524 (1993).

-- Phys. Rev. Lett. 112, 170501 (2014)

-- Phys. Rev. X 6, 041052 (2016)

-- Phys. Rev. A 103, 052201 (2021)

Response: We agree that non-reciprocal phenomenon is not important or novel. So we have removed the part contributed to description and demonstration of such a phenomenon from the manuscript.

4) "with the experimental pulse depicted in Supplemental Material." The pulse scheme shown in the supplementary Fig. 3 can be more detailed. I suggest to specify what all lines do, namely XY, Z, and readout.

Response: Following referee's suggestion, we have specified the functions of all lines. Meanwhile, **we have moved Fig. S3 from the Supplemental Material to the main text (Fig. 2),** which would help the readers better understand the experimental system. **Accordingly, Fig. 2 in the original version is renumbered as Fig. 3 in the present version**.

In the caption of this figure, we have changed "Synthesis of the NH three-qubit model. (a) Experimental implementation." to "Experimental implementation. (a) Synthesis of the NH three-mode system."

5) The paragraph starting with "To reveal the nonclassicality of the eigenstates underling the eigenspectra, we perform quantum state tomography on the output state of the system." is rather unclear.

Response: To make the presentation more clear, we have changed the sentence "To reveal the nonclassicality of the eigenstates underling the eigenspectra, we perform quantum state tomography on the output state of the system" to "To reveal the quantum correlations among the three modes, we perform quantum state tomography on the output state of the system".

6) FIG. S2: is a micrograph, not a diagram. Furthermore, for completeness, add Q1 and Q2.

Response: We have changed "diagram" to "micrograph" in the caption of Fig. S2, and added Q1 and Q2.

7) The fast oscillation in the Hamiltonian reconstruction of the data are fairly difficult to see. I would rather suggest to reduce the line-with to make them more visible, or simply shade the area corresponding to the oscillation amplitude and correspondingly specify it in the text.

Response: As interpreted in detail above, we have removed Fig.3 of the original version, where the data exhibit the fast fluctuations due to thermal noises.

8) Finally, we advise a revision of the language through the paper. For instance, "a huge number of experimental test", " "Hamiltonian" " (why use the "" symbol?), "interacts more or less with the environment" just in the first paragraph.

Response: We have tried our best to improve English. We have changed "a huge number of experimental tests" to "numerous experimental tests", changed " "Hamiltonian" " to "Hamiltonian", and changed "interacts more or less with the environment" to "is inevitably coupled to the environment".

Dear Editor,

First, we would like to thank sincerely both referees for their comments. We are grateful to Reviewer #1 for her/his recommendation of our manuscript for publication; we also appreciate the very valuable comments from Reviewer #2, which are helpful for making the conclusion of our manuscript more convincing.
Below are our responses to the issues raised by Reviewer #2, where the comments are reproduced in blue italic and our responses are given afterwards in black. We hope that each issue has been addressed to her/his satisfaction.

To Reviewer #2

I have read with interest the reply by the authors, and I can say that they have replied to the largest portion of my concerns.

Response: We thank the referee for acknowledging that we have replied to the largest portion of his/her concerns.

*Although the discussion on non-reciprocity was not essential, its complete removal seems kind of drastic in my opinion, and slightly undermines the overall span of the message.
Again, as I previously wrote, the unidirectionality is not essential to the discussion of EPs, but my previous comment was more concerning perspectives and the data actually shown than the overall importance of the finding.*

Response: Indeed, the non-reciprocity is irrelevant to the discussion of EPs. Moreover, the corresponding experimental data are inconsistent with the theoretical prediction after a long time evolution, as the referees pointed out. For these reasons, we remove the discussion on non-reciprocity.

That being said, the previous point 3) raised by myself and Reviewer 3 remains, in my opinion, not completely discussed. What we mean is the following. Consider the model
$$\hat{H} = \omega_x \hat{\sigma}_x + \omega_y \hat{\sigma}_y.$$
This model admits two real eigenenergies, and one could study the diabolical point at $\omega_x = \omega_y =0$*. If I were to apply the winding number proposed by the authors, what result would I get? In other words, would the winding number be able to discriminate between an EP and a DP? This discussion is, in my opinion, essential, as the authors use the winding number as a proxy to quantify the degree of degeneracy of the EP using experimental measurements. If this was to be the case, the article would still be interesting, but it needs to be clarified in the article, especially in the view of possible future follow-ups using this paper as a guide.*

Response: We are sorry for not having well grasped the previous point 3). Following this comment, we have calculated the proposed winding number for the DPs (diabolical points) in both the two-dimensional (2D) and 3D Hermitian system, as well as for the EP2s in a 2D NH system. The result show that the winding number is 0 for these DPs and EP2s. We have added a section at the end of the Supplemental Material, entitled "Winding numbers for DPs and EP2s", where the calculation is presented.

At the end of the third paragraph from the bottom of the main text, we have added the sentences "We further note that the winding number defined by Eq. (3) is zero for the degenerate points in both the two-dimensional (2D) and 3D Hermitian systems, referred to as diabolical points (DPs), as well as for EP2s in a 2D NH system, as detailed in the Supplemental Material. Therefore, such a winding number serves as a homotopy invariant that uniquely characterizes the topology of EP3s."

Upon clarification of this point and demonstration that indeed the article measure features associated with higher-dengerated EPs, I believe this paper reaches the standard of Nature Communications.

Response: As detailed above, we have demonstrated the measured winding number is uniquely associated with three-fold degenerate EPs (EP3s).