Reply

Reply to referee A

- A point that has not been addressed in the paper is that of higher-order derivative corrections, a natural implementation of effective field theory techniques. The authors write a Lagrangian to leading order in a derivative expansion and derive (52)-(54) as well as (66). How would these results change in the presence of higher derivative terms in the Lagrangian?

[Reply] The results presented in the manuscript are exact at leading order in derivatives. Higher derivative corrections to the action would modify the dispersion relation of the phason only at larger momenta/frequencies. In other words, the low-energy result:

$$\omega^2 + i\,\gamma\,\omega = v^2\,k^2 \tag{1}$$

will be corrected by subleading terms $\mathcal{O}(k^4), \mathcal{O}(\omega^4), \ldots$. In summary, the results of the paper would be modified only at high energies. Modulo problems of convergence, one could systematically introduce higher derivative terms in our effective action and compute robustly those corrections. We added a paragraph at the end of section 2 to clarify this point.

- Moreover, are there constraints on the coefficients appearing in (38) coming from the requirement of convergence of the path integral? If so, how are they affecting relations like (54) and (66)?

[Reply] The constraints on the coefficients appearing in (38), which are shown in Eq.(41), do not come from the requirement of convergence of the path integral but rather from imposing the dynamical KMS symmetry of the effective action. The latter is related to the thermodynamics consistency of the theory. For example, such symmetry implies the existence of a well-defined entropy current with non-negative divergence. That being said, there is a constraint that M^{AB} be self-adjoint that comes from unitarity and serves to ensure that the path integral converges. We added a comment on this.

- In the first paragraph on page 3, reference [12] seem to have appeared earlier than [11] while the text is phrased otherwise.

[Reply] The referee is correct. We have changed the sentence referring to Refs.[11]-[12]. Thanks for noticing.

- In the last paragraph on page 3, what are the "diffusive Goldstone bosons"? Is this terminology equivalent to Type II Goldstone bosons mentioned in the previous paragraph?

[Reply] Not exactly. The terminology of Type II Goldstone bosons usually apply to closed systems with no dissipation, to indicate Goldstone modes with dispersion relation $\omega \sim k^{2n}$. Here, the Goldstone modes are diffusive, with purely imaginary dispersion relation at small momenta, and they are characteristic of systems with dissipation. The classification of the modes in those systems has been introduced in `https://arxiv.org/abs/1907.08241` cited in our manuscript.

- In equation 26 there appears to be a typo since what appears in the equation does not correspond to what is written in the text that follows

[Reply] Thanks for noticing. Indeed the last term should read $\partial_t \phi_r$.

- At the beginning of the paragraph containing (38), $\partial_\mu \psi_i$ appears, is this ψ_i^r or ψ_i^a?

[Reply] It is an r field. We fixed it.

- Above (38) it is said that $\partial_\mu \psi_r^4$ has a vanishing expectation value. Why is that the case?

[Reply] The fields ψ_r^A embed the physical 3D space into the quasicrystal 4D space. We chose ψ_r^i to be parallel to the embedded 3D space and ψ_r^4 to be orthogonal. Thus, in equilibrium, the embedding field profiles are $\psi_r^i = x^i$ and $\psi_r^4 = \text{const}$. Thus, $\partial_u \psi_r^4 = 0$

- Below (43), the sentence "The fact that $T_{\mu\nu}$ " seems to be incomplete.

[Reply] Thank you for pointing this out. We have fixed it.

- Eq. (55) is understood as the equation of motion associated with the field ψ^4. It is also understood that the shift symmetry (31) leads to a vanishing Noether current. However, the link between these two statements is not clear. In particular claims such as"Thus, the fact that at low momentum the phason is diffusive is a direct result of the absent Noether current associated... which confirms explicitly the previous arguments." are not evident.

[Reply] We are sorry for the confusion. The Noether current associated to the shift in the 4 direction does not vanish. It is absent in the sense that the corresponding current is not conserved as Eq.(55) shows. What we observe is a typical feature of dissipative/non-hermitian systems in which symmetries of the system can be associated to currents which are not conserved. This is obviously not the case otherwise, as Noether theorem teaches us.

Reply to referee B

- The phason dispersion relation is obtained by performing a split of the quasicrystal modes $\psi^A = (1, .., 4)$ into $\psi^i = (1, 2, 3)$ and ψ^4. In particular, the authors first solve for the dynamics of the quasicrystal fields $\psi_i(i = 1, 2, 3)$, which leads to a partial fix the worldvolume diffeomorphism symmetry, and subsequently solve for ψ^4 . Is a splitting of the ψ^A fields necessary in order to obtain the phason dispersion relation, or does this relation arise also when solving the equations in a manifestly SO(4) -invariant way? It may be good to include a comment on this.

[Reply] It is true that we need not split ψ_r^A into $A = 1, 2, 3$ and $A = 4$. It does, however, make life much easier if we do as it allows us to gauge-fix the fluid worldvolume diff symmetries and simultaneously decrease the number of degrees of freedom. Additionally, ψ_r^i and ψ_r^4 exhibit very different dynamics; the first describes phonon waves, while the second describes diffusive phasons. As a result, there is little practical benefit to treating them on equal footing. We will add a comment to this effect.

- I assume that Poincare' invariance is adopted for simplicity. It would be worth to mention that this assumption can be relaxed, since normally these systems do not enjoy Poincare' symmetry.

[Reply] Our work follows closely the EFT ideology of https://arxiv.org/abs/1501.03845 in which phases of matter are classified accordingly to how they spontaneously break Poincare' invariance. As such,our initial action it is indeed Poincare' invariant and the equilibrium configuration does break it spontaneously. In principle, one could indeed relax that assumption and start directly with systems with Galilean invariance, see for example the recent work https://arxiv.org/abs/2008.03994. We commented on this point.

- On a related note, below eq. (44) it is mentioned that stress-energy conservation is a consequence of gauging Poincare' symmetry. It would be more precise to say that stress-energy conservation is a consequence of gauging spacetime translation symmetry, which is a slightly different statement (for example, the associated background would in general not be a spacetime metric, unlike in the Poincare' case).

[**Reply**] We do agree with the referee. We have changed it and made it more precise.

- Finally, I found a few typos:

[**Reply**] Thanks for noticing them. We have fixed them.