Response to Scipost

We thank the first and the third Referees for their reports and for taking the time to review our manuscript again in such detail. In the following, we provide further clarifications regarding the points raised. We hope that these additional explanations and revisions have improved the clarity of the manuscript, addressing the concerns of the Referees.

We also noticed that the report from the second referee contains several statements and references that appear generic or possibly artificial. For instance, it cites a non-existent paper ("A. Smith et al., Quantized Vortex Models") and raises questions about the ξ-dependence of classical solutions in R_ξ gauges, but it is not clear what gauge symmetry the referee is talking about (no gauge fixing is discussed in our manuscript). For these reasons, we did not address the points raised in that report. We nevertheless considered it appropriate to bring this matter to the editor's attention.

Referee 1

We thank Prof. Abanov for his contributed report, raising many interesting and important points. We detail below the modifications to the manuscript that we have done to address his questions.

Weaknesses

1 *Does not always clearly distinguish between exact results and heuristic arguments. For example, Section 5.2, which plays a crucial role in the subsequent development, is largely heuristic in nature. It would be helpful to acknowledge this explicitly at the beginning of the section.*

We have now included a sentence at the beginning of sec. 5.2, clarifying that the treatment of UV singularities will be largely heuristic.

Report

1 *Section 7 presents possible generalizations to other types of fluids. In Section 7.2, I would appreciate more commentary on the differences between two- and three-dimensional fluids, rather than on the similarities.*

We now included a comment at the end of page 50 where we stress that the symmetry group of the comoving fluid in $3d$ is larger than in $2d$, and thus we do not know how to construct a discretized lattice model that reduces to the $3d$ quantum perfect fluid in the continuum limit.

2 *I find the statement in the Outlook—"suggesting that the quantum theory described here is unlikely to be realized experimentally as is"—to be somewhat confusing (a similar statement appears in the Introduction). Quantum liquids such as liquid helium do exist. If the presented theory is applicable to such systems, then one should expect some of its predictions to manifest experimentally. Even if the degeneracy of vorton states is not truly infinite, it should be large. What, then, limits this degeneracy? Is it*

macroscopically large, or is there a mechanism that invalidates the presented approach and lifts these degeneracies—at least partially? Many such questions remain open.

When describing the quantum perfect fluid as "unlikely to be realized in nature", we mean that the symmetries of the vorton EFT cannot be emmergent in the low energy limit, contrary to the naive expectation for a hydrodynamical system. Indeed, in the analysis of positronium in section 8, we show that an infinite amount of fine tuning is needed to obtain the vorton theory in the low energy limit. We see this as a potential explanation as of why we do not observe vortons experimentally. We have expanded the discussion in the conclusion to reflect this point of view.

On the other hand, as commented in ref. 53, as well as in sec. 7.2 and the outlook of our paper, the regime of highly vortical flows might be experimentally relevant and deserves further study.

Requested changes

1 *Footnote, page 31: The sentence "In fact, as eq. (5.22) shows, vortons exhibit vanishing vorticity as $p \to 0$" is confusing. According to eq. (5.21), the vorticity of a vorton is identically zero. Equation (5.22), on the other hand, describes the vorticity dipole moment of the vorton, which—when the vortex–antivortex pair is well separated—takes the form $d = \ell\gamma$, where ℓ is the separation and γ the circulation. Even if the dipole moment d vanishes, this does not imply that γ vanishes; rather, it indicates that the vortex and antivortex merge $\ell \to 0$, a process governed by UV physics beyond the regime of validity of the effective theory presented. I suggest clarifying this argument. In fact, the explanation based on the symmetry $\Phi \to \Phi + c$, also mentioned in the footnote, is more convincing.*

We added equation 5.21 and modified footnote 30 in the current page 34, to clarify that the vorticity density vanishes at zero momentum.

2 *In the same sentence, it is better to use P instead of p to remain consistent with the notation in equation (5.22).*

We thank the referee for spotting the inconsistency, which has now been eliminated.

3 *In the sentence immediately above equation (8.6), the phrase "spherical harmonics" is somewhat misleading, as only a single angular variable exists in two dimensions. The terms "harmonics" or "angular harmonics" would be more appropriate.*

We changed "spherical harmonics" to "angular harmonics".

4 *In the first sentence of the Outlook section, please change "work affirmatively address the question" to "work affirmatively answers the question".*

Thanks, done.

5 *Please clarify what is meant by "zero temperature fluid" (see the referee report for details).*

We acknowledge that talking about "zero temperature fluid" is misleading and in partial contradiction with the narrative of the paper. We have replaced it by the notion of non-dissipative limit, when it was mentioned in the text.

6 *My understanding is that the form of the action in equation (3.16), as well as the interpretation of the right and left symmetry actions, originates with Arnold. It would be helpful to include a reference to his work at this point.*

We have included references to Arnold's work at the end of section 3.1.

Referee 3

We thank Referee 3 for the report and for the very insightful questions and problems raised. As suggested, we have significantly expanded the outlook section into a conclusion and outlook section summarizing the exotic properties of the vorton theory. We detail below in more details the modifications to the manuscript that we have done to address the questions.

Weaknesses

1 *In not really sure what the conclusion is!! Its not clear what the take-away message is?*

We have now included a conclusion section before the outlook with the intend of clarifying that question. There we stress that the quantum perfect fluid is indeed a predictive theory that can be consistently quantized, which however shows some peculiarities—as we further detail below.

Requested changes

1 *It would be really useful have a conclusion regarding how this EFT differs from canonical ones and whether or not the EFT is in some way non-Wilsonian.*

We agree that, as is the case for fractonic systems, the EFT that we described doesn't break the Wilsonian picture, but rather defies the standard intuition from weakly coupled effective field theory. We have written a conclusion section explaining the exotic properties that lead to this conclusion:

- The spontaneous symmetry breaking at the quantum level that implies a gapless dispersion relation, whose prefactor cannot be inferred from the low energy Lagrangian (it depends on UV information that is not encoded in the Wilson coefficients of the EFT).
- The infinite degeneracy of the spectrum. Importantly, the spectrum and its degeneracy rely on the preservation of the symmetry beyond the cutoff scale.

These two properties imply a stronger sensitivity to the UV physics than in usual EFTs.

2 *There seems to be tension between the following assumptions. The relabelling symmetry is NOT a gauge redundancy (as they emaphasize was mistakingly conclude in ref [2]), and yet they wish to quantize the theory with a bose symmetry which is a gauge symmetry? i.e. you can't label quantum particles. But the symmetry is exactly a re-labelling symmetry. Can the authore please explain this?*

We thank the referee for the question that allowed us to clarify a subtle point. We now address this question in the last two paragraphs of the conclusion, where we discuss the consequences of gauging the symmetry. As formerly discussed in ref. 4, gauging the symmetry reduces the theory of the (comoving) fluid to a superfluid, whose quantization is trival and way less rich. Therefore, while gauging the relabelling symmetry is a consistent option, we believe it is worthwhile to explore the richer alternative developed in this work.

3 *The authors keep referring to the UV-IR connection. This term is often used without a clear definition. As we know there is always a UV-IR connection in the sense that IR coupling are sensistive to UV physics. The authors seem make a point of the fact that the effective dispersion relation has a coefficient which relies on unknown UV parameters, but I am puzzled by this, in the sense that that is always true. So what point are the authors trying to make after equation 5.9? Also the authors seem to be making a clear distinction between the dim. reg. model and the cutoff, but in either case the allowed coefficient in k^2 is sensitive to the UV, it's just that dim. reg. naively sets it to zero. I am sure the authors are aware of this but this needs to be clarified.*

We address these points in the conclusion, explaining in detail what we mean by UV-IR mixing and increased UV sensitivity. In particular, we stress again that the parameter entering the dispersion relation cannot be inferred from finitely many Lagrangian Wilson coefficients, and that the symmetry needs to be preserved up to the cutoff scale for the degeneracy of the vortons to be robust.

In the discussion of the dimensionally regulated model, we simply want to highlight that the fact that dimensional regularization sets the dispersion relation to zero cannot be used as an argument to dismiss the existence of the vortons, as the resulting theory is strongly coupled. Indeed, in the following section we introduce a discretized theory that allows us to obtain rigorous statements about the vorton theory. These are found to be in agreement with the naive cutoff analysis, hence justifying it a posteriori.

4 *On page 36 they state: In conclusion, we argued that the perfect quantum fluid in the Clebsch formulation admits infinitely many light vorton bound states, one for each particle number $n > 0$, with energy given by (5.19). Their existence and degeneracy is a robust consequence of the symmetry algebra. Yet, the precise coefficient of their dispersion relation naively depends on all higher derivative operators, thus defying the standard EFT logic. Given my previous paragraph, I ask the authors to please clarify this seemingly important point they are making as well.*

We have expanded this paragraph in the main text (between eq.s (5.31) and (5.32)) as well as in the conclusion (see also our answer to requested change 1.).

5 *In the papers on fracton models this (UV-IR mixing) term is used, I believe, when the degeracy of the ground state grows with the number of lattice point. I think thats what they author are saying here as well. But this does not in anyway spoil the Wilsonian picture. Its true that such degereracies create challenges to calculating in a Fock space picture. e.g. systems with flat bands, but thats not a violation of Wilsonian reasoning.*

We agree with the above statement. We have added a discussion of the dependence of the degeneracy on the number of lattice site, as well as the similarity with the fractons in the conclusion.

6 *Can the authors put some equations behind these words? Perhaps they have elsewhere and I have missed it. But it would be good to put it here. In particular the action of Q22 on a vorton bound state versus a state of two vortons separated from each other. Don't these two operators generate states with non-zero overlap?*

We added equation (5.32) to clarify the discussion.

7 *Could the authors also explain why the quantum generation of the* ω^2 *term in the energy is not an anomaly? Otherwise why would it not have been included in the classical theory?*

We have added a paragraph in the conclusion highlighting and explaining the fact that the symmetry is not anomalous, since quantum effects do not break it explicitly but only spontaneously. This is manifest in the discretized model where the symmetry charges manifestly commute with the Hamiltonian.

8 We thank the referee for spotting some grammar mistakes, which we have corrected.