Anonymous Report 1 on 2022-11-23 *(Invited Report)*

Strengths

1. Comprehensive discussion of subtle issues in non-Hermitian physics

2. Work of great interest for a broad spectrum of physicists from cond-mat to optics and open quantum systems

3. Reasonably clear presentation in spite of the length and complexity. Of course, some shortening of the text (with no loss of content nor of clarity) would be appreciated.

When revising the manuscript, we simplified some sentences and deleted some redundant portions of text.

Weaknesses

1. A crucial technical issue must be clarified

2. Referencing to previous literature and putting the results in a wider context may be improved

Report

The manuscript by Brunelli et al. provides an interesting and convincing novel perspective on the role of topology in non-Hermitian systems, mostly of optical nature. The subject is very timely and the manuscript shines new light on a hot topic of present-day research. The results are -in my view- original and, if proven correct, may offer a deep insight on the underlying physics: the study critically compares different approaches to the bulk-boundary correspondance in non-Hermitian systems and provides an unified view on the subject, potentially of great utility for follow-up theory as well as for experiments. I expect it will have a significant impact on a wide community of researchers interested in non-Hermitian models from different points of view, from optics to hydrodynamics, as well as on mathematical physicists. Given the complexity of the work, the authors have done a good job in providing a clear and understandable presentation of their results. Based on these arguments, I find that SciPost will eventually be an ideal venue for its publication once the authors have taken into due consideration the following remarks.

We thank the Referee for acknowledging the novelty, timeliness, and significance of our work.

0/ First and most important.

As the authors repeatedly mention, the base point for determining the topology plays a crucial role in their theory, as it determines the distinction between "point gap" and "non-trivial topology". My problem is the following.

On the one hand, a global shift of the frequencies of the site energies (i.e. of omega_c) can be fully compensated by an identical shift of the probe laser frequency (i.e. the oscillation frequency of \Omega_m(t) or, equivalently, of a_{in,m}(t)), giving at the end identical physical results, which is fine.

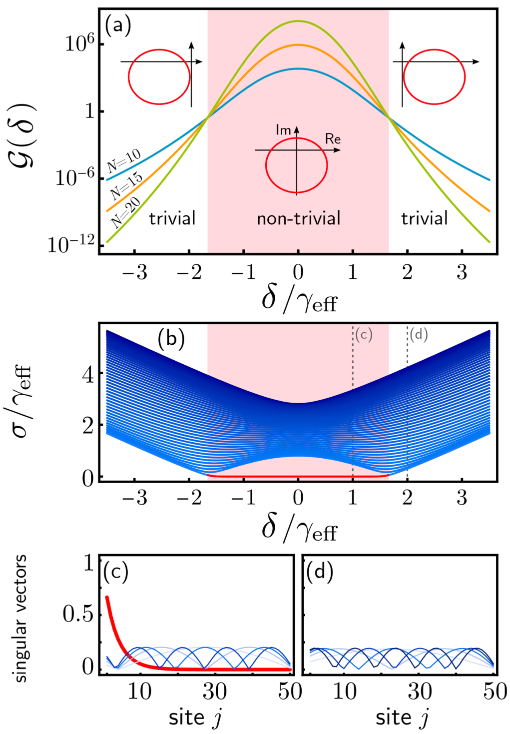
On the other hand, a shift of the relative frequency of the probe viz. the site energies can change the topology. While the choice of setting the pump to the bare site frequency may appear natural in the Hatano-Nelson model, it is far less obvious in more complex lattices with different on-site frequencies.

These arguments, together with some remarks by the authors above eq.(19), push me to think that the topology notion they are introducing is a frequency-dependent one. But if I accept this frequency-dependence of the topology, then I do not understand what is the physical meaning of the ZSMs, since their number can depend on the probe frequency (in contrast to Hermitian lattices, where the BBC is an intrinsic fact of the system and the probe only serves as a measurement tool). And then I would no longer undestand the meaning of the frequency-dependent response (41) calculated by means of the SVD at a given frequency. In a word, I am lost.

The authors must clarify this point in full detail before the manuscript can be considered for publication.

We thank the Referee for stating their concern clearly. Indeed, our definition of the winding number does depend on the relative frequency between cavity resonance and probe laser frequency, i.e., detuning. As the Referee writes, this is in sharp contrast to the Hermitian case, where the topology can be defined on the level of the wavefunction of the closed system.

As we show in the manuscript, in non-Hermitian (point-gapped) systems, the topology tells us how the system (i) responds to a probe and (ii) interacts with the environment. In particular, the frequency at which the system is probed affects its topology. The same holds true for the amount of energy lost to (absorbed from) the environment, namely the loss (gain), which also affects the topology. We can see that from the fact that the constant term \mu_0 in Eq. (15) enters in the evaluation of the topological invariant Eq. (19). In particular, the real part of Eq. (15) leads to a frequency-dependent notion of topology. While we focus in the manuscript on the case of a resonant probe, the system's response does depend on the probe frequency, as shown by Eq. (13). Crucially, such frequency dependence is fully in agreement with the behavior of the ZSMs, and is in fact necessary to reinstate the BBC. Mathematically, this is because the SVD, unlike the standard eigendecomposition, is sensitive to the diagonal shift in Eq. (15). We stress this point e.g. in Sec. XII, where we write "the SVD, unlike the eigendecomposition, is not invariant under shifts. In fact, for non-normal matrices, singular values and vectors can change non-trivially due to a diagonal shift". Physically, the ZSMs count the number of (independent) channels of directional amplification that the array supports under OBC. This is expressed by Eq. (41) and (42) and illustrated in Fig. 6 for the case of a resonant probe and winding number \nu=2. Introducing a detuning (not considered in Fig. 6) would impart a shift, which would be picked up by the SVD in Eq. (41) and affect the ZSMs in (42). If a probe signal is too far off resonant, it will not be amplified; a non-amplified response corresponds to a topologically trivial regime.



To make the dependence of both topology and ZSMs on the frequency more explicit, we plot the gain and the singular values under OBC as a function of cavity-probe detuning below. For clarity, we focus on the case of equal on-site frequencies. The gain grows exponentially in the topologically nontrivial regime, as indicated by the red-shaded region. For small detuning, we have non-trivial topology (non-zero winding) under PBC, which coincides with the OBC regime of exponentially growing gain, subfigure (a) below, and the presence of one ZSM under OBC, subfigure (b); this ZSM is exponentially localized, subfigure (c).

Conversely, for large detuning, we find trivial non-Hermitian topology (zero winding) under PBC, which corresponds to the absence of ZSMs, i.e., no amplified response; all singular vectors are extended, subfigure (d).

We also point out that a frequency-dependent notion of topology is consistent with our previous work on the topic [Nat Commun 11, 3149 (2020); PRL 127, 213601 (2021)] as well as with other works [PRL 122, 143901 (2019); PRA 103, 033513 (2021); PRA 106, L011501 (2022)]. In particular, in [Nat Commun 11, 3149 (2020)], we explicitly consider the case of non-zero detuning and we compute the corresponding topological phase diagram, see figure 3d of [Nat Commun 11, 3149 (2020)] (please note that detuning is denoted by ω there). Furthermore, in the Methods of that reference, we compute the gain as a function of detuning.

Regarding the case of different on-site frequencies mentioned by the Referee: there are two possible cases. (i) Either the on-site frequencies are staggered and preserve translational invariance but with a larger unit cell. This case goes beyond the one-band scenario covered by the current manuscript and will be the subject of future work. (ii) In the second case, the on-site frequencies are disordered and break translational invariance which makes it impossible to calculate a winding number for a single realization of the system. However, it is still possible to recover a correspondence between properties of the spectrum and directional amplification, by picking the extrema of the on-site frequencies (minimum and maximum, respectively) and considering the winding numbers obtained in these two limiting cases. If both are non-trivial, the disordered system displays directional amplification with a gain exponential in system size. If both are trivial, there is no such amplification. If one is trivial and one is non-trivial, the disordered system may behave either way. Phrased differently, the minimal separation between the origin and the complex spectrum of a system without disorder (the NH gap) sets the amount of disorder that can be tolerated before topological features are lost. We have proven these results in Ref. [PRL 127, 213601 (2021)].

In response to this Referee comment, we added the entirely new Appendix C to the revised version of the manuscript, which contains the figure of the gain and singular values as a function of detuning.

In the main text, we added the following sentence in Section V following the paragraph after Eq. (19):
"...there is room for point-gapped spectra with trivial topology. This represents a distinctive trait of our framework.
We discuss the dependence of the topology on detuning in more detail in Appendix C."

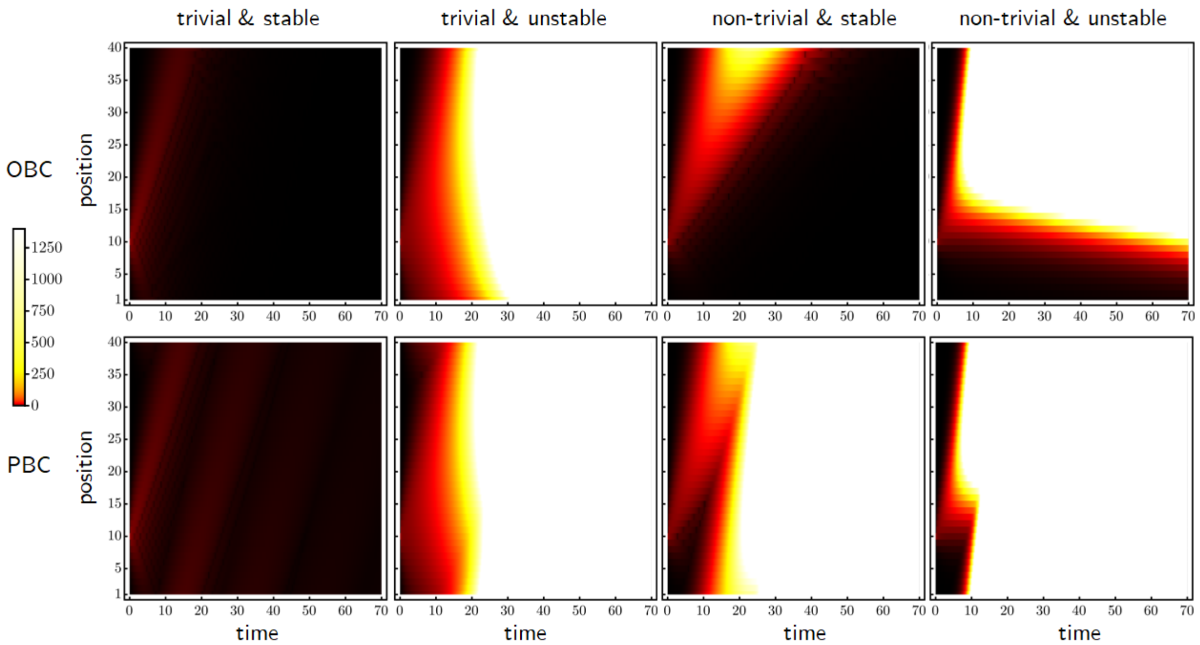
1/ Already several decades ago, quite some literature has addressed the interplay of the conservative and dissipative/gain dynamics in hydrodynamical systems, the so-called absolute vs. convective instabilities. These results were then successfully applied to nonlinear optics. The authors should give proper credit to this early literature, quoting the most significant works and, if possible, adding some good discussion on if and how their results may be of interest also for such hydrodynamic problems.
For instance, I expect that the sentences around eq.(43) would become clearer if a connection was made to absolute vs. convective instabilities, and I expect that similar improvements may be in order in other points of the ms.

As a few entry points into this literature (I am not expert of it, so the authors should make their own bibliographic search), I may mention:

Lovergnaux et al., PRL 102 043901 (2004); Pitaevskii et al., PRL 100 160402 (2008); Santagiustina et al., PRL 79, 3633 (1997); Seclì et al., Phys.Rev.Res. 1, 033148 (2019); R. J. Briggs, Electron-Stream Interaction with Plasmas (MIT Press, Cambridge, MA, 1964); P. Huerre and P. A. Monkewitz, Annu. Rev. Fluid Mech. 22, 473 (1990).

We thank the Referee for bringing to our attention the literature on absolute vs. convective instabilities.

In this literature, a distinction is made between two kinds of instabilities: given a localized perturbation the instability can either grow (**absolute instability**) or it can grow while propagating through the system (**convective instability**). In the latter case, the excitation can eventually leave the region of positive net gain, so the system as a whole can remain stable despite an (exponential) growth of fluctuations.



We analyzed how these two kinds of instability enter our problem. Following the example of Figure 6 in Seclì et al., [Phys. Rev. Res. 1, 033148 (2019)], we calculate the dynamics following a local perturbation (on site #5 at time t = 0). We plot the time evolution of the mean amplitudes in four cases, which we label “trivial & stable”, “trivial & unstable”, “non-trivial & stable”, and “non-trivial & unstable”. (Here, ‘trivial’ and ‘non-trivial’ refer to the NH topology). We highlight the following facts:

- In the ‘trivial & stable’ case, a perturbation decays and the system relaxes back to the vacuum, as expected (as all the eigenvalues have negative imaginary parts, both under PBC and OBC). In the literature, this system is said to be (linearly) **stable.**
- The system can be unstable both in topologically trivial and non-trivial regimes. In both cases, we see in our numerical simulations that an initial perturbation grows both under PBC and OBC. Mathematically, this is related to the fact that local gains due to the incoherent pump are larger than local losses, so $\gamma_{eff} = (\gamma + 2\Gamma - \kappa)/2 < 0$, leading to **absolute instability**.

  In the manuscript, we write that the system is unstable when we refer to this case.
- The most interesting case is the ‘non-trivial & stable’ case, where the PBC eigenvalue spectrum signals an instability (which can be seen in the real-time evolution) whereas the OBC system remains stable, i.e. all the eigenvalues of the OBC system have negative imaginary parts. We see in our numerics that in a non-trivial, but stable system a perturbation leads to exponential growth that moves away from its initial position in a front that under OBC can leave the system at the open boundary. In the literature, this situation is called **convective instability**. In the manuscript, we refer to this case as (dynamically) stable, see Sec. X, discussion above Eq. (43).

Additional insight can be gained from the analytical expressions of the retarded Green’s function of the Hatano-Nelson model in certain limiting cases, which can be found in a recent preprint by G. Lee, A. McDonald, A. A. Clerk, arXiv:2210.14212. This Green’s function characterizes the response of the system to a local perturbation (on a single site). For example, in the limit of perfect non-reciprocity, i.e., unidirectional hopping, the authors find that the growth diverges locally if local gain is larger than local losses, which characterizes an **absolute instability**. On the other hand, if the local losses are larger than the local gain, the authors find that the Green’s function is a sharply-peaked function in the time domain, featuring exponential growth that propagates through the system. This is the defining feature of a **convective instability**, as seen in our numerical simulations.

We have added the following sentence and new references to the discussion around Equation (43):
"In the literature e.g. on non-equilibrium pattern formation [RMP 65, 851 (1993)], solitons [PRL 100, 160402 (2008)], and lasing [PRR 1, 033148 (2019)], this situation is called a convective instability and is distinguished from an absolute instability which in our system corresponds to the case $max_m Im\,\lambda_m > 0$. Such convective type of instability is the physical mechanism through which excitations can leave the system under open boundary conditions, which can stabilize the NH topological phase. In the future, we will explore consequences of our results presented here in the presence of interactions, e.g. in arrays of nonlinear resonators, which allow for a richer hydrodynamic description [RMP 85, 299 (2013)]."

Note that we also fixed a typo in equation (43).

1-bis/ Along similar lines, I suggest the authors to explicitly discuss and clarify as much as possible the difference between "directional amplification" and "lasing". I anticipate that several readers may fall into confusion on such subtle distinctions.

We thank the Referee for bringing this point of potential confusion to our attention. In the regime of directional amplification, the equations of motion are linear and stable under OBC; this is the regime that we characterize in detail in our manuscript. For larger cooperativities, i.e. stronger pumping, the linear equations of motion become unstable even under OBC (absolute instability). Then, they do not suffice to describe the dynamics in the long-time limit and non-linear effects become important. This is the regime of "lasing" that we do not discuss in this paper.

We added the following disclaimer in the Introduction, after the first appearance of the expression NH topological amplification: "This is not to be confused with lasing, which has also been investigated in several topological systems [Harari et al, Science (2018), Bandres et al, Science (2018), Amelio and Carusotto PRX (2020)], the key difference being that topological amplification relies on linear equations of motion."

Note that the list of references on topological lasers is not meant to be exhaustive. In view of the already large number of references in the manuscript, we opted for keeping new references to a minimum.

2/ I personally do not like the expression "unconditional implementation of...". If I understand correctly what the authors are doing, eqs.(3) are just the equations of motion for the expectation value of the field amplitudes <a_m> under the Master equation (2) (which, for a linear system like the authors' one, are equivalent to a tight-binding reformulation of Maxwell's equation in a gain/loss material). Furthermore, all this ms. deals with classical dynamics, so no reason of using a quantum language.

On the of all these arguments, I recomment the authors to look throughout the whole ms. and replace all instances of this misleading terminology.

In our work, the non-Hermitian Hamiltonian determines the dynamics of classical amplitudes. We fully agree with the Referee that Eqs. (3) are nothing but the equations of motion for the expectation value of the field amplitudes. We followed the Referee's recommendation and removed the expression "unconditional implementation of.." in the revised version of the manuscript.

3/ The authors should define the expression "point gap" the first times they use it: for instance on the LHS column of pag.1 saying "winds in the complex energy plane AS K IS SCANNED ACROSS THE FBZ" (or similar) and then again at the beginning of pag.4. This would help non-expert readers to quickly get to the point of the ms.

We appreciate the comment by the Referee. To avoid confusion, besides the detailed definition of the point gap provided in Section V, we added a short description of the 'point gap' to the introduction and in the first point of Sec. II, as suggested by the Referee.

4/ At the beginning of pag.5, the authors say that the non-engineered photon decay \gamma is on each input-output waveguide, but in their calculations they assume \gamma to be present on all sites. These two configurations are contradictory as the presence of input/output waveguides can only affect the sites to which they are connected. The authors should update several points in the text to avoid such a confusion.

We thank the Referee for bringing up this potential point of confusion. Indeed, we connect input-output waveguides to all sites. When we probe the system, we drive one site coherently and we measure the output at any of the N sites. This corresponds to standard transmission measurements, and this is the information contained in the NxN scattering matrix, as shown in Fig. 1.

We clarify this point in the revised manuscript below Eq. (8) where photon decay at rate \gamma is first introduced and added the following sentence in Sec. IV: "When we probe the system, we drive one site and measure the outgoing amplitude at any of the N output ports. This is the information contained in the scattering matrix, see rightmost column of Fig. 1."

5/ For non experts on SVD, the authors may add some reference on the general mathematics of SVD and explicitly mention that the u_j and the v_j form two basis of orthonormal vectors (but do not satisfy any mutual orthogonality condition).

We thank the Referee for the suggestion. We added a reference to [Trefethen, Lloyd N.; Bau III, David. Numerical linear algebra. Philadelphia: Society for Industrial and Applied Mathematics. ISBN 978-0-89871-361-9 (1997)] where we first introduce the SVD above Eq. (24).

6/ In the caption of fig.3, the quantity \mathcal{C}_2 is called "cooperativity" but its definition before eq.(14) is more like a normalized dissipative coupling. The authors should clarify the terminology.

The quantity \mathcal{C}_2 compares the decay rate of an engineered (non-local) dissipator to the total on-site decay rate. We refer to \mathcal{C}_2 as a 'cooperativity' rather than as 'dissipative coupling' to be consistent with Ref. [32] [PRX 5, 021025 (2015)], as well as our previous works, Refs. [31, 79].

7/ After eq.39, the authors classify the topology in terms of a Zak phase. Is this really the optimal way of doing, or isn't it better to use the more standard notion of topology related to systems with chiral symmetry? I understand that the two definitions may reduce to similar expressions involving \phi(k) but I am not sure that there are no gauge dependence issues around.

We thank the Referee for pointing this out. Indeed, the Referee is correct that the Zak phase of a *single* band depends on the gauge. This is why the Zak phase is only related to the winding number mod 2. To define a gauge-independent topological invariant, one has to take the difference between the Zak phases of two bands. In the Hermitian SSH model, this allows the winding number to be extracted from the eigenstates.

In our NH model, this implies that to obtain a topological invariant that is independent of gauge, we have to take the difference between the Zak phases of left and right singular vectors (which corresponds to taking the difference between Zak phases of Hermitian bands). In principle, we could also argue that the winding number defined on H(k), according to the Hermitian doubled matrix Eq. (36), coincides with the winding number of the Hermitian SSH model. Our motivation behind calculating an invariant from the singular vectors has been to provide a topological characterisation based on the SVD alone.

We improved the the definition of the Zak phase (40): "The singular vectors also encode the topological invariant which we obtain as difference between the Zak phases of right and left singular vector

$$\begin{aligned}\Psi_{\mathrm{Zak}} &= -\mathrm{i}\int_0^{2\pi} \mathrm{d}k\, \left(\langle \Psi_+(k)|\partial_k \Psi_+(k)\rangle - \langle \Psi_-(k)|\partial_k \Psi_-(k)\rangle\right) \\ &= -i\int_0^{2\pi} \mathrm{d}k\, \left(\langle v(k)|\partial_k v(k)\rangle - \langle u(k)|\partial_k u(k)\rangle\right) = \pi\,\nu\end{aligned}$$

This corresponds to taking the difference between Zak phases of different bands in the Hermitian SSH model and is therefore gauge invariant."

8/ At the end of Sec.XI I am lost about the role of symmetry in the presence of disorder. At the beginning of the section, I understand that the authors are taking a generic disorder that does not necessarily fulfills the chiral symmetry and are showing that the results on the ZSM hold also on this case. Then, in the final paragraph, they seem to require a chiral symmetry. The authors should clarify what they are doing.

The NH model in Eq. (14) has only one band, see Eq. (17), and has therefore no additional symmetries. However, mapping this NH model to the Hermitian generalized SSH (GSSH) model via the construction of the doubled matrix according to Eq. (34),

$$\mathscr{H} \equiv \begin{pmatrix} 0 & H^\dagger \\ H & 0 \end{pmatrix} = |B\rangle\langle A| \otimes H + |A\rangle\langle B| \otimes H^\dagger \,,$$

we see that the Hermitian model has a chiral symmetry by construction, for *any* type of disorder in the NH model.

This was meant as an additional remark and is not central to the discussion about robustness of NH topological phases. As we wrote at the end of Sec XI: "to highlight the self-consistency of our framework and the role of the NH gap, we discussed the robustness to disorder fully at the NH level."

We have rephrased the paragraph to read: "Finally, we notice that topological robustness can also be addressed by exploiting the mapping to the GSSH of Sec. IX. The GSSH enjoys chiral symmetry $\nobreak{(\sigma_z\otimes\mathbb{1}_N) \mathscr{H} (\sigma_z\otimes\mathbb{1}_N) = - \mathscr{H}}$ which ensures the presence of the gap and then ensures that (Hermitian) topology is robust to perturbations that do not break this symmetry [79]. Disordered NH models still map to the GSSH model for *any* type of disorder so the chiral symmetry of the associated Hermitian model is always preserved. This allows to infer the robustness of NH topological phases for each of the two NH copies. Here, to highlight the self-consistency of our framework and the role of the NH gap, we discussed the robustness to disorder fully at the NH level."

8-bis/ in the same last paragraph of Sec.XI, the authors may add parenthesis around tensor products to clarify the formula of the chiral symmetry.

Fixed.

9/ Among the list of experimental systems where this physics can be potentially realized, the authors should also mention exciton-polaritons in micropillar arrays. For completeness, they may also consider adding references to early works on non-Hermitian photonic systems, e.g. by Longhi.

We thank the Referee for their suggestion. As for exciton-polariton micropillar arrays, we added the following references: Amo and J. Bloch, Comptes Rendus Physique 17, 934 (2016); S. Klembt, et al. , Nature 562, 552 (2018).
We also added the following references by Stefano Longhi on photonic realizations of non-Hermitian systems: M. Pan et al., Nat. Commun. 9, 1308 (2018); H. Zhao et al., Science 365, 6358 (2019).
In view of the already large number of references in the manuscript, we opted for keeping new references to a minimum.

We thank the Referee for their careful review and their positive evaluation of the significance, originality, and clarity of our work.