Reviewers' comments:

Reviewer #1 (Remarks to the Author):

In "Anomalous and normal dislocation modes in Floquet topological insulators", the authors studied various topological modes (ie 0 and pi modes) around dislocation points in a few static and Floquet models. Overall, the study is quite rigorous and deserving of publication. However, in terms of novelty, it is currently not quite clear which results contain novel physics, other than the explicit solution to specific models that have not been studied this way previously.

Some specific comments:

1. In Eqs 3 & 4, a specific kicked Floquet driving term was used. Although its simple and convenient form is evident in Eq S3, the authors should elaborate more about the significance of this choice of the Floquet driving, as well as how the results might change had other Floquet driving protocols been used.

In addition, is "ren" or "renormalized" the correct word to use, since there is no renormalization in the sense of integrating out degrees of freedom.

2. The authors mentioned band inversion momenta and the K_inv\cdot b rule near the beginning, but that isn't quite relevant to the first example on the Chern insulator. Indeed, parts of the first 2-3 pages of the paper seemed to be not too related to the main results. The authors may optionally consider restructuring the paper to make its main results and main novelty clearer.

3. Given recent advancements in various metamaterial platforms etc. photonics, acoustics, electrical circuits, etc, the authors should provide a much more in-depth discussion of how to realize the numerical observations experimentally. There should be examples of specific setups and specific measurements to be made, although the details of the experimental feasibility can referenced to existing experiments. This is important because it is still nontrivial to realize a Floquet driving protocol V(t) like Eq 3, and a solid proposal will boost the novelty of this work.

4. Is it possible to realize the effective Floquet Hamiltonian ie Eq S3-S4, which is highly nonlocal in real-space, with classical or quantum circuits?

5. Last but not least, please check for typos (i.e. the end of Eq S3 is missing an iT)

Reviewer #2 (Remarks to the Author):

See attached file

Reviewer #3 (Remarks to the Author):

This paper presents a theoretical analysis of the effects of line dislocations in Floquet topological insulators. In static (i.e., undriven) topological insulators, it is known that dislocations can generate "dislocation states" that are localized to the dislocation center (which can be understood in terms of the phase shifts introduced by the Burgers vector). Here, the authors investigate the Floquet (temporally driven) case, and find various anomalous features such as phases with zero topological invariants but supporting topological dislocation states.

The technical analysis is generally well done, and the results may be of interest to those working on Floquet topological phases. However, before publication there should be signficant improvements to the presentation.

First, the authors do not motivate the choice of the particular static Hamiltonian (1) to analyze. The Hamiltonian is presented in terms of its momentum space representation, but the real-space (lattice) representation should also be clearly described, since the real-space mode distributions on the lattice are discussed (e.g. in Fig. 3 (e)-(f)). The real-space implementation of the dislocation is also not adequately described; for instance, are the "distorted" (extra-length) bonds assigned the same hoppings as "undistorted" bonds, or different?

Second, some details are missing from the figures and figure captions. In Fig. 1, the meaning of the colors (Chern number? Of which band?) should be stated in the caption. In Fig. 3(e)-(f), 4(e)-(f), and 5(e)-(f), I think it's mode (intensity) distribution that is being plotted, not the local density of states; and is the value really identically zero on the other lattice sites, as the plot seems to indicate? (Surely not.)

Third, the authors should describe some of the key novel features more pedagogically. The notion of "translationally active" phases will not be familiar to readers not steeped in the theory of space groups, so it would be good to explain this and its significance. The concept of "anomalous" dislocation modes is introduced but it is not explicitly stated what precisely is anomalous about them.

Fourth, it would be good for the authors to articulate a bit more clearly, in their conclusion, how they think dislocation states add to the topological classification of Floquet phases that is not captured by the usual approach (e.g., looking at boundary states or higher-order boundary states). After all, is it not already known that Floquet engineering can create a zoo of unusual topological phases? What exactly does looking at dislocation states contribute?

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Response to the Reviewer 1

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We thank the reviewer for writing a report. A specific comment from the reviewer "*Overall, the study is quite rigorous and deserving of publication*", is encouraging to the authors. In response to reviewer's comment on the novelty of this work, we seek to point out that the proposal of probing and classifying Floquet topological phases from their responses to dislocation lattice defects is completely new and bears no resemblance with existing literature (to the best of our knowledge). It is a common practice in scientific community that a new idea is anchored by studying a specific model. However, our proposal is not limited to the model we study nor to two dimensions. It solely rests on the robust $\mathbf{K}\cdot\mathbf{b}$ rule. In the future, we will demonstrate its applicability for other models in 2D as well as in three dimensions, as mentioned in "Discussion". Below we respond to all the enumerated comments and make appropriate changes in the revised manuscript and SI.

1. Note that Eq. (4) is not specific to any drive. It follows the definition of the time evolution operator $U(\boldsymbol{k},T)$, obtained by solving time-dependent Schrödinger equation. In Eq. (3), we picked a simple drive protocol for which analytical results are compact (See Sec. S1 of SI) and compared with numerical analysis. Considering reviewer's question as a constructive criticism, we now perform completely new and extensive numerical analyses with two different driving protocols, and show that dislocation modes are qualitatively insensitive to it and depends only on the $\mathbf{K}\cdot\mathbf{b}$ rule. The results are discussed in Sec. S5 and shown in Figs. S5 and S6 of the SI.

   We remove "ren" or "renormalized" from $\boldsymbol{d}(\boldsymbol{k})$ vector, and replace them with "Flq", representing "Flqouet".

2. Note that the $\mathbf{K}\cdot\mathbf{b}$ rule is fully operative for a static Chern insulator, which supports dislocation mode only in the M phase [where $\mathbf{K}_{\rm inv}=(\pi,\pi)/a$], not in the Γ phase [where $\mathbf{K}_{\rm inv}=(0,0)$]. See Fig. S1 of SI. We relegated this figure to the SI, because focus of this manuscript is on the Flqouet insulators, not on the static insulators.

   The first 2-3 pages of the manuscript cover the following materials. Page 1: Abstract, Introduction and Fig. 1. Page 2: Summary of key results, model and its symmetries, and Fig. 2. First left column of Page 3: Discussion on band inversion, topological invariant (Chern number) and the $\mathbf{K}\cdot\mathbf{b}$ rule. All these components are important to pedagogically expose the readers to the rich, complex and previously unexplored landscape of dislocation in FTIs, especially bearing in mind the non-expert readers. Therefore, it is not clear to us which parts of the discussion in first 2-3 pages can be considered as redundant. Rest of the paper is all about dislocations in Floquet insulators. Nevertheless, considering reviewer's suggestion to highlight the novel part of the results, we state at the end of the second paragraph of the Introduction that *role of dislocation lattice defects in dynamic topological systems remained completely unexplored*, which should promote the novelty of our results. In addition, we also expand the third paragraph of the Introduction, summarizing our key and new results.

3. The best experimental probe for dislocation modes in 2D is LDoS, which in electronic systems can be measured by scanning tunneling microscope (STM) (Ref. 41). In photonic crystals LDoS can be measured by two-point pump probe (Ref. 43) or reflection spectroscopy (Ref. 44). In acoustic lattice LDoS is measured by local mechanical susceptibility measurement at each magnetomechanical resonator (Ref. 45). Dislocations in photonic and acoustic crystals can be engineered by removing a line of optical waveguides (Refs. 42-44) and magnetomechanical resonator (Ref. 45), respectively, ending at the dislocation core. Finally, the on site periodic drive (Eq. (3) and Sec. S5) can be engineered in optical lattices [PRX **4**, 031027 (2014), Ref. 46], for example, where onsite Hubbard repulsion has already been tuned across a quench [Nature **481**, 484 (2012), Ref. 47].

   In the revised manuscript we add new discussion at the end of 'Discussion' summarizing the above paragraph and add two new references (Refs. 46 and 47). Note that our proposal should be experimentally feasible as driven topology has been realized on various material platforms, where lattice defects have also been used to probe static topological phases. Now experimentalists can bring these two frontiers together to probe dynamic topological phases by dislocation defects.

4. We thank the reviewer for suggesting a possible realization of Floquet Hamiltonian from Eq. (S3)-(S4) in quantum circuits. But, we are not experts on quantum circuits, and it is an impossible task to muster the working skills on a completely new field to make educated connections with our proposed scenario. We hope that the reviewer will kindly excuse us from this burden. Note that the Floquet Hamiltonian was constructed solely for a theoretical purpose to compute the Chern number, while all the results on dislocation modes are derived by diagonalizing the time evolution or Floquet operator $U(\boldsymbol{k},T)$, which contains only short-range terms. Therefore, realizing the Floquet operator is more feasible in experiments than implementing the effective Floquet Hamiltonian, containing long-range terms. For this reason, quasimodes of the time evolution or Floquet operator are analyzed in experiments. Nevertheless, specifically in the high-frequency regime the Floquet Hamiltonian simplifies considerably, which then contains only short-range term and therefore can be implemented in metamaterials, at least in principle. At the end of Sec. S2 of SI we add a discussion on it.

5. We thank the reviewer for spotting the typo in Eq. (S3) of the SI. We fix it now. Also we thoroughly searched for typos in the entire manuscript and SI, and fixed all of them (to the best of our knowledge).

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Response to the Reviewer 2

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We thank the reviewer for writing the report. A specific comment from the reviewer "*It is quite an interesting result and as far as I am aware, no similar work has been carried out before. For this reason, I think this work deserves to be published in a good journal*" is encouraging to the authors. Below we respond to all the comments and questions raised by the reviewer, and make appropriate changes in the revised manuscript and SI.

1. Applicability of the $\mathbf{K}\cdot\mathbf{b}$ rule and the Jackiw-Rebbi mechanism for dislocation modes in driven system can be directly inferred from their counterparts in static systems. For example, if the band inversion momentum ($\mathbf{K}_{\rm inv}$) is finite at the Floquet zone center then the system supports normal or zero quasienergy dislocation mode. By contrast, when finite $\mathbf{K}_{\rm inv}$ occurs at Floquet zone boundaries, the system supports anomalous dislocation mode at quasienergies $\pm\omega/2$. Respectively in these two cases the Jackiw-Rebbi mechanism is applicable to the chiral edge modes at quasienergies zero and $\pm\omega/2$ (which are shown explicitly for all cases), yielding normal and anomalous dislocation mode. We summarize this statement in the revised manuscript prominently.

2. In a Chern insulator $d_0(\boldsymbol{k})$ is typically nonzero. But, it does not play any role in topology of static or driven systems. However, in a $p_x + ip_y$ superconductor $d_0(\boldsymbol{k}) \equiv 0$ (identically) due to charge conjugation symmetry. So, throughout we set $d_0(\boldsymbol{k}) = 0$ from the outset. We show the form of $d_0(\boldsymbol{k})$, namely $d_0(\boldsymbol{k}) = [m + t\{\cos(k_x a) + \cos(k_y a)\}]$, in case any reader wishes to incorporate the particle-hole asymmetry in the analysis for a Chern insulator. Since the explicit form of $d_0(\boldsymbol{k})$ led to a confusion, we remove it from the manuscript and show in Sec. S1 of the SI.

3. The reviewer correctly noted that the Chern number does not uniquely quantify responses of FTIs to dislocations. Dislocation modes are guaranteed by the *finite* band inversion momentum $\mathbf{K}_{\rm inv}$, obtained by computing the chiral edge modes in semi-infinite systems, as shown throughout the paper. The purpose of this statement was exactly to point out that two insulators, possessing identical Chern number, can be completely different in nature, which can be probed by dislocation. In the revised manuscript, we sharpen this statement, and add a new table (Table S1 of SI), where we show that FTIs with identical Chern number can response distinctly to dislocations. In this context, we request the reviewer to also consult our response No. 4 to Reviewer 3.

4. We thank the referee for seeking a bulk topological invariant, capturing the existence of chiral edge modes at $k_x = \pi$, stemming from band inversion at $\mathbf{K}_{\rm inv} = (\pi,\pi)$. However, the winding number (W) from Ref. 5 [PRX **3**, 031005 (2013)] only captures the total number of edge modes at stroboscopic time [see Eqs. (4) and (5)], but does not provide any information regarding $\mathbf{K}_{\rm inv}$ or the momentum of the chiral edge modes. Also, the winding number W is related to the total Chern number [see Eq. (13)], which we explicitly compute at the stroboscopic time for all the cases. To the best of our knowledge, the proposed prescription, based on the computation of chiral edge modes in semi-infinite geometry, could possibly be the only way to pin down its momentum and in turn the band inversion momentum ($\mathbf{K}_{\rm inv}$). For this reason, we do not compute W explicitly.

5. Let's consider a 3D topological insulator with $\mathbf{K}_{\rm inv} \neq 0$. Then, in the presence of a screw dislocation electron acquire a nontrivial phase as it goes through the "slipping half-plane" from one layer to its neighbor. As a matter of fact, the first work on dislocation in static topological insulator was done in 3D in the presence of a screw dislocation [Ref. 26]. In this regard, the edge dislocation in 3D is somewhat mundane, which is constructed by stacking 2D edge dislocation. Still 3D edge dislocation hosts topological modes, see PRB **90**, 241403(R) (2014). In the revised manuscript, we sharpen this discussion and cite above two papers.

6. The reviewer's criticism is justified. Dislocation probes "translationally active" Floquet insulators by supporting normal and/or anomalous dislocation modes at quasienergy zero and/or $\pm\omega/2$, respectively. The Floquet insulator can still be topological or trivial, which can be settled by computing the Chern number or the total number of chiral edge modes in semi-infinite systems (set by the winding number from Ref. 5), that in conjunction with the dislocation modes fully classify a Floquet insulator. Nevertheless, it should be noted that whether a Floquet insulator is translationally active or not can only be pinned by dislocation modes, not by the bulk invariants. The newly added Table S1 of SI shows examples of FTIs with identical bulk invariants that respond completely differently to dislocation. In this context, we request the reviewer to also consult our response No. 4 to Reviewer 3. In the revised manuscript we sharpen this statement in 'Discussions'.

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Response to the Reviewer 3

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We thank the reviewer for writing the report. A specific comment from the reviewer "*The technical analysis is generally well done, and the results may be of interest to those working on Floquet topological phases*" is encouraging to the authors. Below we respond to all the comments and suggestions, and make appropriate changes in the revised manuscript and SI.

1. We chose the Hamiltonian in Eq. (1) because it (1) describes paradigmatic topological phases in 2D: Chern insulator and $p_x + ip_y$ superconductor, and (2) supports a definite topological invariant (Chern number) and features insulating phases with the band inversion at the $\Gamma = (0, 0)$ and $M = (\pi, \pi)/a$ points, while only the M phase is translationally active. Even though these facts were stated systematically after Eq. (1), in the revised manuscript, we add new comments around Eq. (1), highlighting the quintessential features of this model.

   We have already shown the real space version of the tight-binding model in the presence of a dislocation in Eq. (S2) of the SI. The magnitude of the hopping elements across the line of missing atoms ending at the dislocation center, which the reviewer called "distorted" bonds, were kept the same as along the "undistorted" bonds, only for simplicity. Notice that dislocation modes appear *solely* due to the nontrivial phase $\mathbf{K} \cdot \mathbf{b} = \pi$ (modulo 2π) around the dislocation core in translationally active phases. Their stability does not depend on the hopping amplitudes along the "distorted" bonds. Now considering different hopping amplitudes along the "distorted" bonds connecting the sites across the line of missing atoms, we perform new numerical analysis and show that existence of dislocation modes is indeed insensitive to it. See Fig. S2 and Sec. S1 of the SI.

2. We believe that the reviewer meant Fig. 2, not Fig. 1, while referring to the Chern number. In Fig. 2, we quote the Chern number of the conduction band. The Chern number of the valence band is exactly the opposite. We specify the details in the caption of Fig. 2.

   In Figs. 3(e)-(f), 4(e)-(f) and 5(e)-(f), we plotted the LDoS, however, with a cutoff 10^{-3}, such that all the sites with LDoS below this cutoff are shown to be empty, which previously we did not specify in the captions. In the revised manuscript, we show all the LDoS plots using a black and white color scheme. Black represents the largest value of LDoS and white to its minimum value, now being exactly equal to 0.00.

3. The definition of "translationally active" should be relatively straightforward for a non-expert reader to grasp. Specifically, any topological phase (either static or dynamic) with the band inversion at a finite momentum is called "translationally active", since such a phase supports robust modes near a dislocation core, which is constructed by breaking the translational symmetry in the bulk of the system, according to the $\mathbf{K} \cdot \mathbf{b}$ rule.

   Regarding the novelty of the results, note that the all the results on dislocation modes in Floquet insulators are completely new and they bear no resemblance with existing literature (to the best of our knowledge). Considering reviewer's comment as a constructive criticism, we highlight novelty in the revised Introduction.

   The term "anomalous" is well known in the community of topological condensed matter physics. It was introduced about 7-8 years back in Ref. 5. It applies to either the edge or dislocation modes, residing at the Flqouet zone boundary at energies $\pm\omega/2$. Nevertheless, keeping non-expert readers in mind, we now properly define "anomalous" in the third paragraph of the revised manuscript.

4. Contribution of dislocation in the classification of FTIs is two fold.

   (I) Dislocation is a unique and only probe for translationally active Floquet insulators that support normal and anomalous dislocation modes when finite momentum Floquet-Bloch band inversion ($\mathbf{K}_{\text{inv}}$) takes place at the Floquet zone center and boundaries, respectively. It should be noted that the bulk topological invariants, such as the Chern number (C) or the total number of chiral edge modes, determined by the "winding number" (W) introduced in Ref. 5, do not provide any information about $\mathbf{K}_{\text{inv}}$.

   (II) There are examples of FTIs that possess identical bulk topological invariants (such as C and W, defined above), which respond *distinctly* to dislocations. Therefore, such FTIs are distinct is nature, which can only be probed by dislocation, but cannot be distinguished by bulk invariants. We show examples of such scenarios, such as the FTIs with $C = \pm 1$, in the newly built Table S1 of the SI.

   In this context, we further sharpen the discussion on $C = \pm 1$ FTIs in low frequency regime, right before 'Discussion'. We also highlight the unique role of dislocations in identifying translationally active FTIs at the end of the third paragraph of the Introduction, and first paragraph of the 'Discussion'.

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Changes in the revised manuscript and SI

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All the changes in the revised manuscript and SI are shown in blue.

- **Changes in response to to Reviewer 1:**

1. After Eq. (3), we mention that dislocation modes are qualitative insensitive to exact drive protocol. In Sec. S5 of the SI, we show new numerical analyses with two completely different drive protocols to anchor this claim. The results are summarized in Figs. S5 and S6 of the SI, which we refer to in the main manuscript.

   Throughout the manuscript and SI, we replace $\boldsymbol{d}_{\rm ren}(\boldsymbol{k})$ by $\boldsymbol{d}_{\rm Flq}(\boldsymbol{k})$.

2. At the end of the second paragraph of Introduction, we state that role of dislocations in dynamic topological systems remained completely unexplored so far. This statement should promote the novelty of the whole work. In addition, we also expand the third paragraph of the Introduction, summarizing all the key and new results.

3. At the end of 'Discussion' we add new discussion on engineering of dislocation and detection of dislocation modes in photonic and acoustic lattices, and electronic materials. We propose a possible experimental route to engineer on site staggered potential drive in optical lattices, and add two new references: Refs. 46 and 47.

4. At the end of Sec. S2 of the SI, we compare the feasibility of implementing the Floquet Hamitlonian and Floquet operator in experiments or materials. We also highlight the possibility of directly implementing the Floquet Hamiltonian in the high frequency regime.

5. We fix the typo in Eq. (S3). We also thoroughly searched for typos in the main manuscript and SI, and fix them, whenever found.

- **Changes in response to to Reviewer 2:**

1. In the third paragraph of the manuscript, summarizing the key results (page 2, left column), we discuss the applicability of the $\mathbf{K} \cdot \mathbf{b}$ rule and Jackiw-Rebbi mechanism for dislocation modes in driven systems.

2. A detailed discussion on the particle-hole asymmetry term $d_0(\boldsymbol{k})$ has now been removed from the main manuscript and relegated to the Sec. S1 of the SI.

3. At the end of page 4 and on page 5 (left column) we sharpen the statement on the dislocation modes for $C = -1$ FTI and its comparison to a static $C = -1$ insulator. There we also mention inadequacy of the Chern number in determining response of a FTI to dislocation and refer to the new Table S1 of the SI.

4. In the same context, we also point out that the "winding number" (W) introduced in Ref. 5 [PRX **3**, 031005 (2013)] only provides the total number of edge modes in the system, not the Floquet-Bloch band inversion momentum $\mathbf{K}_{\rm inv}$, which ultimately governs the response of a Floquet insulator to dislocation. In this context, we also expand the discussion on $C = \pm 1$ FTIs in the low frequency regime (appearing before 'Discussions') to argue that FTIs with identical Chern number and even with equal number of edge modes (set by the "winding number" W) can response *distinctly* to dislocation, while referring to the new table (Table S1) in the SI.

5. In the first paragraph of 'Discussions' we sharpen the discussion on dislocations in three dimensions. In this context, we cite an existing reference (Ref. 26) and add a new reference of PRB **90**, 241403(R) (2014) (Ref. 39).

6. Throughout the manuscript, we appropriately point out that dislocations are the suitable probe for "translationally active" FTIs that otherwise cannot be pinned by the Chern or winding number W (yielding the total number of edge modes) from Ref. 5, especially toward the end of the paper. We also state that translationally active FTIs with identical Chern number and total number of edge modes can be distinct, which can be probed by dislocations. In this context, we also appropriately refer to the new Table S1 from the SI.

- **Changes in response to to Reviewer 3:**

1. We highlight the quintessential features of the model Hamiltonian from Eq. (1) by adding a new brief discussion around Eq. (1).

   We refer to the real space hopping Hamiltonian in the presence of dislocation on page 3 (left column) of the main text. New numerical analysis, showing the stability of dislocation modes against the modified hopping aplitudes along the "distorted bonds" are presented in Fig. S2 and discussed at the end of Sec. S1 of the SI.

2. In the caption of Fig. 2, we state that this figure quotes the Chern number of the conduction band and the Chern number of the valence band is exactly the opposite.

   All the local density of states (LDoS) plots in the main manuscript and SI, are shown in black and white.

3. We define “translationally active” toward the end of the second paragraph of the manuscript (page 2, left column) immediately after the discussion on dislocation modes in static system. In the third paragraph, we again exemplify it, while discussing translationally active phases in driven or Floquet system.

   At the end of the second paragraph we state that *the role of dislocation in dynamic topological systems remained completely unexplored*, which should convince the readers about the novelty of our results, summarized in the third paragraph of the Introduction.

   In the third paragraph of the Introduction, we define “anomalous” immediately after we discuss the dislocation modes, appearing at the Floquet zone boundaries with quasienergies $\mu = \pm\omega/2$.

4. In the Introduction, technical part and Discussion of the paper we highlight that dislocation is a unique probe to identify translationally active FTIs, and together with the bulk invariants they provide more complete classification of FTIs. We also discuss examples of FTIs with identical bulk invariant that can be completely different in nature, which can only be probed by dislocations. In this context, we refer to Table S1 of the SI.

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Report of Reviewer 1

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In "Anomalous and normal dislocation modes in Floquet topological insulators", the authors studied various topological modes (ie 0 and pi modes) around dislocation points in a few static and Floquet models. Overall, the study is quite rigorous and deserving of publication. However, in terms of novelty, it is currently not quite clear which results contain novel physics, other than the explicit solution to specific models that have not been studied this way previously.

Some specific comments:

1. In Eqs 3 & 4, a specific kicked Floquet driving term was used. Although its simple and convenient form is evident in Eq S3, the authors should elaborate more about the significance of this choice of the Floquet driving, as well as how the results might change had other Floquet driving protocols been used.

In addition, is "ren" or "renormalized" the correct word to use, since there is no renormalization in the sense of integrating out degrees of freedom.

2. The authors mentioned band inversion momenta and the $\mathbf{K}_{\rm inv} \cdot \mathbf{b}$ rule near the beginning, but that isn't quite relevant to the first example on the Chern insulator. Indeed, parts of the first 2-3 pages of the paper seemed to be not too related to the main results. The authors may optionally consider restructuring the paper to make its main results and main novelty clearer.

3. Given recent advancements in various metamaterial platforms etc. photonics, acoustics, electrical circuits, etc, the authors should provide a much more in-depth discussion of how to realize the numerical observations experimentally. There should be examples of specific setups and specific measurements to be made, although the details of the experimental feasibility can referenced to existing experiments. This is important because it is still nontrivial to realize a Floquet driving protocol V(t) like Eq 3, and a solid proposal will boost the novelty of this work.

4. Is it possible to realize the effective Floquet Hamiltonian ie Eq S3-S4, which is highly nonlocal in real-space, with classical or quantum circuits?

5. Last but not least, please check for typos (i.e. the end of Eq S3 is missing an iT)

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Report of Reviewer 2

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The authors investigated the emergence of zero or $\omega/2$ quasienergy modes when Floquet topological insulators are subjected to dislocations. The manuscript extends the work of Ref. [26] to periodically driven setting, where the presence of such dislocation modes is predicted via the $\mathbf{K} \cdot \mathbf{b}$ theory and is numerically confirmed by considering an explicit periodically driven (Floquet) 2D Chern insulator. The main finding of this work is the fact that when the system exhibits a pair of chiral edge states at $k_x = \pi$ around zero ($\omega/2$) quasienergy, introducing a dislocation line along a specific direction gives rise to zero ($\omega/2$) quasienergy states localized near the two dislocation ends. It is quite an interesting result and as far as I am aware, no similar work has been carried out before. For this reason, I think this work deserves to be published in a good journal, provided the authors are able to satisfactorily address my comments below:

1. In the introduction section, the authors briefly elucidated how dislocation modes are expected to arise at $\mathbf{K} \cdot \mathbf{b} = \pi$ due to the similarity to the Jackiw-Rebbi mechanism in producing zero modes. Since this seems to constitute the main result of this work and is not very obvious in the periodically driven setting (especially when anomalous dislocation modes are involved), it would be extremely useful to readers if the authors could provide more technical detail about the mechanism at play.

2. On the first column of page 2, the authors wrote "The particle-hole asymmetry is captured by $d_0(k) = [m + t\{\cos(k_x a) + \cos(k_y a)\}]$." Shortly on the same page, they then stated "Then $\{\sigma_\mu\}$ operate on the Nambu or particle-hole indices, and $d_0(k) \equiv 0$ due to the charge conjugation symmetry. As $d_0(k)$ does not play any role in the topology of the above model, throughout we set $d_0(k) = 0$.". This seems to contradict the earlier statement about the chosen value of $d_0(k)$?

3. On page 4, the authors wrote "such $C = -1$ FTIs support anomalous dislocation modes with quasienergies $\mu = \pm\omega/2$, according to the $\mathbf{K} \cdot \mathbf{b}$ rule, but without any analogue in the static system, see Fig. 3(b), (d), (f)." This sentence seems to suggest that the Chern number determines the emergence of anomalous dislocation modes, which is not actually the case as the same Chern number may instead give rise to chiral edge states at $k_x = 0$ that do not support such dislocation modes.

4. Related to the above point, is it possible to derive a new topological invariant determining dislocation modes at zero or $\omega/2$ quasienergy separately? I believe this should be doable by considering a modification of the quasienergy winding number introduced in Ref. [5] to capture the existence of chiral edge states at $k_x = \pi$ only.

5. On the second column of page 5, the authors wrote "Furthermore, the $\mathbf{K} \cdot \mathbf{b}$ rule should be applicable to three-dimensional Floquet phases, capturing their responses to edge and screw dislocations." I do not understand how this generalization works in higher dimensions. In two-dimensions, the $\mathbf{K} \cdot \mathbf{b}$ rule can be related to the phase picked up by an electron surrounding a particular dislocation end. However, in three- and higher-dimensions, such a phase is always trivial since the electron's path can always be deformed to a point.

6. Towards the end of the paper, the authors argued that the observed dislocation modes can be used to probe Floquet topological phases. However, according to the results presented, that is not always the case. In particular, there are some nontrivial phases that do not support dislocation modes at all, such as those involving chiral edge states at $k_x = 0$. Moreover, even those phases that do support dislocation modes are not unique, e.g., a π dislocation mode may correspond to a phase hosting a pair of single chiral edge states at $k_x = \pi$ around quasienergy $\omega/2$ or a phase that also hosts additional pair of chiral edge states at $k_x = 0$. Since this aspect seems to be the main application of the proposed study, the authors should clarify this more.

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Report of Reviewer 3

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This paper presents a theoretical analysis of the effects of line dislocations in Floquet topological insulators. In static (i.e., undriven) topological insulators, it is known that dislocations can generate "dislocation states" that are localized to the dislocation center (which can be understood in terms of the phase shifts introduced by the Burgers vector). Here, the authors investigate the Floquet (temporally driven) case, and find various anomalous features such as phases with zero topological invariants but supporting topological dislocation states.

The technical analysis is generally well done, and the results may be of interest to those working on Floquet topological phases. However, before publication there should be signficant improvements to the presentation.

First, the authors do not motivate the choice of the particular static Hamiltonian (1) to analyze. The Hamiltonian is presented in terms of its momentum space representation, but the real-space (lattice) representation should also be clearly described, since the real-space mode distributions on the lattice are discussed (e.g. in Fig. 3 (e)-(f)). The real-space implementation of the dislocation is also not adequately described; for instance, are the "distorted" (extra-length) bonds assigned the same hoppings as "undistorted" bonds, or different?

Second, some details are missing from the figures and figure captions. In Fig. 1, the meaning of the colors (Chern number? Of which band?) should be stated in the caption. In Fig. 3(e)-(f), 4(e)-(f), and 5(e)-(f), I think it's mode (intensity) distribution that is being plotted, not the local density of states; and is the value really identically zero on the other lattice sites, as the plot seems to indicate? (Surely not.)

Third, the authors should describe some of the key novel features more pedagogically. The notion of "translationally active" phases will not be familiar to readers not steeped in the theory of space groups, so it would be good to explain this and its significance. The concept of "anomalous" dislocation modes is introduced but it is not explicitly stated what precisely is anomalous about them.

Fourth, it would be good for the authors to articulate a bit more clearly, in their conclusion, how they think dislocation states add to the topological classification of Floquet phases that is not captured by the usual approach (e.g., looking at boundary states or higher-order boundary states). After all, is it not already known that Floquet engineering can create a zoo of unusual topological phases? What exactly does looking at dislocation states contribute?

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

In the revised manuscript, the authors have satisfactorily addressed most of my comments, and I believe those of other referees' as well.

There are some minor points, i.e. typos like "phonotic". And the experimental proposal section could have been more comprehensive. But overall, I think the paper has already satisfied the standards of novelty and clarity required for a good journal like Communications Physics.

Reviewer #2 (Remarks to the Author):

See the attached file

Reviewer #3 (Remarks to the Author):

The authors have addressed some of the concerns raised in the previous review, but several concerns remain.

There is still no proper definition of a "translationally active" phase given in the manuscript. It should be noted that this terminology has previously been used by only a small community interested in topological order in systems with dislocations. The sentence inserted by the authors, "The M phase thereby stands as an example of translationally active topological insulator, as it features finite momentum band inversion", does not constitute a proper definition or explanation.

The definition of anomalous topological mode is still likewise undefined. In their rebuttal letter, the authors state that this term is "well known in the community of topological condensed matter physics", referring to Ref. [5]. But in that work, Rudner and co-workers referred to anomalous Floquet PHASES, not modes. An anomalous Floquet topological insulator is one that exhibits protected edge states despite the bands having zero Chern number, in violation of the typical bulk-boundary correspondence. In the present manuscript, the authors refer to "normal" dislocation modes and "anomalous" dislocation modes, without properly explaining how this relates to the concept of anomalous topological phases introduced by Rudner et al.

Given that Comm Phys is supposed to be a broad-scope physics journal, proper care should be taken to define any highly-specialized jargon that is employed.

If the above issues can be rectified, the paper should be suitable for publication.

Here are some other more minor issues the authors should consider dealing with as well:

1. The authors mention Majorana fermions prominently in the abstract (1st paragraph), in a manner that makes it seem important. But it then appears just once in the last paragraph of the

introduction, before never being mentioned again in the rest of the paper; apparently, it is not relevant to the theoretical analysis.

2. The authors make the interesting point that dislocations provide a "unique" probe for certain classes of Floquet topological insulators. In the non-Floquet case, there is now an extensive literature discussing using dislocation modes as a similar experimental signature for bulk topology, e.g. Ref. [27]. The authors should comment on the extent to which the Floquet aspect is unique (or if it is). In other words, are Floquet phases especially difficult to characterize compared to static ones, which makes dislocations more important for the Floquet case?

The authors have satisfactorily addressed my most of my previous comments. In particular, now the motivation for studying dislocation modes is very clear, i.e., to fully classify FTIs when combined with existing topological invariants such as the Chern and dynamical winding number. Before recommending the paper for publication, I however still have some minor comments below:

1. The explanation about the applicability of the K.b rule in Floquet systems is still not very clear to me. So far, the authors stated that such dislocation modes arise due to the hybridization between finite momentum chiral edge modes at quasienergy zero and \omega/2. However, it is still not obvious how the Jackiw-Rabbi mechanism can simply carry over to the time-periodic setting, where one now has two types of chiral edge states, the normal and anomalous one. In particular, the explanation can be improved by convincing the reader: 1) why Jackiw-Rabbi mechanism still holds for the anomalous chiral edge states which are not at zero quasienergy and therefore have no static counterpart, 2) why Jackiw-Rabbi mechanism seems to address normal and anomalous edge states separately, i.e., their coexistence does not cause nontrivial effect on the presence of dislocation modes at zero and \omega/2 quasienergies.
2. In regard to my point 3 in the previous report, I now understand the authors intention to highlight how the presence of dislocation modes distinguish between two phases with the same Chern number. The texts in the revised manuscript (page 4 and left column of page 5), however, seem to give the impression that such dislocation modes only distinguish between an FTI and a static TI with the same C=-1.
3. Given that the motivation for studying these dislocation modes is to classify FTIs, it is natural to ask about the generality of the current scheme. In particular:
   a. In the absence of particle-hole symmetry, chiral edge states can in principle be located at any momentum, rather than at BZ edge or center. How will the presence of such chiral edge states at any nonzero momentum affect the presence of dislocation modes?
   b. In certain FTI systems such as those studied in PRB **97**, 245430 (2018) and PRB **101**, 235438 (2020), multiple chiral edge modes may emerge at the same quasienergy and momentum. How will the presence of various chiral edge states at the BZ edge affect the dislocation modes?

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Response to the Reviewer 1

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We thank the reviewer for writing a second report and supporting our paper for publication in Communications Physics, based on the novelty of the results and the clarity of the presentation. It is also pleasing to know that the reviewer found "*In the revised manuscript, the authors have satisfactorily addressed most of my comments, I believe those of other referees' as well*".

In the revised manuscript, "phonotic" reads "photonic". We also fixed any other typo we could find.

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Response to the Reviewer 2

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We thank the reviewer for writing a second report. Below we respond to the questions raised by the reviewer and make appropriate changes in the revised manuscript and Supplemental Information (SI).

1. The Jackiw-Rebbi mechanism is strictly about the existence of topologically protected midgap states at the domain wall, not so much about its energy. Specifically, when two counter-propagating 1D modes are hybridized by a domain wall mass, robust midgap states appear exactly at the energy where these modes cross each other. If counter propagating 1D modes cross at zero energy, the midgap states appear at zero energy, as in the original paper by Jackiw and Rebbi. If we include particle-hole asymmetry, then such localized midgap states move away from the zero energy, as the counter-propagating 1D modes now cross each other away from zero energy. But midgap states continue to exist as they are topologically protected. Therefore,

   (1) Jackiw-Rebbi mechanism is equally applicable to normal dislocation modes at zero quasienergy ($\mu = 0$), stemming from the hybridization of counter-propagating finite momentum 1D edge modes crossing at $\mu = 0$, as well as for anomalous dislocation modes at $\mu = \pm\omega/2$, arising from the hybridization of finite momentum counter-propagating 1D edges modes crossing at $\mu = \pm\omega/2$. Here ω is the drive frequency.

   (2) Normal and anomalous dislocation modes are separated by the quasienrgy gap $\omega/2$. Therefore, they do not mix. Hybridization of two states is only allowed when their energy or quasienergy are degenerate, in the spirit of the perturbation theory. As long as the normal and anomalous edge modes are stable and exist simultaneously, the resulting normal and anomalous dislocation modes are also stable, when they exist simultaneously.

   We presented ample evidences supporting these statements in the main manuscript and SI. Nevertheless, we sharpen the discussion on this issues in the third and fourth paragraph (page 2) of the Introduction.

2. The primary purpose of the paragraph, previously starting on page 4 and continuing to page 5 is the following. In the high-frequency regime, FTI with Chern number $C = -1$ supports anomalous dislocation modes. But, in static system $C = -1$ Chern insulator does not support any dislocation mode. Hence, even though high-frequency Floquet systems can be compared (at least qualitatively) with static systems, the $C = -1$ FTI and $C = -1$ static Chern insulator respond distinctly to dislocation, despite both of them possessing the same bulk topological invariant $C = -1$ and equal number of edge modes. Then we generalize this observation to a situation, where FTIs with identical Chern number and equal number of edge modes can respond distinctly to dislocations, as shown in Table SI of the SI. The text got bit messed up while editing the draft in the previous round, for which we take the responsibility. Now we fix the text in this part of the revised manuscript.

3. (a) Particle-hole asymmetry only causes an overall shift in energy, but does not change the band inversion momentum $\mathbf{K}_{\rm inv}$, determining the momentum at which counter propagating 1D edge modes cross each other and the phase acquired by electron while encircling a dislocation core $\Phi^{\rm top}_{\rm dis} = \mathbf{K}_{\rm inv} \cdot \mathbf{b}$. The existence of dislocation modes, guaranteed when $\Phi^{\rm top}_{\rm dis} = \pi$ (modulo 2π), is thus not affected by the particle-hole asymmetry. We add a brief discussion in Sec. S1 of the SI, since we already relegated all the discussion on particle-hole asymmetry to the SI. See also our response point (1). On the same token, when edge modes appear at momentum other than the BZ center or edge, dislocation modes can be found as long as the corresponding Burgers vector satisfies $\Phi^{\rm top}_{\rm dis} = \mathbf{K}_{\rm inv} \cdot \mathbf{b} = \pi$ (modulo 2π). We add a qualitative discussion on this issue in "*Discussion*" and cite PRB **97**, 245430 (2018) [Ref. 41], where such modes have been found.

   (b) When more than one edge modes are found at finite momentum, for each pair of counter-propagating 1D edge modes we find one state at the dislocation core. To quantitatively anchor this claim, we consider the drive protocol from the paper the reviewer pointed out PRB **101**, 235438 (2020), and specifically analyzed the situation displayed in Fig. 2(c) of this paper, showing one pair of counter-propagating 1D edge modes at the Floquet zone center and two pairs of counter-propagating 1D edge modes at the Floquet zone boundary at a finite momentum. Consequently, in the presence of dislocation such a Floquet insulator supports one normal and two anomalous dislocation modes, in agreement with the proposed $\mathbf{K} \cdot \mathbf{b}$ rule. In this context, we add a comment in "*Discussions*" of the revised manuscript. New numerical results supporting this claim are shown in Sec. S6 and Fig. S7 of the revised SI. We also cite PRB **101**, 235438 (2020) [Ref. 40].

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Response to the Reviewer 3

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We thank the reviewer for writing a second report. Below we respond to the concerns the reviewer raised and make appropriate changes in the revised manuscript.

1. Previously in the manuscript, we added the following two sentences to define "translationally active" phases (blue text) "*The M phase thereby stands as an example of translationally active topological insulator, as it features finite momentum band inversion. Consequently it supports robust zero-energy mode at the dislocation core, realized by breaking the translational symmetry in the bulk of the system*". However, in the second report the reviewer only quoted the first sentence. Note that only when these two sentences are put together, they constitute the complete definition of "translationally active" phases.

   The above definition is in agreement with Refs. 28 and 30, and all the examples from the manuscript and SI for both static and dynamic systems are consistent with this definition. Even though this phrase was used by a small community in the past, recently B.R. wrote a paper with V.J. (arXiv:2006.04817), one of the key investigators of these two early articles on this topic, and used the same definition for "translationally active" phases. In the revised manuscript we combine and expand these two sentences, such that the definition is presented in one sentence to avoid any possibility of misinterpretation, and also cite Refs. 28 and 30. If the reviewer still feels that this definition is incomplete, we request the reviewer to suggest an alternative one. We are open to happily accept a justified suggestion that is in agreement with all our findings.

2. Even though in the previous rebuttal we said "*The term "anomalous" is well known in the community of topological condensed matter physics*", in the revised manuscript we clearly mentioned that modes at Floquet zone boundary are called "anomalous", bearing in mind non-expert readers who may not be familiar with this term. We admit that it was not stated why the modes at Floquet zone boundary are called "anomalous". In the revised manuscript we add a new sentence in this context that reads "*The later ones are named anomalous dislocation modes as they appear at quasienergies $\mu = \pm\omega/2$ at the Floquet ZB, since they lack any counterpart in static systems, where dislocation modes are found only at zero energy.*" in the third paragraph of Introduction.

   The term "anomalous edge modes" appeared multiple times in Ref. 5, including in its title, and also in the review article RMP **89**, 011004 (2017) [Ref. 7]. The term "anomalous dislocation mode" never appeared in the literature, since, to the best of our knowledge, dislocation modes in Floquet system has not been studied previously. We believe that authors of a paper should have some degree of freedom of introducing new terms as long as they are properly defined and exemplified. We believe that "anomalous dislocation mode" is now properly defined, which is also consistent with all the examples presented in the manuscript and SI.

   We never used the term "anomalous phase" in the manuscript, as all the phases, except the one in Fig. 3(a), are anomalous since all of them support edge modes at the Floquet zone boundary at $k_x = 0$ and/or π. Therefore, introduction of this term does not serve any purpose to distinguish these phases. Still in the revised manuscript, we qualitatively relate "anomalous dislocation modes" with "anomalous phases".

Also note that "translationally active" and "anomalous modes" were not rigidly defined anywhere in the literature, unlike topological invariants. Researchers used them somewhat flexibly in the context of respective problems. Now we respond to the minor comments from the reviewer and make appropriate changes in the revised manuscript.

1. Our model is equally applicable to a Chern insulator and a p_x+ip_y superconductor, as stated above Eq. (1). For this reason, our results on dislocation modes are equally applicable to neutral Majorana fermions, as mentioned in the Abstract and at the end of the Introduction. Nevertheless, the reviewer correctly noted that in the later part of the paper the word "Majorana fermions" did not appear with due credit. In the revised manuscript, we mention the applicability of our formalism and the conclusions for localized Majorana dislocation modes of a driven $p_x + ip_y$ superconductor after Eq. (3) and in "Discussion".

2. Previously we pointed out that the bulk topological invariants, Chern number (C) and dynamic winding number (W), do not provide any information about the band inversion momentum $\mathbf{K}_{\rm inv}$. Finite momentum band inversion can only be probed by dislocations as they host localized topological modes in its core when $\mathbf{K}_{\rm inv}$ is finite, either at the Floquet zone center or zone boundary. We showed it through multiple examples in Figs. 3,4,5, and in the SI. Furthermore, in Table SI of the SI, we showed examples of FTIs with equal Chern and dynamic winding numbers that respond distinctly to dislocations (also summarized right before "Discussions"). For this reason, dislocations, besides being unique probe for translationally active Floquet topological phases, together with the bulk topological invariants provides a more complete classification, as mentioned in the "Introduction" and "Discussion", using almost the same language as above.

   Therefore, we could not figure out, how else we could make these statements more prominent to highlight the importance of dislocations in probing translationally active Floquet phases and classifying them. Still, we further expand the existing statements in the Introduction and Discussion of the revised manuscript. However, we strongly feel that over-specification of new results leads to repetition, which in turn reduces the lucidity of the presentation. We hope that changes we made in this context are adequate.

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Changes in the revised manuscript and SI

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All the changes in the revised manuscript and SI are shown in red.

- **Changes in response to to Reviewer 1:**

1. We fixed any typo we would spot in the manuscript and SI. Now "phonotic" reads "photonic".

- **Changes in response to to Reviewer 2:**

1(a). We sharpen the discussion on the Jackiw-Rebbi mechanism in the context of the realization of normal and anomalous dislocation modes in the third paragraph of the Introduction (Page 2, left Column).

1(b). We argue that any mixing between normal and anomalous dislocation modes are forbidden as they are separated by quasienergy $\sim \omega/2$ in the fourth paragraph of the Introduction (Page 2, right Column).

2. We sharpen the discussion [previously starting on page 4 and continued to page 5, now appearing on page 5 (left column, second paragraph)], contrasting the response of $C = -1$ insulator to dislocation in static and high-frequency Floquet systems.

3(a). We add a brief discussion on the stability of the dislocation modes in the presence of particle-hole asymmetry at the end of the first paragraph of Sec. S1 of the revised SI. We add a brief comment in "Discussion" (third paragraph) on the realization of dislocation modes, when chiral edge modes are found away from the BZ center or edge. In this context we cite PRB **97**, 245430 (2018) [**Ref. 41**].

3(b). In Sec. S6 and Fig. S7 of revised SI, we show that for each pair of chiral edge modes appearing at $k_x = \pi$, there exists one dislocation mode by explcitly considering the specific example from Fig. 2(c) of PRB **101**, 235438 (2020). We also add a comment on this issue in the third paragraph of "*Discussion*". In this context, we cite PRB **101**, 235438 (2020) [**Ref. 40**].

- **Changes in response to to Reviewer 3:**

1. Toward the end of the second paragraph of Introduction, we combine and expand the two sentence from the previous version to define "translationally active" phases, such that the whole definition appears in one sentence. In this context we also cite Refs. 28 and 30, with which our definition is in agreement.

2. In the third paragraph of the Introduction, we define which dislocation modes are called "anomalous" and also why. Toward the end of this paragraph we also establish connection with anomalous phases, introduced in Ref. 5 and reviewed in Ref. 7.

3. We further specify that our formalism for generating dislocation modes are equally applicable to driven $p_x + ip_y$ supercondcutor, after Eq. (3) and in the "Discussion".

4. We extend the existing statements highlighting dislocations as unique probes for translationaly active Floquet phases and their role in a more complete classification of Floquet phases at the end of "Introduction", on page 5 (left column, second paragraph) and in the "Discussion".

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Report of Reviewer 1

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In the revised manuscript, the authors have satisfactorily addressed most of my comments, and I believe those of other referees' as well.

There are some minor points, i.e. typos like "phonotic". And the experimental proposal section could have been more comprehensive. But overall, I think the paper has already satisfied the standards of novelty and clarity required for a good journal like Communications Physics.

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Report of Reviewer 2

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The authors have satisfactorily addressed my most of my previous comments. In particular, now the motivation for studying dislocation modes is very clear, i.e., to fully classify FTIs when combined with existing topological invariants such as the Chern and dynamical winding number. Before recommending the paper for publication, I however still have some minor comments below:

1. The explanation about the applicability of the K.b rule in Floquet systems is still not very clear to me. So far, the authors stated that such dislocation modes arise due to the hybridization between finite momentum chiral edge modes at quasienergy zero and $\omega/2$. However, it is still not obvious how the Jackiw-Rabbi mechanism can simply carry over to the time-periodic setting, where one now has two types of chiral edge states, the normal and anomalous one. In particular, the explanation can be improved by convincing the reader: 1) why Jackiw-Rabbi mechanism still holds for the anomalous chiral edge states which are not at zero quasienergy and therefore have no static counterpart, 2) why Jackiw-Rabbi mechanism seems to address normal and anomalous edge states separately, i.e., their coexistence does not cause nontrivial effect on the presence of dislocation modes at zero and $\omega/2$ quasienergies.

2. In regard to my point 3 in the previous report, I now understand the authors intention to highlight how the presence of dislocation modes distinguish between two phases with the same Chern number. The texts in the revised manuscript (page 4 and left column of page 5), however, seem to give the impression that such dislocation modes only distinguish between an FTI and a static TI with the same $C = -1$.

3. Given that the motivation for studying these dislocation modes is to classify FTIs, it is natural to ask about the generality of the current scheme. In particular:
   (a) In the absence of particle-hole symmetry, chiral edge states can in principle be located at any momentum, rather than at BZ edge or center. How will the presence of such chiral edge states at any nonzero momentum affect the presence of dislocation modes?
   (b) In certain FTI systems such as those studied in PRB **97**, 245430 (2018) and PRB **101**, 235438 (2020), multiple chiral edge modes may emerge at the same quasienergy and momentum. How will the presence of various chiral edge states at the BZ edge affect the dislocation modes?

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Report of Reviewer 3

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The authors have addressed some of the concerns raised in the previous review, but several concerns remain.

There is still no proper definition of a "translationally active" phase given in the manuscript. It should be noted that this terminology has previously been used by only a small community interested in topological order in systems with dislocations. The sentence inserted by the authors, "The M phase thereby stands as an example of translationally active topological insulator, as it features finite momentum band inversion", does not constitute a proper definition or explanation.

The definition of anomalous topological mode is still likewise undefined. In their rebuttal letter, the authors state that this term is "well known in the community of topological condensed matter physics", referring to Ref. [5]. But in that work, Rudner and co-workers referred to anomalous Floquet PHASES, not modes. An anomalous Floquet topological insulator is one that exhibits protected edge states despite the bands having zero Chern number, in violation of the typical bulk-boundary correspondence. In the present manuscript, the authors refer to "normal" dislocation modes and "anomalous" dislocation modes, without properly explaining how this relates to the concept of anomalous topological phases introduced by Rudner et al.

Given that Comm Phys is supposed to be a broad-scope physics journal, proper care should be taken to define any highly-specialized jargon that is employed.

If the above issues can be rectified, the paper should be suitable for publication.

Here are some other more minor issues the authors should consider dealing with as well:

1. The authors mention Majorana fermions prominently in the abstract (1st paragraph), in a manner that makes it seem important. But it then appears just once in the last paragraph of the introduction, before never being mentioned again in the rest of the paper; apparently, it is not relevant to the theoretical analysis.

2. The authors make the interesting point that dislocations provide a “unique” probe for certain classes of Floquet topological insulators. In the non-Floquet case, there is now an extensive literature discussing using dislocation modes as a similar experimental signature for bulk topology, e.g. Ref. [27]. The authors should comment on the extent to which the Floquet aspect is unique (or if it is). In other words, are Floquet phases especially difficult to characterize compared to static ones, which makes dislocations more important for the Floquet case?

REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

The authors have satisfactorily addressed my previous minor points. I just have a very small minor comment about the authors' reply that particle-hole asymmetry only leads to shift in energy. I believe the authors came to this conclusion from the observation that the term d0 breaks particle-hole symmetry. It should be noted however that such a term may not be the only particle-hole asymmetry perturbation. Indeed, the authors stated in the main text that particle-hole operator takes the form P = sigma_1 K. Given the particle-hole symmetry condition P h(k) P^{-1}=-h(-k), it is easy to come up with various symmetry breaking perturbation, e.g., sin(k) sigma_3. I believe the latter may no longer pin the edge states at k=0,pi. Nevertheless, the authors have satisfactorily explained the expected physics when edge states are not confined at k=0 or k=pi, so my above comment is only meant to make the discussion in the revised manuscript more accurate.

As one last thing, I might have missed this, but in the supplemental material, the authors added Table I, but it was not referenced anywhere in the text. It is therefore not very clear if that table refers to the main model considered in this work or the additional models discussed in the supplemental material.

Once the above aspects are addressed, I think the manuscript is truly ready for publication, and I do not need to see the revised manuscript again.

Reviewer #3 (Remarks to the Author):

The revised manuscript sufficiently clarifies the terminology, and there are no more outstanding issues. I recommend publication.

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Response to Reviewer 2

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We thank the reviewer for supporting our manuscript for publication in Communications Physics. Below we provide our responses to the minor comments the reviewer made.

1. Notice that no particle-hole symmetry breaking term is permitted in the effective single particle Hamiltonian for the $p_x + ip_y$ superconductor, as they are forbidden by the charge conjugation symmetry. In the model for the Chern insulator one can in principle add particle-hole symmetry breaking terms, such as $t_x \sin(k_x a)\sigma_3$ or $t_y \sin(k_y a)\sigma_3$, as proposed by the reviewer, besides $d_0(\boldsymbol{k})$, the effects of which we already discussed in details in the Supplemental Information (SI). We performed explicit numerical analysis by adding such terms in the ststic system and found that its phase diagram remains completely unchanged. Moreover, the edge modes are pinned precisely at $k_x = 0$ for $0 < m_0/t_0 < 2$ and at $k_x = \pi$ for $-2 < m_0/t_0 < 0$ when t_x or t_y is as large as 1. Thus addition of either of these two terms does not lead to any change regarding our conclusion about the dislocation modes. If edge modes appear at $k_x \neq 0, \pi$, then one needs to construct a dislocation such that $\mathbf{K} \cdot \mathbf{b} = \pm\pi$ (modulo 2π), which was already mentioned in the third paragraph of "Discussions".

2. In the revised SI, we add the reference of Table S1 at the end of the Supplementary Note 4.

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Response to Reviewer 3

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We thank the reviewer for supporting our manuscript for publication in Communications Physics.

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Changes in the revised Supplemental Information

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In response to Reviewer 2, we made the following changes in the revised Supplemental Information. No changes were required in the main manuscript.

1. At the end of the second paragraph of Supplementary Note 1, we add a brief discussion stating that addition of particle-hole symmetry breaking terms like $t_x \sin(k_x a)\sigma_3$ or $t_y \sin(k_y a)\sigma_3$ does not lead to any change on the phase diagram and dislocation modes in the static system, even when t_x or t_y is as large as 1.

2. At the end of the Supplementary Note 4, we add a reference of Table S1.

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Report of Reviewer 2

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The authors have satisfactorily addressed my previous minor points. I just have a very small minor comment about the authors' reply that particle-hole asymmetry only leads to shift in energy. I believe the authors came to this conclusion from the observation that the term d_0 breaks particle-hole symmetry. It should be noted however that such a term may not be the only particle-hole asymmetry perturbation. Indeed, the authors stated in the main text that particle-hole operator takes the form $P = \sigma_1 K$. Given the particle-hole symmetry condition $Ph(k)P^{-1} = -h(-k)$, it is easy to come up with various symmetry breaking perturbation, e.g., $\sin(k)\sigma_3$. I believe the latter may no longer pin the edge states at $k = 0, \pi$. Nevertheless, the authors have satisfactorily explained the expected physics when edge states are not confined at $k = 0$ or $k = \pi$, so my above comment is only meant to make the discussion in the revised manuscript more accurate.

As one last thing, I might have missed this, but in the supplemental material, the authors added Table I, but it was not referenced anywhere in the text. It is therefore not very clear if that table refers to the main model considered in this work or the additional models discussed in the supplemental material.

Once the above aspects are addressed, I think the manuscript is truly ready for publication, and I do not need to see the revised manuscript again.

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Report of Reviewer 3

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The revised manuscript sufficiently clarifies the terminology, and there are no more outstanding issues. I recommend publication.