Comments of Reviewer 2:

1. The first part is intended to be abstract and general. This approach is appropriate when the hopping ratios decrease with nonlinearity. However, in the case of increasing hopping ratios, a careful reading of the section reveals several additional conditions that have been established. For instance, there is a kind of saturable nonlinearity, where the nonlinearity must maintain the first hopping ratio at 1 once a2 =a1 is reached. Furthermore, they also consider the nonlinearity from the subsequent section. I recommend that the authors clarify that this is not a general case, but rather a specific scenario motivated by their physical context.

We thank the reviewer for the question. We agree with the reviewer that our estimation in Fig.1 does not apply to all kinds of nonlinearities, especially when hopping ratios in A increase with nonlinearity. We do add one additional assumption to derive the corresponding results. However, we would like to clarify that,

(1), It is impossible to derive one deterministic law that is valid regardless of the nonlinear contents, since nonlinearity, in principle, can arbitrarily depend on the amplitude. For this reason, we have focused on nonlinearities with monotonic dependence on amplitudes, a property shared by most of the 'common' nonlinearities.

(2), We impose that the hopping ratio stays at 1 once $a_2 = a_1$ is achieved. This comes from the concern about the physical instability in practical situations, as we state in the manuscript "After that, if nonlinearity still can increase the hopping ratio, ..., inevitably ending with a physical instability of the system.". Our aim of this work is to observe in practice the greatest variation of the nonlinear edge state, we thus restrict ourselves to the case where the system remains physically stable at all nonlinear magnitudes.

(3), we agree with the reviewer that such a 'saturable' type of nonlinear hopping ratio may not include all monotonic amplitude-dependent nonlinearities. However, when the hopping ratio is saturable, the corresponding hopping terms are not limited to the saturation types, they can also be polynomial (quadratic, cubic,...) or even other exotic ones such as exponential. And most importantly, they can also be non-local (as in the case we show in Figs.2-3). Therefore, we still believe that our estimations in Fig.1 can apply to a broad class of nonlinearities, not only to the specific nonlinearity presented in the subsequent section.

(4), When hopping ratios in A are increased with nonlinearity, the sites in B inevitably rise due to the finite dimensions of the systems. However, the variations of the relevant hopping ratios are not unique since nonlinearity in B can theoretically be random (different from that in A), That is why we have utilized the nonlinearity in the concrete case to draw the variation in B. Despite this, the evolution in the sublattice A, the main focus of this section, is not derived depending on a specific case. It can apply to a broad family of nonlinearities, as stated in the manuscript and explained above in (1)-(3).

We corrected the texts in section 3 and added descriptions in the caption of Fig.1 to make our estimations/interpretations clearer and more rigorous.

2. The authors aim to propose a physical system that exhibits the key features of chiral nonlinearity. However, the proposed setting is unnecessarily complicated. This is evident in Fig. 4, where at the frequency fH , the linear identical resonators (Helmholtz resonators) behave like an open circuit. By the way for the not specialists in electronics, the authors should clarify why this behavior occurs at high frequencies rather than at low frequencies. Wouldn't a variation of the approach presented in Phys. REVIEW APPLIED 20, 014022 (2023), using only membranes and active control, suffice to achieve the desired outcomes?

We thank the reviewer for the comments. We reply in the following points,

(1), We would like first to point out that, the behaviors of the resonators at low and high frequencies are just about the fact that the capacitor behavior of a single-degree-of-freedom resonator is dominant at low frequencies. In contrast, its mass behavior is dominant at high frequencies. This is not relevant to any further derivations such as the mentioned PRApplied. We added more explanations in the paragraph describing the corresponding Fig.6 in Appendix B.1.

Then, we would like to clarify that at high frequencies, the dominant mass behaviors of the linear resonators LF_n cause their impedance to be large. However, assuming this impedance is close to infinity (as an open circuit) is actually rude and uncritical. We thus corrected Fig.6 to more precisely approximate the linear resonators as masses at high frequencies, rather than as open circuits. These linear resonators do have an impact, they change the frequency f_H of the edge state, despite the predominance of the nonlinear resonators HF_n therein, as can be derived from Eq.(A.5) in Appendix A.1.

(2), We demonstrated with Fig.6 that our system is equivalent to a topological lattice made of single resonant unit cells at two different frequencies. That is to say, when simplifying it by removing the linear resonators LF_n in the lumped element circuit (the Helmholtz resonators in acoustic system), the same recurrent relations between the sites a_{n+1} (b_{n-1}) and a_n (b_n) can be obtained as in the current system (Eq.(4)). From this point of view, the reviewer is right that the simplified system can also allow the nonlinear topological edge state to be generated.

(3), In contrast to the simplified case, the advantage of the current complex system is that, within one single lattice, one can achieve two topological edge states with completely different features: the one we show in the main text (at the frequency f_H) is very robust in response to all kinds of losses, as evidenced by the experimental results. However, the second one (at the frequency f_L) is very sensitive to losses, leading to severe distortion in its shape and frequency. From our knowledge, such dual-band topological edge states achieved in one single system have hardly been demonstrated in the existing research in nonlinear topology. This is the reason for our choice, which we may not have explicitly emphasized in the main texts.

To describe and evidence our choice, we added more explanations in Section 4 and Appendix B.1 (in paragraph for Fig.6). A theoretical comparison between the two edge states is provided in Fig.8 in Appendix B.1, where the loss in each constituent element is considered in turn. We additionally added two figures (Figs.17 and 18) in Appendix B.3 to showcase the experimental results for the second edge state at f_L, from which one can notice the clear difference with the one at f_H.

3. Considering a setting like finite SSH chains with medium hopping ratios, one would expect to observe two edge modes, coupled and with energies

both below and above zero energy. It appears that the authors do not take this into account. Although the spectral figures, such as Fig. 2 and Fig. 3, show a dominant peak that is not at the frequency fH (the "zero" energy) but rather below it. The figure captions indicate that "the whole system starts and ends with the controlled loudspeakers," suggesting that the authors are addressing a driven case rather than a Hamiltonian case, given the presence of loudspeakers at both ends of the structure. In this context, the edge profiles reflects the driven response of the system rather than the eigenmodes derived from the Hamiltonian's eigenvalues. This distinction may confuse the readers, especially since the first "general" section is based on the Hamiltonian framework. I recommend that the authors clarify these points in the text,

We thank the reviewer for identifying this confusion. To be clearer,

(1), We state that "the whole system starts and ends with the controlled loudspeakers", this is necessary to derive the Schrodinger equations $t_1 b_{n-1} + t_0 b_n = 0$ and $t_1 a_{n+1} + t_0 a_n = 0$ we show in Eq.(4), which is not related to whether the system is driven or non-driven. The driving source is actually included in the boundary b_0 (left end) of the lumped-element system.

(2), We do consider a driven configuration in the theoretical investigation in Section 4, which is necessary for the experimental realizations of edge states. Indeed, after the first 'general' section, we aim to directly explore a realizable case (a realizable system in a practically feasible configuration), rather than a "purely" theoretical derivation with eigenmodes of the Hamiltonian. Meanwhile, we would like to stress that, the excitation we define, combined with the non-reflecting boundary conditions we employ, play the same role as the theoretically required $\mathrm{b}_0 = \mathrm{a}_\mathrm{e} = 0$ in the Hamiltonian (non-driven) case, as demonstrated in Fig.7 in Appendix B.1. Therefore, the edge states profiles we show reflect indeed the non-driven responses that are obtained in an equivalent realizable driven manner.

We agree with the reviewer that the above points were not clearly stated in the manuscript. To remove ambiguities, we corrected Fig.2a and added one paragraph in Section 4 (the first paragraph after Fig.2) to describe the overall boundary conditions under consideration, including the driven situation, the details of which are completed in Appendix A.2.

4. Lines 217-223 lack clarity, particularly regarding the authors' discussion of instability. While they reference several points related to instabilities, there are no results demonstrating how these instabilities are involved. For example, they state, "The limit of only a1 being dynamic cannot be observed, as instability arises first, which is in accordance with time-domain analysis (Appendix B.2, Fig. 9)." However, I do not see any evidence of instability dynamics in this figure. Identifying nonlinear unstable solutions is crucial, particularly in terms of how these instabilities manifest. Are the authors referring to wave instabilities or those arising from the active elements? Clarifying these points would enhance the reader's understanding.

We thank the reviewer for pointing out the unclarity. The stability we are discussing therein refers to a physical instability that manifests itself in wave amplitude irregularities and tends to infinity. It is caused by a time delay unavoidable in real-time feedback active control on the loudspeakers, which inject energy into the closed system. We clarify this point in the corresponding paragraph.

5. In line 302, the authors state, "If $\Delta_t = 0$ is possible." It is crucial for the validity of the results that Δ_t be zero, yet it is not clear under what conditions this occurs or if it is always the case. Please clarify this point clearly to ensure a better understanding of its implications for the results.

We thank the reviewer for indicating the deficiency. We added more derivations and explanations in corresponding Appendix A.1. Indeed, $\Delta_t = 0$ can be transformed in the frequency domain at the fundamental frequency where $\frac{d}{dt} = i\omega$. This leads to a fourth-order differential equation on ω, in which two solutions exist, see Appendix A.1 in the new version of the manuscript for details. We additionally state that the higher harmonic generations are consistently lower than 1% in generating the edge state, thus $\frac{d}{dt} = i\omega$ holds directly, i.e., the corresponding results are always true.

6. A final and significant comment: From Eq. A5, it appears that the nonlinearity results in non-reciprocal hopping. If this is indeed the case, the authors should address this explicitly. Please explain the constraints that led to this outcome. Additionally, could a reciprocal nonlinear chiral system be proposed along the lines of the electronic setup described in Nat Elec-

tronics 178, 2018, "Self-induced topological protection"? Addressing these points would enhance the comprehensiveness of the discussion.

We thank the reviewer for the observations that we had not addressed. We confirm by double-checking that we have $\mathrm{t_{0a}} \neq \mathrm{t_{0b}}$ in the nonlinear regime, i.e., the hopping in our system is non-reciprocal. We added discussions about such non-reciprocity in Section 4 (in the second paragraph after Fig.2).

We would like to point out,
(1), In our system, the nonlinear part of the hopping terms for b_n (a_n) arise from the difference in the nonlinearities of $\mathrm{V}_{2k-1}^{(\mathrm{NL})}$ ($\mathrm{V}_{2k+1}^{(\mathrm{NL})}$) and $\mathrm{V}_{2k}^{(\mathrm{NL})}$, as can be seen from the dynamic equations in Eq.(A.4) in Appendix A.1. In this case, if one would like to derive a reciprocal hopping, the above nonlinearity differences should depend on a_n and b_n in the same manner respectively. However, knowing that due to the periodicity of the system, $\mathrm{V}_{2k-1}^{(\mathrm{NL})}$ and $\mathrm{V}_{2k+1}^{(\mathrm{NL})}$ present the same nonlinear law while depending sequentially on different sites, such a requirement is eventually not trivial to satisfy. By contrast, introducing nonlinearities leads to breaking reciprocity in the hopping, this is intuitively more like what could happen.

(2), The system studied in Nat. Electron. 178, 2018 presents alternatively mounted linear and nonlinear capacitors, corresponding to HF_{2k-1} linear and HF_{2k} nonlinear in our system. In that work, the authors have made several approximations in deriving Eq.(1) in the manuscript, as detailed in their Supplementary Information (Eqs.S5-S8). If removing all these assumptions while using only the basic circuit principles for their system shown in Fig.1, one will eventually arrive at Eq.(1) plus additional nonlinear terms depending both on a_n and b_n. Therefore, their achievements of nonlinear Schrodinger equations with reciprocal hopping relations involve approximate results, the nonlinear topological edge states are not produced precisely.

In contrast to it, our derivations haven't introduced any assumptions or approximations, the proposed nonlinear system allows the exact generations of nonlinear edge states (exact derivations of nonlinear Schrodinger equations), it also rigorously satisfies chiral symmetry regardless of the nonlinear magnitudes, and the non-reciprocal hopping relations are consistently present. To stress this point, we added the corresponding comments in Section 4.