We would like to thank both the referees for their detailed reports which led to several improvements in our manuscript. In the following we address all the points.
To simplify their work of review, we have kept all the modifications in the manuscript in **red** color.

1 Report 1

The authors study the behaviour of black hole solutions in a specific class of models corresponding to massive gravity theories in order to test the validity of several conjectured (upper and lower) bounds on certain observables in the dual field theory. Such models have been used in the literature for modelling spontaneous and explicit breaking of translation invariance in holographic matter.

Identifying and understanding universal bounds on observables in Nature is certainly of great importance. Here, particular emphasis is give to the conjectured KSS bound for the shear viscosity to entropy density ratio, the lower bound on diffusion constants introduced by Hartnoll and extended by Blake, the upper bound on diffusion constants by Hartman et al as well as more recent bounds related to the speed of sound by Hohler et al. The authors carry out a set of numerical computations in order to test these bounds in field theories with (explicit or spontaneous) breaking of translations. These tests are new and, to the best of my understanding, correct. No new bounds have been introduced in this work.

We thank the referee for the positive and useful report.

At several points in the paper the authors claim that they have numerical proved" the conjectured bounds. The authors are reminded that numerically one can only disprove a conjecture by finding a counter-example. If the bound is not violated numerically, it simply means that in the specific model studied and for the specific values used, e.g. (N,μ,T), the bound was respected. Having this in mind, the claims of the authors are too strong and should thus be toned down, in order to actually reflect the computations done. As the paper stands, the claims are too strong.

We totally agree with the referee's view. The concept simply got lost in translation. We have modified the various sentences where "*proved*" was appearing and substituted them with milder expressions such as"*confirmed the validity*", "*checked*", "*verified*", etc...

Let me now turn to more specific points (following the order of the paper): 1. It is well known that finite-N effects violate the KSS bound (but not finite λ corrections). This itself suggests that this bound can not be a universal, regardless of the status of translation invariance. See 1108.0677 for a review.

The referee is correct. There are explicit perturbative computations which show a violation of the KSS bound given by $1/N$ effects. Nevertheless, the violation is not parametric (like in the case of broken spacetime symmetries) but it simply modifies the value of $1/4\pi$ to a different lower number (e.g. $16/(25*4\pi)$ for GB corrections), which interestingly enough is usually forced by causality or other consistency requirements. Therefore, we do not find fair to treat the two type of violations on the same footage. We nevertheless agree with the referee that this point deserves further explanation in the main text. We have added a paragraph to clarify this issue at the end of the introduction. Notice that the same argument can be also run for the diffusion constants.

2. τ_{eq} is the *local* equilibration time and it's related to the applicability of hydrodynamics. This is different from the time needed to reach *global* equilibrium, so please adjust the text. Furthermore, by now it's understood that the scale that sets τ_{eq} is not the distance to the first gapped quasinormal mode, but instead the location of the critical point (see e.g. 1904.12862) which can be much larger. How does this observation affect your results related to the upper bound?

We agree with the referee about the role of τ_{eq} and we have corrected the wrong statement. We are aware of the recent work that the referee mentions. In principle, the computation of the curve $P(k,\omega)$ is analogous to that of the Green's functions and it is therefore doable in our class of models. However, we believe (without any proof at the moment) that the two different definitions will not differ consistently but just modify the $\mathcal{O}(1)$ number appearing in the inequality. Some of these critical collision points were for example observed in https://arxiv.org/abs/1807.10530 in the context of the linear potential $V(X) = X$. From there, it is evident that the position of the first gapped mode and that of the critical point are not far from each other but always of the same order. We dont know if this is the general case and we definitely take this comment of the referee as a possible input for future work. We have added a comment about this definition in the main text to make the reader aware of this latest development.

3. For a system with both energy and charge density one needs to be more careful when defining the speed of sound, see e.g. 1205.5040

The referee is correct, the speed of longitudinal sound corresponds to $v_s^2 = \partial p/\partial\epsilon$ only in a simple uncharged relativistic fluid. Nevertheless, we never use such formula in our computations. We have added a comment about it in footnote 1. Notice how the introduction of finite elastic constant is exactly another case in which the simple formula above is not valid anymore and gets corrections.

4. In the last sentence of Section 1, you say you will discuss the implications of the absence of a UV cutoff, but I can not find this discussion. Please indicate where this is done.

We have expanded the conclusions to include a short discussion on this point.

5. Please add a small appendix with the details of the numerical calculations that you carried out. This will add clarity and completeness to the paper and allow readers to follow easier. Also, add a few comments in the main text where you discuss the difference between explicit and spontaneous breaking in connection with the value of N – currently this is only done for the explicit case.

We have added a short appendix about the numerical computations and a paragraph in the text clarifying the role of N in determining the nature of the translational symmetry breaking (see end of section 2).

6. Figure 2, right panel: remove the y-axis label (since you display 2 different quantities on the same plot)

We have fixed fig.2.

7. Page 7, point V, first 2 sentences: Essentially all the points on page 6 and 7 indicate that it's not clear what is the right definition for the shear viscosity when translations are broken. Given that, any conclusion for the violation or not of the KSS bound in this context can be attributed to simply using the wrong definition and thus not to carry any physical importance. This comment also applies to the orange line in Figure 2. I understand and I agree with the rest of the argument V regarding the mass of the shear mode.

Indeed, there has been a lot of discussion regarding whether the usual Kubo formula applies or not. Notice that this subtlety is related to the explicit breaking of translations and does not apply to the spontaneous one in which the conservation of the stress tensor remains intact. Therefore the apparent violation of the KSS bound (at least for the SSB case) cannot be attributed to this technical point. Moreover, Ref.[60] addressed this problem and concluded that a violation of the KSS bound appears independently of the definition of η. Therefore, we can exclude the possibility that the violation is simply due to a wrong definition of the transport coefficient. We added a clarification in point (IV).

8. Page 21: the fact that the stiffness becomes negative signals a dynamic instability.

That is subtle. The stiffness becoming negative would signal a dynamic instability only if there is a corresponding propagating sound mode whose speed is set by the stiffness. Notice that in our case that is never happening because for $N < 5/2$ there is no propagating sound mode (momentum dissipation destroys it) and for $N > 5/2$ the speed of sound is not given by the stiffness because of the presence of finite elastic moduli. This has been verified explicitly for $N = 1$ in Ref.[130]. We clarified this in the text.

9. It would be nice to add a table in your discussion section where you summaries the bounds and whether or not they are violated for spontaneous or explicit breaking of translations in your model.

We added a summary table in the final section of the paper.

10. The Hawking-Page transition has been associated to the confinement transition by Witten more than 10 years ago. How does this fit with the concept of the melting temperature?

We had a short comment in the discussion. The idea of connecting the melting process to the Hawking-page transition was put forward in our Ref.[122]. The idea is simply that the holographic solid would cease to be the favourable solution at a certain critical temperature and undergoes a first order phase transition to the dual of thermal AdS. We find this proposal interesting and we are planning to revisit it in our model following the methods of Ref.[132].

11. Please check the text again for typographic errors. Furthermore, in several points the authors summarise the lack of deeper understanding of certain concepts in the form of questions, one after the other. This clearly shows their excitement for the topic, which is highly appreciated and valued, but it can be disruptive to the reader – please consider formulating in a different way.

We have tried, specially in the conclusions, to remove the direct questions and to contain our excitement.

Given the above, I recommend this paper for publication in Sci.Post, after the authors have addressed the points raised here.

We have addressed all the points of the referee which were very useful and accurate. We believe that our manuscript has improved a lot and we hope the referee will find it now suitable for publication in SciPost.