nature portfolio

Peer Review File

Dynamic control of 2D non-Hermitian photonic corner skin modes in synthetic dimensions

Corresponding Author: Dr Mahmoud Jalali Mehrabad

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

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
In this work, the authors have extended the time-multiplexed lattices into higher dimensions by incorporating two additional side loops. Based on this construction, the authors have demonstrated several key results: dynamically tuning of the localization of the topological corner states, adiabatic steering of the corner states along predefined trajectories and examination of the robustness of the non-Hermitian skin effect against disorder.

Overall, the manuscript is well-written, and the results are very interesting, showing excellent agreement with the theoretical predictions. I am certainly inclined to recommend the publication of this work. But before I do so, I have some technical questions that I hope the authors can clarify:

1. The extension of 'two-loop' architecture into 'four-loop' is not new, which has already been theoretically proposed (Science 336, 55-58 (2012)) and experimentally studied (Phys. Rev. Lett. 123, 253903 (2019)) in earlier works. One major difference between the current experimental platform with the previous one is that instead of allocating the loop length equally in the four loops (30 km, 30 km, 30 km + 600m, 30 km + 6m), here the authors have chosen a different way of allocation (3 km, 3 km +100 m, 0 m, 3 m). Is there a practical concern over this length allocation difference?

2. In Fig. S1(a), the authors have just showed 1 PD for the up channel in the entire loop configuration. Should there be another PD for the down channel as well? I am also quite curious about the role of the EDFA directly before the PD.

3. I am particularly surprised with the experimental results in Fig.3. Based on the background color, it seems that the signal to noise ratio decreases from step 1 to step 17, but increases from step 17 to 37, both for the dynamical control and tweezing cases. This is quite counter-intuitive, as I expect the signal to noise ratio to be always decreasing with respect to time in such a non-Hermitian system, as demonstrated in Fig. 2(b) and Fig. S6. If this is the case here, is there any explanation behind this observation?

4. Recently there have been works demonstrating how NHSE could shape the wavefunctions of topological modes in a mechanical system (Nature 608, 50-55 (2022)). It seems that similar SSH lattices can also be realized in the current 2D set-up (changing the 50/50 coupler to a variable beamsplitter). The results would be much richer if you can take this into consideration.

Reviewer #2

(Remarks to the Author)
This paper experimentally realized a specific type of two-dimensional non-Hermitian tight-binding lattice using pulsed light in time bins in fiber loops and demonstrated a two-dimensional non-Hermitian effect that can be controlled dynamically by slowly varying the lattice Hamiltonian parameters.

While this work presents some new results in (i) the realization of dynamical control of 2D non-Hermitian skin effect and (ii) the realization of 2D non-Hermitian skin effect on this specific experimental platform (synthetic time dimension), the significance of these results is limited to the field of topological photonics. 2D non-Hemermitian skin effect has been realized in [Zhang, X., Tian, Y., Jiang, JH. et al. Observation of higher-order non-Hermitian skin effect. Nat Commun 12, 5377 (2021)]. The dynamical control of the 2D non-Hermitian skin effect does not give rise to any qualitative difference. Therefore, I don't find the significance and potential impact of the paper sufficient to be published in Nature Communications. After adequately addressing the following comments, the paper may be publishable in a more specialized journal.

(1) The authors wrote, "two-dimensional non-Hermitian photonic skin effect, that is, corner states," which I find misleading as corner states usually refer to the boundary state from 2D high-order topological insulator (HOTI). The latter are bound states (discrete energies outside the continuum), while the skin effect, 1D or 2D, comes from bulk states (continuum).

(2) The authors claim that their dynamical variation of parameters for such a non-Hermitian system is adiabatic. However, the adiabatic theorem is based on having no crosstalk between eigenstates. In such a non-Hermitian system with open boundary conditions, the eigenstates are not orthogonal, and thus, the adiabatic condition is not fulfilled, or at least not well defined. It is better to term it as a gradual control.

(3) In the results reported in Fig. 4, I don't find it too meaningful to test the robustness of the skin modes against disorders. As these skin modes are just a localized version of the bulk states, they are not necessarily robust against disorders. Unless the authors can provide or refer to a convincing theoretical argument on the robustness of non-Hermitian skin effects against disorders, this part of the results could be misleading.

Reviewer #3

(Remarks to the Author)
In the manuscript by Zheng and co-authors, entitled “Dynamic control of 2D non-Hermitian photonic corner states in synthetic dimensions”, the authors experimentally realize a synthetic non-Hermitian lattice using time-multiplexed optical setup. They successfully demonstrate emergence of higher-order non-Hermitian boundary states localized at the corner of four domains with different complex hopping.
Time-multiplexed latices have indeed been shown to be a versatile platform for realization of topological phenomena, and this work is another demonstration of how such optical platform can be used to realize fascinating physical properties, in this case emerging from nontrivial topology in complex energy space. Here the authors predict that in their platform they can attain nontrivial topological characteristic (winding of the complex energy bands), which gives rise to the emergence of corner states.
More importantly, here the authors also show two other interesting results directly stemming from the versatility of the experimental platform 1) the possibility to dynamically control topology of synthetic dimensional domains to move corner modes around and change degree of their localization and 2) to prove topological robustness by introducing disorder into non-Hermitian parameters of the lattice. These are truly impressive results showcasing how powerful the experimental platform is.

The paper is very nicely written, and the supplement aids understanding when the main text appears vague. The results are very clearly presented. Therefore, I have only a couple of minor suggestions which I believe would further improve readability and rigor of the work.
1) In Fig. 1(b) loops in planes corresponding to several fixed values of k_y are shown, however, the plotting style gives an illusion that the loops are not laying in planes. I am afraid this may cause a confusion of a reader. I recommend replotting the loops on planes and stacking these planes vertically to make it clear that they lay in planes of constant k_y.
2) The manuscript would benefit from stating some sort of general argument, a bulk boundary correspondence principle, which would explain how flipping complex hopping in four domains leads to the formation (or even guarantees) emergence of the corner states. While the authors cite earlier works, this could make text more complete and accessible to a novice reader.
3) In Fig. 5 the authors plot average displacements as curves, which is odd. Please use some scatters and also show standard deviations. (This is also Nature Comm policy, I believe).
4) Finally, I suggest softening a very strong claim the authors made in the abstract about time-multiplexed latices, namely “effects free of geometric restrictions”. Time-multiplexed latices have their own geometric constraints, which is evidenced by the limited size of the domain the authors realize. Please rewrite the sentence in the abstract.

To summarize, provided the above minor revision is done, I believe this work will be of a major interest to very broad research community. I am also looking forward to seeing how this experimental platform will evolve to showcase even more exciting non-Abelian and Floquet topological phases.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The authors have addressed my questions satisfactorily. I recommend this work for publication.

Reviewer #2

(Remarks to the Author)
The paper has benefited from the revision. While I still don't think the robustness of the corner skin modes against disorder is "similar to that of Hermitian topological systems where two regions with different winding numbers (or topological invariants) show edge modes" as the authors claim in the revised supplementary text, I would not oppose the publication of this paper in Nature Comms.

Reviewer #3

(Remarks to the Author)
The authors have fully addressed my criticism as well as adequately answered questions and points raised by other reviewers, some of which were rather unsubstantiated and based on too vague/generic statements. I again would like to strongly support publication of this work, this time in its present form.

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Reviewer #1
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In this work, the authors have extended the time-multiplexed lattices into higher dimensions by incorporating two additional side loops. Based on this construction, the authors have demonstrated several key results: dynamically tuning of the localization of the topological corner states, adiabatic steering of the corner states along predefined trajectories, and examination of the robustness of the non-Hermitian skin effect against disorder.

Overall, the manuscript is well-written, and the results are very interesting, showing excellent agreement with the theoretical predictions. I am certainly inclined to recommend the publication of this work. But before I do so, I have some technical questions that I hope the authors can clarify:

Answer:
We thank the reviewer for their very positive feedback on our manuscript.

1. The extension of ‘two-loop’ architecture into ‘four-loop’ is not new, which has already been theoretically proposed (Science 336, 55-58 (2012)) and experimentally studied (Phys. Rev. Lett. 123, 253903 (2019)) in earlier works. One major difference between the current experimental platform with the previous one is that instead of allocating the loop length equally in the four loops (30 km, 30 km, 30 km + 600m, 30 km + 6m), here the authors have chosen a different way of allocation (3 km, 3 km +100 m, 0 m, 3 m). Is there a practical concern over this length allocation difference?

Answer:
We thank the referee for pointing out this subtle difference. First, we would like to clarify that it is the net difference between the lengths of the loops that set the time bins. In that sense, our setup is similar to that of the mentioned PRL 123 (2019) paper. We use a 3 m length difference for lattice sites along the X-axis and a 100 m difference for sites along the Y-axis. The PRL paper cited by the Referee uses 6 m and 600 m for time-bins along the X and Y axis, respectively. The total fiber length in each loop is set by the response time of the modulator used. In PRL 2019, the authors used acousto-optic modulators. Their slow response time necessitated the use of very long fiber lengths (30 km) so that the modulators could be switched within time bins. In contrast, we use electro-optic modulators with a response time of ~ 100 ps.

This allows us to use an order of magnitude smaller fiber lengths of ~ 3 Km, and therefore, reduce the losses in the fiber loops.

To highlight this difference, we have revised the text as follows:
“{We note that in our designed allocation configuration, in particular, the time delay durations, we have chosen shorter delays than in previous 2D synthetic works} \cite{schreiber20122d,muniz20192d}. This is because, unlike the previously mentioned works where AOMs were used for switching, we have used EOMs which are much faster and can accommodate shorter delays.”

2. In Fig. S1(a), the authors have just shown 1 PD for the up channel in the entire loop configuration. Should there be another PD for the down channel as well? I am also quite curious about the role of the EDFA directly before the PD.

Answer:
We thank the referee for pointing out two more subtle features of our experiential setup, which we address one by one below:

PD for the down channel:
Indeed as the referee mentioned, one can append another PD in the down channel to measure the response of the down channel. Nevertheless, as we show below using simulations for the example of localization of light at the corner skin mode, the second PD will simply give similar results as the other channel. Therefore, in our experiment, we don’t use another PD in the down channel. We have included this discussion in the revised Supplementary Section on Experimental Setup.

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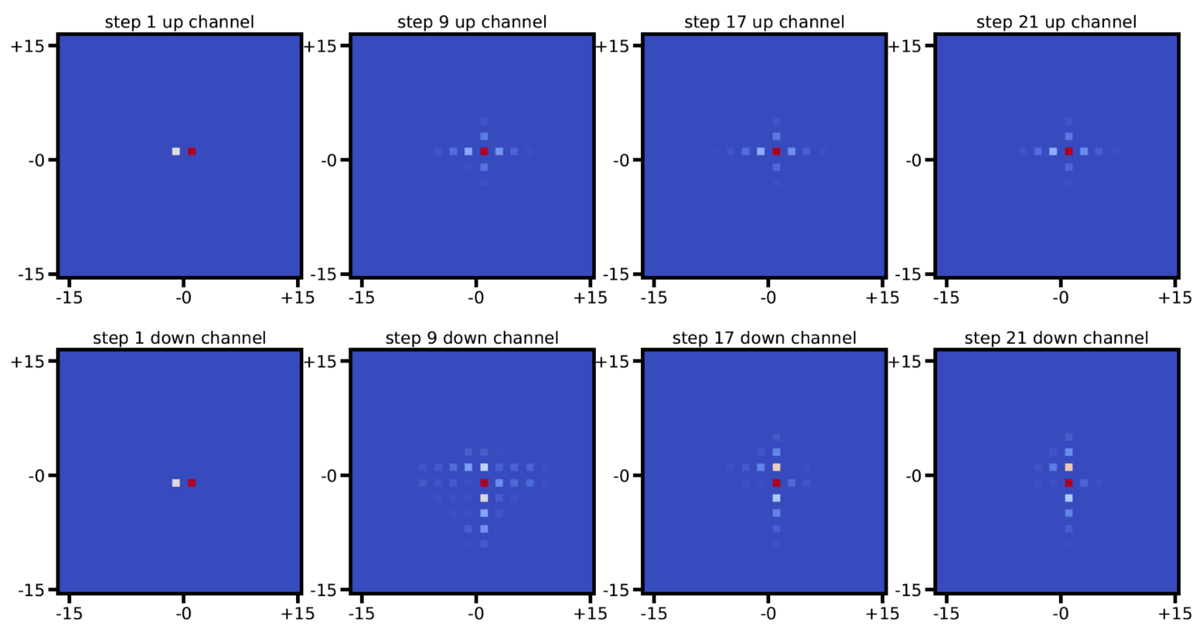
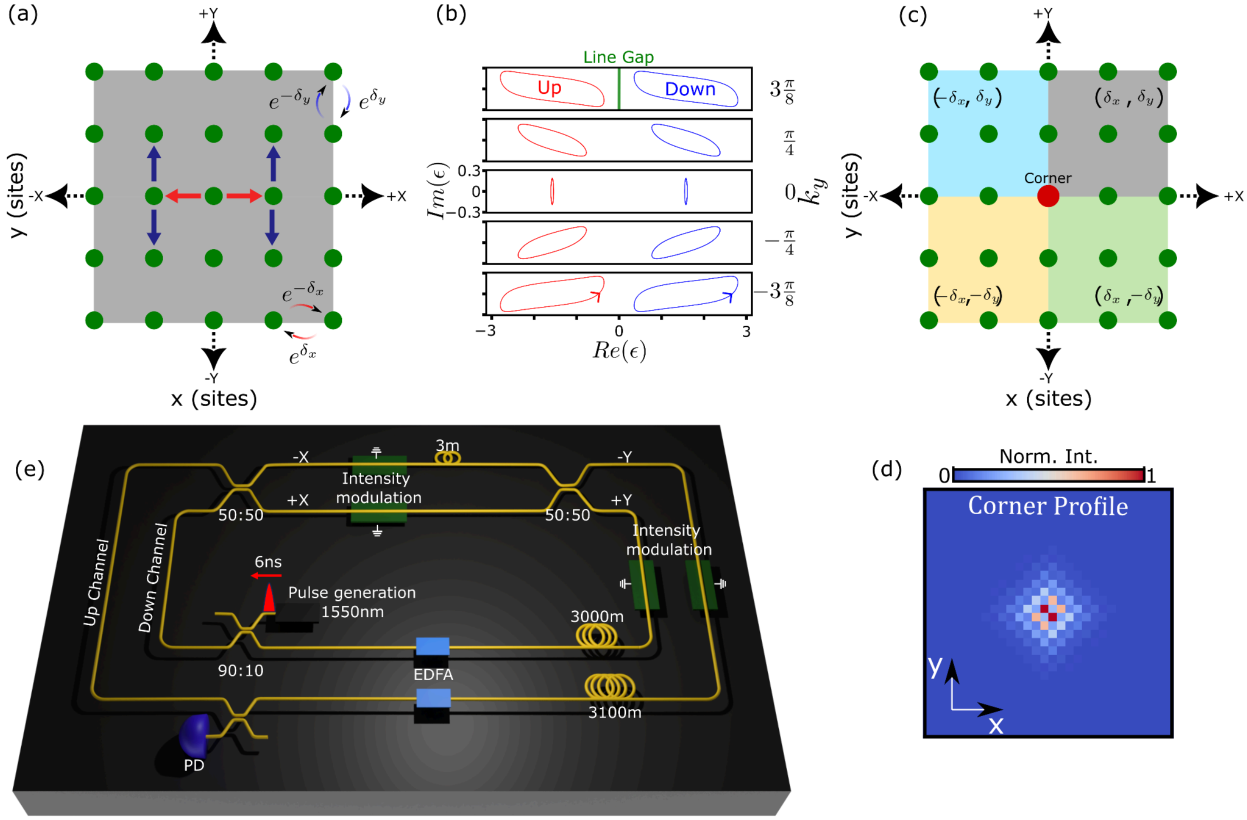


Moreover, motivated by the referee's question, to avoid this confusion, we have removed the down channel's PD from the Schematic of the setup in Fig 1, as shown below in panel (e), and updated Fig 1 in the revised manuscript accordingly.



The role of the EDFA before the PD:
In our setup, the EDFA before the PD is merely used for the amplification of the light pulses coming out of the quantum walk experiment, making it easier for the PD to detect it.

To address this question, we have added a clarifying sentence to the experimental setup section of the SI as follows:

"Note that the EDFA placed immediately prior to the up channel's PD is merely used to amplify the light pulses coming out of the quantum walk experiment, making it easier for the PD to detect it."

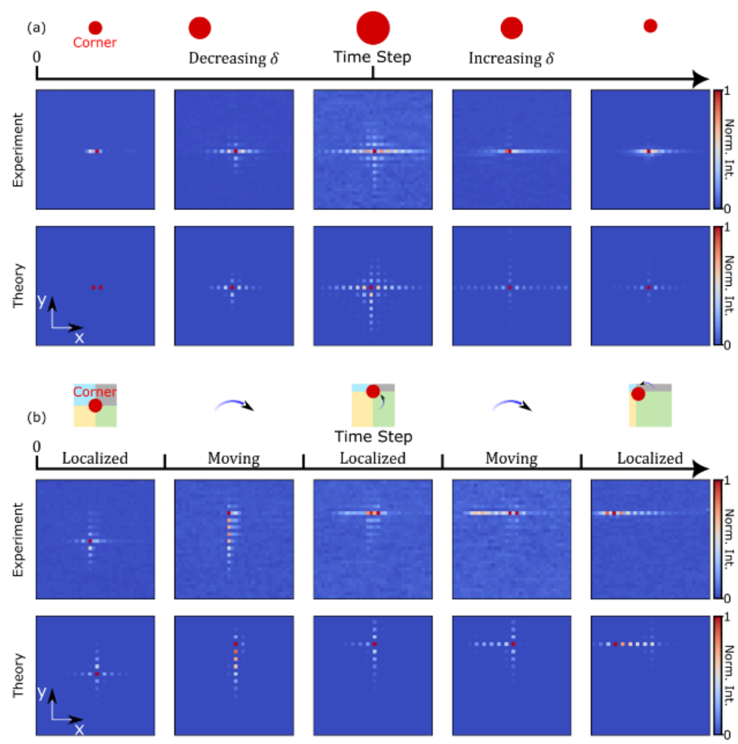
3. I am particularly surprised with the experimental results in Fig.3. Based on the background color, it seems that the signal to noise ratio decreases from step 1 to step 17, but increases from step 17 to 37, both for the dynamical control and tweezing cases. This is quite counter-intuitive, as I expect the signal to noise ratio to be always decreasing with respect to time in such a non-Hermitian system, as demonstrated in Fig. 2(b) and Fig. S6. If this is the case here, is there any explanation behind this observation?

Answer:

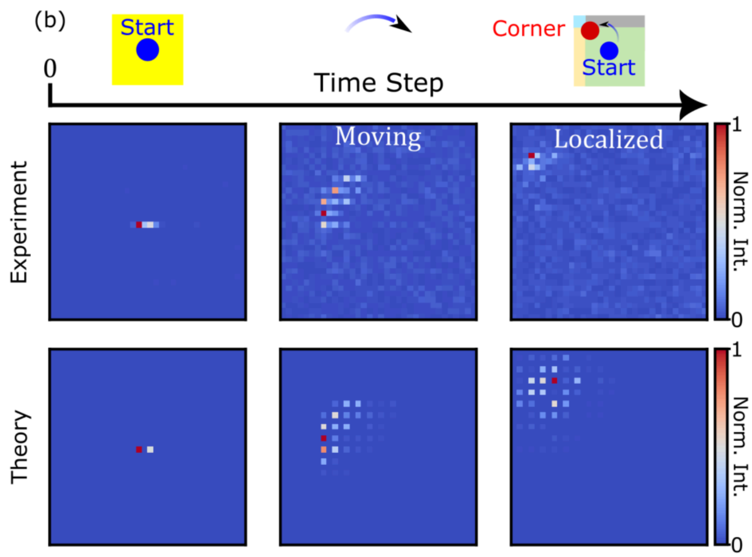
In response to the referee's comment, below we explain the SNR in every figure that the referee has referred to.

For Figure 3, we note that the **SNR** is determined by **two factors**: 1- the absolute value of the signal pulse amplitude (importantly, for which the degree of localization needs to be taken into account) and 2 - the absolute value of the noise amplitude. For our experiment related to Figure 3, we can see that the amplitude of the noise is gradually decreasing as a function of the time step, as expected. However, from step 1 to step 17, the light gets delocalized (i.e. less confined at the corner). Namely, from step 1 to step 17, the signal pulses transform from a few high-amplitude pulses located at the corner to a larger number of pulses distributed in the lattice whose maximum amplitude is smaller. Thus, the maximum pulse amplitude (red dot) at step 17, which represents the largest amplitude of the pulses, corresponds to a smaller value compared to the red dot at step 1, and hence the noise becomes more visible at step 17. From step 17 and onwards, the process from step 1 to step 17 is reversed and the pulses return to a pattern of a few high amplitude pulses relocalized at the corner, so the noise becomes less visible.

For the tweezing case, there is a similar explanation as for Figure 3. The absolute value of the noise simply decreases as expected as a function of the time step, but for those steps for which the absolute value of the maximum pulse amplitude is relatively smaller than other times steps, the noise becomes more visible. For example, note that for the last column of the figure panels for the tweezing case, since we have waited for a long enough step number, the absolute value of the highest pulse amplitude becomes high so that the noise becomes less visible. During the tweezing process, however, the pulses can be less localized, so that the absolute value of the highest signal pulse is smaller, and the noise becomes more visible.



Finally, for Figure 2(b) and related data shown in the supplements, the same argument as above applies. For the panels in the middle column, light is diffusively spreading out, and for the last column, the light is being stabilized at the corner. Importantly, the largest amplitude of the pulses is smaller during the spreading than that of the localized cases, and therefore the noise becomes more visible.



To address this question, we have added a clarifying discussion in the SI as follows:
"Importantly, we also note that while the signal-to-noise ratio is always decreasing as a function of the time step as seen in all the static control measurements, the power intensity distribution in the lattice also changes as a function of the degree of localization of light. This is particularly important in dynamic control over the localization of light at the corner as well as tweezing measurements when the light is localized at the corner after spreading."

4. Recently there have been works demonstrating how NHSE could shape the wavefunctions of topological modes in a mechanical system (Nature 608, 50-55 (2022)). It seems that similar SSH lattices can also be realized in the current 2D set-up (changing the 50/50 coupler to a variable beamsplitter). The results would be much richer if you can take this into consideration.

Answer:
We thank the reviewer for bringing this relevant paper to our attention which we have cited in the revised version of the manuscript. Indeed the referee is correct that such NHSE-enabled morphing of topological modes can be implemented in our platform, likely via tuning the RF signal for the intensity modulators.

To implement this suggestion, we have added a sentence in the outlook as follows:
"Another intriguing direction can be exploring NHSE-enabled morphing of photonic topological modes which was recently demonstrated in mechanical lattices \cite{wang2022non}."

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Reviewer #2
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This paper experimentally realized a specific type of two-dimensional non-Hermitian tight-binding lattice using pulsed light in time bins in fiber loops and demonstrated a two-dimensional non-Hermitian effect that can be controlled dynamically by slowly varying the lattice Hamiltonian parameters.

While this work presents some new results in (i) the realization of dynamical control of 2D non-Hermitian skin effect and (ii) the realization of 2D non-Hermitian skin effect on this specific experimental platform (synthetic time dimension), the significance of these results is limited to the field of topological photonics. 2D non-Hermitian skin effect has been realized in [Zhang, X., Tian, Y., Jiang, JH. et al. Observation of higher-order non-Hermitian skin effect. Nat Commun 12, 5377 (2021)]. The dynamical control of the 2D non-Hermitian skin effect does not give rise to any qualitative difference. Therefore, I don't find the significance and potential impact of the paper sufficient to be published in Nature Communications. After adequately addressing the following comments, the paper may be publishable in a more specialized journal.

Answer:
We first thank the referee for their evaluation of our work. The main result of our work is the realization of dynamic controls over topological photonic modes in synthetic dimensions. These results have broad significance for robust reconfigurable control over the flow of light in synthetic dimensions, as well as understanding the fundamental dynamic interplay between topology and NHSE in photonics. To address the referee's comment, we highlight the significance and impact of our work more specifically and more clearly as detailed below:

The significance of dynamic control for reconfigurable light steering:
Reconfigurable light steering is crucial for a broad range of applications such as transmission links of light fully using the entire footprint of a photonic device. Combining these capabilities with topological effects would lead to novel phenomena such as robust waveguiding. For example, Zhao et al (Science 265 (2019)) showed that dynamical control in a spatial lattice enables such robust and reconfigurable light steering that cannot be done statically. The extension of these effects to synthetic dimensions would enable novel phenomena that could not be done in spatial dimensions, such as photonic devices with higher dimensions as well as the realization of complex photonic effects with a small number of photonic components (L Yuan et al Optica 5 (2018)).

The impact of the 2D non-Hermitian skin effect (NHSE) in photonics:
Combining NHSE with topology can enable exotic new phenomena that are inaccessible to trivial counterparts. For example, Wang et al (608 Nature 2022) showed that NHSE enables the morphing and reshaping of topological wavefunctions in a mechanical system. Combining

NHSE with dynamic control in a topological photonics platform can enable the realization of photonic analog of such dynamic and robust topological wavefunction engineering which is of fundamental importance in studying the interplay between topology and photonic NHSE in a tunable manner.

Application of photonic NHSE for temporal mode-locked lasing phenomena:
Combining photonics NHSE with synthetic time dimensions can give rise to unique topological temporal mode-locking phenomena. For example, Leefman et al (20 Nature Physics 2024) showed that NHSE enables temporally mode-locked lasers in synthetic dimensions. Combining such lasing effects with our dynamic control can enable the realization of robust and reconfigurable lasers where the lasing mode can be dynamically tuned across the full footprint of the system.

To emphasize the novelty of this work we have added additional discussion and citations as outlined below:

"Our results demonstrate that useful control mechanisms in spatial landscapes such as reconfigurable light steering \mbox{\cite{zhao2019non}} can be extended to synthetic dimensions."

"Further, one can create an analogue of on-site interaction by imposing a nonlinear phase shifter after the linear optical transformations, and investigate non-Hermitian models of interacting particles \cite{gliozzi2024many}. Such nonlinearities could also have implications in the recently discovered regime of topological frequency combs \mbox{\cite{mittal2021topological,jalali2023topological,flower2024observation}} as well as temporal mode-locked lasers \mbox{\cite{leefmans2024topological}} due to the periodic temporal pulses that define our platform."

"Another intriguing direction can be exploring NHSE-enabled morphing of photonic topological modes which was recently demonstrated in mechanical lattices \mbox{\cite{wang2022non}}."

(1) The authors wrote, "two-dimensional non-Hermitian photonic skin effect, that is, corner states," which I find misleading as corner states usually refer to the boundary state from 2D high-order topological insulator (HOTI). The latter are bound states (discrete energies outside the continuum), while the skin effect, 1D or 2D, comes from bulk states (continuum).

Answer:
We thank the referee for raising this important point. We agree with the referee and in order to enhance the accuracy of terminology for localized states formed in our system, we can refer to these states as Corner Skin Modes which consist of the terminology used in recent works such as Zhang et al **Nature Com.** 13 (2022) and Zhou et al **Nature Com.** 14 (2023).

To address this point, we have replaced the term "corner state" with "corner skin mode" throughout the revised manuscript.

(2) The authors claim that their dynamical variation of parameters for such a non-Hermitian system is adiabatic. However, the adiabatic theorem is based on having no crosstalk between eigenstates. In such a non-Hermitian system with open boundary conditions, the eigenstates are not orthogonal, and thus, the adiabatic condition is not fulfilled, or at least not well defined. It is better to term it as a gradual control.

Answer:
We thank the referee for raising another important point. We agree that gradual control here is a more accurate label as adiabatic.

To address this point, we have replaced the term "adiabatic" with "gradual" throughout the revised manuscript.

(3) In the results reported in Fig. 4, I don't find it too meaningful to test the robustness of the skin modes against disorders. As these skin modes are just a localized version of the bulk states, they are not necessarily robust against disorders. Unless the authors can provide or refer to a convincing theoretical argument on the robustness of non-Hermitian skin effects against disorders, this part of the results could be misleading.

Answer:
In response to the Referee's concern regarding the robustness of non-Hermitian skin modes, we would like to point to ref. 9 (Gong et al **PRX** 6 (2018)) which shows that the non-trivial winding in complex energy plane (for a finite periodic system) guarantees the existence and the robustness of skins modes in a finite system. As we show in our manuscript, the skin modes are robust against disorder in the complex hopping strengths. This robustness of skin modes has been demonstrated in several 1D non-Hermitian systems, for example, see Fig 3 in Gong et al **PRX** 6 (2018).

To address this point, we have added the following sentence to the revised manuscript:
"A detailed discussion of the robustness of the corner skin modes is available in Ref. \mbox{\cite{gong2018topological}}."

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Reviewer #3
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In the manuscript by Zheng and co-authors, entitled "Dynamic control of 2D non-Hermitian photonic corner states in synthetic dimensions", the authors experimentally realize a synthetic non-Hermitian lattice using time-multiplexed optical setup. They successfully demonstrate emergence of higher-order non-Hermitian boundary states localized at the corner of four domains with different complex hopping.

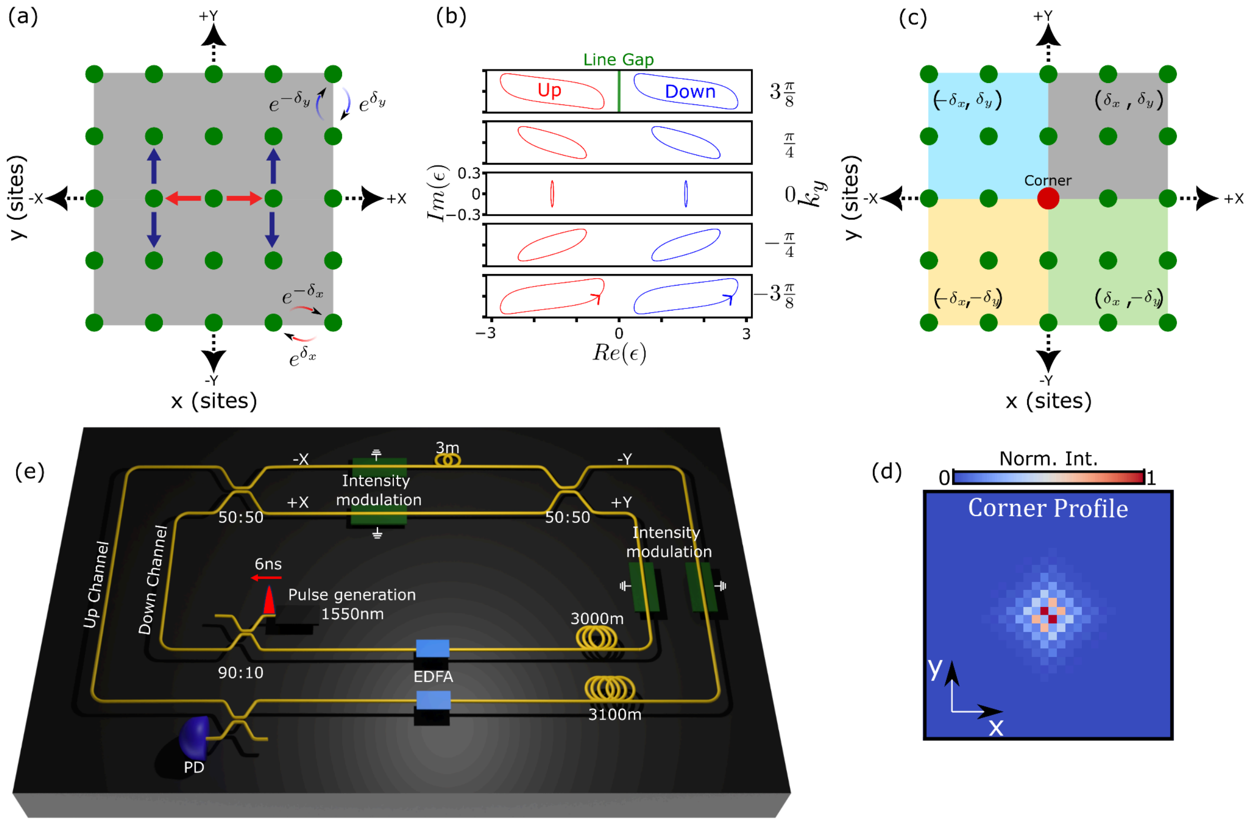
Time-multiplexed lattices have indeed been shown to be a versatile platform for realization of topological phenomena, and this work is another demonstration of how such optical platforms can be used to realize fascinating physical properties, in this case emerging from nontrivial topology in complex energy space. Here the authors predict that in their platform they can attain nontrivial topological characteristics (winding of the complex energy bands), which gives rise to the emergence of corner states.
More importantly, here the authors also show two other interesting results directly stemming from the versatility of the experimental platform 1) the possibility to dynamically control topology of synthetic dimensional domains to move corner modes around and change degree of their localization and 2) to prove topological robustness by introducing disorder into non-Hermitian parameters of the lattice. These are truly impressive results showcasing how powerful the experimental platform is.

The paper is very nicely written, and the supplement aids understanding when the main text appears vague. The results are very clearly presented. Therefore, I have only a couple of minor suggestions which I believe would further improve readability and rigor of the work.

Answer:
We thank the reviewer for their very positive feedback on our manuscript.

1) In Fig. 1(b) loops in planes corresponding to several fixed values of k_y are shown, however, the plotting style gives an illusion that the loops are not laying in planes. I am afraid this may cause confusion for readers. I recommend replotting the loops on planes and stacking these planes vertically to make it clear that they lay in planes of constant k_y.

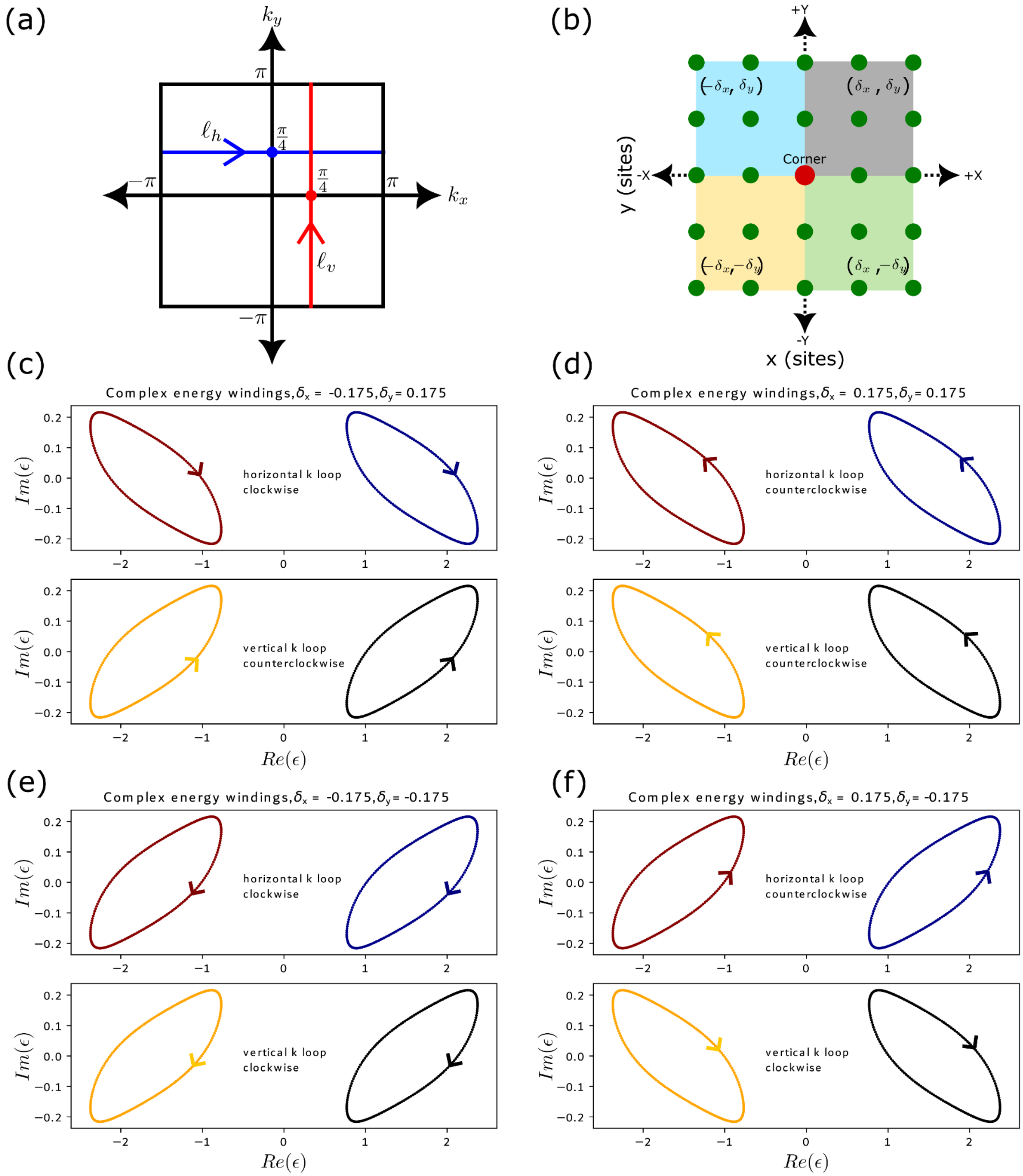
Answer:
We thank the referee for this suggestion. To address this, we have replotted the loops in the 2D complex energy plane as shown below in panel (b) and updated the figure in the revised manuscript accordingly.



2) The manuscript would benefit from stating some sort of general argument, a bulk boundary correspondence principle, which would explain how flipping complex hopping in four domains leads to the formation (or even guarantees) emergence of the corner states. While the authors cite earlier works, this could make text more complete and accessible to a novice reader.

Answer:
We thank the referee for their suggestion. Indeed, the formation of corner skin modes in our semi-infinite system is guaranteed by the non-trivial winding in the complex energy plane. In particular, the four domains with different complex hoppings exhibit different directions of winding. At an interface between domains with different windings, we observe skin modes. This phenomenon is similar to that of Hermitian topological systems where two regions with different winding numbers (or topological invariants) show edge modes. In the revised supplementary, we have shown these windings for different domains, as shown in the figure below.

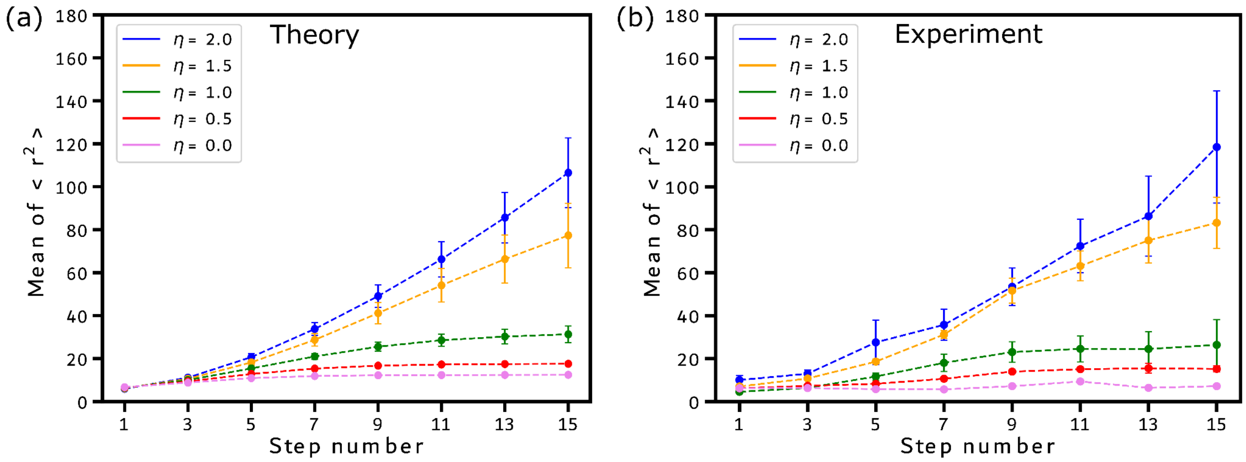
To implement the referee's suggestion, we have added the following explanation to the SI:
"We note that the formation of corner skin modes in our semi-infinite system is guaranteed by the non-trivial winding in the complex energy plane. In particular, the four domains with different complex hoppings exhibit different directions of winding. At an interface between domains with different windings, we observe skin modes, as shown in Fig.1d in the main text. This phenomenon is similar to that of Hermitian topological systems where two regions with different winding numbers (or topological invariants) show edge modes."



3) In Fig. 5 the authors plot average displacements as curves, which is odd. Please use some scatters and also show standard deviations. (This is also Nature Comm policy, I believe).

Answer:

We thank the referee for this suggestion. To address this, we have replotted the average displacements in scatter forms and also added the standard deviations as shown below, and updated the figure in the revised manuscript accordingly.



4) Finally, I suggest softening a very strong claim the authors made in the abstract about time-multiplexed latices, namely “effects free of geometric restrictions”. Time-multiplexed latices have their own geometric constraints, which is evidenced by the limited size of the domain the authors realize. Please rewrite the sentence in the abstract.

Answer:
We thank the referee for their suggestion and agree with it. To address this, we have revised the last sentence of the abstract as follows:

“This opens avenues for topological classification, quantum walk simulations of many-body dynamics, and robust Floquet engineering in synthetic landscapes.”

To summarize, provided the above minor revision is done, I believe this work will be of a major interest to very broad research community. I am also looking forward to seeing how this experimental platform will evolve to showcase even more exciting non-Abelian and Floquet topological phases.

Answer:
We thank the reviewer for their positive remarks, and we hope that the revisions we have made according to their comments are satisfactory. Moreover, we also indeed are excited to see the application of our platform for future non-Abelian and Floquet topological effects.