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Peer Review File

Reconfigurable All-Solid-State Topological Lasing at Arbitrary Sites

Corresponding Author: Professor Feng Chen

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

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The manuscript reports the realization of an all-solid-state, reconfigurable topological laser based on an SSH waveguide array in Nd:$BaLaGa_3O_7$. The authors demonstrate single-mode lasing with output powers exceeding 100 mW, which they claim represents an improvement of more than two orders of magnitude over conventional semiconductor-based topological lasers. Furthermore, lasing is observed not only at conventional edge sites but also at trivial lattice terminations and arbitrarily reconfigurable interface positions, attributed to non-Hermitian PT-symmetry breaking. These findings are potentially important for the field of topological photonics.

I am not convinced by the current manuscript. Howerer, I can reconsider the work if they can make major revisions. Here are some detailed comments and questions.

The authors point out that current topological lasers suffer from limitations such as low optical gain and limited output power. By implementing an all-solid-state, reconfigurable topological laser based on a SSH waveguide array platform, they claim to overcome these challenges and demonstrate a significant improvement in performance. However, the manuscript does not provide a clear theoretical explanation for why this approach should lead to much higher output power. The authors should explicitly clarify the underlying mechanism beyond simply benefiting from the inherently higher gain of solid-state media.

The authors claim that their output power is two orders of magnitude higher than that of conventional topological lasers. However, this statement is not entirely rigorous, as the output power can be strongly influenced by factors such as device size, pump power, and other conditions. For example, Ref. 35 (Nat. Nanotechnol. 15, 67–72 (2020)) reports tens-of-micrometer devices already reaching mW-level emission, whereas the present structures employ a gain medium length of ~1 cm, which naturally results in higher collected output. The authors should provide the relevant comparison conditions, or alternatively, a direct comparison of external quantum efficiency would be more meaningful.

The evidence in Fig. 3f,g seems insufficient to clearly confirm lasing. I suggest adding spectral linewidth data to show narrowing from spontaneous to stimulated emission. If the below-threshold spectrum is too weak, self-interference of the emission pattern could be used to demonstrate coherence.

Some other technical comments:
1.In Fig.3f, the Light–Light curve under varied pump power below threshold should also be shown, as this is important for analyzing laser dynamics. Using a log–log scale would make the variations below threshold more clearly visible. For clarity, I suggest indicating the pump power for each spectrum in Fig. 3g.
2.Please clarify whether Fig. 3c and Fig. 3d show simulated or experimental data, and provide the same clarification for Fig. S9b and S9c.
3.In the sentence “two edge modes coalesce at an exceptional point (PT)” on line 93 of the main text, the abbreviation appears to be incorrect; it should be (EP) to match “exceptional point” and avoid confusion with PT symmetry.
4.In the Fig. 2 caption (line 117), the phrase “at leftmost lattice site together (b)” has the wrong label — it should be (a), not (b).
5.In Fig. S5(c), the blue fitting curve extends beyond the plotted data range and even outside the figure frame. If the curve

was derived strictly from experimental data, why does it exceed the data boundaries? The authors should clarify the fitting procedure and correct the figure.

Reviewer #2

(Remarks to the Author)
Femtosecond laser writing has successfully created diverse topological photonic waveguides in glass, spanning symmetric/asymmetric lattices and non-Hermitian systems. Yet glass material constraints currently preclude experimental realization of topologically amplified waveguides. This manuscript reports on the demonstration of topological lasing in a novel platform, the all-solid-state laser system (self-grown disordered laser crystal Nd:BaLaGa3O7). Both theoretical analyses and experimental validations confirm that high net optical gain can induce non-Hermitian PT phase transitions at trivial edge sites and intermediate positions. This study enables localized reconfigurable topological lasing at arbitrary locations, which not only provides new insights for fundamental non-Hermitian topological photonics but also holds important value for practical applications in integrated laser systems. Both the novel platform and its findings are interesting, and the manuscript is well-organized. I recommend this paper for publication in Nature Communications after some revisions.

1) The authors employed a self-grown disordered laser crystal as the substrate material, successfully achieving fabrication within the gain medium and constructing the gain-based non-Hermitian photonic lattice system, ultimately realizing topological lasing applications. It is worth further exploring whether similar experiments can be achieved for traditional rare-earth ion-doped laser glasses (such as Nd:glass) or common laser crystals (such as YAG, YAP, etc.). How does the choice of substrate material affect the experimental results? Do the disordered laser crystals grown by the authors possess unique physical or optical properties (such as higher nonlinear coefficients, better gain performance, or stronger damage resistance)? Are the bulk material gain properties preserved in waveguide region? It is suggested that the authors emphasize the advantages of this material in the text to highlight the innovation and contribution of this research.

2) While femtosecond laser direct writing has enabled the fabrication of single-mode waveguides and waveguide arrays with exceptional longitudinal coupling properties in crystalline materials (e.g., SBN, LiNbO3), their optical stability remains a critical challenge. For instance, laser-induced refractive index modifications in these materials often exhibit gradual relaxation at room temperature, leading to degraded waveguide performance over time. Thus, for laser devices, thermal stability is a critical performance parameter. Can the disordered laser crystal material employed in this study effectively maintain stable waveguide characteristics?

3) In Figure 2f, the theoretical simulation indicates that excitation occurs at Site 1 (trivial position) in the lattice. However, experimental results (Figures 3c-d) show excitation applied at Site 1 (topological non-trivial) and Site 15 (trivial), respectively. While this difference might not fundamentally alter the conclusions—since in a periodic SSH model, edge excitations are expected to behave similarly regardless of the exact site—the inconsistency between simulation and experiment could mislead readers when comparing the predicted and observed behaviors. It is suggested to re-optimize or change the annotation

4) In the theoretical analysis, the authors primarily focus on non-Hermitian PT phase transitions under gain conditions. However, losses are inevitable in real systems: can similar transitions be achieved solely by tuning losses (e.g., via periodic dissipation)? What is the equivalence to the gain-driven mechanism?

5) Additionally, some formatting and detail issues need to be addressed: a) I suggest swapping the positions of Figure 2e and 2f to better align with the logical progression of data or the sequence of experimental results. b) Supplementary Figure S6, the excitation site labels require correction: The current annotation for “Sites 1, 7, 9, and 15” should be replaced with “Sites 1, 7, 8, and 15” to accurately represent the experimental configuration.

Reviewer #3

(Remarks to the Author)
In this work, authors present an experimental realization of a topological SSH waveguide array laser based on Nd:BaLaGa_3O_7 crystal. They experimentally and theoretically study the active topological lasing from arbitrary sites of their structure. Experimentally reached output powers are greater than that of known reported semiconductor-based topological lasers, which makes this solid-state platform promising for the in-depth study of topological effects related to non-Hermiticity and nonlinearity.
The main novelty claimed by the authors is the topological phase transition induced by the non-Hermitian gain. Although I do not doubt the validity of the experimental observation and its interpretation, essentially the same effect was already discussed in https://journals.aps.org/prl/abstract/10.1103/PhysRevLett.116.046402, where the authors show not only the breaking of SSH-chain into two distinct parts by the spatially profiled gain, which is accompanied with the emergence of couple of topological edge states, but also discuss spontaneous formation of such topological defects in the nonlinear regime according to the Kibble-Zurek mechanism. Based on this, I cannot recommend this work for publication in Nature Communications. However, the advantages of the proposed implementation of active topological SSH chain will ignite

interest in a more specialized community of topological photonics.
In addition:
1) The authors describe the main effect of gain-induced topological transition with the reduced effective non-Hermitian Hamiltonian model, where parts of the SSH chain are replaced with a single state. The validity of this approach is illustrated with a qualitative similarity of the numerically calculated PT-symmetry breaking transition with the coupling parameter v in the two cases. Although, it appears evident that sufficiently strong coupling results in a transition in both cases, the absence of clear analytical mapping leaves the question of their equivalence somewhat open. The advantage of the proposed simplified model is especially unclear since direct numerical diagonalization of a non-Hermitian effective Hamiltonian is easily achievable in both cases.
2) It is somewhat unclear why the Fibonacci quasi-periodic positional modulation was used to simulate the effect of structural disorder. Is there any reason why it was preferred over the random positional anti-diagonal disorder affecting the coupling between neighbouring sites or a random variation of waveguide cross-section size simulating diagonal disorder by affecting the resonant mode energies?

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The manuscript now effectively addresses the concerns, and the technical details have been clearly improved. I have no additional criticisms and support the manuscript for publication in Nature Communications.

Reviewer #2

(Remarks to the Author)
I recommend it for publication.

Reviewer #3

(Remarks to the Author)
The authors addressed all the raised points and improved the quality of the revised manuscript. However, in some cases I found their argumentation unconvincing and factually incorrect. Most notably, in the reply to the first raised comment:
1) In the cited reference, the observed edge state arises from the region separation caused by finite potential barriers, which is an effect that can exist in Hermitian systems. By contrast, the emergence of edge and interface states in our work is exclusively to non-Hermitian systems. Specifically, in Fig.2(c) of the cited reference, nonresonant pumping is introduced in the bulk to excite localized modes of polariton condensate. However, as explained in that paper, the formation of the edge states results from the separation of the left and right chains by a Hermitian reservoir potential, rather than from the non-Hermitian effects.
The statement in the first sentence is incorrect: in the cited work the effect of spectrum modification and lasing from localized modes in the vicinity of an excitation spot was caused by a local non-Hermitian potential term UR+iγ in Eq. (4), accounting for both local self-energy shift and local gain, induced by the nonresonant optical excitation. The authors then proceed by elaborating on the incorrect statement and comparing the effect a local purely Hermitian perturbation with that of the non-Hermitian one. This comparison is by itself interesting but somewhat irrelevant to my original comment: the effect of lasing from localized modes near the excitation spot in SSH chains, described by a local non-Hermitian perturbation of a topologically nontrivial Hamiltonian, was proposed in 2016.
2) From a platform perspective, to the best of our knowledge, the potential barrier-induced localized states of polariton condensation at arbitrary sites discussed in the cited reference remain purely theoretical and have not been experimentally realized on any platform. In contrast, our work demonstrates experimental realization of laser amplification at arbitrary lattice sites on a novel all-solid-state platform, as also noted by Reviewer #2. This platform enables stable, high-power output, as confirmed by our experimental result, and paves the way for reconfigurable, on-chip, and actively controlled photonic devices.
Indeed, the Hermitian effect related to a local potential barrier, addressed by the authors, has not been demonstrated in the polariton platform to the best of my knowledge. However, the effect proposed in the cited work [Solnyshkov, Nalitov & Malpuech, PRL 2016] is non-Hermitian, so the authors continue elaborating on an incorrect statement. The effect of local lasing in the bulk of an SSH chain due to spatially focused optical excitation spot, on the contrary, was observed in polaritonics (see, for example, a recent work [Georgakilas et al., Sci. Adv. 11, eadt8645 (2025)]). Without doubt, the new material platform brought forward by the authors, has certain advantages for applied photonics, but I do not see how that addresses my comment.
3) In summary, while the two works display similar phenomena, they are based on fundamentally different mechanisms and platforms—one theoretical and the other experimentally realized. To better highlight the merits and novelty of our work, we have further emphasized the underlying mechanism and the platform in the revised manuscript (see Page 3 marked in red). We believe these distinctions underscore the novelty and importance of our study.
Although the revised manuscript acknowledges the earlier theoretical prediction, it incorrectly explains the difference between the two effects on the basis of Hermiticity of the former. As explained above, the earlier theoretical prediction was made with the non-Hermitian model.
In the reply to the second point the authors explain in detail the employed approach, which, as now is clear, reduces to a

partial block diagonalization of the Hamiltonian, resulting in a second-order perturbation theory energy correction (5), which is indeed in remarkable agreement with the exact result. The added supplementary sections greatly elucidate the theoretical framework.
Answering my last comments, the authors experimentally compared the effect of the previously employed Fibonacci disorder with that of a statistically uncorrelated one and found no measurable difference, which answered my question. This point is now also clarified this point in the Supplementary Materials.
Although the revised work shows great improvement over the initial submission, the reply letter shows that the authors misinterpreted the earlier theoretical prediction belonging to the same class of non-Hermitian topological effects, which is still reflected in the revised manuscript. As the main novelty claim is based on the first effect observation of the effect, which has been previously shown, it should also be significantly diluted. Demonstration of a new photonic platform for implementation of topological SSH chains, in my opinion, is more suitable for a a more specialized journal on photonics.

Response Letter

We are deeply grateful for the insightful comments and valuable suggestions provided by the reviewers, which have guided our revision. Our point-by-point responses to all comments raised by the reviewers are below, with reviewers' comments in black and our response in blue. In the revised manuscript and Supplementary materials, all amendments are marked in red and cross-referenced to the corresponding comments. We hope that these revisions and responses adequately address the reviewers' comments.

Responses to Reviewer 1

Reviewer #1: The manuscript reports the realization of an all-solid-state, reconfigurable topological laser based on an SSH waveguide array in Nd:$BaLaGa_3O_7$. The authors demonstrate single-mode lasing with output powers exceeding 100 mW, which they claim represents an improvement of more than two orders of magnitude over conventional semiconductor-based topological lasers. Furthermore, lasing is observed not only at conventional edge sites but also at trivial lattice terminations and arbitrarily reconfigurable interface positions, attributed to non-Hermitian PT-symmetry breaking. These findings are potentially important for the field of topological photonics.

Answer: We sincerely thank Reviewer #1 for carefully reviewing our work and the positive assessment of our manuscript.

I am not convinced by the current manuscript. However, I can reconsider the work if they can make major revisions. Here are some detailed comments and questions.

Answer: We sincerely thank the reviewer for the insightful comments and professional questions. In response, we have revised the manuscript and supplementary materials accordingly.

1. The authors point out that current topological lasers suffer from limitations such as low optical gain and limited output power. By implementing an all-solid-state, reconfigurable topological laser based on a SSH waveguide array platform, they claim to overcome these challenges and demonstrate a significant improvement in performance. However, the manuscript does not provide a clear theoretical explanation for why this approach should lead to much higher output power. The authors should explicitly clarify the underlying mechanism beyond simply benefiting from the inherently higher gain of solid-state media.

Answer: We thank the reviewer for their valuable comments and insightful suggestions. Compared to semiconductor laser diodes, solid-state lasers generally offer a better combination of high power and excellent beam quality. The major performance improvement of our topological laser over previously reported systems stems mainly from fundamental technical differences between solid-state and semiconductor laser platforms. Several key reasons are outlined below:

1. ***High Gain Efficiency***: Solid-state lasers use rare-earth-doped crystals/glasses (e.g., Nd^{3+}, Yb^{3+}), where emission comes from sharp 4f energy level transitions. Shielded 4f electrons yield narrow gain spectra (<1 nm) and large emission cross-sections (~10^{-19} cm^3), enabling efficient amplification. In contrast, conventional semiconductor lasers rely on broad interband transitions (gain spread over tens of nm) with emission cross-sections 10^2-10^3 times smaller, requiring high carrier densities for similar gain and thus lower efficiency.
2. ***Low Intrinsic Loss***: Solid-state lasers exhibit low intrinsic loss, with loss mainly from impurities or scattering. Semiconductor lasers suffer from free-carrier absorption, defect scattering, and facet losses, which counteract gain.
3. ***Superior Thermal Performance and Beam Quality***: Solid-state gain media offer high thermal conductivity and uniform gain distribution, enabling power scaling and stable single-mode operation by suppressing nonlinear effects and spatial hole burning. In contrast, semiconductor lasers suffer from thermal and nonlinear limitations, exciting higher-order modes that degrade beam quality and restrict output power. They also exhibit inferior stability at room temperature and are prone to catastrophic optical damage (COD). For instance, polariton lasers may dissociate at high temperatures, causing sharp gain reduction and severely limiting maximum power.

Therefore, due to the high gain efficiency, low loss, and excellent thermal performance, solid-state lasers can convert pump energy into laser output more efficiently. Moreover, when operating at the same power level, all-solid-state lasers achieve higher beam quality, resulting in substantially brighter output—defined as power per unit area per unit solid angle—than their direct diode counterparts. This allows them to overcome the efficiency bottlenecks and beam quality issues faced by semiconductor materials at high power levels, ultimately enabling higher output power. As a result, solid-state lasers could be considered as ideal platforms to achieve high power and high-quality beams, such as non-Hermitian topological lasers.

We have revised the manuscript to include concise explanation of the underlying mechanism responsible for high-power output in solid-state lasers platform (Page 3, paragraph 1, marked in red). In addition, a detailed theoretical discussion has been added in Supplementary Note S5 to further clarify the physical origin of the observed phenomena.

2. The authors claim that their output power is two orders of magnitude higher than that of conventional topological lasers. However, this statement is not entirely rigorous, as the output power can be strongly influenced by factors such as device size, pump power,

and other conditions. For example, Ref. 35 (Nat. Nanotechnol. 15, 67–72 (2020)) reports tens-of-micrometer devices already reaching mW-level emission, whereas the present structures employ a gain medium length of ~1 cm, which naturally results in higher collected output. The authors should provide the relevant comparison conditions, or alternatively, a direct comparison of external quantum efficiency would be more meaningful.

Answer: We thank the reviewer for the reminder and suggestion.

We apologize for the lack of clarity in the original manuscript regarding this point. We would like to clarify our system is based on a continuous-wave (CW) laser, not a pulsed source. For pulsed lasers, lasing power is characterized by two distinct parameters: peak power and average power. In the referenced study (i.e., Ref. 35, Nat. Nanotechnol. 15, 67–72, 2020), the reported 2 mW output refers to the peak power of a pulsed topological laser, not the average power—a point that has been confirmed by the corresponding author. As indicated in Table 1 of that work, the authors used a nanosecond pulsed pump laser (1064 nm, 5 ns pulse width, 12 kHz repetition rate) with a peak pump intensity of 15 kW cm^{-2}, corresponding to an average pump power of 0.09 W cm^{-2}, to generate a 2 mW peak output. Without considering thermal relaxation or temporal response effects, the calculated average output power of such a pulsed laser with 2 mW peak power is only about 9 μW. This value is substantially lower than the 100 mW CW output power achieved in our present work. We have revised in the page 3, paragraph 1, sentences in red.

Additionally, we have calculated the external quantum efficiency for both types of topologically structured lasers. The expression for the external quantum efficiency of the light-light pumped laser is:

$$\eta_{eqe} = \frac{P_{out} / h\nu_{laser}}{P_{absorbed} / h\nu_{pump}} = \frac{P_{out}\lambda_{laser}}{P_{absorbed}\lambda_{pump}}$$

where P_{out} is the laser output power, $P_{absorbed}$ is the pump light power absorbed by the gain medium/semiconductor, and hv is the energy of the output laser photons. According to the above formula, the external quantum efficiency in our topological laser system reaches ***17.86%***, whereas the efficiency of semiconductor laser system in Ref. 35 is ***0.14%***. In summary, the performance of topological laser in our work as a photonic device is superior to that from Ref. 35.

3. The evidence in Fig. 3f,g seems insufficient to clearly confirm lasing. I suggest adding spectral linewidth data to show narrowing from spontaneous to stimulated emission. If the below-threshold spectrum is too weak, self-interference of the emission pattern could be used to demonstrate coherence.

Answer: We thank the reviewer for the valuable suggestion.

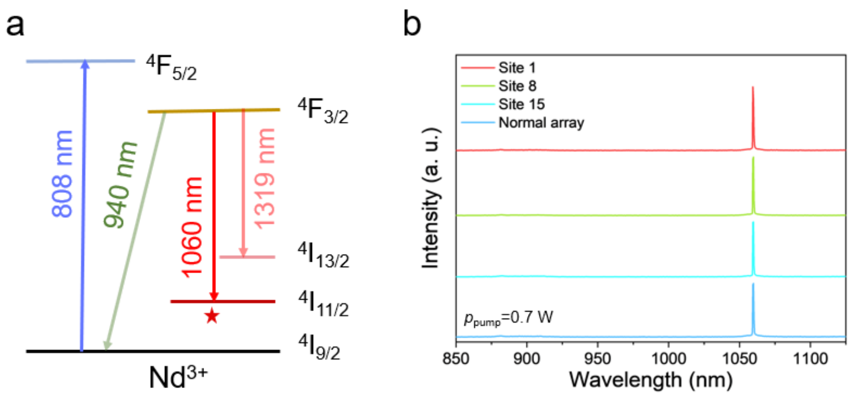

Fig. R1 (a) Energy level diagram of Nd^{3+}. (b) Laser spectra of different sites in SSH model and normal array.

To properly identify lasing behavior in our solid-state waveguide platform, two pieces of background knowledge need to be established.

First, the mechanism of solid-state lasing. In our platform, under 808 nm pumping, electrons excite to the $^4F_{3/2}$ level, producing broadband spontaneous emission. A Fabry–Perot resonator formed by coated mirrors on the crystal ends (Supplementary Fig. S9a) provides wavelength selectivity. Multiple photon modes then compete for finite gain from the population inversion. The primary transition pathway of Nd^{3+} ions is $^4F_{3/2} \rightarrow {}^4I_{11/2}$ (1060 nm). Consequently, among the broadband noise photons generated via spontaneous emission, those near 1060 nm experience preferential amplification through stimulated emission, while photons at other wavelengths are suppressed and dissipate due to mode competition. Figure R1b (Fig. 3g in main text) shows the output spectrum of a typical Nd-doped laser, consistent with other reference (e.g. *Laser Physics Letters* **2013,** *10*, 105815, *Optics & Laser Technology* 2020, 125, 106018.).

Second, the unique features of topological laser based on waveguide system. Waveguide lasers possess a number of unique features in comparison with bulk lasers (e.g., see review article Grivas *Progress Quantum Electron*. 35, 159-239 (2011)). Waveguiding phenomenon can generate light emission with some laser-like characteristics. For example, light may resonantly leak into the substrate due to resonance effects, and these "leaky waves" propagate along the waveguide, forming a narrow-band emission spectrum. It is important to note that waveguiding is inherently a linear optical process. Therefore, the most straightforward and effective method to distinguish between waveguided spontaneous emission and lasing is to check for the presence of threshold behavior (see reference *Nat. Photonics* 3, 546 (2009) “How to recognize lasing”).

Therefore, to differentiate between laser emission and fluorescence signals in our solid-state waveguide laser system, we not only characterized the optical-optical curve (Fig.

3f in main text) but also provided the output spectrum near the lasing threshold, our solid-state lasers doped with neodymium (Nd^{3+}) ions (Fig. R1a) exhibit distinct linewidth behaviors during the processes of spontaneous and stimulated emission, as we will demonstrate in Fig. R2. Specifically, as the pump power approaches the threshold, a narrow peak emerges at 1060 nm. Simultaneously, we observe a broad fluorescence peak around 900 nm with significant intensity fluctuations, confirming the inability to achieve stable laser transition (cyan curve in Fig. R2). When the pump power exceeds the threshold, this broadband signal is overwhelmed by a sharp and stable high-intensity peak at 1060 nm, demonstrating that the lasing process consumes the majority of excited-state particles and suppresses spontaneous emission (blue curve in Fig. R2). Single-wavelength laser oscillation is thereby established. This spectral transition strongly supports the conclusion that the output originates from a photon competition mechanism—which selectively amplifies a single mode while suppressing others—providing clear evidence of mode-selective amplification in the laser cavity. Relevant data and detailed analyses are provided in the Supplementary Materials Note S8 and marked in red.

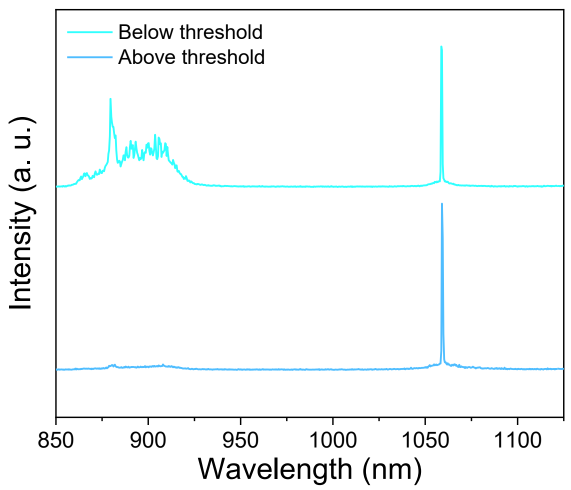

Fig. R2 Emission spectrum before and after the laser generation threshold. Since the laser power surges abruptly upon generation, spectral signal measured above the threshold is recorded after undergoing power attenuation.

To further strength our evidence, we employed a beam profiler to capture the spatial distribution characteristics of the optical field both below and above the lasing threshold. As illustrated in Fig. R3a, when the pump power is below the threshold, the fluorescence generated by spontaneous emission exhibit no directivity and cannot be effectively confined within the waveguide structure. Consequently, the majority of photons are dissipated into the surrounding environment. At this stage, only those spontaneously emitted photons whose propagation direction closely aligns with the waveguide's axial direction and whose wavelength lies near 1060 nm (corresponding to the gain spectrum peak) can undergo multiple total internal reflections within the waveguide and achieve a certain degree of round-trip transmission. This enables them to gain net amplification, thereby establishing the initial conditions for laser oscillation. Once the pump power exceeds the lasing threshold, as shown in Fig. 3b, a sufficient population inversion is achieved, leading to the dominance of stimulated emission and the eventual generation of a stable, highly collimated, and intense laser output.

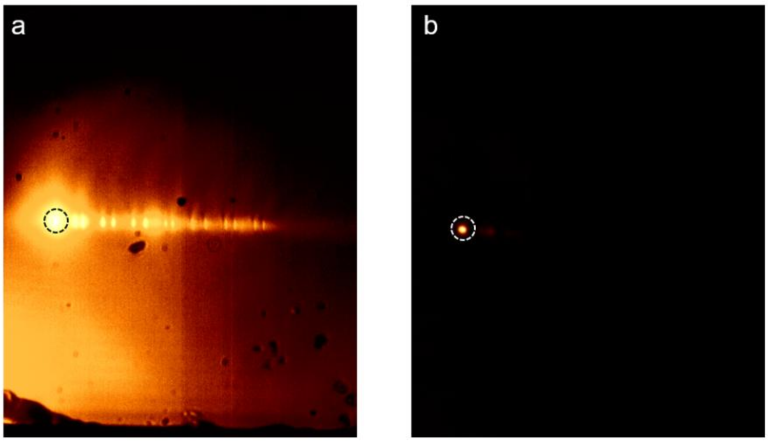

Fig. R3 Normalized optical field intensity distribution before and after laser generation.

Additionally, we also tested the polarization characteristics of the single-line waveguide laser and found that the polarization of the output laser remained well-aligned with that of the pump laser, and is polarization-dependent, as shown in the Fig. R4. This observation further confirms that the output is a gain laser, rather than fluorescence from spontaneous radiation. We have included detailed descriptions of the relevant experiments in the Supplementary materials Note S8.

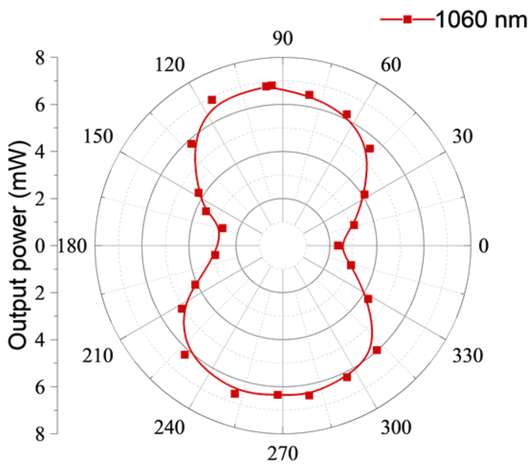

Fig. R4 Diagram of output laser power as a function of pump light polarization.

Some other technical comments:

1. In Fig.3f, the Light–Light curve under varied pump power below threshold should also be shown, as this is important for analyzing laser dynamics. Using a log–log scale

would make the variations below threshold more clearly visible.

Answer: We thank the reviewer for the useful suggestion.

In solid-state lasers, the transition from spontaneous to stimulated emission can generally be identified by a distinct kink in the output characteristics under linear-scale representation. Below the threshold, the output power remains extremely low (dominated by spontaneous emission fluorescence) and increases only gradually with pump power. On a linear scale, such weak signals are compressed near the origin, making subtle variations difficult to resolve. In contrast, a logarithmic scale significantly expands the display range in the low-power region, allowing clear visualization of sub-threshold behavior.

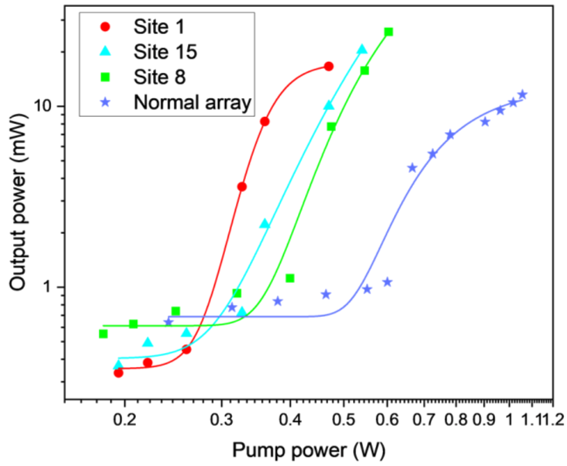

Fig. R5 Relationship between pump intensity and output power in a semi-logarithmic coordinate system. Scatter points represent data points, while the curve is a fitted curve.

To better reveal these subtle variations below threshold, we have included a log-log scale plot in Fig. R5. In this representation, the region below threshold appears as a straight line with constant slope, corresponding to spontaneous emission, while above threshold, the curve rises sharply, indicating the onset of stimulated emission. The transition point, or kink, marks the laser threshold.

We have included relevant spectral data in the Supplementary materials note S8 (sentences in red), which clearly demonstrate the nonlinear variation in output power before and after the threshold.

For clarity, I suggest indicating the pump power for each spectrum in Fig. 3g.

Answer: We thank the reviewer for the good suggestion. The pump power for all spectra in Fig. 3g was consistently maintained at 0.7 W, and now has been clearly indicated in the figure and its caption.

2.Please clarify whether Fig. 3c and Fig. 3d show simulated or experimental data, and provide the same clarification for Fig. S9b and S9c.

Answer: both Fig. 3c and Fig. 3d and Fig. S9b and S9c (now Fig. S13) present experimental data, and we have modified the according text to emphasize them in the revised manuscript and marked in red at the corresponding locations.

3.In the sentence "two edge modes coalesce at an exceptional point (PT)" on line 93 of the main text, the abbreviation appears to be incorrect; it should be (EP) to match "exceptional point" and avoid confusion with PT symmetry.

Answer: We thank the reviewer for pointing out the typo, we have revised it in the updated manuscript (Page 5, sentence in red).

4.In the Fig. 2 caption (line 117), the phrase "at leftmost lattice site together (b)" has the wrong label — it should be (a), not (b).

Answer: We have changed the labels accordingly (Page 6, sentence in red).

5.In Fig. S5(c), the blue fitting curve extends beyond the plotted data range and even outside the figure frame. If the curve was derived strictly from experimental data, why does it exceed the data boundaries? The authors should clarify the fitting procedure and correct the figure.

Answer: We thank the reviewer for the helpful question. We conducted a thorough analysis and fitted the raw data. The fitted curve was generated using MATLAB software based on the experimental data, as shown in the Fig. R6. The resulting equation is: $t=5383^{\times}e^{-0.2168\times d}$.

We then re-imported the data into Origin and used this equation to regenerate the plot. Although no boundary errors occurred during the display or plotting process in Origin, the exported PDF files showed the equation curve exceeding the boundaries. This was an oversight, which has now been corrected in the Supplementary materials and marked in red.

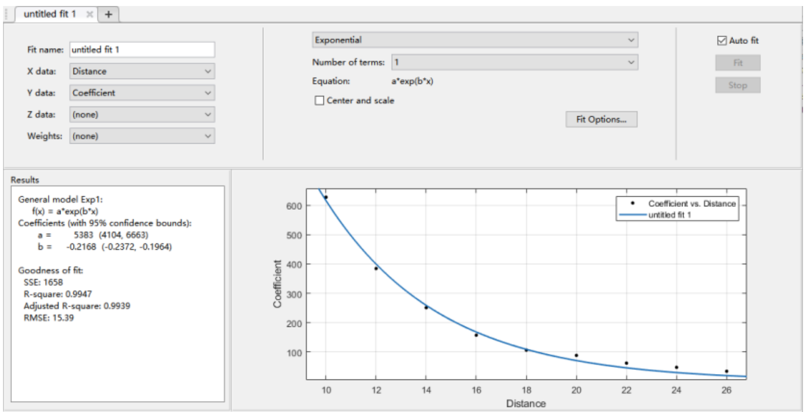

Fig. R6 Screenshot of output for MATLAB fitting.

Responses to Reviewer 2

Reviewer #2: Femtosecond laser writing has successfully created diverse topological photonic waveguides in glass, spanning symmetric/asymmetric lattices and non-Hermitian systems. Yet glass material constraints currently preclude experimental realization of topologically amplified waveguides. This manuscript reports on the demonstration of topological lasing in a novel platform, the all-solid-state laser system (self-grown disordered laser crystal Nd:BaLaGa3O7). Both theoretical analyses and experimental validations confirm that high net optical gain can induce non-Hermitian PT phase transitions at trivial edge sites and intermediate positions. This study enables localized reconfigurable topological lasing at arbitrary locations, which not only provides new insights for fundamental non-Hermitian topological photonics but also holds important value for practical applications in integrated laser systems. Both the novel platform and its findings are interesting, and the manuscript is well-organized. I recommend this paper for publication in Nature Communications after some revisions.

Answer: We thank Reviewer #2 for providing positive feedback and valuable suggestions.

1) The authors employed a self-grown disordered laser crystal as the substrate material, successfully achieving fabrication within the gain medium and constructing the gain-based non-Hermitian photonic lattice system, ultimately realizing topological lasing applications. It is worth further exploring whether similar experiments can be achieved for traditional rare-earth ion-doped laser glasses (such as Nd:glass) or common laser crystals (such as YAG, YAP, etc.). How does the choice of substrate material affect the experimental results? Do the disordered laser crystals grown by the authors possess unique physical or optical properties (such as higher nonlinear coefficients, better gain performance, or stronger damage resistance)? Are the bulk material gain properties preserved in waveguide region? It is suggested that the authors emphasize the advantages of this material in the text to highlight the innovation and contribution of this research.

Answer: We thank the reviewer for the insightful question. Theoretically, experiments can certainly be conducted with laser glass or laser crystals. In practice, we have attempted processing experiments with various solid-state laser materials. However, several challenges arise during actual processing.

First, for waveguide-based topological lattice systems, inter-waveguide coupling must be precisely optimized, which requires controlled refractive index modification (commonly known as Type I optical waveguide). In laser glass, rare-earth ion doping increases the material's light damage threshold, thereby narrowing the fabrication window for producing high-quality Type I waveguide. Even when successfully fabricated, such waveguides often suffer from significant losses. Moreover, laser glass typically exhibits a lower gain per unit length compared to laser crystals, further constraining its practical applications. In contrast, crystal materials possess intrinsic lattice order, which makes the stable fabrication of Type I waveguides inherently challenging. To address these challenges, we developed a non-traditional crystal structure—a disordered laser crystal—that lies structurally and functionally between conventional glass and crystals.

In disordered laser crystals, specific ions randomly substitute others at certain lattice sites, forming a disordered structure. This structure retains the long-range order of crystals while exhibiting the spectral broadened, inhomogeneous characteristics of glass, thus enabling efficient energy storage. Among these materials, ABC3O7-type disordered crystals belonging to the tetragonal crystal system (where A =Ba, Sr, Ca; B =La, Gd; and C = Ga, Al) have gained significant attention in ultrafast laser research. Taking $Nd:BaLaGa_3O_7$ (Nd:BLGO) as a representative example, Ba^{2+} and La^{3+} ions are randomly distributed in the interlayer lattice sites with a 1:1 ratio, resulting in a smaller emission cross-section and extended fluorescence lifetime. Additionally, the thermal conductivity of Nd:BLGO disordered crystals increases with temperature, exhibiting abnormal glass-like behavior. This anomalous thermal behavior, which is opposite to traditional trends observed in typical crystals, enhances the thermal stability of Type I modified waveguides induced by femtosecond laser processing.

Fluorescence spectrum measurements further confirm that, although the fluorescence emission intensity in the laser-modified region is weaker than that of the bulk material, as shown in Fig. R7, it remains sufficiently strong to meet the requirements of our experiments.

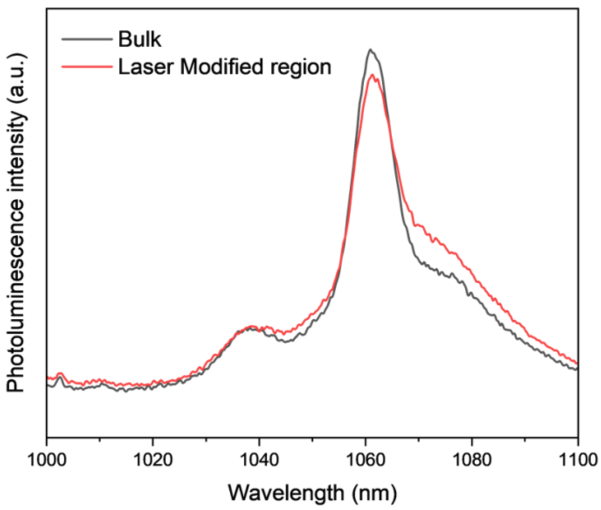

Fig. R7 Fluorescence intensity before and after laser modification

We have supplemented and clarified the relevant content in both the revised main text (Page 3, paragraph 1, marked in red) and Supplementary materials Note S6.

2) While femtosecond laser direct writing has enabled the fabrication of single-mode waveguides and waveguide arrays with exceptional longitudinal coupling properties in crystalline materials (e.g., SBN, LiNbO3), their optical stability remains a critical challenge. For instance, laser-induced refractive index modifications in these materials often exhibit gradual relaxation at room temperature, leading to degraded waveguide performance over time. Thus, for laser devices, thermal stability is a critical performance parameter. Can the disordered laser crystal material employed in this study effectively maintain stable waveguide characteristics?

Answer: We thank the reviewer for the professional question. The processing area in the disordered crystal is highly stable. The waveguide we prepared three years ago, which has been stored at room temperature, still maintains excellent transmission and laser characteristics, as shown in Fig. R8. Notably, the maximum output power of the laser has not changed significantly over this period.

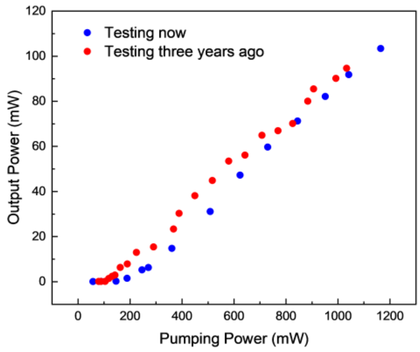

Fig. R8 Comparison of the single-line waveguide laser output characteristics: past (three years ago) vs. present

3) In Figure 2f, the theoretical simulation indicates that excitation occurs at Site 1 (trivial position) in the lattice. However, experimental results (Figures 3c-d) show excitation applied at Site 1 (topological non-trivial) and Site 15 (trivial), respectively. While this difference might not fundamentally alter the conclusions—since in a periodic SSH model, edge excitations are expected to behave similarly regardless of the exact site—the inconsistency between simulation and experiment could mislead readers when comparing the predicted and observed behaviors. It is suggested to re-optimize or change the annotation

Answer: We thank the reviewer for carefully examining the figures and pointing out the inconsistency between the annotations in the simulation and experimental results. We have redrawn Figure 2 to ensure consistency between them.

4) In the theoretical analysis, the authors primarily focus on non-Hermitian PT phase transitions under gain conditions. However, losses are inevitable in real systems: can similar transitions be achieved solely by tuning losses (e.g., via periodic dissipation)? What is the equivalence to the gain-driven mechanism?

Answer: We thank the reviewer for these interesting questions. As per the reviewer's second suggestion, while our work focuses on the impact of local gain modulation on the PT phase transitions of edge and interface modes, the same phenomenon can also occur in pure lossy system, provided that local non-Hermitian modulation is introduced. To verify this, we performed eigenvalues calculations for the SSH model with both the gain-driven ($\Gamma > 0$) and the loss-driven ($\Gamma < 0$) regime. As illustrated in Fig. R9(a, b), the eigenvalue spectra of the gain-driven and loss-driven regimes are complex-conjugated of each other, confirming they share the same underlying mechanism.

Regarding the question of whether similar transitions can be achieved solely by tuning losses, we have added a global loss of 0.3 to the models in Fig.R9(a, b), simulating the losses that typically exist in real systems, and their corresponding band structures are present in Fig. R9(c, d). The spectra experience an overall downward shift along the imaginary axis, while PT phase transition point remains unchanged. This result indicates that PT phase transition can indeed be realized in realistic systems through local loss modulation. However, in practice, loss-driven systems present experimental challenges: when the losses are sufficiently large, light decay rapidly, making the desired modes difficult to observe.

To clarify the questions raised by the reviewer, we have included these results in the updated Fig. S2 in Supplementary materials and marked in red, where Fig.S2(c) and Fig.S2(d) introduce global losses based on the configurations of (a) and (b) respectively, to simulate realistic systems.

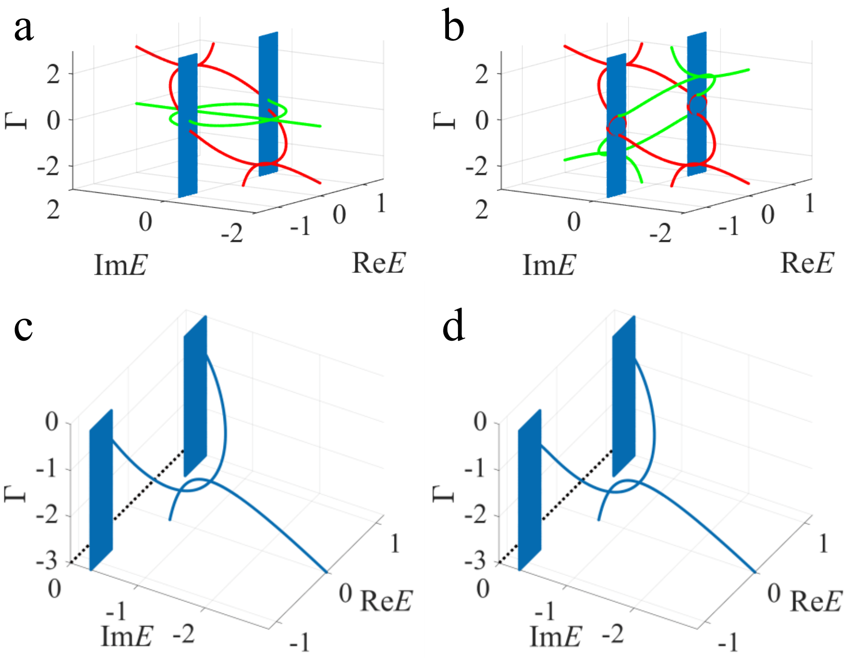

Fig. R9 The updated Fig. S2 in the Supplementary materials. The added figures (c) and (d) introduce global losses of 0.3 based on the models in (a) and (b), respectively.

5) Additionally, some formatting and detail issues need to be addressed: a) I suggest swapping the positions of Figure 2e and 2f to better align with the logical progression of data or the sequence of experimental results.

We thank the reviewer for the kind reminder, we have made the corresponding adjustments in the updated figure 2.

b) Supplementary Figure S6, the excitation site labels require correction: The current annotation for “Sites 1, 7, 9, and 15” should be replaced with “Sites 1, 7, 8, and 15” to accurately represent the experimental configuration.

We thank the reviewer for spotting the typo, we have changed the annotations accordingly in Supplementary Figure S6.

Responses to Reviewer 3

Reviewer #3: In this work, authors present an experimental realization of a topological SSH waveguide array laser based on Nd:$BaLaGa_3O_7$ crystal. They experimentally and theoretically study the active topological lasing from arbitrary sites of their structure. Experimentally reached output powers are greater than that of known reported semiconductor-based topological lasers, which makes this solid-state platform promising for the in-depth study of topological effects related to non-Hermiticity and nonlinearity. The main novelty claimed by the authors is the topological phase transition induced by the non-Hermitian gain. Although I do not doubt the validity of the experimental observation and its interpretation, essentially the same effect was already discussed in https://journals.aps.org/prl/abstract/10.1103/PhysRevLett.116.046402, where the authors show not only the breaking of SSH-chain into two distinct parts by the spatially profiled gain, which is accompanied with the emergence of couple of topological edge states, but also discuss spontaneous formation of such topological defects in the nonlinear regime according to the Kibble-Zurek mechanism. Based on this, I cannot recommend this work for publication in Nature Communications. However, the advantages of the proposed implementation of active topological SSH chain will ignite interest in a more specialized community of topological photonics.

Answer: We thank Reviewer #3 for the constructive feedback and for recognizing the validity and interpretation of our experimental results, and the advantages of our platform.

We also thank the reviewer for pointing out the interesting reference, which indeed demonstrates similar phenomena to our work, and we have accordingly cited it in the revised manuscript (Ref. 47, see Page 3 marked in red). While that work attributes the emergence of edge states to the Hermitian reservoir potential in a zigzag chain of polariton micropillars, our study focuses on topological lasing at arbitrary site on all-solid-state platform, with fundamentally different underlying physics. We have provided a detailed comparison below.

In the cited reference, the observed edge state arises from the region separation caused by finite potential barriers, which is an effect that can exist in **Hermitian systems**. By contrast, the emergence of edge and interface states in our work is exclusively to **non-Hermitian systems**. Specifically, in Fig.2(c) of the cited reference, nonresonant pumping is introduced in the bulk to excite localized modes of polariton condensate. However, as explained in that paper, the formation of the edge states results from the separation of the left and right chains by a Hermitian reservoir potential, rather than from the non-Hermitian effects.

To illustrate this mechanism, we simulate the reservoir potential via introducing an onsite energy of 1.5 at the intermediate site in the SSH model, as shown in Fig.R10. The onsite term acts as a potential barrier separating the left and right chains, producing **single** edge state localized on either side of the pumping spot, corresponding to the "diagonal" (D) and "antidiagonal" (A) direction in Fig. R10(a, b), respectively. The difference map, $(I_D - I_A)$, shown in Fig. R10(c), reproduces the spatial localization on both sides of pumping spot, consistent with the cited reference.

In contrast, the non-Hermitian edge states and their associated PT phase transition in our system originate from a local non-Hermitian perturbation and can be explained within the framework of non-Hermitian theory. The gain site and the adjacent lattice site effectively form a two-level PT-symmetric subsystem that couple to their bulk environment through self-energy corrections, as depicted in Fig. 2(b) of the main text. Consequently, the gain-induced edge states always appear in **pairs,** both before and after the exceptional point (EP), as shown in Fig.R11 and in Fig. 2(e,f). Before the EP, the two edge states are localized both at the gain site and v-coupled neighboring site, exhibiting equal amplitude but different phase (Fig. R11, a). After the EP, the PT symmetry is broken, and the two edge states become complex-conjugated partners that are predominantly localized at individual sites (Fig. R11, b). Thereby, both the number of edge states and their spatial localization features in our system are distinct from those in the cited reference.

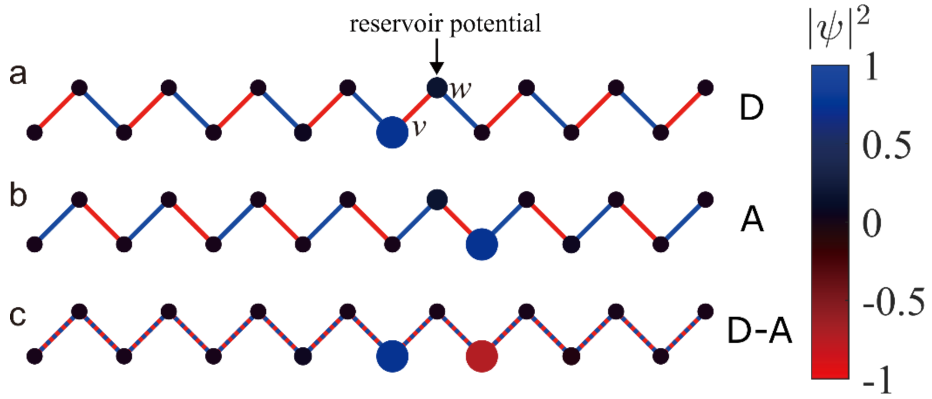

Fig. R10 Spatial distribution of interface state in SSH model with an onsite potential of 1.5 introduced at the intermediate site. The parameters are set as (a) v=0.95 and w=0.208, and (b) v=0.208 and w=0.95. (c) The field-intensity difference between (a) and (b).

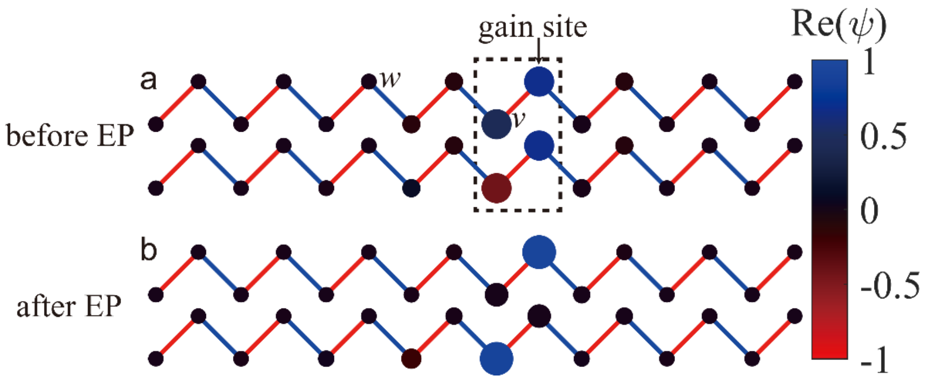

Fig. R11 The spatial distribution of interface state in SSH model with gain introduced at the intermediate site, where (a) is before the EP and (b) is after the EP. v=0.95 and w=0.208. Gain parameter is taken Γ=1.5 before EP and Γ=3 after EP.

From a platform perspective, to the best of our knowledge, the potential barrier-induced localized states of polariton condensation at arbitrary sites discussed in the cited reference remain purely theoretical and have not been experimentally realized on any platform. In contrast, our work demonstrates **experimental realization** of laser amplification at arbitrary lattice sites on a novel *all-solid-state* platform, as also noted by Reviewer #2. This platform enables stable, high-power output, as confirmed by our experimental result, and paves the way for reconfigurable, on-chip, and actively controlled photonic devices.

In summary, while the two works display similar phenomena, they are based on fundamentally different mechanisms and platforms—one theoretical and the other experimentally realized. To better highlight the merits and novelty of our work, we have further emphasized the underlying mechanism and the platform in the revised manuscript (see Page 3 marked in red). We believe these distinctions underscore the novelty and importance of our study.

In addition:

1) The authors describe the main effect of gain-induced topological transition with the reduced effective non-Hermitian Hamiltonian model, where parts of the SSH chain are replaced with a single state. The validity of this approach is illustrated with a qualitative similarity of the numerically calculated PT-symmetry breaking transition with the coupling parameter v in the two cases. Although, it appears evident that sufficiently strong coupling results in a transition in both cases, the absence of clear analytical mapping leaves the question of their equivalence somewhat open. The advantage of the proposed simplified model is especially unclear since direct numerical diagonalization of a non-Hermitian effective Hamiltonian is easily achievable in both cases.

Answer: We thank the reviewer for the insightful comment.

Regarding the comment on the absence of clear analytical mapping, we provide a rigorous proof that a general semi-infinite tight-binding model can indeed be mapped onto our reduced effective model, as detailed below. Since the bulk Hamiltonian and edge Hamiltonian are coupled through the hopping matrix *T*, the eigenvalue equations of the semi-infinite system can be written as

$$\begin{cases} H_{edge}\psi_{edge} + T\psi_{bulk} = E\psi_{edge} & (1) \\ T^{\dagger}\psi_{edge} + H_{bulk}\psi_{bulk} = E\psi_{bulk} & (2) \end{cases}.$$

From Eq. (2), we obtain

$$\psi_{bulk} = (E - H_{bulk})^{-1}T^{\dagger}\psi_{edge} \ (3).$$

Substituting Eq. (3) into Eq. (1) gives:

$$\left(H_{edge} + T(E - H_{bulk})^{-1}T^{\dagger}\right)\psi_{edge} = E\psi_{edge} \ (4).$$

The second expression in the right side of Eq. (4) is similar to the self-energy expression:

$$\Sigma(E) = T(E + i\eta - H_{bulk})^{-1}T^{\dagger} \ (5),$$

where η represents causality and is inversely proportional to the length of the system.

As the length approaches infinity, we have:

$$\lim_{\eta \to 0}\Sigma(E) = T(E - H_{bulk})^{-1}T^{\dagger} \ (6)$$

which is the second term in the right side of Eq. (4). Thus, we obtain the eigenvalue equation for the reduced Hamiltonian containing self-energy in Eq. (5)

$$H_{eff}\psi_{edge} = E\psi_{edge} \ (7)$$

where $H_{eff} = H_{edge} + \Sigma(E)$. Therefore, we can use Eq. (7) to solve the boundary spectrum for a finite system.

To further validate the accuracy of our analytical approach, we plot in Fig. R12 the variation of the critical gain parameter Γ_c at the PT-symmetry transition (exceptional point) as a function of the intercell hopping *w,* with intracell hopping *v* fixed. The black dots represent results obtained from direct numerical diagonalization, while the red solid line and the blue dashed line denote the real and imaginary parts of the analytical results, respectively. The excellent agreement between the two confirms the accuracy and reliability of our analytical model.

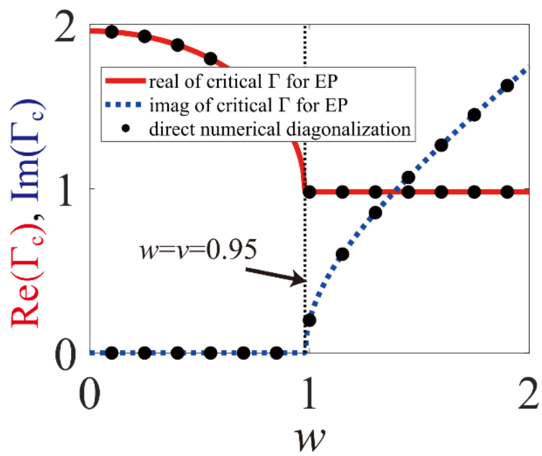

Fig. R12 The critical Γ for EP under various w from 0 to 2, fixing v=0.95. (a) Introduce gain at the boundary site, (b) Introduce gain at the intermediate site. The red solid line represents the real of Γ, while the blue dashed line represents the imaginary of Γ, both obtained using analytical methods. The black dots are calculated by direct numerical diagonalization.

Regarding the reviewer's question on the advantage of the reduced Hamiltonian, our analytical approach not only simplifies the complex system into an effective two-level PT-symmetric model, but also yields explicit criteria (e.g. Eq. (S3.3) and Eq. (S4.4)) for determining the condition of PT phase transition of edge and interface states. These analytical relations allow one to readily assess whether a PT phase transition can occur under a given local non-Hermitian perturbation. For instance, Eq. (S3.3) shows that the critical Γ_c becomes complex in the topologically nontrivial regime ($w>v$), indicating that a PT transition cannot be achieved solely by varying the gain strength. In contrast, Eq. (S4.4) shows that, when gain is introduced at an intermediate site, the PT transition always occurs regardless of whether $w>v$ or $w<v$, consistent with the numerical results. We have detailed the relevant content in Supplementary Material S2 and S3 and highlighted it in red.

2) It is somewhat unclear why the Fibonacci quasi-periodic positional modulation was used to simulate the effect of structural disorder. Is there any reason why it was preferred over the random positional anti-diagonal disorder affecting the coupling between neighbouring sites or a random variation of waveguide cross-section size simulating diagonal disorder by affecting the resonant mode energies?

Answer: We thank the reviewer for the great question.
In femtosecond laser-written topological waveguide arrays, conventional disorder models typically introduce random perturbations to lattice sites. However, practical fabrication imperfections—such as motion stage backlash—often result in spatially correlated errors over extending multiple sites. These correlations are more accurately captured by quasi-periodic modulation of Fibonacci sequences, rather than by

uncorrelated randomness. Critically, Fibonacci based "disorder" offers a distinct experimental advantage: it enables deterministic and reproducible control over waveguide positions or coupling coefficients via programmable laser writing, thereby avoiding the impracticality of independently tuning random parameters and the associated experimental errors. Remarkably, our measurements revealed neither spectral broadening nor power instability despite the disorder. The laser linewidths consistently maintained sub-nanometer resolution ($\Delta\lambda \approx 0.6$ nm), and variations in lasing thresholds and peak power across lattice sites were negligible. This robustness conclusively demonstrates the topological protection of gain-guided modes against both engineered and accidental disorder.

In fact, introducing Fibonacci sequence-based disorder in finite lattice sites yields results nearly indistinguishable from those obtained with purely random disorder. To confirm this, we have implemented random positional disorder within the fabricated lattice grid and carried out measurement on the new sample. Experimental results show again the stable laser outputs at localized sites 1, 8 and 15 in the Fig. R13, demonstrating the intrinsic resilience of the topological mode lasing to disorder irrespective of its statistical nature. We have added relevant discussion in Supplementary Material Note S8 and marked it in red.

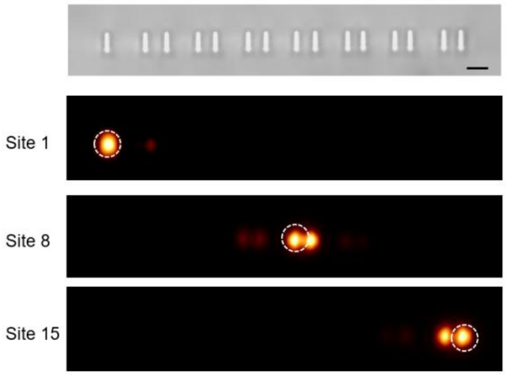

Fig. R13 Active laser output map under random position disorder conditions. Maximum position offset: 2 μm.

Response Letter

We sincerely thank Reviewer 1 and Reviewer 2 for their positive feedback on our work. We are also very grateful to Reviewer 3 for the insightful comments, which are definitely helpful to improve the quality of our paper. We have carefully addressed each of the reviewer's points—their original comments are provided in black, and our corresponding responses are highlighted in blue. The changes made in the revised manuscript have been marked in red. We believe that these revisions and clarifications have fully addressed the reviewer's concerns and questions.

Reviewer #3 (Remarks to the Author):

The authors addressed all the raised points and improved the quality of the revised manuscript. However, in some cases I found their argumentation unconvincing and factually incorrect. Most notably, in the reply to the first raised comment:
1) In the cited reference, the observed edge state arises from the region separation caused by finite potential barriers, which is an effect that can exist in Hermitian systems...
The statement in the first sentence is incorrect: in the cited work the effect of spectrum modification and lasing from localized modes in the vicinity of an excitation spot was caused by a local non-Hermitian potential term $U_R+i\gamma$ in Eq. (4), accounting for both local self-energy shift and local gain, induced by the nonresonant optical excitation. The authors then proceed by elaborating on the incorrect statement and comparing the effect a local purely Hermitian perturbation with that of the non-Hermitian one. This comparison is by itself interesting but somewhat irrelevant to my original comment: the effect of lasing from localized modes near the excitation spot in SSH chains, described by a local non-Hermitian perturbation of a topologically nontrivial Hamiltonian, was proposed in 2016.

We thank the reviewer for the careful assessment of our response letter, and apologize for the earlier misunderstanding of the reviewer's comment.

We agree with the reviewer that comparing a purely Hermitian local perturbation with the non-Hermitian pumping-induced mechanism in the cited work does not directly address the reviewer's original point. Our intention in the earlier response was to highlight that localization can also occur in Hermitian settings, but we now recognize that this discussion was tangential and risked obscuring the key non-Hermitian physics in the cited study. We have therefore removed this comparison from the revised manuscript.

After carefully re-examining the cited reference, we fully agree with the reviewer that the cited paper is the first to propose lasing from localized modes near the excitation spot in SSH chains through a local non-Hermitian perturbation applied to a topologically nontrivial Hamiltonian. We have now included an explicit acknowledgement of this contribution in the Introduction of the revised manuscript (page 3, paragraph 2, sentences in red). We have also ensured that no statements in the manuscript imply priority over this established result. We hope these revisions can address the reviewer's concerns.

2) Indeed, the Hermitian effect related to a local potential barrier, addressed by the authors, has not been demonstrated in the polariton platform to the best of my knowledge. However, the effect proposed in the cited work [Solnyshkov, Nalitov & Malpuech, PRL 2016] is non-Hermitian, so the authors continue elaborating on an incorrect statement. The effect of local lasing in the bulk of an SSH chain due to spatially focused optical excitation spot, on the contrary, was observed in polaritonics (see, for example, a recent work [Georgakilas et al., Sci. Adv. 11, eadt8645 (2025)]). Without doubt, the new material platform brought forward by the authors, has certain advantages for applied photonics, but I do not see how that addresses my comment.

We sincerely thank the reviewer for bringing the recent work by Georgakilas et al. (Sci. Adv. 11, eadt8645 (2025)) to our attention. After carefully studying this paper, we agree that it presents an important experimental demonstration of selective polariton condensation in SSH lattices via localized optical excitation. We have now cited this work in the revised manuscript. At the same time, we would like to clarify a key distinction between their results and the non-Hermitian lasing phenomena investigated in our work: whether localized pumping excites topological interface modes in the SSH chain.

In the cited work, polariton condensation is observed either in the topological edge mode at the boundary or in trivial bulk modes when the excitation is positioned near the center of the chain. As explicitly stated by the authors (see their text and Fig. 2C,E,F,H, Fig. 3A, middle and right panels): "Exciting at the center of the chain allows for changing between the binding and antibinding s- and p-band bulk modes … by adjusting the resonance." And "The highest intensity of the condensate appears at the edges in the case of the edge state (Fig. 3A, left panel) and at the center for the bulk modes (Fig. 3A, middle and right panels)". Thus, the modes excited in the bulk of the SSH chain are the trivial bulk modes, rather than topological interface modes inside the bulk. These observations are physically distinct from the phenomena we report. In our work, spatial localization and lasing occur in **newly formed edge** and **interface modes** created by a local non-Hermitian perturbation—states that do not exist in the Hermitian SSH chain.

For this reason, we firmly agree that the polariton work is relevant and have cited it appropriately (We have revised in the page 12, paragraph 2, sentences in red), but we maintain that its experimental observations may not constitute local lasing of interface states in the SSH chain. We thank the reviewer again for pointing us to this important reference, and we believe the clarifications above strengthen the discussion and improve the accuracy of our manuscript.

3) In the reply to the second point the authors explain in detail the employed approach, which, as now is clear, reduces to a partial block diagonalization of the Hamiltonian, resulting in a second-order perturbation theory energy correction (5), which is indeed in remarkable agreement with the exact result. The added supplementary sections greatly elucidate the theoretical framework.
Answering my last comments, the authors experimentally compared the effect of the previously employed Fibonacci disorder with that of a statistically uncorrelated one and found no measurable difference, which answered my question. This point is now also clarified this point in the Supplementary Materials.

We sincerely thank the reviewer for the positive assessment of our revised theoretical analysis and the newly added experiments.

Although the revised work shows great improvement over the initial submission, the reply letter shows that the authors misinterpreted the earlier theoretical prediction belonging to the same class of non-Hermitian topological effects, which is still reflected in the revised manuscript.

We thank the reviewer for pointing out the remaining ambiguity in our interpretation of the cited theoretical reference. In response, we have carefully revised the relevant statements in the Introduction of the revised manuscript to ensure an accurate and unambiguous description of the prior work. Specifically, we now explicitly acknowledge that **"We notice that an earlier work reported the emergence of interface modes at the centre of a zigzag chain of polariton micropillars via local non-Hermitian pumping in an SSH chain"** (the page 3, paragraph 2, marked in red). This revision removes the earlier misinterpretation and properly situates our contribution within the reported class of non-Hermitian topological effects. We appreciate the reviewer's guidance, which has helped us improve the precision and clarity of the manuscript.

As the main novelty claim is based on the first effect observation of the effect, which has been previously shown, it should also be significantly diluted. Demonstration of a new photonic platform for implementation of topological SSH chains, in my opinion, is more suitable for a a more specialized journal on photonics.

We thank the reviewer for the comment regarding the novelty claim. While we fully acknowledge that the cited work first proposed the concept of generating a localized interface state in an SSH chain via local non-Hermitian pumping, our work advances this direction in several fundamental and experimentally validated ways.

First, the physical origin of the localized state in the cited work remains ambiguous: it is unclear whether the non-Hermitian term $i\gamma$ in the local potential UR+$i\gamma$ is essential for the formation of the interface mode, or whether the phenomenon could also arise from purely Hermitian perturbations. As we showed in our earlier responses, similar localization can indeed occur under a local Hermitian potential in a tight-binding model. By contrast, in our work we explicitly demonstrate both theoretically and experimentally that the emergent interface states originate purely from non-Hermitian effects and cannot be reproduced within any Hermitian description.

Second, the prior work does not clarify how the non-Hermitian perturbation modifies the system's topological properties or how the new eigenstates emerge from a non-Hermitian band theory perspective. In our manuscript, we provide a developed theoretical framework based on non-Hermitian topology and a simplified PT-symmetric model, offering a clear and quantitative explanation of how non-Hermitian terms reshape the spectrum and generate new interface states.

Third, our work presents the first experimental realization of reconfigurable, site-selective topological lasing at trivial edge or interface modes that do not exist in the Hermitian SSH chain. The effect proposed in the cited theory has not yet been experimentally realized in polariton platforms, as we explained in detail in our prior response.

For these reasons, namely, the clarified non-Hermitian origin, the development of a non-Hermitian PT-symmetric model, and the first experimental demonstration of reconfigurable topological lasing, we believe that the novelty and breadth of our results go beyond a platform-specific advance and meet the merits of *Nature Communications*.

The authors addressed all the raised points and improved the quality of the revised manuscript. However, in some cases I found their argumentation unconvincing and factually incorrect. Most notably, in the reply to the first raised comment:

1) In the cited reference, the observed edge state arises from the region separation caused by finite potential barriers, which is an effect that can exist in Hermitian systems. By contrast, the emergence of edge and interface states in our work is exclusively to non-Hermitian systems. Specifically, in Fig.2(c) of the cited reference, nonresonant pumping is introduced in the bulk to excite localized modes of polariton condensate. However, as explained in that paper, the formation of the edge states results from the separation of the left and right chains by a Hermitian reservoir potential, rather than from the non-Hermitian effects.

The statement in the first sentence is incorrect: in the cited work the effect of spectrum modification and lasing from localized modes in the vicinity of an excitation spot was caused by a local *non-Hermitian* potential term $U_R+i\gamma$ in Eq. (4), accounting for both local self-energy shift and local gain, induced by the nonresonant optical excitation. The authors then proceed by elaborating on the incorrect statement and comparing the effect a local purely Hermitian perturbation with that of the non-Hermitian one. This comparison is by itself interesting but somewhat irrelevant to my original comment: the effect of lasing from localized modes near the excitation spot in SSH chains, described by a local non-Hermitian perturbation of a topologically nontrivial Hamiltonian, was proposed in 2016.

2) From a platform perspective, to the best of our knowledge, the potential barrier-induced localized states of polariton condensation at arbitrary sites discussed in the cited reference remain purely theoretical and have not been experimentally realized on any platform. In contrast, our work demonstrates experimental realization of laser amplification at arbitrary lattice sites on a novel all-solid-state platform, as also noted by Reviewer #2. This platform enables stable, high-power output, as confirmed by our experimental result, and paves the way for reconfigurable, on-chip, and actively controlled photonic devices.

Indeed, the Hermitian effect related to a local potential barrier, addressed by the authors, has not been demonstrated in the polariton platform to the best of my knowledge. However, the effect proposed in the cited work [Solnyshkov, Nalitov & Malpuech, PRL 2016] is non-Hermitian, so the authors continue elaborating on an incorrect statement. The effect of local lasing in the bulk of an SSH chain due to spatially focused optical excitation spot, on the contrary, was observed in polaritonics (see, for example, a recent work [Georgakilas et al., Sci. Adv. 11, eadt8645 (2025)]). Without doubt, the new material platform brought forward by the authors, has certain advantages for applied photonics, but I do not see how that addresses my comment.

3) In summary, while the two works display similar phenomena, they are based on fundamentally different mechanisms and platforms—one theoretical and the other experimentally realized. To better highlight the merits and novelty of our work, we have further emphasized the underlying mechanism and the platform in the revised manuscript (see Page 3 marked in red). We believe these distinctions underscore the novelty and importance of our study.

Although the revised manuscript acknowledges the earlier theoretical prediction, it incorrectly explains the difference between the two effects on the basis of Hermiticity of the former. As explained above, the earlier theoretical prediction was made with the *non-Hermitian* model.

In the reply to the second point the authors explain in detail the employed approach, which, as now is clear, reduces to a partial block diagonalization of the Hamiltonian, resulting in a second-order perturbation theory energy correction (5), which is indeed in remarkable agreement with the exact result. The added supplementary sections greatly elucidate the theoretical framework.

Answering my last comments, the authors experimentally compared the effect of the previously employed Fibonacci disorder with that of a statistically uncorrelated one and found no measurable difference, which answered my question. This point is now also clarified this point in the Supplementary Materials.

Although the revised work shows great improvement over the initial submission, the reply letter shows that the authors misinterpreted the earlier theoretical prediction belonging to the same class of non-Hermitian topological effects, which is still reflected in the revised manuscript. As the main novelty claim is based on the first effect observation of the effect, which has been previously shown, it should also be significantly diluted. Demonstration of a new photonic platform for implementation of topological SSH chains, in my opinion, is more suitable for a more specialized journal on photonics.