Dear Dr. Millev,

Thank you for arranging the review of this manuscript. We are pleased that the referee looked favorably on the manuscript and recommended publication. In the revised manuscript, we have addressed all the issues raised by the referee, as explained in detail below. In what follows, the referee report is in blue and our response is in black.

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Report of the Referee -- BM13999/Stahl

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This paper gives physical arguments for the possibility and

impossibility of spontaneous breaking of multipolar symmetries in

various dimensions, at T=0 or T>0, and in both clean systems and

systems with quenched disorder. The work is timely and interesting,

particularly in the context of the flurry of recent work on fractons

and theories with multipolar symmetries. It is generally well-written,

although some of the discussion is a bit terse or hand-wavy (see later

comments). I would recommend the paper for publication in PRB,

although I have some suggestions that I believe would improve the

paper’s clarity.

We thank the referee for this favorable assessment.

1) I find the discussion about domain wall nucleation following Eq.

(11) to be lacking in detail to the point of being confusing. Why is

the energy cost minimized by varying \phi like a polynomial of degree

a+1? This hand-waving argument makes it seem like one could obtain a

kinetic energy cost that is exactly zero by choosing to vary \phi like

a polynomial of degree a or lower. It would be helpful to elaborate on

this argument.

The domain wall discussion is meant to be a heuristic argument to help with intuition prior to the correlation function calculations. We have clarified the argument and also emphasized that it is auxiliary to the correlation function calculations.

2) How many of the results in this paper can be generalized to other

theories with soft (putative) Goldstone modes which do not arise from

a multipolar symmetry? It appears that, for example, in the T=0

Mermin-Wagner argument for full multipolar breaking, the only

information that plays a role in determining the critical dimension is

the fact that the dispersion goes as \omega \sim k^{a+1}; the fact

that the symmetry is multipolar does not obviously play any other

role.

Indeed, other soft Goldstone modes can lead to higher critical dimensions. Multipole theories are interesting in this respect because they do not require fine-tuning to achieve soft modes. We have clarified this in the introduction.

3) I find Section IV.A quite opaque. What, precisely, is the result?

We are considering breaking M^a\_{max} down to what subgroup in

general? And how many Goldstone modes are there, in what

circumstances? I think a significant part of my confusion arises from

why we start from talking about breaking M^a\_{max} to an arbitrary

subgroup and then suddenly talk only about breaking M^b\_{max}.

We have updated sections IV.A and IV.B to provide a clearer picture of the results of symmetry breaking in our models. In particular, section IV.B now says in which dimensions it is possible to break any maximal multipole group to any of its maximal subgroups. We have included a note on the subtleties of counting massless modes and of deciding which such modes are true Goldstone modes.

4) Are there any results arising \*directly\* from the lattice models

showing, for example, how the different phase transitions have

different critical dimensions? At the moment, the discussion of the

lattice model is limited to stating the phases one would expect in

various regimes, and then stating that some of those phases cannot

exist in certain dimensions due to the prior abstract arguments.

The lattice model does not give any new results. Instead, we use it as a concrete model where we can apply the results of the paper. This is now clarified at the beginning of section VI.

We thank the referee for detailed and useful comments, and trust the manuscript will now be judged suitable for publication.

Sincerely,

Charles Stahl, Ethan Lake, and Rahul Nandkishore