Review of "A Maxwell-Elasto-Brittle rheology for sea ice modelling" by Dansereau et al.

This manuscript details the development of a new viscoelastic rheology for sea ice modelling that includes fracturing behaviour through the use of a continuum damage variable. A Maxwell type viscoelastic model is adopted, with the elastic and viscous terms coupled in series. A continuum damage variable is adopted for representing fractures in a continuum sense. The damage variable serves to decrease both the effective elastic modulus and the effective viscosity in the Maxwell model. The constitutive relation for developing and modifying the damage variable is based on a Mohr-Coulomb criterion that accounts for both principal stress components (in 2D plan-view) and different levels of ultimate strength in tension versus compression. A healing scheme for damage is introduced, governed by a characteristic timescale for damage healing. The resulting governing equation for damage evolution thus includes both damage production according to the current stress state and damage reduction (healing) through time. The effective elastic modulus is decreased according to the current level of damage in a similar manner as is commonly adopted in purely elastic damage models. The effective viscosity, however, is decreased according to a power law such that the viscous relaxation time of the material decreases with increasing damage.

The manuscript is incredibly detailed and generally very well written. The behaviour of the model is thoroughly explored using an idealized, uniaxial compression experiment. The model is capable of reproducing a number of features that are consistent with observations of sea ice deformation, including heterogeneous deformation that localizes along faults/leads and intermittent deformation and damage at different spatial and temporal scales. Care is taken to point out the limitations of the model setup and directions for future development.

Most of my comments are relatively minor, and mainly have to do with the presentation and description of the model and the results.

General comments

1. The thoroughness and detail of the manuscript are commendable, although in some places the presentation was somewhat confusing as a result. I would recommend splitting the Introduction into separate Introduction and Background/Theory sections. A shorter Introduction section that clearly outlines the motivation for the new model, the most relevant context, and the general approach would benefit the reader. As it is written, the Introduction currently is very long and detailed but in some places confusing in terms of both the writing and the relevance of this level of detail. In other places, relevant details seem to be left out, and it seems to be left to the reader to be familiar with all of the references in order to understand certain points. It seems to be an excellent review of the state of sea ice modelling, and demonstrates that the authors have a good grasp of the field, but as a reader I found myself a little "lost in the weeds" at times.

2. Even in a fully viscous model of ice deformation, the stress balance can be non-local (for instance, in a glaciology context, viscous ice stream or ice shelf deformation is described by a stress balance that is inherently nonlocal, (e.g. *MacAyeal*, 1989)). You seem to be implying in several places that "long-range" interactions must come from elastic deformation (e.g. p 4, l 14). Am I reading this wrong, or are you indeed stating that long-range interactions can only be accounted for by elastic interactions?

3. The results of the model are mesh-dependent, as damage localizes to the scale of an individual element. This is often viewed as a negative result, because the results of the model thus depend on how the user sets it up. However, many different approaches for nonlocal regularization of damage models have been proposed and adopted in a variety of settings (e.g. *Bažant and Jirásek*, 2002; *Borstad and McClung*, 2011). In these approaches, the stresses/strains/constitutive relation are computed by integrating over an intrinsic length scale related to the scale of heterogeneity of fracturing of the material. As long as the element size is smaller than this intrinsic length scale, the results of the model are independent of

the resolution of the mesh. I think the authors should mention this type of approach in the manuscript, and discuss whether it might be feasible to produce mesh-objective results.

4. The discussion of the damage formulation is a bit confusing. You mention in the text (p 14, l 24-26) that stresses outside the failure envelope are non-physical. However, unless I am missing something, you seem to be calculating your damage variable according to the distance *beyond* the failure envelope. It seems, then, that damage is a sort of constitutive post-processing to "correct" the stress level such that it lies directly on the failure envelope. Some clarification is needed in the text on this point, since your schematic representation of the failure envelope in Figure 2 seems to contradict what you state in the text. Damage is based on the distance of the stress state outside of the envelope, and yet a stress state outside the envelope is non-physical...

5. I'm confused as to why a separate term for the ice concentration (A) is needed, as this seems at least partially redundant with damage. Why is it necessary to have both a concentration term and a damage term that modify both the elastic modulus and the viscosity? Isn't there some redundancy here, as a damaged fault/lead will necessarily have a reduced ice concentration? The ice concentration term seems to be simply added to the model at the very end of the model discussion, without much explanation.

Specific comments

- l 25: "or"
- p 4, l 6-9: how have these VP hypotheses been found inconsisent? Can you summarize these for the reader?
- check English spelling throughout the document, a number of words are misspelled (looses instead of loses, euqation, it's instead of its, dependance, assymptotes,...)
- p 10, l 20: there is some inconsistency in the text formatting of the different versions of "I" for the identity tensors
- p 13, l 22-23: I was confused about what h is here
- p 19, l 18: might there be other contexts or model setups (e.g. realistic domains) for which the minimum value of d might come into play? The results of a damage model can be quite sensitive to this choice.
- p 20, l 11-13: Some motivation or explanation is needed for why you choose to write the momentum equation in terms of internal stress rather than the vertically integrated stress tensor, especially if you are departing from what is more commonly done in the sea ice modelling community.
- p 23, l 6-9: why not perform a sensitivity analysis as you describe then?
- p 26, l 5: you previously described the inertial term as being negligible, so why is it here? some clarification is needed.
- The first part of the Results section is not really results, but background.
- p 29, l 23-24: this would be helpful to state also in the figure caption
- p 30, l 26-27: well, the localization scale is the element scale, so the choice of resolution dictates the localization of damage
- p 31, l 6-11: but you didn't introduce disorder initially, so you cannot claim this here.

- p 31, l 27: "...has already been investigated in depth..." is another example of the reliance on the reader to be familiar with all of the literature you are citing. It would be more helpful to summarize the findings. What did these investigations find?
- p 34, l 25: some word has been omitted here
- Figure 5: it doesn't look like the elements are getting smaller from the top row to the bottom row of panel (b), but isn't the resolution supposed to be getting finer moving down in the figure? Also, the damage rate axis in panel (a) is missing a numerical scale other than the zero.
- Figure 6 is presented in the discussion of heterogeneity, the dependence on the spatial scale of observation. It's still not clear to me how this is represented in the figure, which only shows one realization of the experiment at one resolution.

References

Bažant, Z. P., and M. Jirásek (2002), Nonlocal integral formulations of plasticity and damage: Survey of progress, *J. Eng. Mech. - ASCE*, *128*(11), 1119–1149, doi:10.1061/(ASCE)0733-9399(2002)128:11(1119).

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MacAyeal, D. (1989), Large-scale ice flow over a viscous basal sediment: Theory and application to Ice Stream B, Antarctica, *J. Geophys. Res.*, *94*(B4), 4071–4087.