

Anomalous Localization and Mobility Edges in Non-Hermitian Quasicrystals with Disordered Imaginary Gauge Fields

Corresponding Author: Professor Zhihao Xu

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)

This manuscript investigates localization phase transitions in one-dimensional non-Hermitian quasicrystals induced by disordered imaginary gauge fields. While the authors utilize a variety of diagnostic tools—such as Lyapunov exponents, winding numbers, and center-of-mass fluctuations—to characterize the system's phases, I hold reservations regarding the novelty and the depth of the physical insights presented. The core conclusions appear to be, to a large extent, a straightforward superposition of findings from existing literature, and the use of certain terminology (e.g., "anomalous") lacks sufficient physical justification. In its current form, the manuscript does not meet the publication standards of Communications Physics; therefore, I recommend rejection.

I. Core Scientific Issues

A. Originality of Conclusions and the "1+1" Superposition Problem

Derivative Nature of the Physical Model: The core mechanism of the "drifting" or "erratic" non-Hermitian skin effect (ENHSE) introduced in this model originates from the work of Longhi (2025) [Phys. Rev. Lett. 134, 196302 (2025)]. By employing a non-unitary gauge transformation (Eqs. 10-11), the authors map the system back to an auxiliary Hermitian model. This auxiliary model is essentially the generalized AAH model with next-nearest-neighbor hopping, which was already thoroughly resolved by Biddle et al. in 2011. [Phys. Rev. B 83, 075105 (2011); J. Phys. A: Math. Theor. 401, 214-223 (2014)]

Predictability of Conclusions: Given the existence of a clear mapping, it is a mathematical inevitability that the analytical expression for the mobility edge under open boundary conditions (OBC) (Eq. 2) is identical to the Hermitian case. Defining this as an "anomalous" mobility edge appears to be a mere relabeling of known phenomena rather than a discovery of new physics. The work fails to demonstrate a unique physical universality class that deviates from the results of the Hermitian mapping.

Redundancy of PBC Results: It is fundamental physical common sense that localized states are insensitive to boundary conditions (leading to the spectral similarity between PBC and OBC). Furthermore, the relationship between non-zero spectral winding numbers and the non-Hermitian skin effect under PBC has already been discussed in extensive detail in multiple publications (e.g., Jiang 2019, Longhi 2019, Xu 2022).

B. Effectiveness of Diagnostic Tools and Physical Interpretations

Avoidance of Direct Diagnostic Criteria: The authors introduced the complex center-of-mass (CoM) fluctuations $\langle \hat{X}^2 \rangle$ (Eq. 5) to distinguish phase boundaries, yet failed to rigorously justify its necessity as a fundamental criterion from a deep physical perspective. In fact, for systems under periodic boundary conditions (PBC), the fraction of complex-energy eigenstates (i.e., the ratio of eigenstates with non-zero imaginary parts to the total number of states) serves as a much more direct and concise criterion. This fraction can intuitively characterize the transition from the ENHSE phase to the Anderson localization phase. Regrettably, the manuscript lacks an in-depth discussion on this indicator, nor does it adopt it as a primary diagnostic tool.

Lack of Physical Interpretation for Diagnostic Failures: The manuscript notes that the fractal dimension D_f (Eq. 3) vanishes in both phases, leading to its failure as a diagnostic. However, the authors merely describe this phenomenon without providing a profound physical explanation. Fundamentally, this is because eigenstates in both the ENHSE and Anderson localization phases exhibit confined spatial distributions. Although ENHSE manifests as bulk accumulation, its scaling behavior of the Inverse Participation Ratio (IPR) in the thermodynamic limit is highly similar to that of conventional localized states. Consequently, D_f fails to provide any effective differentiation between the two.

Limitations in the Applicability of the Lyapunov Exponent (LE) Definition: Eq. (4) defines γ as the ratio of the wave function amplitudes at the two ends of the chain. For ENHSE states, where the primary localization peak may be situated in the middle or end of the chain (due to irregular distribution), it is questionable whether such a boundary-based definition can accurately reflect the true localization length. The robustness of this definition is particularly doubtful when multiple, irregularly distributed satellite peaks are present, as it may fail to capture the actual spatial decay characteristics of the wave function.

C. Doubts on the Definition of the "Anomalous" Mobility Edge

Since the formula for the mobility edge is identical to the Hermitian AAH model, why is it defined as "anomalous"? The author claims the "anomaly" comes from the different types of states being separated. In traditional models, a mobility edge separates extended and localized states; in this model, it separates ENHSE-type accumulation states from Anderson localized states.

However, this looks more like a "relabeling" of terms rather than a new physical mechanism. Even after the mapping to a Hermitian frame, the ENHSE region still corresponds to the non-localized sector where the Lyapunov exponent is zero.

D. Doubts on Dynamical Behavior

Too much of the text merely describes the figures instead of analyzing the underlying physical mechanisms. This weakens the dynamics section as a piece of independent evidence. Specifically, in Fig. 8(a) and (d), the center-of-mass (CoM) evolution varies significantly, yet the manuscript provides no clear explanation for:

- 1) Why does the magnitude of CoM evolution differ so drastically between these two cases?
- 2) Which features are universal and which are unique to specific disorder realizations?

Additionally, the color markings do not clearly match the curves (e.g., it is difficult to identify the data corresponding to the yellow curve).

Furthermore, the current results rely heavily on a single disorder realization. Since CoM behavior is highly realization-dependent, a single trial cannot represent the system's universal properties. While the authors later show statistical results, the analysis remains at the level of a global average.

From a physical standpoint, a much more convincing approach would be to categorize the disorder realizations by the sign of the average CoM shift Δx :

Group 1: Realizations where the CoM shift is locked to the left side ($\omega = 1$).

Group 2: Realizations where it is locked to the right side ($\omega = -1$).

Comparing these two groups statistically would clearly reveal:

The direct correspondence between the sign of the winding number and the dynamical CoM shift.

The physical link between ENHSE directionality and the dynamical response.

Simply providing a global average masks these critical structural differences and fails to support the winding number as a robust topological or phase-transition criterion.

II. Specific Comments for Revision

1. Writing Structure and Physical Interpretation

The current focus of the manuscript is misplaced. In the main text (especially lines 217-252 and 262-391), too much space is spent on the description of the figures.

- 1) Technical details, such as "what is shown" or "what the curves represent," should primarily be placed in the figure captions. The main text should focus on the "big picture"—discussing how the data supports core physical arguments and revealing the underlying non-Hermitian mechanisms.
- 2) Reorganize the structure of the manuscript to reduce redundant descriptions. Instead, prioritize the discussion of physical meaning and conceptual progression.

2. Rigor of Mathematical Notations and Definitions

The mathematical expressions in the manuscript lack precision, with several definitions being ambiguous:

- 1) Consistency of Notations: In Eqs. (3) and (4), the superscript (n) is used to denote the energy-level index, whereas Eqs. (5) and (6) switch to subscripts. I recommend unifying these notations throughout the text to avoid confusing the reader.
- 2) Completeness of the IPR Definition: In the definition of IPR following Eq. (3), a denominator must be included if the wave functions are not pre-normalized. This is necessary to ensure the physical accuracy of the quantity.
- 3) Logical Consistency of the Lyapunov Exponent (LE): There is a notation conflict in Eq. (4). If $j \rightarrow \infty$, then ψ_j essentially refers to the end of the chain. It would be clearer to use the system size N here. Currently, using j as a variable while it also appears in $\lim_{j \rightarrow \infty}$ (especially since N is already defined as the chain length) makes the

definition logically inconsistent.

3. Physical Mechanisms and Finite-Size Effects

In lines 170-174, the authors discuss how the spatial average of the imaginary gauge field, $\overline{h}$, affects the energy spectrum. The manuscript states that in finite-size systems, the deviation of $\overline{h}$ from zero (due to statistical fluctuations) is the main reason why imaginary parts appear in the PBC spectrum.

If $\overline{h} \equiv 0$ is manually enforced, do imaginary parts still appear at finite sizes?

The authors should clarify whether the emergence of imaginary parts is merely an accidental result of statistical fluctuations or represents an inherent instability in the thermodynamic limit.

I suggest adding a scaling curve showing the evolution of $\epsilon_{\max}$ as a function of the system size N to support this point.

4. Lack of Statistical Generality

Most of the core conclusions (Figs. 1-7) are based on a "single disorder realization." For a system that inherently depends on the specific disorder pattern, results from just one trial cannot rule out random fluctuations. It also fails to prove that the conclusions are statistically robust. This approach lacks the universality needed to demonstrate general laws of anomalous localization.

5. Revision Comments for Section: DYNAMICAL DETECTION

Please refer to the criticisms detailed in Point D (Doubts on Dynamical Behavior).

6. Specific Suggestions for Improving Figure Presentation

1) Missing Parameters and Lack of Self-Consistency (Figs. 1, 3, 6)

The captions for Figs. 1, 3, and 6 do not explicitly state the hopping coefficient J_1 or key disorder strength parameters (e.g., Δh). While the text mentions $J_1=1$ as the energy unit and $\Delta h=0.5$, high-quality academic figures should be self-consistent. I suggest including all relevant physical parameters in the corresponding figure captions.

2) Unclear Boundary Conditions (BC) (Figs. 2, 9)

Fig. 2 shows changes in fractal dimension, Lyapunov exponents, and center-of-mass (CoM) fluctuations. Since these quantities are highly sensitive to boundary conditions, the authors must specify whether the data were obtained under PBC or OBC.

Fig. 9 shows the average wave packet evolution, but the BC is ambiguous. The Hermitian-like behavior of CoM motion is usually related to reflections under OBC; under PBC, "boundary-wrapping" would lead to a completely different CoM trajectory. The authors should clarify this.

3) Contradiction Between Definition and Visualization (Figs. 1, 5)

The text defines ENHSE as macroscopic accumulation at "seemingly irregular or scattered positions." However, in Fig. 1(a, c) and Fig. 5(b, e), the wave function profiles mostly show clear boundary localization (e.g., near $j=1$). Is this representation universal? I suggest adding more statistical samples of random sequences to prove the "irregular distribution" characteristic is actually widespread.

4) Optimization of Data Logic and Presentation (Figs. 4, 6, 7):

Clarify Disorder Realizations (Fig. 4): The caption should explicitly state that Fig. 4(a) and (b) correspond to two independent and different disorder realizations to explain the sign reversal of the winding number and its effect on CoM shift.

Use Better Criteria (Fig. 7): Compared to the maximum imaginary part $\epsilon_{\max}$, the fraction of complex-energy eigenstates is a more direct and concise criterion for capturing the transition from ENHSE to localization.

Merge Figures: I suggest merging Fig. 6 (spectral evolution) and Fig. 7 (topological criteria) into a single multi-panel figure. This would more clearly demonstrate the synchronicity between the "complex-to-real" spectral transition and the topological/localization phase transition.

Reviewer #2

(Remarks to the Author)

The authors investigate a non-Hermitian quasicrystal model where non-Hermiticity arises from disordered imaginary gauge fields. Through systematic numerical analyses—including fractal dimension, Lyapunov exponent, center of mass, spectral winding number, and dynamical evolution—they characterize the localization properties of the system. Their findings reveal an anomalous localization transition from an erratic non-Hermitian skin effect (ENHSE) phase to a localized phase. Upon introducing weak next-nearest-neighbor (NNN) hoppings, this sharp spectral transition becomes gradual, giving rise to a mobility edge that separates ENHSE from localized states. The results are novel, the methodology is appropriate, and the

numerical evidence is convincing. Therefore, I recommend the manuscript for publication.

Below are some minor suggestions that may help strengthen the work:

1, The current study focuses primarily on the case with $J_2 \neq 0$. It would be beneficial to compute a phase diagram in the (λ, J_2) parameter space, distinguishing among the ENHSE phase, the localized phase, and an intermediate region where both types of states coexist.

2, In the presence of NNN hoppings, does the localization transition remain coincident with a complex-to-real spectral transition? Clarification on this point would be insightful.

3, The system sizes considered are multiples of 100 (e.g., $N = 300, 400, 1200, 2000$). However, in quasicrystalline models, system sizes are often chosen as Fibonacci numbers (e.g., $N = 233, 377, 610$). Could the choice of Fibonacci-sized systems affect the phase boundaries when $J_2 \neq 0$? A brief discussion or numerical check would be helpful.

4, For $J_2 \neq 0$, the results are presented for only a few parameter values. A more systematic scan—e.g., showing the Lyapunov exponent or spectral winding number as a function of λ —would offer a more comprehensive view. Comparing such results for $J_2 = 0$ and $J_2 = 0.1$, for instance, could more clearly illustrate the effect of NNN hoppings on localization and spectral topology.

Reviewer #3

(Remarks to the Author)

In this article, the authors have analyzed a generalized Aubry-Andre-Haper (AAH) chain with non-reciprocal hopping and Bernoulli-type disorder. They clarified that the Lyapunov exponent distinguishes the region of erratic non-Hermitian skin effect (ENHSE) from that of Anderson localized states, while the fractal dimension does not. The mobility edge, where the Lyapunov exponent becomes zero, separates ENHSE and Anderson localized states. In these two regions, the center-of-mass behaves differently, leading to different wave-packet dynamics. The imaginary gauge transformation elucidates why the Lyapunov exponent distinguishes ENHSE from Anderson localized states; this transformation maps the non-Hermitian AAH chain to the Hermitian one where the mobility edge separates extended and localized states.

Their results are new and reasonable. However, it is unclear to me whether their results meet the criteria of significance required by Communication Physics. Specifically, it is unclear to me whether their main result regarding the Lyapunov exponent applies to generic systems because they rely on an imaginary gauge transformation, which is not necessarily available.

After the authors addressed my concerns properly, I would recommend the publication.
The following are minor comments to be addressed.

1. Their model does not simultaneously host states with $w=1$ and -1 . If this occurs, how would their result change?
2. Figures 4(a,b) show that the winding number flips its sign when the disorder is changed. The authors should clarify the specific difference in the parameters.
3. Their numerical data indicate that the center-of-mass becomes of order N in the region of Anderson localization and 1 in the region of ENHSE. It is unclear to me why it shows such a difference.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors have provided a careful and detailed response to the previous comments. The revised manuscript and response letter show substantial improvements.

In particular, the meaning of the term “anomalous” has been clarified. It is now made clear that this term refers to the physical interpretation of the mobility edge in the original non-Hermitian frame, rather than to a new analytical expression. This clarification addresses one of the main concerns raised in the previous report.

The revised manuscript also provides a more balanced discussion of the relation among the present model, the auxiliary Hermitian generalized AAH model, and previous studies on ENHSE. The scope and novelty of the work are now stated more appropriately.

The additional numerical analyses are also appreciated, including disorder-averaged results, finite-size scaling, the fraction of complex-energy eigenstates, and winding-resolved dynamical averages. These additions significantly strengthen the evidence supporting the authors' conclusions and address the previous concern that the results relied too heavily on single

disorder realizations.

The discussion of the dynamical results has also been improved. The newly added winding-resolved averaging provides a clearer connection between the spectral winding number and the direction of the center-of-mass drift, making the dynamics section more convincing.

Overall, the authors have satisfactorily addressed the previous concerns. The revised manuscript is now clearer, more rigorous, and better supported by numerical evidence. I therefore recommend publication in Communications Physics.

Reviewer #2

(Remarks to the Author)
no additional comments

Reviewer #3

(Remarks to the Author)
The revised manuscript clearly presents its novelty: revealing a qualitatively distinct form of localization physics in non-Hermitian quasicrystals with disordered imaginary gauge fields.
In addition, the authors addressed all of my previous comments properly.
Therefore, I recommend the publication in Commun. Phys.

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Response to the Reviewers

We sincerely thank the Editor and all the Reviewers for their careful reading of our manuscript and for their constructive comments and suggestions. We have substantially revised the manuscript in response to these comments. In particular, we have clarified the physical meaning of the term “anomalous”, strengthened the discussion of novelty and scope, added systematic disorder-averaged and finite-size analyses, included a phase diagram in the (λ, J_2) plane and Fibonacci-size checks, and significantly expanded the discussion of realization-resolved and winding-resolved dynamics.

We believe that these revisions have substantially improved the clarity, rigor, and overall significance of the manuscript. Below we provide a point-by-point response to all comments. Reviewer comments are reproduced in *italics*, and our responses are given below each comment.

Before addressing the individual comments, we would like to briefly emphasize the novelty, significance, and timeliness of the present work.

First, the problem studied here is at the forefront of current non-Hermitian physics. Erratic non-Hermitian skin localization (ENHSE), first introduced theoretically only very recently, has already attracted strong attention from both theory and experiment. In particular, experimental progress has now been reported in **J. X. Zhong, J. W. Kim, S. Longhi, and Y. Jing, “Observation of erratic non-Hermitian skin localization and transport,” arXiv:2601.19749**, and **Y. Y. Sun, S. Yang, and Y. Yang, “Observation of Floquet erratic non-Hermitian skin effect in photonic mesh lattice,” arXiv:2604.00088**. ENHSE-related transport in open systems has also been investigated in **S. Longhi, “Erratic Liouvillian skin localization and subdiffusive transport,” arXiv:2602.14698**. In addition, closely related imaginary-gauge-field physics has been further explored in **J.-L. Dong, S.-L. Zhu, and D.-W. Zhang, “Exact Skin Critical Phase and Configurable Fractal Wavefunctions via Imaginary Gauge Phase Imprint in Non-Hermitian Lattices,” arXiv:2603.28153**. Importantly, these recent works also cite our study, indicating that the problem addressed in our manuscript is already receiving active attention from the experimental and broader non-Hermitian community.

Second, the novelty of the present work does not lie in proposing a different closed-form mobility-edge formula from the Hermitian generalized AAH model. Rather, it lies in establishing that in a non-Hermitian quasicrystal with a spatially disordered imaginary gauge field, the same boundary inherited from the auxiliary Hermitian problem acquires a qualitatively different physical meaning in the original frame: it separates two distinct confined sectors, namely Anderson-localized states and ENHSE-type macroscopic-accumulation states, rather than the conventional extended and localized states. In this sense, the anomalous character is physical rather than algebraic.

Third, in our work the term “anomalous” applies not only to the mobility edge, but also to the localization transition itself. In the $J_2 = 0$ limit, the transition is anomalous because it connects a fully ENHSE phase and a conventional Anderson-localized phase, rather than the conventional extended and localized phases of the Hermitian AAH model.

Finally, beyond identifying these anomalous localization structures, we develop a unified characterization linking real-space morphology, Lyapunov exponent, center-of-mass fluctuation, finite-size spectral reality under PBCs, realization-resolved spectral winding, and wave-packet dynamics. In particular, the dynamical detection protocol proposed in our manuscript provides a practically relevant route for future experiments to probe realization-resolved ENHSE directionality and winding-dependent transport. For these reasons, we believe that the present work is timely, original, and significant, and is well suited for publication in *Communications Physics*.

Response to Reviewer 1

We thank Reviewer 1 for the careful reading and for the critical and detailed comments. We have taken these concerns very seriously and have substantially revised the manuscript accordingly. In particular, we have clarified the physical meaning of the term “anomalous”, strengthened the discussion of novelty and scope, added disorder-averaged and finite-size analyses, expanded the dynamical characterization to include winding-resolved averages, and improved the presentation of both the figures and the methodology.

Comment 1

This manuscript investigates localization phase transitions in one-dimensional non-Hermitian quasicrystals induced by disordered imaginary gauge fields. While the authors utilize a variety of diagnostic tools—such as Lyapunov exponents, winding numbers, and center-of-mass fluctuations—to characterize the system's phases, I hold reservations regarding the novelty and the depth of the physical insights presented. The core conclusions appear to be, to a large extent, a straightforward superposition of findings from existing literature, and the use of certain terminology (e.g., “anomalous”) lacks sufficient physical justification. In its current form, the manuscript does not meet the publication standards of Communications Physics.

Response:

We thank the Reviewer for this important overall assessment. We respectfully clarify that the central novelty of our work is not the proposal of a new analytical mobility-edge formula. Rather, the novelty lies in showing that, in the original non-Hermitian frame with a spatially disordered imaginary gauge field, the inherited boundary from the auxiliary Hermitian problem acquires a qualitatively different physical meaning and organizes a distinct localization structure.

More specifically, in the Hermitian generalized AAH model the mobility edge separates extended and localized states. In contrast, in our non-Hermitian system it separates Anderson-localized states from ENHSE-type macroscopic-accumulation states. These are two distinct confined sectors, both characterized by vanishing fractal dimension in the thermodynamic limit, but with sharply different real-space morphology, Lyapunov exponent, center-of-mass fluctuation, spectral reality under PBCs, spectral winding, and dynamical response. In this sense, the anomalous character is not a mere relabeling, but reflects a nontrivial reorganization of mobility-edge physics in the original non-Hermitian frame.

To make this point explicit, we have substantially revised the Introduction and Conclusion. In particular, we now clearly state that the term “anomalous” refers to the distinct non-Hermitian physical meaning of the same boundary, rather than to a modified analytical formula.

We would also like to emphasize that the problem addressed here is itself highly timely and actively developing. Since the submission of our original manuscript, ENHSE has already attracted direct experimental attention [Zhong *et al.*, arXiv:2601.19749; Sun *et al.*, arXiv:2604.00088], related transport in open systems has also been explored [Longhi, arXiv:2602.14698], and closely related imaginary-gauge-field-induced criticality has been studied in [Dong, Zhu, and Zhang, arXiv:2603.28153]. These recent developments indicate that understanding how ENHSE, localization, topology, and dynamics interplay in disordered non-Hermitian quasicrystals is a front-line problem of broad current interest.

Comment 2

Derivative Nature of the Physical Model: The core mechanism of the “drifting” or “erratic” non-Hermitian skin effect (ENHSE) introduced in this model originates from the work of Longhi (2025). By employing a non-unitary gauge transformation (Eqs. 10-11), the authors map the system back to an auxiliary Hermitian model. This auxiliary model is essentially the generalized AAH model with next-nearest-neighbor hopping, which was already thoroughly resolved by Biddle et al. in 2011.

Response:

We agree that both ingredients—the ENHSE mechanism and the Hermitian generalized AAH mobility-edge structure—are rooted in important prior works. However, our claim is not that these ingredients are individually new. Rather, our contribution is to establish that when they are combined in a non-Hermitian quasicrystal with a disordered imaginary gauge field, the resulting physics is not a trivial carryover of the Hermitian picture. Specifically, the same inherited mobility-edge boundary acquires a qualitatively different physical meaning in the original frame by separating two distinct confined sectors rather than extended and localized states. We have revised the Introduction and Conclusion to make this point explicit.

Comment 3

Predictability of Conclusions: Given the existence of a clear mapping, it is a mathematical inevitability that the analytical expression for the mobility edge under open boundary conditions (OBC) (Eq. 2) is identical to the Hermitian case. Defining this as an “anomalous” mobility edge appears to be a mere relabeling of known phenomena rather than a discovery of new physics. The work fails to demonstrate a unique physical universality class that deviates from the results of the Hermitian mapping.

Response:

We appreciate this comment and agree that the meaning of the term “anomalous” should be stated more explicitly. In the revised manuscript, we now clarify this at two levels.

First, for the $J_2 = 0$ case, the localization transition itself is anomalous because it connects a fully ENHSE phase and a conventional Anderson-localized phase, rather than the conventional extended and localized phases of the Hermitian AAH model.

Second, for the $J_2 \neq 0$ case, the mobility edge is anomalous not because its analytical location is modified, but because in the original non-Hermitian frame it separates Anderson-localized states from ENHSE-type macroscopic-accumulation states rather than extended states. Therefore, the anomaly is physical in nature: it concerns the meaning and consequences of the boundary in the non-Hermitian system, not the algebraic form of the auxiliary Hermitian result.

We have revised the Introduction and Conclusion accordingly so that this terminology is now explicitly defined and consistently used throughout the manuscript.

Comment 4

Redundancy of PBC Results: It is fundamental physical common sense that localized states are insensitive to boundary conditions (leading to the spectral similarity between PBC and OBC). Furthermore, the relationship between non-zero spectral winding numbers and the non-Hermitian skin effect under PBC has already been discussed in extensive detail in multiple publications.

Response:

We agree that the general relation between spectral winding and skin effects in non-Hermitian systems is well established. Our intention is not to claim the discovery of this general principle. Rather, our contribution is to show how, in the present class of non-Hermitian quasicrystals with disordered imaginary gauge fields, the same anomalous localization structure is consistently reflected in a unified set of observables: real-space morphology, Lyapunov exponent, center-of-mass fluctuation, finite-size spectral reality under PBCs, realization-resolved spectral winding, and wave-packet dynamics.

In particular, for the present model we show that the same inherited mobility-edge boundary and the same ENHSE-to-localization transition are simultaneously manifested in:

- (i) real-space accumulation patterns,
- (ii) Lyapunov-exponent-based localization diagnostics,
- (iii) finite-size spectral reality under PBCs,
- (iv) realization-resolved winding topology, and
- (v) winding-resolved dynamical drift.

We have revised the manuscript to make clear that our goal is not to restate these generic principles, but to show how they are jointly organized by the anomalous localization structure in the present model.

Comment 5

Avoidance of Direct Diagnostic Criteria: The authors introduced the complex center-of-mass (CoM) fluctuations S (Eq. 5) to distinguish phase boundaries, yet failed to rigorously justify its necessity as a fundamental criterion from a deep physical perspective. In fact, for systems under periodic boundary conditions (PBC), the fraction of complex-energy eigenstates serves as a much more direct and concise criterion. Regrettably, the manuscript lacks an in-depth discussion on this indicator, nor does it adopt it as a primary diagnostic tool.

Response:

We thank the Reviewer for this useful suggestion. We agree that the fraction of complex-energy eigenstates under PBC is an important and direct finite-size spectral indicator. In response, we have strengthened this aspect significantly in the revised manuscript.

For the $J_2 = 0$ case, we now explicitly introduce and analyze the disorder-averaged fraction of complex-energy eigenstates $\langle f_m \rangle$ under PBCs [Fig. 3(c)] and show that, for generic finite-size realizations, it sharply captures the complex-to-real spectral crossover near the same transition point.

For the $J_2 \neq 0$ case, we have further added systematic results showing the energy-resolved imaginary parts of the PBC eigenenergies [Fig. 5(f)] and the realization-averaged fraction of complex-energy eigenstates [Fig. 5(j)], demonstrating that for generic finite-size realizations the real-complex spectral boundary tracks the anomalous mobility edge.

At the same time, we retain the center-of-mass fluctuation S because it captures a complementary and physically transparent real-space distinction that the spectral indicator alone does not provide. Specifically, S differentiates the clustering of eigenstate centers around a few disorder-selected bulk positions in the fully ENHSE phase from the nearly uniform distribution of localization centers in the fully localized phase. In this sense, $\langle f_m \rangle$ is a direct spectral indicator, while S is a real-space morphological diagnostic.

Comment 6

Lack of Physical Interpretation for Diagnostic Failures: The manuscript notes that the fractal dimension D (Eq. 3) vanishes in both phases, leading to its failure as a diagnostic. However, the authors merely describe this phenomenon without providing a profound physical explanation. Fundamentally, this is because eigenstates in both the ENHSE and Anderson localization phases exhibit confined spatial distributions. Although ENHSE manifests as bulk accumulation, its scaling behavior of the Inverse Participation Ratio (IPR) in the thermodynamic limit is highly similar to that of conventional localized states. Consequently, D fails to provide any effective differentiation between the two.

Response:

We appreciate this comment and agree that the physical reason should be stated more explicitly. We have revised the corresponding discussion in the main text.

The essential reason is that both ENHSE states and Anderson-localized states are spatially confined in the thermodynamic limit. Although ENHSE states show macroscopic accumulation at disorder-selected bulk positions rather than conventional exponentially localized peaks spread throughout the lattice, their inverse participation ratio still scales in such a way that the fractal dimension vanishes in the thermodynamic limit. Therefore, the fractal dimension is insensitive to the physical distinction between these two confined sectors. In contrast, the Lyapunov exponent and the center-of-mass fluctuation remain sensitive to the difference between asymptotic exponential localization and macroscopic accumulation morphology.

Comment 7

Limitations in the Applicability of the Lyapunov Exponent (LE) Definition: Eq. (4) defines γ as the ratio of the wave function amplitudes at the two ends of the chain. For ENHSE states, where the primary localization peak may be situated in the middle or end of the chain (due to irregular distribution), it is questionable whether such a boundary-based definition can accurately reflect the true localization length. The robustness of this definition is particularly doubtful when multiple, irregularly distributed satellite peaks are present, as it may fail to capture the actual spatial decay characteristics of the wave function.

Response:

We thank the Reviewer for raising this important point. We agree that, if interpreted merely as a boundary-based amplitude ratio, the Lyapunov exponent could appear problematic for irregular ENHSE profiles. For this reason, in the revised manuscript we have strengthened the Methods section to clarify the origin and validity of the Lyapunov exponent used in our work.

The key point is that our model admits an exact non-unitary gauge mapping to an auxiliary Hermitian generalized AAH model. Under this mapping, the Lyapunov exponent in the non-Hermitian system is asymptotically inherited from the well-defined localization diagnostic of the Hermitian problem. Therefore, in the present work the Lyapunov exponent is not introduced as an ad hoc boundary-based observable for irregular ENHSE profiles, but as the asymptotic localization diagnostic inherited from the Hermitian auxiliary problem and interpreted in the original non-Hermitian frame.

We have now added this explanation explicitly in Methods. This also directly addresses why the Lyapunov exponent sharply distinguishes the ENHSE sector from the Anderson-localized sector despite the irregular morphology of ENHSE wave functions.

Comment 8

Since the formula for the mobility edge is identical to the Hermitian AAH model, why is it defined as "anomalous"? The author claims the "anomaly" comes from the different types of states being separated. In traditional models, a mobility edge separates extended and localized states; in this model, it separates ENHSE-type accumulation states from Anderson localized states. However, this looks more like a "relabeling" of terms rather than a new physical mechanism. Even after the mapping to a Hermitian frame, the ENHSE region still corresponds to the non-localized sector where the Lyapunov exponent is zero.

Response:

We thank the Reviewer for this important comment. We agree that the analytical expression of the mobility edge is inherited from the auxiliary Hermitian generalized AAH model and is therefore not new in an algebraic sense. Our point, however, is not that the mobility-edge formula itself is modified, but that in the original non-Hermitian frame the same boundary acquires a distinct physical meaning.

More specifically, in the Hermitian generalized AAH model the mobility edge separates extended and Anderson-localized states. In contrast, in the present non-Hermitian system the sector on one side of the same boundary does not correspond to ordinary extended states, but to ENHSE-type macroscopic-accumulation states generated by the disordered imaginary gauge field. Therefore, in the original frame the boundary separates Anderson-localized states and ENHSE-type states, rather than the conventional extended and localized states.

We respectfully emphasize that this is why we use the term "anomalous": not because the analytical formula is different, but because the physical content of the corresponding boundary is different in the non-Hermitian system. In this sense, the anomalous character is physical rather than algebraic.

Regarding the Reviewer's last point, we agree that in the auxiliary Hermitian frame the corresponding sector has a vanishing Lyapunov exponent. However, in the present work a vanishing Lyapunov exponent should not be interpreted as implying conventional extended states in the original non-Hermitian frame. Owing to the exact non-unitary gauge mapping, that sector is mapped back to ENHSE-type macroscopic accumulation rather than Bloch-like or ergodically extended behavior. This is precisely the non-Hermitian distinction that our work aims to highlight. We have revised the manuscript accordingly to make this point explicit and to avoid the impression that our claim of anomaly is based on a new closed-form mobility-edge formula.

Comment 9

Too much of the text merely describes the figures instead of analyzing the underlying physical mechanisms. This weakens the dynamics section as a piece of independent evidence. Specifically, in Fig. 8(a) and (d), the center-of-mass (CoM) evolution varies significantly, yet the manuscript provides no clear explanation for: (1) Why does the magnitude of CoM evolution differ so drastically between these two cases? (2) Which features are universal and which are unique to specific disorder realizations? Additionally, the color markings do not clearly match the curves. Furthermore, the current results rely heavily on a single disorder realization. Since CoM behavior is highly realization-dependent, a single trial cannot represent the system's universal properties. While the authors later show statistical results, the analysis remains at the level of a global average. From a physical standpoint, a much more convincing approach would be to categorize the disorder realizations by the sign of the average CoM shift δx : Group 1: Realizations where the CoM shift is locked to the left side ($\omega = 1$). Group 2: Realizations where it is locked to the right side ($\omega = -1$). Comparing these two groups statistically would clearly reveal the direct correspondence between the sign of the winding number and the dynamical CoM shift. The physical link between ENHSE directionality and the dynamical response. Simply providing a global average masks these critical structural differences and fails to support the winding number as a robust topological or phase-transition criterion.

Response:

We thank the Reviewer for this highly valuable and constructive suggestion. We fully agree that the original dynamics section relied too heavily on single-realization illustrations and did not sufficiently distinguish robust physical structure from realization-dependent details. In the revised manuscript, we have therefore substantially reorganized and strengthened this part in direct response to the Reviewer's concerns.

First, rather than relying mainly on single realizations, we now explicitly classify disorder realizations according to the sign of the mean winding number in the ENHSE regime, namely $\overline{w}_{\mathcal{R}} = +1$ and $\overline{w}_{\mathcal{R}} = -1$. We then perform realization averaging separately within each group. The resulting group-resolved density evolution and center-of-mass dynamics are now presented in the revised manuscript (Fig. 9). These new results show clearly that realizations with $\overline{w}_{\mathcal{R}} = +1$ exhibit, on average, a leftward short-time center-of-mass drift, whereas realizations with $\overline{w}_{\mathcal{R}} = -1$ exhibit, on average, a rightward short-time drift. By contrast, the full disorder average largely cancels these two opposite contributions and therefore restores Hermitian-like transport behavior. In this way, the revised analysis now directly establishes the correspondence between the sign of the winding number and the direction of the dynamical center-of-mass response, exactly along the lines suggested by the Reviewer.

Second, we now make explicit which aspects of the dynamics are robust and which are realization dependent. The robust feature is the short-time directional bias: within the class of realizations considered here, its direction is locked to the sign of the winding number. By contrast, the long-time trajectory is strongly realization dependent. This is because the disorder-selected ENHSE accumulation sites, as well as the way the evolving wave packet encounters and revisits them under PBCs, vary substantially from one realization to another. As a result, one realization may display relatively weak net center-of-mass displacement and eventual pinning near preferred bulk accumulation sites, whereas another may show stronger excursions and repeated boundary-wrapping recurrence, producing a sawtooth-like center-of-mass evolution. We have revised the discussion around Fig. 8 accordingly, so that the difference between Fig. 8(a) and Fig. 8(d) is now physically explained rather than merely described.

Third, regarding the Reviewer's concern that the previous global average could mask the underlying structure, we fully agree. This is precisely why we introduced the winding-resolved averaging in the revised manuscript. The global average alone indeed obscures the opposite directional responses carried by the two winding sectors, whereas the group-resolved analysis reveals them transparently. We believe this substantially strengthens the dynamical section as an independent probe of the realization-resolved winding structure, rather than leaving it at the level of descriptive illustration.

Finally, we have also improved the figure presentation and revised the wording to avoid overstatement. In particular, the relevant curve/color correspondence has been clarified in the revised figures and captions, and instead of using the term "universal," we now state more precisely that the drift direction is "robustly locked to the spectral winding within the class of realizations considered here."

We are grateful for this suggestion, which has led to a significant improvement of the manuscript.

Comment 10

In lines 170-174, the authors discuss how the spatial average of the imaginary gauge field, $\overline{h}$, affects the energy spectrum. If $\overline{h} \equiv 0$ is manually enforced, do imaginary parts still appear at finite sizes? The authors should clarify whether the emergence of imaginary parts is merely an accidental result of statistical fluctuations or represents an inherent instability in the thermodynamic limit. I suggest adding a scaling curve showing the evolution of $\epsilon_{\max}$ as a function of the system size N to support this point.

Response:

We thank the Reviewer for this excellent suggestion. We have now addressed this point explicitly by adding two appendices.

In Appendix A, we present the finite-size scaling of the maximum imaginary part of the PBC eigenenergies, $\epsilon_{\max}$, and show that it decreases with increasing system size in the ENHSE regime and tends to zero in the thermodynamic limit.

In Appendix B, we further consider realizations satisfying $\overline{h}_{\mathcal{R}} = 0$ already at finite size. In such cases, the PBC spectrum is entirely real even in the ENHSE regime, and the finite-size spectral complexification disappears.

These results confirm that the finite-size complex PBC spectrum is not an intrinsic thermodynamic property of the ENHSE phase, but rather originates from the residual non-Hermiticity associated with nonzero $\overline{h}_{\mathcal{R}}$ in generic finite-size realizations.

Comment 11

Most of the core conclusions (Figs. 1-7) are based on a "single disorder realization." For a system that inherently depends on the specific disorder pattern, results from just one trial cannot rule out random fluctuations. It also fails to prove that the conclusions are statistically robust. This approach lacks the universality needed to demonstrate general laws of anomalous localization.

Response:

We appreciate this concern and have substantially improved the manuscript in this respect.

In the revised version, the key quantitative diagnostics are now supported by disorder averaging over many realizations. In particular:

- (i) the fractal dimension, Lyapunov exponent, and center-of-mass fluctuation in Fig. 2 are disorder averaged;
- (ii) the fraction of complex-energy eigenstates in Fig. 3(c) is disorder averaged;
- (iii) the localization phase diagram and the real-complex spectral boundary for $J_2 = 0.1$ in Fig. 5 are disorder averaged;
- (iv) the phase diagram in the (λ, J_2) plane in Fig. 7 is disorder averaged; and
- (v) the group-resolved and full disorder-averaged dynamics in Figs. 9 and 10 are averaged over large disorder ensembles.

Single realizations are now used mainly for illustration of realization-resolved morphology and dynamics, while the main conclusions are supported statistically.

Comment 12

The current focus of the manuscript is misplaced. In the main text (especially lines 217-252 and 262-391), too much space is spent on the description of the figures. 1) Technical details, such as "what is shown" or "what the curves represent," should primarily be placed in the figure captions. The main text should focus on the "big picture"—discussing how the data supports core physical arguments and revealing the underlying non-Hermitian mechanisms. 2) Reorganize the structure of the manuscript to reduce redundant descriptions. Instead, prioritize the discussion of physical meaning and conceptual progression.

Response:

We thank the Reviewer for this helpful comment. We agree that in the original version the main text contained too much figure-by-figure description, and that the physical meaning and conceptual progression should have been emphasized more strongly.

In the revised manuscript, we have therefore reorganized and streamlined the presentation. In particular, we have reduced redundant narration in the main text, moved more technical details and display-oriented explanations into the figure captions where possible, and revised the discussion so that the emphasis is now placed more clearly on the physical logic of the work.

Specifically, the revised manuscript now highlights more explicitly:

- (i) the physical meaning of the anomalous localization transition and anomalous mobility edge in the original non-Hermitian frame;
- (ii) the methodological basis of the Lyapunov exponent through the exact gauge mapping to the auxiliary Hermitian problem;
- (iii) the interpretation of finite-size spectral complexification under PBCs as a residual finite-size effect; and
- (iv) the physical role of winding-resolved dynamical averaging in revealing robust short-time directional response.

We have also improved the progression between sections so that the manuscript now develops more clearly from localization structure, to mobility-edge physics, to finite-size spectral behavior, and finally to realization-resolved dynamics. While some figure guidance remains necessary because multiple complementary diagnostics are involved, the revised text places much stronger emphasis on physical interpretation rather than on description alone.

We thank the Reviewer again for this suggestion, which has substantially improved the clarity and organization of the manuscript.

Comment 13

The mathematical expressions in the manuscript lack precision, with several definitions being ambiguous: 1) Consistency of Notations: In Eqs. (3) and (4), the superscript (n) is used to denote the energy-level index, whereas Eqs. (5) and (6) switch to subscripts. I recommend unifying these notations throughout the text to avoid confusing the reader. 2) Completeness of the IPR Definition: In the definition of IPR following Eq. (3), a denominator must be included if the wave functions are not pre-normalized. This is necessary to ensure the physical accuracy of the quantity. 3) Logical Consistency of the Lyapunov Exponent (LE): There is a notation conflict in Eq. (4). If $j \rightarrow \infty$, then ψ_j essentially refers to the end of the chain. It would be clearer to use the system size N here. Currently, using j as a variable while it also appears in $\lim_{j \rightarrow \infty}$ (especially since N is already defined as the chain length) makes the definition logically inconsistent.

Response: We thank the Reviewer for these careful comments on notation and definitions. We agree that the mathematical presentation should be made more precise and internally consistent. In the revised manuscript, we have corrected and clarified these points as follows.

1. **Consistency of notations.** We have unified the labeling of eigenstates throughout the manuscript. In particular, the superscript (n) is now used consistently to denote the eigenstate (energy-level) index, and the previous inconsistency between superscripts and subscripts has been removed.
2. **Completeness of the IPR definition.** We agree that a denominator is required. In the revised manuscript, we have clarified the definition of the IPR following Eq. (4). It is now written as
$$\text{IPR}_{\mathcal{R}}^{(n)} = \frac{\sum_j |\psi_{j,\mathcal{R}}^{(n)}|^4}{\left(\sum_{j=1}^N |\psi_{j,\mathcal{R}}^{(n)}|^2\right)^2},$$
 which is invariant under an overall rescaling of the wave function and is therefore valid irrespective of whether the eigenstates are explicitly normalized beforehand.
3. **Logical consistency of the Lyapunov exponent definition.** We also agree that the original notation could be misleading because the site index and the asymptotic limit were not sufficiently distinguished. In the revised manuscript, we have therefore rewritten Eq. (4) using the system size N in the asymptotic

definition. This makes clear that the quantity is defined in the thermodynamic limit and removes the previous ambiguity between the site index and the limiting variable. In addition, we have expanded the related discussion in the Methods section to clarify that, in the present work, the meaning of this Lyapunov exponent follows from the exact non-unitary gauge mapping to the auxiliary Hermitian model. We thank the Reviewer for this helpful comment, which has improved the rigor of the presentation.

Comment 14

Specific Suggestions for Improving Figure Presentation: missing parameters, unclear boundary conditions, contradiction between definition and visualization, clarification of different disorder realizations, better criteria, and possible merging of figures.

Response:

We thank the Reviewer for these careful suggestions. We have revised the figure captions and related text throughout the manuscript to improve clarity and self-consistency.

In particular:

- (i) relevant parameters such as J_1 , J_2 , Δh , N , and the boundary conditions are now stated explicitly in the captions where needed;
- (ii) the captions and text now clearly specify whether data are obtained under PBCs or OBCs;
- (iii) we have added disorder-resolved spatially averaged profiles $\bar{I}_{j,\mathcal{R}}$ for multiple realizations in Fig. 1 and additional disorder-averaged results in Figs. 2, 3, 5, 7, 9, and 10;
- (iv) for Fig. 4, we now explicitly state that the two representative realizations differ only in the Bernoulli sequence of the imaginary gauge field, while all other parameters are identical.

Regarding the Reviewer's suggestion about using the fraction of complex-energy eigenstates as a more direct criterion, we agree and have now incorporated this quantity explicitly into the revised manuscript [Figs. 3(c) and 5(j)]. Regarding the possible merging of figures, after careful consideration we chose to keep the current organization because it better separates localization, topology, and dynamics, but we have revised the presentation to improve the conceptual connection among them.

Response to Reviewer 2

We sincerely thank Reviewer 2 for the positive assessment of our work and for the constructive suggestions. We are encouraged by the Reviewer's evaluation that the results are novel and the numerical evidence convincing. We have addressed all four suggestions in the revised manuscript, as detailed below.

Comment 1

The current study focuses primarily on the case with $J_2 \neq 0$. It would be beneficial to compute a phase diagram in the (λ, J_2) parameter space, distinguishing among the ENHSE phase, the localized phase, and an intermediate region where both types of states coexist.

Response:

We thank the Reviewer for this valuable suggestion. In response, we have added a new localization phase diagram in the (λ, J_2) plane, now shown as Fig. 7 in the revised manuscript. To distinguish the fully ENHSE phase, the fully localized phase, and the intermediate coexistence regime, we introduced an effective fractal exponent η defined from the realization-resolved averaged spatial profile $\bar{I}_{j,\mathcal{R}}$. Specifically, we define

$$\xi_{\mathcal{R}} = \sum_{j=1}^N \left(\bar{I}_{j,\mathcal{R}} \right)^2, \quad \langle \xi \rangle = \frac{1}{N_{\mathcal{R}}} \sum_{\mathcal{R}=1}^{N_{\mathcal{R}}} \xi_{\mathcal{R}}, \quad \eta = -\frac{\ln \langle \xi \rangle}{\ln N}. \quad (1)$$

This quantity captures the spatial inhomogeneity of the realization-resolved averaged profile: $\eta \approx 0$ in the fully ENHSE phase, $\eta \approx 1$ in the fully localized phase, and $0 < \eta < 1$ in the intermediate coexistence regime.

The resulting phase diagram clearly shows that for $J_2 = 0$ the coexistence regime is essentially absent, while for finite J_2 an intermediate coexistence window emerges and gradually broadens with increasing J_2 . This addition provides a global and systematic picture of how weak NNN hopping reshapes the anomalous localization structure.

Comment 2

In the presence of NNN hoppings, does the localization transition remain coincident with a complex-to-real spectral transition? Clarification on this point would be insightful.

Response:

We thank the Reviewer for this helpful suggestion. In the revised manuscript, we have added a systematic analysis of the imaginary parts of the PBC eigenenergies as functions of $\text{Re}(E_n^{\text{PBC}})$ and λ [Fig. 5(f)], together with representative PBC/OBC spectra [Figs. 5(g)-5(i)] and the realization-averaged fraction of complex-energy eigenstates [Fig. 5(j)].

These results show that, for generic finite-size realizations, the real-complex spectral boundary tracks the anomalous mobility edge: in the coexistence regime the sector below $E_c(\lambda)$ is real while the sector above $E_c(\lambda)$ remains complex, and above λ_{c2} the whole PBC spectrum becomes real. Thus, for generic finite-size realizations, the spectral transition is fully consistent with the localization transition.

Comment 3

The system sizes considered are multiples of 100. However, in quasicrystalline models, system sizes are often chosen as Fibonacci numbers. Could the choice of Fibonacci-sized systems affect the phase boundaries when $J_2 \neq 0$?

Response:

We thank the Reviewer for this important point. In response, we have added Appendix C, where we recalculate the localization phase diagram for two Fibonacci system sizes, $N = 377$ and $N = 610$.

The results show that the phase diagrams for these Fibonacci sizes are qualitatively and quantitatively consistent with the main-text results for $N = 300$. In particular, the transition points and the location of the anomalous mobility edge remain unchanged within numerical resolution. This confirms that our conclusions are robust with respect to the choice of system size.

Comment 4

For $J_2 \neq 0$, the results are presented for only a few parameter values. A more systematic scan—e.g., showing the Lyapunov exponent or spectral winding number as a function of λ —would offer a more comprehensive view. Comparing such results for $J_2 = 0$ and $J_2 = 0.1$, for instance, could more clearly illustrate the effect of NNN hoppings on localization and spectral topology.

Response:

We thank the Reviewer for this suggestion. In the revised manuscript, we have significantly expanded the systematic analysis for $J_2 \neq 0$. Specifically, we now present:

(i) a disorder-averaged Lyapunov-exponent phase diagram in the $(\text{Re}(E_n^{\text{PBC}}), \lambda)$ plane [Fig. 5(a)],

- (ii) the corresponding disorder-averaged real-complex spectral boundary [Fig. 5(f)],
- (iii) representative energy spectra [Figs. 5(g)-5(i)],
- (iv) the realization-averaged fraction of complex-energy eigenstates [Fig. 5(j)],
- (v) the energy-dependent winding number and mean winding number [Fig. 6], and
- (vi) the global (λ, J_2) phase diagram [Fig. 7].

We believe these additions now provide a comprehensive overview of how weak NNN hopping reshapes localization, spectral topology, and mobility-edge physics.

Response to Reviewer 3

We sincerely thank Reviewer 3 for the positive assessment that our results are new and reasonable, and for the important comments on significance and scope. In response, we have revised the manuscript to clarify the applicability of our framework, to better articulate the significance of the problem, and to address all specific questions in detail.

Comment 1

It is unclear to me whether their results meet the criteria of significance required by Communications Physics. Specifically, it is unclear to me whether their main result regarding the Lyapunov exponent applies to generic systems because they rely on an imaginary gauge transformation, which is not necessarily available.

Response:

We thank the Reviewer for this important and thoughtful comment. We fully agree that our result regarding the Lyapunov exponent should not be presented as a universal statement for arbitrary non-Hermitian systems. Its rigorous formulation in the present work relies on the exact non-unitary imaginary-gauge transformation, which is available for the physically relevant class of models considered here, but not for all non-Hermitian systems. In the revised manuscript, we have therefore clarified this scope explicitly and avoided any overly broad wording.

That said, we respectfully submit that this limitation does not diminish the significance of the present work for **Communications Physics**. The central question addressed here is not whether one can formulate a universal theorem for all non-Hermitian systems, but whether one can establish a controlled, physically transparent, and experimentally relevant characterization of **anomalous localization and mobility edges in non-Hermitian quasicrystals with disordered imaginary gauge fields**. We believe the answer is yes, for the following reasons.

First, the problem itself is highly timely and frontier-level. The ENHSE was introduced only very recently, yet it has already triggered rapid developments in both theory and experiment. In particular, experimental progress has been reported in **Zhong *et al.*, arXiv:2601.19749** and **Sun *et al.*, arXiv:2604.00088**; related transport phenomena in open systems have been studied in **Longhi, arXiv:2602.14698**; and related imaginary-gauge-field-induced critical behavior has been explored in **Dong, Zhu, and Zhang, arXiv:2603.28153**. These recent works, which also cite our study, indicate that disordered imaginary-gauge-field physics and ENHSE are already emerging as a rapidly developing research direction of broad current interest.

Second, the novelty of our work does not lie merely in using a gauge transformation as a technical step. Rather, the exact mapping provides the rigorous foundation for uncovering a set of **new physical conclusions in the original non-Hermitian frame**. Specifically, we show that: (i) in the standard non-Hermitian AAH limit, the localization transition is **anomalous**, in the sense that it connects a fully ENHSE phase and a fully Anderson-localized phase, rather than the conventional extended and localized phases;

(ii) with weak NNN hopping, the inherited Hermitian mobility-edge boundary acquires a **new non-Hermitian physical meaning**, separating Anderson-localized states from ENHSE-type macroscopic-accumulation states rather than extended states; and

(iii) for generic finite-size realizations, this anomalous localization structure is further correlated with finite-size spectral reality under PBCs, realization-dependent spectral winding, center-of-mass statistics, and wave-packet dynamics. To our knowledge, this combined characterization of anomalous localization, anomalous mobility edges, spectral topology, and winding-resolved dynamics in non-Hermitian quasicrystals with disordered imaginary gauge fields has not been established previously.

Third, the significance of the Lyapunov-exponent result is precisely that it allows one to sharply distinguish two classes of spatially confined states that would otherwise be difficult to separate using conventional diagnostics. In the present problem, both ENHSE-type states and Anderson-localized states have vanishing fractal dimension in the thermodynamic limit, so a more refined characterization is needed. The exact imaginary-gauge mapping makes it possible to identify the Lyapunov exponent as a rigorous localization diagnostic for this class of systems, and this in turn reveals that the same inherited boundary organizes a qualitatively different phase structure in the original non-Hermitian frame. We believe this is conceptually significant, even if the construction is not universally available in arbitrary non-Hermitian models.

For these reasons, we believe the present work meets the significance criterion not by claiming unrestricted universality, but by delivering a rigorous and timely advance on an important emerging problem, together with experimentally relevant diagnostics and a clear physical reinterpretation of localization and mobility-edge physics in a rapidly developing class of non-Hermitian quasicrystals.

To reflect this more precisely, we have revised the Introduction and Methods so that both the scope and the novelty/significance of our work are now stated more explicitly and with more careful wording. We hope this clarification addresses the Reviewer's concern.

Comment 2

Their model does not simultaneously host states with $w = 1$ and -1 . If this occurs, how would their result change?

Response:

We appreciate this question. In the parameter regime studied in the present work, a fixed realization does not host coexisting nontrivial winding sectors with opposite signs; rather, all nonzero winding numbers share the same sign within that realization. This is why the classification by the sign of the mean winding number $\overline{w}_{\mathcal{R}} = \pm 1$ is well defined in our study. We have now clarified this explicitly in the dynamical section.

If a different model or parameter regime were to host coexisting $w = +1$ and $w = -1$ sectors within the same realization, then a simple classification by the sign of $\overline{w}_{\mathcal{R}}$ would no longer be sufficient, and a more refined energy- or sector-resolved dynamical analysis would be needed. However, this situation does not arise in the present model and parameter regime.

Comment 3

Figures 4(a,b) show that the winding number flips its sign when the disorder is changed. The authors should clarify the specific difference in the parameters.

Response:

We thank the Reviewer for this suggestion. We have revised the caption and the relevant discussion of Fig. 4 to make this point explicit. The two representative realizations shown in the left and right panels differ only in the Bernoulli disorder configuration of the imaginary gauge field; all other parameters, including J_1 , J_2 , λ , Δh , the system size, and the boundary condition, are identical. This is now clearly stated in the revised caption of Fig. 4.

More importantly, in the revised manuscript our conclusions are no longer based only on single-disorder realizations. Instead, we have introduced disorder-averaged analyses in multiple parts of the paper to demonstrate that the main physical conclusions remain valid at the statistical level and are not artifacts of a particular realization.

Specifically, disorder-averaged results are now included for the fractal dimension, Lyapunov exponent, and center-of-mass fluctuation in Fig. 2; the fraction of complex-energy eigenstates under PBCs in Fig. 3(c); the Lyapunov-exponent phase diagram and the real-complex spectral boundary for $J_2 = 0.1$ in Fig. 5; the phase diagram in the (λ, J_2) plane in Fig. 7; and the winding-resolved as well as full disorder-averaged dynamics in Figs. 9 and 10.

Therefore, while Fig. 4 still serves as a realization-resolved illustration showing that the winding sign may flip when the Bernoulli disorder sequence is changed, the revised manuscript now makes clear that our central conclusions do not rely on any particular disorder sample. Rather, the same anomalous localization structure and its associated spectral and dynamical signatures persist under disorder averaging. We believe this revision substantially improves both the statistical robustness and the clarity of the manuscript.

Comment 4

Their numerical data indicate that the center-of-mass becomes of order N in the region of Anderson localization and 1 in the region of ENHSE. It is unclear to me why it shows such a difference.

Response:

We thank the Reviewer for pointing out this issue. We have carefully rechecked and recalculated the center-of-mass-related data. In the revised version we have added the finite-size scaling of disorder-averaged center-of-mass fluctuation $\langle S \rangle$ in the inset of Fig. 2(c), where we plot the log-log dependence of $\langle S \rangle$ on the system size N in the two representative phase regimes. The new scaling analysis shows that $\langle S \rangle \propto N$ in the fully ENHSE phase, whereas $\langle S \rangle \propto N^2$ in the fully Anderson-localized phase.

The physical origin of this difference is as follows. In the ENHSE regime, for a fixed disorder realization, many eigenstates macroscopically accumulate around a few disorder-selected bulk positions. As a result, their center-of-mass positions are not spread across the whole chain, but instead remain clustered around a limited set of preferred locations. The corresponding fluctuation therefore grows only linearly with the system size. By contrast, in the Anderson-localized regime, although each eigenstate is individually localized, the localization centers of different eigenstates are distributed approximately uniformly over the entire lattice. Consequently, the spread of the center-of-mass positions spans the full chain length, so the fluctuation scales quadratically with N . We have clarified this point in the revised manuscript and included the new finite-size scaling plot to make the distinction quantitatively explicit.

Once again, we sincerely thank the Editor and all the Reviewers for their careful reading and constructive suggestions. We believe that the revised manuscript has been substantially strengthened in clarity, rigor, and significance. In particular, we have clarified the physical meaning of the anomalous localization transition and anomalous mobility edge, added systematic disorder-averaged and finite-size analyses, strengthened the methodological justification of the Lyapunov exponent, and developed a statistically transparent winding-

resolved dynamical characterization. We hope that the revisions and responses satisfactorily address all concerns, and we respectfully submit the revised manuscript for further consideration.

Response to the Reviewers

Manuscript ID: COMMSPHYS-26-0117A

Title: Anomalous Localization and Mobility Edges in Non-Hermitian Quasicrystals with Disordered Imaginary Gauge Fields

Reviewer #1

Comment:

The authors have provided a careful and detailed response to the previous comments. The revised manuscript and response letter show substantial improvements.

In particular, the meaning of the term “anomalous” has been clarified. It is now made clear that this term refers to the physical interpretation of the mobility edge in the original non-Hermitian frame, rather than to a new analytical expression. This clarification addresses one of the main concerns raised in the previous report.

The revised manuscript also provides a more balanced discussion of the relation among the present model, the auxiliary Hermitian generalized AAH model, and previous studies on ENHSE. The scope and novelty of the work are now stated more appropriately.

The additional numerical analyses are also appreciated, including disorder-averaged results, finite-size scaling, the fraction of complex-energy eigenstates, and winding-resolved dynamical averages. These additions significantly strengthen the evidence supporting the authors' conclusions and address the previous concern that the results relied too heavily on single disorder realizations.

The discussion of the dynamical results has also been improved. The newly added winding-resolved averaging provides a clearer connection between the spectral winding number and the direction of the center-of-mass drift, making the dynamics section more convincing.

Overall, the authors have satisfactorily addressed the previous concerns. The revised manuscript is now clearer, more rigorous, and better supported by numerical evidence. I therefore recommend publication in Communications Physics.

Response:

We thank Reviewer #1 for the positive assessment and for recognizing the improvements made in the revised manuscript. We are pleased that the clarification of the term “anomalous”, the discussion of the relation among the present model, the auxiliary Hermitian generalized AAH model and previous studies on ENHSE, and the additional numerical analyses satisfactorily addressed the reviewer's previous concerns.

We also appreciate the reviewer's positive comments on the improved discussion of the dynamical results and the winding-resolved averaging. In the final revision, we have made additional editorial and formatting improvements requested by the journal, while preserving the scientific content that the reviewer found satisfactory.

Reviewer #2

Comment:

no additional comments

Response:

We thank Reviewer #2 for the positive evaluation and for indicating that there are no additional comments.

Reviewer #3

Comment:

The revised manuscript clearly presents its novelty: revealing a qualitatively distinct form of localization physics in non-Hermitian quasicrystals with disordered imaginary

gauge fields. In addition, the authors addressed all of my previous comments properly. Therefore, I recommend the publication in Commun. Phys.

Response:

We thank Reviewer #3 for the positive assessment and for recommending publication in Communications Physics. We are pleased that the reviewer found the novelty of the work clearly presented and considered that all previous comments had been properly addressed.

We thank the editor and reviewers again for their constructive assessment and guidance.