Dear Editor,

Thank you for handling our manuscript and for sending us the referee reports. We have taken these reports very seriously, and responded to their comments in a complete and exhaustive manner. Before we address their comments below, we would like to make several remarks to you specifically. One reason for this is that they relate to the nature of some of the referees' reservations, which you have mentioned in your message to us. The second reason is that they deal with what we believe are differences in viewpoint, and as such cannot be simply addressed by making specific changes to the manuscript. Both of the referees acknowledge the novelty of our work, and neither of them criticizes its validity, or the correctness of our results. Their main criticisms relates to the significance of our work, which is by nature quite subjective, and on which we strongly disagree.

— The first remark deals with the area of physics that our work belongs to: single-particle physics. The first referee considers it a weakness that our work "does not generalize in any trivial manner to interacting systems." Similarly, for the second referee the "concern lies with how specific the model is to the kind of models considered here (free-fermions)." The second referee considers this to be a "major reason" for thinking that the manuscript is not suitable for SciPost Physics. In this respect, please note that the study of non-interacting systems is an active field of physics. Many selective journals, including SciPost Physics, regularly publish and advertise papers which do not include many-body effects and do not trivially generalize to interacting systems. The main reason that such a study is not purely of academic interest is Landau's great insight over 60 years ago that (weakly-)interacting fermions essentially behave like non-interacting fermions with renormalized parameters. This insight is the basis of the band-structure model for materials, the classification of topological insulators, etc. We kindly ask you, when determining the quality of our paper, to please not judge this field of research, non-interacting topological systems, as being one of low significance.

— The second remark deals with the indirect method we have developed for simulating Floquet systems, which is listed as a weakness by the first referee. They state that "simulating Floquet systems on a computer has similar value to the 'analog' simulation technique" we present. Our work proposes a novel meta-material platform: using reflection matrices to simulate topological Floquet systems. Note that the reflection matrix of an insulating system is unitary by 'construction' and as such is a direct (though artificial) implementation of a Floquet system. We agree that this is an analog simulation technique (different from the simulation on a computer which is digital). It is akin to how photonic crystals, electric circuits, or other artificial systems can be used to simulate non-interacting topological matter. For instance, the Maxwell equations can be used to simulate non-interacting topological systems in photonic crystals [theory: Phys. Rev. Lett. **100**, 013905 (2008), experiment: Nature **461**, 772–775 (2009)]. The Kirchhoff equations can be used to simulate single-particle topological systems in electronic circuits [theory: Commun. Phys. **1**, 39 (2018), experiment: Nature Phys. **14**, 925–929 (2018)]. Finally, the current-phase relation of a Josephson junction can be used to simulate non-interacting topological semimetals [theory: Nature Commun. **7**, 11167 (2016), experiment: Nature Nanotech. **11**, 1055-1059 (2016)]. All of the above-cited papers contain artificial methods of obtaining topological systems, and none of them easily generalize to interacting systems.

Some of our friends and collaborators are of an opinion similar to that of the first referee. They consider these meta-material platforms simply as "analog differential-equation simulators," a task which could be equally well, if not better performed on a computer. Others consider this an important, emerging field of physics, as evidenced by publications in highly selective journals, a few of which we have cited above. We believe that this difference of viewpoint constitutes an important debate in the physics community. We kindly ask you, when judging the significance of our paper, to please consider both sides of this debate, even if they are not equally represented among the referees.

— Finally, we would like to show two specific ways in which this paper, which we consider to be one of our best works, fulfills the expectations of SciPost Physics. We use the SciPost Physics expectations as they are listed on `https://scipost.org/SciPostPhys/about`.

1. We *present a breakthrough on a previously-identified and long-standing research stumbling block*, namely the problem of decoherence due to noisy driving in Floquet systems. As shown by the evidence we provide in our reply to the second referee, noise-induced decoherence is an important limitation to the experimental realization of non-interacting, unitary topological phases. Please see our detailed discussion on this when answering the referee's 5^{th} question. Further, by the same evidence, there is an active theoretical effort in identifying methods to reduce this effect. In our work, we have found a novel way of removing it completely: noisy driving cannot lead to decoherence if unitary topological phases are realized without any driving.

2. We *open a new pathway in an existing or a new research direction, with clear potential for multipronged follow-up work*. This is achieved by introducing a new meta-material platform, which simulates topological systems using the reflection matrix. This allows for multiple follow-up theoretical investigations which are outside the scope of our current paper. They include looking at different types of topological systems, in different dimensions and symmetry classes, but also thinking about how to include many-body effects. In addition, our work opens the possibility of experimentally investigating these novel meta-materials in a variety of settings. We have already contacted and presented our results to some of the authors of the experimental, meta-material paper Nature Materials **18**, 1292–1297 (2019), our Ref. [49], who were excited about our findings. Further, we have already begun scientific collaborations with two other experimental groups, who are now starting to fabricate the devices required to test our theoretical predictions, in two very different types of physical system.

In light of the above, and given that we have fully and thoroughly addressed the comments of the referees below, we are firmly convinced that our work is suitable for publication in SciPost Physics. We hope that with this reply, you will be convinced of this as well.

Sincerely,

Selma Franca, Fabian Hassler, and Ion Cosma Fulga

Report of the First Referee

We thank the referee for agreeing to review our paper and for their comments. Based on the scores given by the referee in different categories, they consider our work to be of *top validity*, *good originality*, and *high clarity*. However, the referee considers our paper to be of *low significance*. We will comment on this while addressing their points below.

First Referee: *Strengths*

1. The paper offers a new perspective on the topological nature of HOTI boundary phenomena, that of scattering matrices.

2. It may, with the reservation below, lead to interesting connections between the classification of HOTIs and Floquet systems.

3. It provides a new, although somewhat indirect, way for experimenting with Floquet topological phases.

Our response: We thank the referee for pointing out several strengths of our paper. Based on their selection of strengths, we have realized the importance of better stressing one of the main strengths of our work: we find a way of eliminating decoherence due to driving noise in Floquet topological phases. We have further emphasized this point in our conclusion section (see list of changes).

First Referee: *Weaknesses*

1. The mapping between HOTI experiments and Floquet experiments is somewhat artificial and indirect. If one is truly interested in witnessing Floquet related effects, one can argue that simulating Floquet systems on a computer has similar value to the "analog" simulation technique presented here.

Our response: We fully agree with the referee that our proposal for simulating Floquet systems is somewhat artificial and we respect their opinion that it is of similar value to a computer simulation. We are familiar with this point of view, as it is also shared by some of our friends and collaborators. However, as we have mentioned to the editor above, this is one side of a debate currently being being held in the physics community. The other side of the debate, which we have asked the editor to also take into account, considers these 'artificial' experiments as an important, emerging field of physics, and points to many publications in highly-selective journals to support this argument.

We do not believe that specific changes to our manuscript will change the referee's point of view on this matter. However, we have expanded the conclusion section in order to better highlight the emerging field of topological meta-materials, and to better point out that our work in fact introduces a novel meta-material platform: simulating topological phases using reflection matrices.

First Referee: *2. It does not generalize in any trivial manner to interacting systems where richer physics can emerge.*

Our response: Our work belongs to the research field of non-interacting topological phases. We have not included any many-body effects in our discussion, and generalizing our work to interacting systems is a nontrivial task, as the referee correctly points out. We respectfully disagree with the referee's opinion that a paper is weak if it does not generalize in any trivial manner to interacting systems. Our point of view is that the ease with which many-body effects can or cannot be included should not be judged as either a strength or a weakness when it comes to how suitable a paper is for publication. The evidence we provide for this is as follows:

1) SciPost Physics, or any other selective journal, does not list this as part of their expectations or acceptance criteria.

2) Looking at the content of papers published in top journals (a few of which are cited in our remarks to the editor above), most studies on topological systems, especially those dealing with unitary operators, are performed on a purely single-particle level, and do not easily generalize to interacting systems.

We have not included many-body effects in the revised version of the paper, as they are outside of the scope of our current work. However, in the new conclusion section (see list of changes), we have mentioned that also for interacting systems, some work exists on the properties of the reflection matrix, and that topological invariants of the reflection matrix have been successfully defined also for some interacting systems [Phys. Rev. Lett. **113**, 057003 (2014); Phys. Rev. B **93**, 125433 (2016); Phys. Rev. B **96**, 205442 (2017); Phys. Rev. Research **2**, 023243 (2020)]. We hope that our work will motivate others to study many-body generalizations of our results.

First Referee: *3. The paper envisions a physically-inspired mapping/dimensional-reduction between static topological phenomena and Floquet, via scattering matrices. However, it is not clear to me that such a mapping can exist. For instance, I believe that scattering of the HOTI edge considered in that work would coincide with scattering from (the bulk and boundary of) an SSH chain. The authors should clarify whether this envisioned mapping between static and Floquet phenomena can indeed be 1 to 1.*

We thank the referee for raising this point, which has helped us to improve the quality of our work. According to the topological classification of HOTIs developed in Phys. Rev. B **97**, 205135 (2018), HOTIs fall within two large classes. The first are called *intrinsic* HOTIs, which require lattice symmetries and host protected modes (e.g. at corners/hinges) associated to a bulk topological invariant. The second class is that of *extrinsic* HOTIs. The latter do not require lattice symmetries, and can host topologically protected corner states even in the presence of a topologically trivial bulk. The corner states of extrinsic HOTIs are instead protected by the nontrivial, strong topological invariant of the system's *surface*. Extrinsic HOTIs help explain the robustness of these systems against disorder, which breaks all of the lattice symmetries enabling bulk topology, but does not destroy the corner states. This is because the latter are in fact protected by the nontrivial strong topology of the surface. Examples of papers where we have studied extrinsic HOTI with trivial bulks and nontrivial surfaces are Phys. Rev. B **100**, 075415 (2019) and Phys. Rev. Lett. **123**, 266802 (2019).

Regarding the referee's question about scattering from a single SSH chain. Consider an infinite waveguide, which hosts propagating modes and which is intersected by a single, one-dimensional SSH chain. Since the SSH chain is very narrow, the propagating waveguide modes encountering it will not be fully back-reflected, but instead they will be partially transmitted. In the strictest 1D limit, the potential barrier encountered by propagating waves will be infinitely narrow. Thus, the resulting reflection matrix will not be unitary, and cannot simulate a Floquet topological phase. However, if the 1D SSH chain is attached to a wide insulating barrier (an insulating bulk) then waves will be back-reflected, and the reflection matrix will be unitary up to exponential precision. In this case, the insulating bulk which hosts a topologically nontrivial surface (the SSH chain) is precisely an extrinsic HOTI.

Notice that even for a waveguide which is only semi-infinite, and simply terminates with an SSH chain, the embedding of the 1D SSH chain into a higher-dimensional space is required to produce a unitary reflection matrix. There needs to exist a semi-infinite insulating barrier (or bulk) on the other side of the SSH chain, such that the SSH chain forms the boundary of this insulating barrier. It is of course possible to study the semi-infinite waveguide problem while neglecting the degrees of freedom characterizing the insulating barrier, by assuming that the latter have a gap much larger than all other energy scales. In this case, the problem becomes analogous to that of studying the effective low-energy description of the surface of an extrinsic HOTI.

In response to the referee's question, we have clarified the notions of intrinsic and extrinsic HOTIs in the revised version of our paper (see list of changes). We have explained that our approach yields topologically nontrivial unitary phases even if the lattice symmetries protecting the bulk invariant of intrinsic HOTI phases are broken, and that a topologically nontrivial surface (meaning an extrinsic HOTI) is sufficient to simulate Floquet phases. As we point out in the revised version of the paper, Section 5, in which we study the effects of disorder, is precisely an example of this.

Finally, concerning the referee's question about our mapping between static and Floquet systems, we comment on this point later in the reply, since this question appears both in the report and in the list of requested changes.

First Referee: *Report*

The current work studies the reflection of a boundary of a 2d HOTI and shows that the topological nature of the edge is reflected in the spectrum of the back-scattering matrix. This is then suggested as a means to simulating topological Floquet systems, and also as a new dimensional reductions scheme between topological phenomena in different dimensions.

I'm somewhat on the fence with this work. The dimensional reduction technique *suggested is novel as far as I know. However the authors, in my mind, do not describe the detail and potential of such a mapping in sufficient detail. It is clear, to some extent, that some topological features of the edge state should leave signatures on the reflection matrix but are these signatures or a real mapping between two seemingly different topological phenomena?*

An aspect of the work which is promoted more strongly concerns the simulation of topological Floquet systems. However, this way of experimenting with Floquet system seems somewhat indirect to me and also limited to non-interacting systems where simulation techniques are more abundant, most notably on a computer.

Given these two points, I believe this work falls slightly below the acceptance threshold. However, I'd reconsider this if the authors flesh out their dimensional reduction technique in more detail (see requested changes below).

Requested changes

1. As a test of their dimensional reduction approach, can the authors show a matching between classification tables of Floquet topological phases and HOTIs? One which, obviously, translates the symmetries in a clearly prescribed manner.

Our response: We thank the referee again for their report. We have already commented above on our difference of opinion concerning non-interacting topological phases as a significant field of research, as well as concerning metamaterial platforms as an indirect, but important means of probing topology. We appreciate very much the fact that the referee is still willing to reconsider our work despite these different viewpoints, provided we better describe the dimensional reduction map that we introduce.

In the revised version of our manuscript, we have expanded the appendix to include a more rigorous statement of the dimensional reduction map (see list of changes). The statement is as follows:

- Using our dimensional reduction map, every d-dimensional Hermitian TI of order $d \geq 2$, in any symmetry class S, maps to a $(d-1)$-dimensional unitary TI of order $(d-1)$, in the same symmetry class S, provided that the parent system's topological corner states are robust against lattice-symmetry breaking.

We now explain this statement and outline the new appendix of our revised manuscript. A TI in d dimensions is said to be of order N if it hosts topologically protected, gapless boundary modes of dimension $(d-N)$. The original, strong TIs have an order $N = 1$, so they are called first-order topological phases, since their bulk has dimension d and their gapless surface states have dimension $d-1$. A HOTI with zero-dimensional topological corner states, the focus of our work, is thus a HOTI of order d in d dimensions.

In the new version of our paper, we have explicitly shown that our dimensional reduction map preserves the symmetry class of the tenfold way classification of Altland and Zirnbauer, by separately treating time-reversal, particle-hole, as well as chiral symmetry. Thus, if a Hermitian system is in symmetry class S, the dimensionally-reduced unitary will still be in symmetry class S.

Our dimensional reduction map applies to HOTIs with corner states, in any dimension for which corners can exist (that is $d \geq 2$), as long as they do not rely on lattice symmetry to be protected. This includes all extrinsic HOTI phases, as well as all intrinsic HOTI phases which are converted to extrinsic HOTIs when lattice symmetries are broken. Based on the classification results of Phys. Rev. B **97**, 205135 (2018) and Phys. Rev. X **9**, 011012 (2019), such d-dimensional TIs of order d with $d \geq 2$ are possible in the following symmetry classes: D, DIII, AIII, BDI, and CII. We then provide a generic construction algorithm based on the reflection matrix, which works in a model-independent way, and produces a unitary TI of dimension $d-1$ and order $d-1$ in the same symmetry class. We show that this unitary TI must host topologically protected π-modes at its boundaries, a consequence of its topologically nontrivial nature.

Finally, we comment on the referee's earlier question on the 1-to-1 nature of this map. This dimensional reduction map is not 1-to-1. If it were, this would imply the existence of an inverse, dimensional raising map, which starts from a reflection matrix and produces a higher-dimensional static HOTI. There is no unique way of doing this, since the reflection matrix encodes only the properties of states close to the Fermi level, whereas the full static HOTI contains also degrees of freedom far from the Fermi level. We note that a lack of invertibility is not a detriment when it comes to establishing connections between topological phases. For instance, the HOTI classification of Phys. Rev. B **97**, 205135 (2018), as well as the original, "tenfold way" classification of TIs in New J. Phys. **12**, 065010 (2010) are based on dimensional reduction maps which are not invertible in general. The reason is the same as in our case: the map "throws away" high-energy degrees of freedom. We wish to thank the authors of those two papers, Max Geier and Shinsei Ryu, for clarifying this last point for us. We have included their names in our list of acknowledgments.

Report of the Second Referee

We thank the referee for agreeing to review our paper, and regret the technical difficulties they encountered during the submission of their report.

Second Referee: *The manuscript entitled "Simulating Floquet topological phases in static systems" attempts to offer a new platform for realizing Floquet topological phases on edges of static higher-order topological phases. While the idea appears to be new, my concern lies with how specific the model is to the kind of models considered here (free-fermions) and its limited significance since such models, driven or otherwise, can be simulated quite efficiently. Moreover, there seems to be a fair amount of conceptual issues that have not been touched upon in this work, either in the way of connecting to previous work or distinguishing the current one from them.*

Our response: We note that the referee recognizes the novelty of our work, and does not criticize the validity or correctness of our results. The referee lists two concerns with regard to our paper. These are:

1. Our work is specific to non-interacting systems.

2. Based on the claim that "such models can be simulated quite efficiently," the referee considers our work to be of limited significance.

Our work is indeed specific to non-interacting systems, as are the majority of papers dealing with topological phases, and especially with unitary topological phases. As mentioned both to the editor and to the first referee above, we do not consider this to be either a strength or a weakness of a paper. The evidence we have provided in support of our claim is the number of highly-cited papers published in selective journals, including SciPost Physics, which are specific to single-particle systems. In response to the referee's comment, in our revised version we have included a discussion on the possibility of generalizing this work to interacting systems (see list of changes and reply to the first referee).

The second concern of the referee is based on the claim that such models can be simulated quite efficiently. We will come back to this point and discuss it in detail when we address the referee's fifth question.

In the following, we fully address all of the conceptual questions of the referee.

Second Referee: *1. The authors use the fact that the reflection matrix on a d-1 edge of a higher-order TI (with gapless topological states at the d-2 corners) is analogous to a d-1 Floquet unitary with topological modes at zero or pi quasienergy (or both). However, it is not at all clear from the manuscript how would that translate to some non-trivial response, such as the frequency dependence of spectral functions, characteristic of non-trivial Floquet phases?*

Our response: In Floquet phases obtained by periodic driving, the frequency dependence of spectral functions is a direct consequence of the driving. The frequency itself is conventionally measured in units of the inverse driving period. In our proposal, unitary TIs which are analogous to Floquet phases are realized without any driving, so the topological response is not a frequency dependence. Instead, the topological response is the quantized phase difference of π (or 0) occurring when waves are backscattered from the boundaries of the waveguide, as shown in our Figs. 1 and 2. The different form of the topological response is characteristic to meta-materials. For instance, in a Chern insulator the topological response is the quantized electric Hall conductivity. When the Chern insulator is simulated in a photonic crystal, there is no electric Hall conductivity, and the topological response is instead given by the unidirectional propagation of light.

In response to the referee's question, we have better clarified in the revised paper that the quantized phase difference of backscattered waves is a topological response. We now state this both in the conclusion section, as well as when presenting our results on page 6 (see list of changes).

Second Referee: *2. The non-trivial topology of the reflection matrix comes about due to the pi phase picked up at the corner states. How does this generalize to richer and arguably, more non-trivial Floquet phases, such as those in Z_3 parafermion chains [Phys. Rev. B 94, 045127 (2016)]?*

Our response:

We thank the referee for pointing out this work, we have cited it in the revised version of our paper. Our work does not trivially generalize to interacting systems, as was correctly pointed out also by the first referee. However, the reflection matrix and its topological invariants have been analyzed in certain many-body systems [Phys. Rev. Lett. **113**, 057003 (2014); Phys. Rev. B **93**, 125433 (2016); Phys. Rev. B **96**, 205442 (2017); Phys. Rev. Research **2**, 023243 (2020)]. While the inclusion of interactions remains outside of the scope of our paper, we have expanded the conclusion section to discuss this possibility (see list of changes).

Second Referee: *3. The authors use a specific invariant based on scattering matrices to characterize the topology of the reflection matrix and say that they cannot use the usual winding invariants used for Floquet unitaries as they don't have access to the instantaneous eigenmodes. This begs the question that whether the results presented in the manuscript can be interpreted as genuine Floquet topological modes or just a constructed unitary which mimicks them. After all, the non-trivial topology in the latter is due to how the quasienergies wrap around the quasienergy-momentum Brillouin zone.*

Our response: It is true that our work does not propose to realize Floquet topological modes as obtained by periodic driving. As the referee correctly points out, we show how to construct a unitary which mimics them, thus simulating Floquet topological phases in static systems. Our work introduces a novel meta-material platform, a fact we have better emphasized in the revised version (see list of changes).

Nontrivial topology in unitary systems can be related to the way in which quasienergies wrap around the Brillouin zone during the time evolution, as the referee correctly points out. However, this is a one-way implication, not an equivalence: Quasi-energy winding implies a topologically nontrivial unitary, but not the other way around. One example of this is the translation operator e^{ik}. It is unitary, has a nonzero winding number as a function of k, but cannot be expressed in the language of quasienergy wrapping around the Brillouin zone during some time-evolution process. For e^{ik} one cannot define some fictitious time-evolution operator with $U(t = T) = e^{ik}$ and T the driving period. This is because there does not exist a path connecting it to the identity matrix, $U(t = 0) = \mathbf{1}$, while preserving both unitarity and continuity.

However, when a an operator can be continuously connected to the identity matrix while preserving unitarity, then its topology can indeed be associated with quasi-energy winding. In response to the referee's question, we now show a specific example of this in the revised version of our paper (see list of changes). Starting from the reflection matrix, we construct a parametrization which unitarily and continuously deforms it to the identity matrix, thus defining a fictitious time-evolution process. The quasienergy winding during this deformation matches the reflection matrix invariant we use in the main paper.

Second Referee: *4. Regarding the issue of dimensional reduction, it is well known that a d-dimensional Floquet system can be mapped onto a (d+1)-dimensional static system [Phys. Rev. 138, B979 (1965); Phys. Rev. A 7, 2203 (1973)]. Is the idea of dimensional reduction described in this work related to the aforementioned old works? If yes, then the authors should comment on the connection; if not then how is their idea different?*

Our response: Our dimensional reduction procedure is different from the mapping described in these works. The latter show how to construct a Floquet Hamiltonian for periodically driven systems in an extended Hilbert space, a procedure commonly referred to as the "extended zone scheme." As explained in Phys. Rev. 138, B979 (1965), the extension of the Hilbert space can be seen physically as arising from the interaction of a system with a quantized driving field. The Hilbert space extension corresponds to the number of photons in the driving field, and transitions between states of the Floquet Hamiltonian correspond to processes which involve the exchange of photons between the system and the driving field (either absorption or emission). This is different from our dimensional reduction procedure, because we consider systems in the absence of any driving field. Instead, our work deals with simulating Floquet topological phases in static systems.

In response to the referee's question, we have added a sentence to the paper explaining that our dimensional reduction scheme is different from these works, since the latter consider the interaction between a system and a driving field, whereas we do not have a driving field.

Second Referee: *5. Finally, the authors comment that one benefit of their work is that it does not suffer from decoherence or heating due to driving. I am not sure of the significance of this comment since free-fermionic systems anyway do not heat up and studying Floquet topological phases therein does not suffer from that problem. The authors claim that in reality, driving is noisy and it could lead to heating. This comment is somewhat unfair as at the end of the day, these are all model studies, and in reality fermions aren't non-interacting either.*

Our response: The referee states that they are uncertain of the significance of our discussion on driving-induced decoherence. The reason for this uncertainty is their claim that "free-fermionic systems anyway do not heat up." Concerning the issue of noisy driving, the referee characterizes our statements as being "unfair," and provides two arguments to support their claim. These arguments are:

1. "at the end of the day, these are all model studies"
2. "in reality fermions aren't non-interacting either"

We begin by clarifying the general issue of decoherence in single-particle systems, and then address its implications for specific experiments.

Non-interacting periodically-driven systems decohere due to unavoidable noise in their driving. This poses an important limitation on the time scales accessible in experiment, since usually the decoherence due to driving noise is *exponentially fast*. Starting from early experimental studies of the quantum kicked rotor in the 90s, this issue and its importance has been recognized both experimentally and theoretically, and an active research direction has emerged from the attempt to reduce the rate of noise-induced decoherence. The evidence we provide in support of this statement is the following list of papers:

Phys. Rev. Lett. **80**, 4111 (1998).
Phys. Rev. Lett. **81**, 1203 (1998).
Phys. Rev. E **62**, 3461 (2000).
Phys. Rev. Lett. **87**, 074102 (2001).

Chaos, Solitons and Fractals **16**, 409 (2003).
Phys. Rev. E **70**, 036217 (2004).
New J. Phys. **16**, 113039 (2014).
Phys. Rev. Lett. **117**, 144104 (2016).
Phys. Rev. A **95**, 013401 (2017).
Phys. Rev. Lett. **118**, 174101 (2017).
New J. Phys. **19**, 083003 (2017).
Phys. Rev. Lett. **120**, 216801 (2018).
Phys. Rev. B **98**, 214301 (2018).
Phys. Rev. B. **99**, 014301 (2019).
Phys. Rev. Research **2**, 033495 (2020).
Nature Physics **16**, 1058 (2020).
arXiv:2006.10736
arXiv:2010.10073

All of these papers, both theoretical and experimental, study noise-induced decoherence in non-interacting driven systems, or attempt to find methods to reduce this effect. In our work, we have found a novel method of eliminating this effect completely: driving noise cannot lead to decoherence in unitary topological phases if the latter are realized without any driving. Thus, we *present a breakthrough on a previously-identified and long-standing research stumbling block*, as stated in our comments to the editor.

We now turn to the implications of noise-induced decoherence for experiments on Floquet topology, and address the previous comment of the referee: "such models can be simulated quite efficiently." Due to the fact that it is exponentially fast, noise-induced decoherence is an important limiting factor when it comes to the time scales typically possible in experiments on Floquet topology. This can be seen, for example, in experimental realizations of so called "anomalous Floquet topological phases," which have only been achieved in meta-materials so far. In the first experiments, Nature Commun. **8**, 13918 EP (2017) and Nature Commun. **8**, 13756 EP (2017), the non-interacting Floquet phase is simulated using photonic crystals, and the total amount periodic driving is limited to two, and three driving cycles, respectively. In the more recent, Nature Physics **16**, 1058 (2020), a non-interacting anomalous Floquet phase is simulated in an optical lattice loaded with potassium atoms. Even with the large amount of control available in cold atom experiments, the authors still find that the topological states decay exponentially fast, and have a lifetime which is two orders of magnitude smaller than that of the static non-interacting system, roughly 10-15 driving periods (see page 5 of their Supplementary).

In light of the evidence presented above on the experimental limitations of noise-induced decoherence, we do not consider that such systems can be simulated quite efficiently. On the contrary, we consider it a remarkable achievement that such topological effects have been measured in spite of these limitations. One of the strengths of our paper is that we do not treat unitary topology only on the level of a "model study," but we also consider some of the real-life experimental limitations of meta-material platforms. We address the issue of decoherence, and include a section on the experimental feasibility of our proposal.

In the revised version of our paper, we have expanded the conclusion section to contain a more detailed discussion on the importance of decoherence due to driving noise, and we now cite the above papers. We hope that the evidence above clarifies that single-particle systems do indeed decohere, that this poses an important limitation to experiments, and that it is not unfair to recognize this fact.

Second Referee: *Overall, I think the manuscript is well-written but has quite a few gaps pertinent to earlier concepts on Floquet systems as well as analysis of the current model. However, the major reason why I think the manuscript in its present form is not suitable for SciPost is due to the limited applicability and significance of the results.*

Our response: We thank the referee for their statement that our work is well-written. We have fully answered all of the questions raised by the referee above, and made appropriate changes to the paper in response. Also the major reasons for the referee's opinion that our work is not suitable for Scipost Physics have been thoroughly clarified. The first was that our work is specific to non-interacting systems. We have addressed this point in the beginning of our reply to the referee, as well as in our reply to the first referee and in our beginning message to the editor. The second was an assessment that our work is of limited significance, based on the claims that "such models can be simulated quite efficiently" and that "free-fermionic systems anyway do not heat up." We have provided a detailed, evidence-based rebuttal to these claims when answering the referee's fifth question.

List of changes

— In response to comments by both referees, we have rewritten the conclusion section. It now discusses the emerging field of topological meta-materials, the importance of noise-induced decoherence, and the existence of works dealing with reflection matrix topological invariants in strongly interacting systems. Further, the new conclusion specifies the topological response of the reflection matrix (which is also stated on page 6), as a response to the second referee's question.

— We have followed the advice of the first referee and described in detail the dimensional reduction map between Hermitian and unitary topological phases. We have added a new Appendix (App. B) which discusses how time-reversal, particle-hole, and chiral symmetry translate across the dimensional reduction, thus proving that our map preserves the Altland Zirnbauer symmetry class. Further, in the new Appendix C we describe in detail this dimensional reduction procedure, showing clearly what is its range of validity, as requested by the first referee.

— Also in reply to the first referee's report, we now define the notions of intrinsic and extrinsic HOTI phases at the end of Section 3, clarifying how they relate to our reflection matrix construction. The disorder-induced change from an intrinsic to an extrinsic HOTI phase is now also mentioned at the end of Section 5.

— To respond to the second referee's question about quasi-energy winding in Floquet systems, we have added a new Appendix section (App. F). In it, we show how to deform the reflection matrix to the identity operator, thus defining a fictitious time-evolution process. This deformation is also included in the new version of the code which we have uploaded together with the paper. The invariants obtained in this way match the scattering matrix invariants we use in the main text. Further, we have modified the beginning of Section 4 to refer to this appendix and to mention the possibility of deforming the reflection matrix.

— Finally, at the end of Appendix C describing our dimensional reduction map, we have addressed the question of the second referee by mentioning that our procedure is different from that of [Phys. Rev. 138, B979 (1965); Phys. Rev. A 7, 2203 (1973)]. These papers deal with driven systems, whereas we consider static systems.