Answers to tc-2019-2010 RC3

February 15^{th}, 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.*

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.

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

Other general comments:

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

>> The abstract was re-written to better reflect the manuscript's content, also as per the comment of reviewer #1.

*) 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.

>> We have simplified somewhat the description of the model by eliminating some of the repetitions. The new model description is still more detailed than what the reviewer would like to see however. Sea ice models have been developed mainly by engineers (e.g. Hibler, Flato, Weiss, Sulski); yet they are used by a climate community composed mainly of physicists. While we agree that the model description could be shortened and make reference to previous work, we decided to present a detailed (stand-alone) description of the model, including details that are often trivial to engineers but less so for the climate community. This point is evident when looking at the development of sea ice modeling as used by the climate community in the last 40 years: as of today, most Global Climate Models use a modification of the standard VP model of Hibler published in 1979. Our goal is to make the model physics more accessible to the broader community such that improvement in future GCM relates to model physics (e.g. using the Elastic Anisotropic Plastic (EAP) rheology, the MEB rheology, VP rheology with Mohr-Coulomb and dilatation or the Elastic Cohesive rheology, etc.), not just the numerics.

*) 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)

>> We agree that the cohesion scales with the scale of features as documented in Weiss et al., (2007) and Schulson et al., (2009). Here, we propose a cohesion value that is consistent with ice bridge observations, which are at a scale typical of current sea ice models (10-100km). While Rampal et al., (2016) document the scaling of deformations in the MEB model, it is not clear that the model resolution impacts the cohesion of sea ice. This was tested by repeating the simulation using different spatial resolution, showing no change in the results.

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.

>> We agree with the reviewer. Note that we now use vertically integrated strength parameters. These changes in ice thickness will influence the ice bridge stability in the model, as it should be.

*) 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).

>> We agree. Our findings described here raise interesting questions that we are currently working on, using the same numerical set-up as Ringeisen et al, (2019), and will be publish in a subsequent paper. This is clarified in the revised manuscript at L487-489:

"This raises the question whether the lines of fracture may be influenced by the stress correction path

used in the damage parameterization, which determines the stress state memory. These questions will be addressed using a uniaxial compression setup (such as in Ringeisen et al., 2019) in future work."

*) 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.

>> The conclusions are now clearly differentiated between demonstrated results, speculated results and future work.

Specific comments:

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

>> This line was removed in the revised manuscript.

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.

>> Brittle is not a type of plasticity, but refers to a mode of fracture with little prior plastic deformation (see Crandall et al., 1978 for a reference book) before fracture. However, we agree that "brittle" should not be used in contrast to plasticity, as brittle materials can undergo plastic deformation after fracture (e.g. glass). This is corrected in the revised manuscript at L51-52:

"The simulated stable ice arches in Dansereau et al. (2017) are located downstream of either Smith Sound or Kennedy channel (see orange curve in Fig 1). These locations differ from the observed ice arch positions in Nares Strait upstream of these channels (e.g., see Fig 1) or in the Lincoln Sea (Vincent, 2019), which are well reproduced by standard VP or EVP models (e.g., Dumont et al., 2008; Rasmussen et al., 2010)."

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

>> Corrected as suggested by the reviewer.

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.

>> Clarified as suggested by the reviewer. The revised text now reads:
"*we present our implementation of the MEB rheology on the FD numerical framework of the McGill VP sea ice model.*"

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.
>> The viscous term in the MEB model is always present with different relative magnitude, not only post-fracture. While the viscous deformation is very small before the fracture, it is present and slowly dissipates the elastic stress memory, stabilizing the model. This is clarified at L119-124 and L497-498 in the revised manuscript. For example, using the model without the damage parameterization, a sustained internal stress of 50kN/m induces a viscous creep deformation of order 10^{-5}.

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

>> This was rephrased, as suggested by the reviewer. Note however that a brittle material is defined as a material that breaks with little prior elastic deformation and without significant plastic deformation. In the MEB model, the development of the fracture is not instantaneous and the damage increases over several time steps during which the deformations progress but not the stress state. As such, as in the VP model, the development of brittle fractures in the MEB model is parameterized as a plastic deformation. The models differ in the deformation rule (a flow rule is used in the VP model, while the stress-strain relation remains visco-elastic in the MEB) and in the post-fracture deformations. We add these clarifications at L203-218 in the revised manuscript.

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.
>> We removed self-repetition in this section but did keep some material included in earlier work for the sake of completeness and for the general reader. We also added clarifications on the strain hardening statement, that relates to the use of vertically integrated equations rather than to an actual hardening of the ice material, which is not parameterized in the model.

L207: Replace lowercase nphi with uppercase nPhi (as well as throughout the rest of the text I believe)

>> Corrected as suggested by the reviewer.

L223: Missing unit for nlambda_0

>> The units (s) were added, as suggested by the reviewer.

L224: I don’t think it’s true that for high enough nlambda_0 MEB becomes EB. At any rate, Bouillon and Rampal (2015) and Rampal et al., (2016) are the wrong references for such a statement.

>> The reference has been corrected, as suggested by the reviewer. This can be seen in a simple scale analysis. In the limit where λ_0 tends to infinity, the viscous relaxation term tends to zero, which makes the system of equations reduce to that of the EB model. This is, for example, mostly the case in landfast ice, where $\lambda = \lambda_0 = 10^5$, making the viscous term orders of magnitude smaller than other terms. In damaged ice, however, λ is reduced by 8-9 orders of magnitude such that the viscous term becomes important unless an unrealistically high λ_0 is used.

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.

>> As we specified above, while we agree that the model description could be shortened and make reference to previous work, we decided to present a detailed (stand-alone) description of the model, including details that are often trivial to engineers but less so for the climate community. Our goal is to make the model physics more accessible to the broader community such that improvement in future GCM relates to model physics (e.g. EAP, MEB, VP with Mohr-Coulomb and dilatation, Elastic

Cohesive, etc.), not just the numerics.

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.

>> This was clarified at L220-225 of the revised manuscript, which now reads:

"*… and presents a significant change from previous implementations that use Finite Element methods with a triangular mesh (Rampal et al., 2016, Dansereau et al., 2016} and/or Lagrangian advection scheme (Rampal et al., 2016)}*."

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.

>> The difference is mainly in the IMplicit-EXplicit treatment of the ice thickness, concentration and damage within the non-linear iterative solver. In Dansereau 2016, the set of equation is solved using (h,A,d) from the previous time-step. Here, we use the IMEX method for these variables, where the explicit equations for (h,A,d) are moved inside the outer loop, such that the solution correspond to a fully implicit solution. This is specified at L294-295 of the revised manuscript:

"This numerical scheme differs from that of Dansereau et al. (2017) who solve the equations using tracers (h, A, d) from the previous time level."

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

>> There was a duplicate of one figure in the manuscript that led to confusion in the automatic latex-referencing. We apologize for not noticing this before submission. We have corrected all remaining issues as proposed by the reviewer. Thanks for noticing this.

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.

>> We agree with the reviewer that this is inferred but not demonstrated in the paper. This is the subject of a future paper where we clarify this statement. This is clarified at L484-489. The comments about the flow rule are re-written to better reflect our conclusions at L481-483; the revised text now reads:

"The fact that different angles of internal friction yield the same fracture orientation (...) indicates that the orientation is not directly associated to the yield criterion in the MEB rheology (there is no flow rule in the MEB rheology)."

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.

>> This was corrected, as suggested by the reviewer. The intuitive weakening of cracked ice is already simulated by the damage parameter, which increases the effective stress resulting from a given forcing.

While it could be argued that the damage could impact the vertically integrated cohesion, it is misleading to state that this coupling between the damage parameter and the critical stress is justified from observations.

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

>> Corrected as suggested by the reviewer.

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.
>> Based on this and other reviewers' comments, we opted to use the vertically integrated yield criterion. This solves the issue discussed here and in the original manuscript, as it was mainly the consequence of using vertically integrated stress but a non-integrated yield criterion. That being said, we are not aware of a physical motivation for the inclusion of the pressure term in the momentum equation as in Rampal et al., 2016. It is also explicitly specified in their paper that this term is used to prevent excessively large ridges, therefore is included for numerical reasons, not for physical reasons.

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.

>> We agree with the reviewer. We have now created a new section 4 where we collated the text related to the error analysis.

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.

>> Corrected as suggested by the reviewer.

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.

>> We agree with the reviewer. This has been rephrased in the revised manuscript.

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

>> We do not claim to demonstrate it here, but explain that it is not possible to quantify it in non-symmetric simulations. The wording is modified to better reflect this in the conclusions of the revised manuscript.

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

>> This sentence states this as a "possible solution". We removed this suggestion and leave it for future

work in the revised manuscript.

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.

>> We have removed the bullet-point recommendations as they are repeating the previous text. However, these conclusions are not only meaningful in our model setup; they directly relate to the damage parameterization itself. For instance, we clearly show a mathematical instability in the damage parameterization equations, which are the basis of the MEB model. There are no reasons to believe that this instability is absent in other implementations, unless some undocumented dissipating factors are used.

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

>> This has been rephrased to focus on the need to mitigate the instabilities rather than giving a specific tolerance criterion. As stated above, there is no reason to believe that a mathematical instability would not be present in other simulations, unless a different stress correction scheme is used.

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

>> As stated in earlier comments, we are now using the vertically integrated cohesion in our model. This recommendation is now removed, and the vertically integrated cohesion is now part of the parameterization and should have been present in the first place.

Crandall, S. H., N. C. Dahl and T. J. Lardner, eds., An Introduction to the Mechanics of Solids, 2nd ed., McGraw-Hill, New York, 1978.

Dansereau, V., Weiss, J., Saramito, P., Lattes, P., and Coche, E.: A Maxwell-Elasto-Brittle rheology for sea ice modeling, Mercator Ocean Quarterly Newsletter, pp. 35–40, 2015.

Dansereau, V., Weiss, J., Saramito, P., and Lattes, P.: A Maxwell elasto-brittle rheology for sea ice modelling, The Cryosphere, 10, 1339–1359, https://doi.org/10.5194/tc-10-1339-2016, 2016.

Dansereau, V.,Weiss, J., Saramito, P., Lattes, P., and Coche, E.: Ice bridges and ridges in the Maxwell-EB sea ice rheology, The Cryosphere, 11, 2033–2058, 2017.

Rampal, P., Bouillon, S., Ólason, E., and Morlighem, M.: neXtSIM: a new Lagrangian sea ice model, The Cryosphere Discussions, 9, 735 5885–5941, https://doi.org/10.5194/tcd-9-5885-2015, http://www.the-cryosphere-discuss.net/9/5885/2015/, 2015.

Rampal, P., Dansereau, V., Olason, E., Bouillon, S., Williams, T., and Samaké, A.: On the multi-fractal scaling properties of sea ice deformation, The Cryosphere Discussions, 2019, 1–45, https://doi.org/10.5194/tc-2018-290, https://www.the-cryosphere-discuss.net/tc-2018-290/, 2019.

Ringeisen, D., Losch, M., Tremblay, L. B., and Hutter, N.: Simulating intersection angles between conjugate faults in sea ice with different viscous–plastic rheologies, The Cryosphere, 13, 1167–1186, https://doi.org/10.5194/

Schulson, E., & Duval, P. (2009). Creep and Fracture of Ice. Cambridge: Cambridge University Press. Doi: 10.1017/CBO9780511581397

Weiss, J., Schulson, E. M., and Stern, H. L.: Sea ice rheology from in-situ, satellite and laboratory observations : Fracture and friction, Earth and Planetary Science Letters, 255, 1–8, https://doi.org/10.1016/j.epsl.2006.11.033, 2007.