The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-210-RC3, 2019

Interactive comment on "The material properties of ice bridges in the Maxwell Elasto-Brittle rheology" *by* Mathieu Plante et al.

Anonymous Referee #3

Received and published: 23 December 2019

Review of "The material properties of ice bridges in the Maxwell Elasto-Brittle rheology" by M. Plante, B. Tremblay, M. Losch, and J-F. Lemieux.

The manuscript introduces an implementation of the MEB rheology in the McGill sea-ice model and outlines an idealised test-case studied with the model. The paper discusses the experiment results in relation to expected results from theoretical physical grounds, as well as outlining a few sensitivity experiments done on key parameters. The paper is generally well written and understandable. The science is reasonably interesting and good enough to warrant publication in The Cryosphere. I must say though that the paper quite esoteric, caters to a very narrow audience, and has relatively weak conclusions. As with all idealised, large-scale sea-ice experiments, this one suffers from the fact that comparison to theory, as well as the generalisation of the results, is

very difficult.

It is interesting to see a new implementation of the MEB rheology, which as far as I know has so far only been implemented by Danserau et al (2016) and Rampal et al (2019). Also, even though the setup is virtually the same as that of Dansereau et al (2017), the authors of this paper still to point out some interesting characteristics of the MEB rheology, as their approach is sufficiently different from that of Dansereau et al (2017). The main weakness of the paper is that even though there are some interesting points made (e.g. about the lack of strain hardening and the presence of numerical errors), then those are largely lost to other less interesting aspects (e.g. attempts to estimate physical parameters which should be estimated from a realistic setup). Ideally, the authors should reassess what is really interesting and novel here and focus on those aspects.

Other general comments:

*) The abstract should be rewritten, as it does not fit well enough the contents of the paper itself.

*) The description of the MEB model is much too detailed. It should suffice to briefly describe those parts of the model that are particularly relevant to the experiments conducted here, as well as those points where the current implementation differs from that of Dansereau et al and Rampal et al. The differences should also be justified.

*) The discussion of the cohesion (3.2.1) should take the following into account: Cohesion scales with the model resolution, so you cannot recommend one cohesion value for all resolutions (Weiss et al., 2007, Schulson et al 2009, Rampal et al 2016) Comparing ice bridges across different straits should take the ice thickness into account. Ice bridges longer than 100 km were probably a regular feature of the Kara Sea fast-ice cover (Divine et al., 2005, Olason, 2016) - although this is changing with a thinning ice cover there.

*) Angle of internal friction (3.2.2): I'm not convinced this is an appropriate setup to discuss the internal angle of friction. I would at least have wanted to see variations in the domain geometry, or better yet a model run with the setup from Ringeisen et al (2019).

*) Conclusions: You have a tendency to restate speculations from the text as demonstrable conclusions in the conclusions section. This is a serious fault which cannot be allowed to stand.

Specific comments:

L45: "minimum viscosity" should be "maximum viscosity"

L72: The term "brittle" refers to a certain type of plasticity, so you cannot contrast brittle and plastic (as in "i.e. Brittle [sic] in the MEB, plastic in the EVP"). Sea ice is a brittle plastic, but it can be argued that the (E)VP gives a (too) ductile behaviour to accurately represent sea ice.

L75: It should be "an MEB rheology", not "a MEB rheology"

L75: "implemented in an Eulerian finite difference VP model" - you should elaborate to make this clearer. I didn't understand what you meant before reading your section 2.3.

L122: Strike ", or creep," as creep usually refers to very slow viscous deformation of the ice (ductile deformation), but the viscous part of the MEB represents the stress relaxation that occurs after a brittle rupture.

L125: Rewrite the sentence "This brittle component . . ." emphasising that both models are plastic, but MEB is brittle while VP is ductile.

Section 2.2.1 seems unnecessary (or at least needlessly long) as it's a repetition of previous work.

Ditto for section 2.2.2, except for the point about the lack of strain hardening, a point I don't recall being discussed before in the literature. The authors would do well to

develop this point further and highlight it in their experiments.

L207: Replace lowercase \phi with uppercase \Phi (as well as throughout the rest of the text I believe)

L223: Missing unit for \lambda_0

L224: I don't think it's true that for high enough \lambda_0 MEB becomes EB. At any rate, Bouillon and Rampal (2015) and Rampal et al., (2016) are the wrong references for such a statement.

Section 2.3: After 5 pages of model description we (finally) have something novel. I dare say only the most attentive reader will make it this far, which would be a pity. You should highlight sections 2.3.1 and 2.3.3 and severely shorten everything else in section 2.

L293: Both Rampal et al. (2016) and Dansereau et al. (2016) use the finite element method for the spatial discretisation. Rampal et al., however, use a Lagrangian advection scheme. Section 2.3.3: How does your approach differ from the fixed point iteration used by Dansereau et al. (2016)? As always for numerics the practical implications of performance and accuracy are paramount.

Section 3: The figures should appear in the order they are referred to in the text.

L356: "This deviation results from the absence of a flow rule in the MEB model" This is a very strong statement, but you never sufficiently show this to be the case.

L369: The statement "[n]ote that unless . . . critical stress" is true, and a key aspect of the MEB model as fracturing increases the damage but does not influence the critical stress. Changing the critical stress would be a completely different approach. You need to justify the "in contrast to real ice features" much, much better for that statement to stand.

L378: I find the use of the word "point" in relation to the figures confusing. Can you use “panel” instead?

L397: The sentence “A physical solution . . .” cannot be allowed to stand as it is. It implies that the approach of Rampal et al. is unphysical, without stating why this is so. It also implies that the suggested approach is physical, but the support given is meagre in terms of physics. What is more, I see no physical reason to relate the yield curve parameters to ice thickness.

L402: I found this discussion interesting, but it’s tagged onto a very descriptive part of the paper and unlikely to receive much attention as it stands.

L536: I would not describe sea ice as being granular here. It can be, but the central pack, which MEB should describe, is not - nor is the unbroken ice cover the fractures are propagating through.

L535: Here you state that the discrepancies between simulated and expected fracture angles are due to the use of a scalar damage parameter. However, in the text itself, you appropriately say that you _speculate_ that this is the case. You should also use this formulation in the conclusions, as you never conclusively show why you don’t get the fracture angles you expect.

L544: You never showed that these errors are not detectable in a different configuration.

L547: You don’t show that the use of a damage tensor and a different stress correction scheme would solve the problem.

L558: Recommendations for who? You’ve only shown idealised experiments, so it is very hard to recommend anything to people wanting to run a realistic setup.

L559: Again, in the idealised setup you need this - but what is the impact in other scenarios? You should at least make that distinction clear.

L562: You never show this to be the case, it, therefore, doesn’t belong to the conclusions, and certainly not to your recommendations.

L564: Ditto for this point; you never show this to be the case, it, therefore, doesn’t belong to the conclusions, and certainly not to your recommendations.

References: I dislike typesetting dois as URLs or the inclusion of both a doi and URL for a single reference. There is also a discrepancy in the capitalisation of the paper titles.

L674: Remove an extraneous ˆ

L735: The full version (not the “discussions” one) is 2016.

Figures:

Figure 5: The phrase “Numbers indicate the location . . .” is not descriptive. Please rewrite.

Figure 8: The last word of the caption is misspelt ("componant", instead of "component").

Figure 10: Please write what the arrows on the yield curve plot indicate.

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