# Referee Report: Ideal Fracton Superfluids

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The paper adopts a hydrodynamic approach in order to characterize the behavior of many-body quantum systems with emergent fracton symmetries. The topic is timely and interesting for the high-energy community (at least) but most probably beyond it. Indeed, the paper is well connected with the literature, both recent and less so.

The paper is extensive, detailed and both systematic and pedagogic in aim. However, the current version falls short in these latter aspects. A revision in the presentation of some aspects seems to be in order to the purpose of improving clearness and to ease the understanding. Besides, it appears there are some issues in relation to some of the reported results. The remarks here below go in the direction of improving on these aspects and I regard it necessary to address them before recommending the manuscript for publication on SciPost.

I close the report with some curiosity whose answering is not necessary for the purpose of publication. Let me strongly encourage the authors to pursue on, especially in the direction of a deeper understanding of the relations among the modes that they study with the Nambu-Goldstone structure of fracton theories, for instance their counting and dispersion properties.

My congratulations for the work done.

## Overall remarks

- The message conveyed by the manuscript on two of the main aspects it deals with is not completely clear, namely the hydrodynamic expansion for fractons and the no-flow theorem. As discussed in the manuscript, the hydrodynamic expansion could have a non-trivial relation with the UV/IR mixing of fracton models. What are the requirements or checks on which one can rely in order to be granted that a hydrodynamic gradient expansion is altogether sound for fracton models? The no-flow theorem claims the impossibility to have a normal fluid flow for fractons in line with expectation coming from mobility constraints for isolated charges when dipole symmetry is enforced. However, the reason why a similar conclusion is reached for the $p$-wave phase seems at odds with the normal understanding that charges can move when interchanging dipoles with a dipole condensate and/or background.

- One of the motivation of the paper is that of having a characterization of fracton effective descriptions in view of chasing them in experiments. It is not clear how the results of the paper improve on this aspect, some more detailed discussion of this could be desirable. In general, hydrodynamic modes can *per se* carry very "compressed" and incomplete information about the microscopic model. Similar patterns of low-energy modes can arise in very different ways. In particular, the reference to magnon-like modes appears to be confusingly suggestive, unless motivated better.

- Even more importantly, the hydrodynamic gradient expansion seems to lack a clear definition in some cases like the $s$-wave superfluid. In the words of the authors, the gradient expansion is conflated with phenomenological assumptions. This makes it non-predictive.

- The appendices contain some parallel developments to the main text. For example, hydrodynamics in more standard contexts is developed there, whereas, in the main text, hydrodynamics is applied to the fracton context. I have found it necessary to repeatedly jump from the main text to the appendices in order to follow the line of reasoning. This hampers the understanding. In line with the punctual observations below, I recommend some overall revision to streamline the presentation both in the main text and in the appendices.

# 1 Punctual remarks

1. Introduction. The end of the second paragraph recalls that fracton dynamics in curved geometry is consistent only on certain special backgrounds. The end of the third paragraph seems to aim at a general description of fracton hydro on generic curved background. The two things would clash. Furthermore, in the paper only linear fluctuations about flat backgrounds are studied. The end of the third paragraph seems thus to need revision.

2. In the introduction and Subsection 2.4 it is stated that, imposing vanishing $U(1)$ curvature corresponds physically to the absence of elementary dipoles. The statement is argued on the basis of a counting of degrees of freedom and in line with a similar observation made in [JJ22]. I think the statement is not correct. The identification of elementary dipoles is ambiguous and related to improvement transformations of the currents, similarly to the spin current in the energy momentum for standard field theory. The argument goes like this. The total dipole is

$$\mathrm{d}^i = \int dx^3 \left( x^i J^t + J^{ti} \right) \ . \tag{1}$$

The Ward identities are

$$\partial_\mu J^\mu = 0 \ , \tag{2}$$
$$\partial_\mu J^{\mu a} = J^a \ . \tag{3}$$

Consider the improvement

$$\tilde{J}^\mu = J^\mu + \partial_\nu \chi^{\nu\mu} \ , \tag{4}$$
$$\tilde{J}^{\mu a} = J^{\mu a} + \chi^{\mu a} \ , \tag{5}$$

with $\chi^{\mu\nu} = -\chi^{\nu\mu}$. The improvement respects the Ward identities and allows one to set

$$\tilde{J}^{ta} = 0 \ , \tag{6}$$

$$\tilde{J}^{[ia]} = 0 \ . \tag{7}$$

In particular, the density of elementary dipoles is vanishing.

This seems to solve another issue. In Subsection 2.4, in order to work with $A_{\mu\nu}$ instead of the full $\tilde{A}^a_\mu$, a constraint on the $U(1)$ curvature is imposed. However, according to (2.32), such a constraint would not be gauge invariant. Thereby, relaxing or imposing it should not change the physical content of the theory. Instead, as stated in the last paragraph of the subsection, depending on the enforcing of the constraint, the theory would/would not contain elementary dipoles.

3. Introduction, second paragraph of pag.4. It refers to the *consistent* definition of the chemical potential. This seems related to the statement below (4.12) where it is said that the *typical ordering* of background gauge field is $\mathcal{O}(1)$. Are these remarks just saying that, if we consider non-trivial chemical potentials, we normally want them to enter at ideal order? The word *consistent* in the intro seems to refer to something more that I am possibly not getting. Some rephrasing would help.

4. In Section 3.1 it would be helpful to say that one turns to Landau grand-potential, otherwise (3.2) seems to clash with comments before it.

5. The are some issues in the argument below (3.5). First, it would be helpful to explain the relation $m = 2\partial P/\partial\vec{u}^2$, maybe connecting to (B.9) and (B.10) (see next point). Then, the argument seems to need to be expressed in a different order, first stating that $u^i$ is invariant and, from that, arguing that $m$ is invariant too. It also seems to lack the extra information coming from the invariance of $J^\mu$ under dipole (2.39). Specifically, being $\rho$ and $J^i = \rho u^i$ both invariant, then $u^i$ is invariant.

6. There seems to be a source of confusion in some adopted notations. In Section 3.1, $u$ represents the frame velocity, namely the spatial equivalent of a chemical potential. In Appendix B, the 4-vector containing the chemical potential and the velocity components is instead indicated with $\xi$, while $u$ assumes a dual meaning according to (B.6) and (B.7).

7. Introduction. The description of quadratic dispersion relations is confusing. First, the coefficient $a$ entering a dispersion relation as $\omega = ak^2 + ...$ is systematically referred to as "velocity" but it is not a speed at the dimensional level. Secondly, the term magnon suggests physical aspects which are not present here. Magnons are spin-waves whose quadratic behavior is related to a lack of time-reversal symmetry. Here neither spin nor time reversal is concerned. As far as I am understanding "magnon-like" is used just as a synonym of "quadratic" which would be preferable.

8. Introduction, fig.1. Why does not the pinning translate into a gap for a mode? This is the customary sense of the word "pinning" in condensed matter. Pinned charge-density-waves, for example, have a gapped sliding mode.

9. The derivation of (2.38) is not explained clearly. It seems that (A.3) is used without saying and possibly also the Ward identities (2.41) derived only later.

10. The derivation of (B.5) is not easy to follow. First, the same symbol $\xi$ is used for the superfluid velocity and for the vector generating a generic diffeomorphism. (B.5) is a consequence of the last of (B.4) and its variation, $\delta\delta_K\phi = 0$ (this could be said to help the reader). However, the meaning of $\delta$ in (B.5) is different from that just used in (B.1), in fact one is using $\delta$ in the sense of $\delta\phi = \mathcal{L}_\xi\phi + \sigma$, comprehending both a diffeo and a gauge transformation.

    This said, I find it not clear how (3.9) could descend from (3.8) as suggested by the text between them. The last of (3.9) is analogous to (B.5) which needed the last of (B.4), absent in (3.8).

    The second of (3.9) is got from $\delta\delta_K B_\mu = 0$ using the second of (3.8) and the last of (3.9). It could be useful to explain more explicitly how to get (3.9).

11. Subsection B.1.1. In (B.6) the parameter $T_0$ is introduced, but then is considered only $T_0 = 1$ without saying. Eq. (B.7) is actually the definition giving rise to the third of (B.6), the phrasing between (B.6) and (B.7) is confusing.

12. Plugging (B.9) into (B.14) and differenciating, there is a problem with the sign of the term in $d\xi^2$. Does this propagate to (B.13)?

13. In (5.10) there seems to be a typo, $\theta$ has not been introduced before. It is probably a $\phi$ and, using an expression analogous to that given below (B.7), it justifies the statement $\mu_p = u^\mu \mathcal{B}_\mu$ given just afterwards. Some more in-line explanation would be useful.

14. The constraint on charge mobility due to dipole symmetry is valid only when a charge is isolated and, in particular, when it cannot exchange dipoles with a background or a dipole condensate. Why then should the $p$-wave fracton superfluid feature a sort of no-flow theorem like that emerging from (4.17)? Relatedly, since both the $p$- and $s$-wave fracton superfluids are on the same footing as far as the dipole symmetry and its breaking are concerned, why should there be such a qualitative phenomenological difference about the possibility to flow? I believe these points need to be discussed more in the paper.

15. Why does (5.1) feature a minus sign with respect to an analogous equation in (B.1)?

16. The comparison of the term proportional to the scalar Goldstone variation between (5.25) and (B.18) seems problematic. In (5.25) there appear a calligraphic kappa, while in (B.18) there is a normal $K$.

17. The steps at pag.29 seem to me correct, however I have found it hard to follow them. Maybe some effort to streamline would be useful.

18. The identification $p_j = -n\Psi_j$ in (4.45) is commented at pag.30 but it appears to be confusing. Momentum as a physical quantity should be gauge invariant, a Goldstone field is not gauge invariant.

19. (5.21) is analogous to (B.13). What is the analog of (B.12), which one should use to get to (5.21)?

20. Well-defined gradient expansion and UV/IR mixing. The parts within Sections 5.2, 5.4 and Section 6 that are concerned with the gradient expansion are confusing. Apparently, having $\mathcal{A}_{\mu\nu} \sim \mathcal{O}(\partial)$ gives the hydrodynamic series a structure of double expansion in both the wave-number $k$ and $\mathcal{A}_{\mu\nu}$. From the comments given in the manuscript, it is not clear whether one is free of considering different regimes like $\mathcal{A}_{\mu\nu} \ll k$ or $\mathcal{A}_{\mu\nu} \gg k$, or if one is obliged to consider $\mathcal{A}_{\mu\nu} \sim k$. Above (5.17), it is stated that higher order scalars built from $\mathcal{A}_{\mu\nu}$ can affect lower orders in wave-numbers. This would relate to $\mathcal{A}_{\mu\nu} \gg k$ and has the downside of making the hydro expansion completely unpredictive. This is in line also with the comments given above (5.33) where, including higher powers of $\xi$, can modify the lower order coefficients in $k$ in the dispersion relations. In the page change from 47 to 48, however, it is said that the gradient expansion followed in the main text reconciles the gradient expansion with that in wave-number, therefore $\mathcal{A}_{\mu\nu} \sim k$. Contrasting with the subsequent sentences where it is said that higher derivative terms also affect lowers orders in $k$. All in all, it is not clear whether one could just take an expansion in orders of $\partial$.

    Note that at the end of pag.47 the gradient expansion done for the $s$-wave with the same scaling hypothesis as the $p$-wave is discarted precisely because of possible "mixing effects" among the orders, when indicating that the higher-derivative term related to $m$ affects the IR-regime.

# 2 Curiosities

1. In the paper, only fractonic symmetries associated to multipolar symmetries are considered, what about sub-system symmetries?

2. The mass/pinning of the Goldstone in the $s$-wave fracton superfluid is very interesting. What is the relation of such pinning to the Goldstone counting problem and/or inverse Higgs constraints?

# 3 Typos

1. The indexes of the $\tilde{A}\,e$ combinations in (2.30) are wrong.

2. Last paragraph of 2.4, the removed components are $d(d-1)/2$ and not $d(d+1)/2$.

3. First sentence of Subsection 2.6, "which" $\to$ "in which".

4. Between (5.14) and (5.15), "thos" $\to$ "those".

5. Just before (4.27), “must a” → “must be a”.
6. Before (4.47), the transformation $f_s \to -f_s$ is referred to as a “shift”, but it is not.
7. Text between (5.18) and (5.19). “second variation (5.3)” → “second variation of (5.3)”.
8. Below (5.21), “as the for $p$-wave” → “as the one for $p$-wave”.

# References

[JJ22] Akash Jain and Kristan Jensen. Fractons in curved space. *SciPost Physics*, 12(4), apr 2022.