Review of egusphere-2022-790 - Round 2

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I thank the authors for their answers to my comments.

I think this manuscript has improved and is almost ready for publication. I recommend this manuscript be accepted with minor revisions, after the authors address my last remaining comments which I list below.

Note:

- *My previous comments are in italic*
- **The author's answers are in bold**
- My new comments are in a normal typeface.

Comments

- *Page 3, L58 to L63: This paragraph is unnecessary. The titles of the subsections are sufficient, and these section introductions are unnecessary.*
  - **We will consider removing it.**
  - **We removed the summary paragraph.**
    - There is some confusion here. I think the paragraph you removed is necessary, I was referring to the paragraph between the title of the section (2 Methods) and the title of the subsection (2.1 Phase-field model of brittle fracture.) I think these section introductions are not necessary, but you decided to keep them in the rest of the manuscript, and I accept your choice. However, I think the summary paragraph is necessary, especially the first sentence defining your paper's study. Please add the summary paragraph back.
- *This manuscript only shows tensile tests, although observations of sea ice show more compressive or shear deformations. Is there a reason for this choice? I would like to see how the model behaves with shear and compressive tests, with one example of each. It would strengthen the manuscript to have more examples of how sea ice behaves. I would guess these are forcing situations that are very likely to happen in the ICEx 2018 datasets.*
  - **We will study shearing and compression experiments when we update the manuscript, and add some of these tests to the paper. If one of the experiments (we believe the shearing experiment is more likely to be promising) proves to be physically interesting, we will perform a statistical study with random ice impurities similar to what we did for the tension test**

and add it to the manuscript. Thanks for the suggestion—we agree that only tension tests is somewhat restrictive.

- **We ran simulations of both compressive and shearing displacements and shear forcing. We found the shear displacement produced crack geometry different from compressive experiments. We added a new figure (Fig. 3) with images of the shearing cracks along with discussion in lines 198–204.**
    - I am happy that you (the authors) took the time to perform the additional shear and compression experiments. I find the results of the shear experiments of Fig 3 to be very interesting! However, I do not see a mention of the compression experiments in the manuscript. Is it because you observed the same fracture pattern as in tension experiments? Or do you run into other difficulties in the compression regime?

- *There is a confusing notation that needs to be addressed. The authors use the notation on L66 for the displacement vector. Then, they define as the velocity vector on L140. Per convention and common use, is better suited for velocity. I suggest using something like for the displacement field and keeping for the velocity field. This way, any confusion with the sea ice VP models, where the strain-rate with the velocity, would be avoided.*
    - **We will clarify the notation when we revise the manuscript. Thanks for pointing this out.**
    - **We did not intend to define the vector on L140 (first version) as a velocity. It is a displacement. We have fixed this typo.**
        - I see that $\mathbf{u}$ is used in Griffith fracturation process from Bourdin et al. (2008) as a displacement, I understand that you keep it, but I still find it confusing.

- *To improve the figure, you could use two panels, one for critical force and and one for average thickness and α.*
    - **The figure will be improved. We agree that it looks confusing.**
    - **We have added separate scatter plots and changed to color map of the original figure to improve the contrast.**
        - Thank you for these changes, it is much better.

- *I could imagine doing similar tests with a VP model. However, it would take an enormous amount of computer time because the numerical convergence of the solver is very slow, especially at a 5m resolution. The cost is discussed on page 12, but this model seems much more efficient than the VP, with which I cannot fathom doing 1000s of simulations. I would be interested to know how fast the model presented here can predict a fracture, e.g., how much time it takes per processor per 1000 random samples.*
    - **We appreciate your comments, but believe that such a comparison in favor of the approach presented here is not fair towards VP models. The phase field approach we present is instantaneous, so it does not include time evolution (like VP), and it's also only useful for initially undamaged ice floes.**

As discussed in the manuscript, if such a phase field model is part of a discrete element model, then in absence of a faster approximate model, many phase field computations for floes in each (or at least many) time steps are necessary, and the time evolution still needs to be taken care of by another model (e.g., the DEM model).

- Thanks for the precisions, I did not realize that it was so fast, I thought it would still take some time to find the fracture fields as you have iterations in Alg. 1 and describe a solver in Sect. 2.4. What it the usual number of iteration that it takes? You could also give the values used for ϵ and N of Alg. 1, for reproducibillity.