Requested changes

1. I am a bit confused about the following statement on page 5: "This adds substantial complexity to solving the problem, and effects have also been observed in experiments", which refers to the inclusion of quantum jumps. I find the second part of this statement somewhat vague. What kind of effects are the authors referring to?

Answer: We thank the referee for pointing out the issue. In this statement, we intended to emphasize why it is necessary to incorporate quantum jumps into our problem. To this end, we wanted to highlight that quantum jumps have been observed experimentally. However, we agree with the referee that the sentence was not complete. We have now rewritten this part in the revised manuscript.

Changes in the manuscript: We have modified/added the sentences: "*This adds substantial complexity to solving the problem, as we discuss below. Furthermore, quantum jumps have also been observed in experiments [98–102], and thus, incorporating the effect of quantum jumps is necessary for open system.*" in Section 2.2.

2. Above Eq.(4), the authors state "For simplicity, we consider a simple form of the onsite loss, which can become experimentally feasible." Could the authors comment on this? This also relates to my next point.

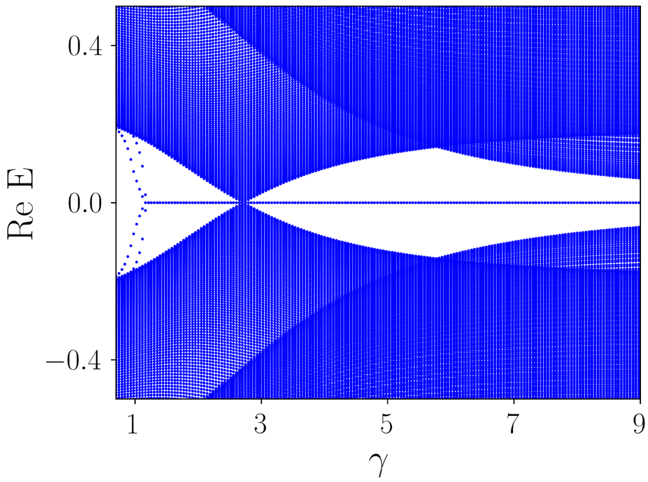
Answer: In our work, we have considered a loss that is onsite in nature. Thus, we call it simple. In principle, one can also consider different and more complicated forms of the loss operator, such as hopping losses. Nevertheless, we only consider a specific form of jump operators in our work due to the already large complexity of studying open systems. It might also be intriguing to find the form of loss by considering different types of coupling with the environment, but that we have to leave for a future investigation.

Changes in the manuscript: We have now added a sentence: "*The form of loss may also be possible to adapt and it would be intriguing to find the form of loss by considering different types of coupling with the environment.*" in Section 4.

3. In Eq.(4) the authors introduce the jump operator they use for their model. In their choice, they assume uniform loss in the entire system. Would it not be more realistic to have different loss rates on different lattice sites? Could the authors comment on how that would alter their results?

Answer: We thank the referee for this question. We agree with the referee that it might be more realistic to consider different loss rates at different lattice sites. In fact, our work contains both uniform loss on all lattice sites (main text) and single-site loss (appendix). Both types of loss give rather similar results, and based on that, we can draw the overall conclusion that some variation of the loss rates will not significantly change the results. We have also now explicitly checked our results for different loss rates at different lattice sites. We do not observe any significant changes to our results. We now mention this while discussing the effect of disorder, in the revised manuscript.

To demonstrate these results, we show a scenario where the loss rate γ is not constant but rather has a form: $\gamma_i=\gamma+\delta\gamma_i$; with $\delta\gamma_i$ is randomly distributed in [-W/2,W/2]. Here, i represent the lattice sites, and w stands for the strength of the loss variations or disorder (not to be confused with the Anderson disorder we also use in the manuscript). We show the disorder-averaged real part of the Lindblad spectra for W=0.3 as a function of γ below. We see that both the RZMs and MZMs remain stable against non-uniform loss at different lattice sites.



Changes in the manuscript: We now have added a few sentences: “*We also investigate the effect of non-uniform dissipation strength at each lattice site. In particular, we consider the case where the dissipation strength $\gamma_i = \gamma + \delta\gamma_i$ with $\delta\gamma_i$ is randomly distributed in [−W/2, W/2] and W being the strength of the dissipation variation (or disorder). However, as long as W is small, we do not observe any substantial changes to the phase diagram that we obtain in Fig. 3(a). Thus, both the RZMs and MZMs are robust against the non-uniform loss across the NW.*“ as a new paragraph at the end of Section 3.2.1.

4. Below Eq.(12), the authors state that \tilde{\tau} and \tilde{\sigma} are newly defined Pauli matrices. For the sake of completion, could they add the explicit form of these new Pauli matrices to the appendix?

Answer: We apologize for this confusion. \tilde{\tau} and \tilde{\sigma} are not any new Pauli matrices, but we just wanted to use different symbols to indicate that they encode other degrees of freedom from before. We have now removed the phrase “newly defined“ in the revised manuscript to avoid any further confusion.

5. In the beginning of section 2.3, the authors introduce a symmetry they call pseudo-anti-Hermiticity symmetry. I want to note that within the framework introduced by Kawabata et al. (PRX 9, 041015 (2019)) this symmetry is referred to as chiral symmetry. It may be worthwhile pointing out that this symmetry appears under a different name as well.

Answer: We thank the referee for pointing out this. We have mentioned this in our revised manuscript.

Changes in the manuscript: We have now added the sentence: “*or an NH system, this symmetry is also called chiral symmetry [66].*“ in Section 2.3.

6. The labels and texts in the insets in the figures are very hard to read because they are very small. For example, it is very difficult to read the labels in the inset in Fig.2(b) or to read the legend in Fig.3(b).

Answer: We thank the referee for these suggestions. We have increased the font size of the labels and the texts in the insets of all the figures in our revised manuscript.

7. For the model discussed in Figure 3, the authors find four so-called RZMs (robust zero-energy modes) induced by two second-order EPs (EP2s) on the left side, i.e., for gamma a bit larger than one. It is known that EP2s always come in pairs and are connected via so-called (i-)Fermi arcs, i.e., via branch cuts at which the real (imaginary) part of the energy is degenerate. This behaviour is also visible in Fig.3(b) for the red and black curves in the bottom. As such, the appearance of the RZMs to me looks like the Fermi arcs one would expect to see between EP2s. Is this indeed a correct observation? If so, the set of Fermi arcs, which would amount to a four-fold degeneracy in this case in line with the observation that four RZMs exist, must terminate at another set of EP2s. Do such EP2s appear at the bulk-gap closing points? Am I correct when the imaginary part of the energy also disappears for the RZMs? Also, does the symmetry in the model force the EP2s and the RZMs to sit at zero energy? Seeing that there is a double set of EP2s, I would expect they could in principle sit away from zero energy as long as they preserve the spectral symmetry,

i.e., appear as epsilon and -epsilon. Could there be a scenario where the EP2s and RZMs could move away from zero?

Answer: We thank the referee for these queries. The EP-induced zero-modes, which we call RZMs, are different from Fermi arcs. Fermi arcs appear as a function of momentum. In contrast, here, in Fig. 3(a), we plot the eigenvalue spectra for a system obeying open boundary condition as a function of the dissipation strength, and the RZMs appear as in-gap states. Thus, they are different from Fermi arcs.

Moreover, we obtain EP2s at the left edge of the yellow region in Fig. 3(a), which we also show explicitly in Fig. 3(b). The referee correctly points this out. However, we do not obtain any EPs at the right edge of the yellow region in Fig. 3(a), which also coincides with the bulk gap closing point and the beginning of the topological MZM phase.

Also, the RZMs are not four-fold degenerate. Only the real parts of the eigenvalues of the RZMs are four-fold degenerate, while their imaginary parts are pair-wise two-fold degenerate. One can see this feature in Fig. 4(a), where we explicitly see two sets of RZMs, with each set containing one pair of RZMs with degenerate imaginary parts. The imaginary parts of the RZMs are also not zero, which is again evident from Fig. 4(a). All this speaks against the RZMs being of Fermi arc origin.

Regarding the symmetry protection of the RZMs, we do not know what is the exact symmetry that pins them at zero (real parts). This can be due to the particle-hole symmetry or the chiral symmetry. However, we intend to identify the origin of the RZMs from analytical calculations in the future, possibly in a simpler model as the current setup is possible to elaborate to solve analytically. By doing this, we might possibly be able to comment on the symmetry responsible for the pinning of the RZMs at zero (real parts). Still, we can based on our current results, already conclude that the RZMs are not connected to the bulk of the system.

Moreover, we do not observe any scenario in our study, varying a range of parameters, where the RZMs are shifted from zero-energy (real parts). They are also pinned at zero energy (real parts) even in the presence of disorder, see Fig. 6. Although the referee points out a very interesting scenario of moving the EPs and possibly RZMs from zero energy, but we would refrain from commenting on this at this point. We believe that further and more detailed investigations are needed to fully understand the emergence of the RZMs, which we have also emphasized in the Conclusions and Outlook section.

8. The dark stars in Figs.3(d) and (e) in the red regions are very hard to spot. Could the authors choose a different colour?

Answer: We thank the referee for the suggestion. We have now changed the color map of Figs. 3(d,e). Now, the dark stars can be more easily spotted.

9. In the appendix, the authors study the very interesting case of having single-site losses. I believe some of these results may need to featured in the main text because they are quite fascinating.

Answer: We thank the referee for finding the results in the appendix to be interesting. We did seriously consider to have the single-site loss in the main text as well. However, we find that many results in the single-site loss have large similarities with single-site defects (which have been investigated thoroughly in the literature already) or have simple analogies with the uniform loss results. Also, one of the goals of the present work is to obtain dissipation-induced MZMs, which we do not observe in the presence of a single site loss (as expected since changing the topology should not be possible by only local processes). Based on all this, we decided to only report on uniform loss in the main text in order to keep that as a coherent and well-contained story but still provide the single-site loss results in the appendix as an interesting complement. Although some features of the single-site loss are indeed quite interesting, we believe they are perhaps not too surprising, taking the uniform loss and single-site defect behavior into account, and we, therefore, choose to keep them as an appendix. We hope the referee may now understand our reasoning for this choice of organization of our manuscript.