Answers to tc-2020-354 RC3

June 21st, 2020

Note :

- The referee comments are shown in black,
- The authors answers are shown in blue,
- *Quoted texts from the revised manuscript are shown in italic and in dark blue.*

- Note that the exact pages and line numbers in our responses are subjected to change as the revised manuscript is being prepared.

In this paper the authors introduce a modification of the MEB rheology in the form of a generalized damage parameterisation. They then proceed to test this new parameterisation using an idealised uniaxial loading setup. They find that the new parameterisation influences the resulting fracture angle, bringing it in the range of observations. The paper is well written and clear, using good English and sentence structure, and a logical flow from section to section and paragraph to paragraph.

The introduction of a modification of the MEB rheology is a niche topic, but potentially an important one and certainly one relevant for publication in the Cryosphere. As it stands, the paper has some faults I would like the authors to address. I expect they can do this adequately and that the resulting work will be fit for publication in the Cryosphere.

We thank the referee for his or her thorough review of the manuscript and constructive comments.

Major comments:

It is not clear why the authors are proposing this addition to the MEB. Is it numerics or physics, or something else? You say something general at the start, but it's vague and really only says what your modification does, not why you want to do it in the first place. This point should be crystal clear and guide the entire paper. Ideally the authors should say something like: "we want to introduce this scheme because we know it represents better the physics (and is incidentally better for the numerics). We see this by looking at the fracture angles (or some other measure)". Such a statement at the top would make this paper very strong. An admittedly overly harsh evaluation of the current state is that the authors change something for dubious reasons and get a different response – so why should we care? Is this the right result, but for the wrong reasons? I don't think that's a fair assessment, but unless the motivation is clearer it will be the impression a critical reader gets.

>> We re-wrote the introduction to better state our objectives. The goal of the study is to reduce the integration errors in the MEB rheology and study the sensitivity of the model to the stress correction scheme, given that the exact path along which the super-critical stresses should be returned to the yield

curve is not known a priori. This is clarified in the abstract, at L60-L64 and at L136-L137 in the revised manuscript.

We also add that our assessment of the sea ice deformations resulting from the use of a damage parameterization in the MEB model contributes to the current effort to assess the difference between different rheologies in reproducing satellite-derived sea-ice deformations (the FAMOS Sea-Ice Rheology Experiment (SIREx), https://epic.awi.de/id/eprint/48616/, with two papers currently under review in JGR). This is mentioned in the revised introduction.

Related to this lack of clear focus, I find it difficult to understand why you do the experiments that you do, so reading sections 4 and 5 is more demanding of the reader than it need be.

>> There is a clear need to standardized simple idealized experiments to test/evaluate different rheological models. This was identified at the workshop "Defining a cutting-edge future for sea-ice modelling" (Laugarvatn, Iceland, 2019) and again recently at the online workshop Modeling the Granular Nature of Sea Ice (https://seaicemuri.org/workshop.html). In both workshops, the simple uni-axial loading test (used in Ringeisen et al. (2019, 2020) received good acceptance for the community. This is now clarified in the introduction of the revised paper.

I also question the fact that the authors don't introduce heterogeneity into their model. They even point out themselves that it "is responsible for much of the brittle material behaviour in progressive damage models" and indicate that the residual errors are not important in a heterogeneous field – which is what MEB is supposed to give. This choice needs to be much better justified than is currently done.

>> We did not include heterogeneity in order to clearly identify the model performance (both numerics and physics). The issues related to the error growth leading to asymmetry in a problem with full symmetry and their impact on the fracture angles could not be addressed using heterogeneity. This was clarified in the revised manuscript at L339-344. We also clarify at L339-341 in the revised manuscript that the heterogeneity is responsible for the localisation and intermittency of sea ice, properties that are not investigated in our manuscript.

Finally, there's almost a hostile tone towards the MEB rheology in the discussion and conclusion section. The authors are practically gleeful in pointing out various faults of the model that are not relevant to the modifications they propose. It is of course fine to point out the faults of MEB - which apparently are plentiful – but the way it is done here borders on un-professional, in my opinion.

>> This is a serious accusation (unprofessionalism). We would ask that the reviewer identify the offending sentences and we will respond promptly whether the paper is accepted or not.

Clearly, this is not our point of view. We disagree that our tone is hostile towards the MEB model. We developed the only (to our knowledge) implementation of this rheology in a finite difference framework in order to be able to study the difference in physics independent of the numerics (other MEB implementations are done in Finite Element). Our study of the numerical and mechanical behaviour of this rheology is in the prospect of better understanding how the damage parameter simulates the deformations and to identify the key elements that can be useful to other models as we aim for higher resolution products. The current paper is a follow-up to an earlier paper where those issues were raised but not addressed. Our goal is to improve sea ice modeling in general and we believe that a multi-model approach towards this goal is very useful.

Minor comments:

L16: The formulation makes it sound as if leads and LKFs are interchangeable, but they are not.

>> We agree and removed to mention to LKF in this sentence.

L120: Shouldn't the cohesion be a function of resolution (see Weiss, 2007)? If that's the case, how do you get the same value from large scale and the lab?

>> The material strength is a function of the resolution, and we do expect smaller values at the large scales (kms in our model) than in laboratory experiments, which usually find strengths that are one or more orders of magnitude larger than what we use in our study (10 kN m^{-2}). Our choice of cohesion is based on results from the ice bridge experiments of Plante et al. 2020, and coherent with what has been used in other studies using the MEB rheology (e.g. Dansereau et al. 2016, 2017, 2019, and also in Rampal et al. 2016, 2019).

L135: What's the physical justification for proposing this generalised stress correction?

>> As we mentioned above, we develop the generalised stress correction in part to improve the issues identified in our previous paper, and in part to assess the influence of the super-critical stress correction on the simulated fractures and deformation, with minimal chances to the damage parameterization. This is clarified in the revised manuscript at L60-64 and at the beginning of section 3.

Note that in the original parameterization, the choice of defining the damage parameter in terms of the amount of stress in excess of the yield curve was made to offer numerical robustness and simplicity. In a perfect model for instance, this overshoot would approach zero. A physically meaningful definition of the damage parameter could involve thermodynamics relations as the stress state approaches the yield curve (see for instance Murakami 2012), or use discrete cycling methods (as in the models of Main., 2000, Amitrano and Helmstetter., 2006, Carrier et al., 2015), but would represent a significant modification of the damage parameterization. This is considered for future model development but out of the scope of this paper.

L222: Mohr-Coulomb and Roscoe theories both concern granular materials, but hear we're dealing with the fracturing of a solid. Are they still valid? Please elaborate.

>> Sea ice is a granular material. Sea for instance books from Leppäranta (2011), Weiss (2013), and the recent workshop "Modeling the Granular Nature of Sea Ice" (https://seaicemuri.org/workshop.html), bringing scientists from all around the world working on this topic, for reference.

L248: There's a lot of information in figure 4 and the reader needs more help in deducing why you created it and what it's supposed to tell us.

>> Additional information is included in the figure caption of the revised manuscript, which now reads:

"Scatter plots of local stress invariants (σ_{I} vs. σ_{II}, in kN m^{-1}, left column), normal stresses and scaled strain rate invariants (σ_{I} vs. $(1-d)^3 \dot{\epsilon}_{II}$, right column) in heavily damaged (d > 0.9) grid cells, at t = 60 min (shortly after the fracture, top row), t = 120 min (~1 hour after the fracture, middle row), and t = 180 min (~2 hours after the fracture, bottom

row). Color indicates the local damage. The strain rates are normalized to account for the non-linear dependency of the viscosity η on the damage parameter. The gradual alignment of the points in the σ_{I} vs. $(1-d)^3 \dot{\epsilon}_{II}$ diagram indicate the development of a linear-viscous stress-strain relationship over time."

L276: A reference to the contrasting results is needed.

>> We add the reference to Ringeisen et al. (2019) for the VP model and to Bardet (1991), Balendran and Nemat-Nasser (1993), for granular materials.

L291: A reference for what is typical for granular material is needed (a textbook will suffice).

>> We added the reference to the book from J. Duran (1999), and to Bardet (1991). See references below.

L316: This entire paragraph is a bit up-side-down to me. You start by saying the MEB is not good enough, for various reasons (begging the question of why you use it in the first place, actually) - and then you say how your new addition will not save it. A more natural way to write this is to first say that although the decohesive stress tensor can do some things it cannot fix everything, including etc.

>> We do not state that the MEB is not good enough. We mention the differences in behaviour with respect to the more commonly used VP models and discuss these differences in terms of potential limitations that should be taken into account in future model developments. We do believe that the use of several different models raises questions that would not be raised with the use of a single model, even if that model is better. Our community has suffered from a monopoly in approach with the standard VP model, until only very recently when new approaches were developed. This is made clear in the introduction of the revised manuscript.

We also believe that this discussion is made clearer in the revised manuscript by specifying in the abstract, at L60-64 and L136-L137 that we developed the generalized damage parameterization in part to investigate the influence of the return algorithm on the simulated fractures.

L329: This paragraph is off topic, discussing experiments not introduced before and not relevant to the introduction of the decohesive stress tensor. Please remove.

>> We argue that this paragraph serves to put our results in context with other MEB model studies, which use different material parameters. In the revised manuscript, we widen the discussion to integrate the effect of grid resolution, sample aspect ratio, advection and heterogeneity, and clarify the context of this discussion.

L353: Now I'm confused, did you want to solve the ridging problem by introducing the decohesive stress? Again, a more natural way to present your results would be to first state what works and then what remains.

>> We now specify that the generalized damage parameterization modification is used to tackle issues that we raised on the damage parameterization in a previous paper (Plante et al. 2020) but also to investigate the influence of the return algorithm on the simulated fractures. The dominance of the post-fracture deformations in the MEB rheology is an important finding in our experiments, which contrasts

with the behaviour in the VP and EVP rheologies. This is clarified in the conclusion at L346-348, but also in the abstract, at L60-L64 and at L136-L137in the revised manuscript.

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