Answers to tc-2019-210 RC1

February 15th, 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.*

The manuscript named “The material properties of ice bridges in the Maxwell Elasto-Brittle rheology” test the MEB sea-ice rheology in a traditional finite difference framework. The aim is to investigate the damage parameterisation. This is achieved with an idealized model setup of a channel that is narrow in the middle and wider in the two ends. I think that this is a very relevant to study the damage parameterisation as this (at least in my opinion) is important for new developments within sea-ice dynamics. The manuscript is in general well written and therefore fairly easy to read.

I could wish for a better organization of the figures. In addition, the result section would be much easier to read if the figures were numbered in the order they are referenced. At last I think that the simplifications made in terms of zero ocean currents/sea surface tilt may have bigger impact, especially in some of the examples mentioned within the Canadian archipelago, where tides are significant. This is not necessary to include within this study but a better discussion of the limitation would be nice.

The results in general seems less impressive than other studies using the MEB rheology. A discussion of the performance of these would also be nice. Many of these points are mentioned but I think that it would be beneficial to collect these in a discussion and maybe spend a few more words.

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

Major revisions:

I think that the focus of the abstract is a bit off and I would like this to be revised. It is not that important that the framework of the sea-ice model originally was build for VP dynamics as this is not mentioned in the manuscript. The eulerian/lagrangian implementation is more relevant. I would like the abstract to include line 77-82 as these
fit well into a summary/abstract and less into the introduction. Corrected as suggested by the reviewer

>> As suggested by the reviewer, the abstract was re-written to better reflect the manuscript's content.

We have kept however the sentence about the implementation of the MEB model on the Eulerian, finite differentiation framework, because this allows for a direct comparison with other models commonly used in GCMs. This is not a trivial task and many have tried without success. This is considered a significant contribution for the ice modeling community that is worth reporting in the abstract. Instead, we have put more emphasis on this in the body of the paper to justify its inclusion in the abstract.

Discussions are scattered around in the manuscript. I would like a collected discussion.

>> We added a new section where we collected our discussions on the damage parameterization, its instabilities and the orientation of the lines of fractures.

- What improvements/limitations are there when the framework moves from a Lagrangian to an Eulerian approach.

>> The limitations of moving from a Lagrangian to a Eulerian framework is mainly linked with the advection of tracers within the model. A Lagrangian approach allows to follow deforming elements within the domain resulting in low numerical diffusion. The disadvantage of this approach is that one must recalculate a new grid periodically (called regridding) when the elements are too distorted, requiring the use of interpolation techniques that leads to diffusion of resolved features and requiring a significant amount of CPU time. In a Eulerian framework, higher order advection schemes are typically used (but not here) to limit numerical diffusion and the model grid remains the same throughout the full integration, resulting in lower CPU cost. Note also that the use of a Eulerian scheme is not novel here: the MEB model was originally implemented in a Eulerian scheme in Dansereau et al., 2016, although using Finite Element Methods.

The goal here is not so much about model improvements (or limitations), but rather about advantages of having two different models on the same platform. This has been clarified at L221-226 in the revised manuscript. Namely, the advantages of coding the MEB model using a Eulerian, finite differentiation framework is that it allows for a direct comparison of the MEB model physics (rheology, yield curve, deformations) with that of the standard viscous plastic approach (or variation thereof) used in the vast majority of GCMs and coupled ice-ocean models, independently of the differences in the numerical framework (i.e. Lagrangian vs Eulerian advection scheme, regridding, the use of Finite Element Methods and triagular mesh). The caveats of using the Finite Difference scheme is also discussed in section 2.3.2, at L267-283.

The study limits the effects of the ocean (eg. tides) by neglecting it. For an idealized study like this it is fine just to look at the wind. But in terms of comparisons with real data then this restricts the value og the study. Tides are very high from Kanes Basin and southwards. This is an important factor when the stability of the last fast ice is to be considered and compared with real life. This is briefly mentioned on line 500, however I would like a bigger discussion of this.

>> We agree with the reviewer that the tides and thermal stresses are important in the landfast ice break up process. Here, we consider our “wind forcing” to be representative of the combined surface forces on the ice from both winds and ocean currents including tides, which vary depending on the location. This was clarified in the revised manuscript by re-writing the surface forcing into a landfast ice forcing term independent of the ice velocity and a water drag term that is only significant in drifting ice. As such, the forcing imposed on the ice bridge is no longer assumed to originate from the winds. The simulations are now discussed in terms of forcing values, rather than wind magnitude. Note that our ideal simulations and cohesion estimates are not sensitive to the source of the forcing, only to its

magnitude and direction. This is now clarified at L85-L96. The wind forcing and surface current values associated to the forcing used to derive the material properties of sea ice is also indicated at L410-411. We believe that a lengthy discussion on these factors is outside of the scope of this paper, which is concerned with the simulation of ice arches in the MEB framework and the influence of the material properties in their formation and stability. We also clarified the scope of the study in the introduction so as to not create expectations that are not met in the body of the paper.

Figures are very inconsistent when labeling. These should be changed. I have suggested updates to almost all of the figures. These comments are in the minor correction/technical correction part.

>> There was a duplicated figure in the manuscript that led to confusion in the automatic latex-referencing. This addressed several of the comments raised by the reviewer. We apologize for not noticing this before submission. We have corrected all other issues as proposed by the reviewer.

Minor details

Line 2- Please revise sentence. An example is provided: The effect of the material parameters on ice arches in a numerical framework that includes both the Maxwell Elasto-Brittle (MEB) including a damage parameterization and the Viscous-Plastic (VP) dynamics.

>> The abstract was completely rewritten.

Following lines after line 2: I assume that this is MEB but it is a little unclear

>> The abstract was rewritten.

Line 20 I think that this should be reformulated. For instance, ice keel don't protect sea-ice from forcing. It creates a friction that resist the forcing. I would reformulate this

>> This was reformulated as suggested by the reviewer, and now reads:

Typically, large landfast ice areas can form and remain stable due to the presence of islands or by the grounding of ice keels on the ocean floor.

Line 35 Ice thickness anomaly is this in time or space? I guess that the influencing factor is the current ice thickness, spatial variation (anomaly).

>> This was referring to ice thickness anomalies from year to year. This is clarified in the revised manuscript and now reads:

A variety of studies suggest that the inter-annual variability in presence or absence of ice arches in given locations are influenced by several factors, such as ice thickness anomalies [...]"

Line 47: replace new rheology with new rheologies

>> Corrected as suggested by the reviewer.

Line 67. References to figures in other articles makes it hard to read. Please either add the location on figure 1 or add a map where this can be shown.

>> The locations were added in Figure 1, as suggested by the reviewer.

Line 77 to 82: This part would be well suited for conclusions and/or abstract. The introduction should be more overview of previous studies and an overview of what will be presented. Not results.

>> It is correct that such statements are usually only included in the abstract or conclusions. However, including those at the end of the introduction situates the reader up front and allow him/her to focus on "how we arrived at those conclusions". It is a style adopted by many authors both in oral presentations and written papers, and recommended in the book by Joshua Schimel "how to write papers that get cited and proposals that gets funded". We believe it leads to a more active form of presentation that is more engaging for the reader. For this reason, we have opted to leave it in the introduction.

Line 95: Nares Strait has strong tides in the part near Baffin Bay, thus the ocean currents would most likely be an important factor especially before and probably after the fast ice region has formed. Therefore, this should be mentioned in the discussion.

>> As we state in response to the general comments above, the simulations are not sensitive to the source of the forcing, only to the total magnitude. We also rephrase the results so that we refer to the forcing magnitude, rather to the wind speed throughout the revised manuscript. Note also that the regions of high tidal forcing downstream of Nares Strait is rarely landfast (see Hannah et al., 2009, Vincent 2019). This is also clarified at L413-414:

"Note that higher forcing may be frequent in areas associated with strong tides, although these locations correspond to unstable landfast ice areas and recurrent polynyas (Hannah et al., 2009)."

Coriolis is only zero when the ice is not moving.

>> We are mainly concerned with the loading of landfast ice until the break-up and in the derivation of constraints on the mechanical properties of landfast sea ice. It is correct that the subsequent motion after break-up will have small errors (of the order of 10%, Turnbull et al. 2017) given that ice is relatively thin and that the Coriolis term scales with ice thickness. This is clarified on L80-81 of the revised manuscript:

"We assume the ocean to be zero and ignore the Coriolis term, as it is identically zero for immobile ice. These assumptions are appropriate for landfast ice, but could result in small errors in drifting ice (Turnbull et al., 2017)."

The discussion of the influence of the ocean is too small.

>> See comment above. The analysis was re-written such that the forcing used on the ice bridge is not exclusively coming from the atmosphere, but can also originate from the ocean. This is clarified at L85-96 in the revised manuscript.

Equation 6 ":" in the equation?. This is described in equation 8. This should
be moved here (first place that it is used)

>> Corrected as suggested by the reviewer.

Line 119 lhs and rhs should be written without using a abbreviation.
>> Corrected as suggested by the reviewer.

Line 120- 124. These sentences are a bit hard to follow. Please revise.
>> These sentences were clarified, as suggested by the reviewer. They now read:

Eq. 6 indicates that in the visco-elastic regime (before fracture), the deformations are dominated by a fast and reversible elastic response (first term on the left hand side of Eq. 6), with a slow viscous dissipation acting over longer timescales (second term on the left hand side). The reversibility of the elastic deformations implies that the elastic strains return to zero if all loads are removed. This results from a memory of the previous elastic stress and strain states given by the time-derivative in Eq. 6. The Maxwell viscosity term, although orders of magnitude lower that the other terms in the visco-elastic regime, leads to a slow viscous dissipation of this elastic stress memory over long timescales determined by λ.

Line 265 Figure 3 No need to show a ARAKAWA grid. This is a standard. I would remove the figure.

>> This figure is removed, as suggested by the reviewer.

Line 317 Remove "-" 2 times

>> Corrected as suggested by the reviewer.

Line 404 In short the physical solution did not converge until the tolerance is lower than 10ˆ-10. How many iterations are required? Is this important for the computational time (how important?).

>> The errors are not due to a difficulty in solving the equations, but rather to the fact that the residual errors are accumulating in the memory terms (instability). The MEB rheology actually converges rapidly, especially given the small time step required by the CFL criterion. The convergence is most time reached within 6-8 outer-loop iterations (fgmres converges in only 1 iteration, given the very small changes in the solution in 0.5s.). This is clarified at L496-500 in the revised manuscript.

Also, using a very low residual tolerance does not solve the problem. This is now better illustrated in Fig. 15 in the revised manuscript. Note that the small timestep, however, is a burden in terms of total time of integration, especially if longer-term simulations are needed. Using a low tolerance increases that burden. For example, on a standard computer (Quad Core Intel Xeon E5-1630 v3, L2 cache of 10.0 MiB with a RAM of 62.63 GiB), the 10h simulations are completed in 4h30 when using a tolerance of 10^-10, 2h30 when using a tolerance of 10^-4, and 1h30 when using the VP model and a time step of 10min, a tolerance of 10^-3 and a maximum of 500 outer loop iterations.

Line 447 Nature is a bit more complex than just wind. Orography ocean currents etc. also play a role, thus values like the cohesion of sea ice should be smaller than 21KN/m seems to be a very rough estimate based on parts of the momentum equation. Admitted wind is normally the dominant factor along with the resistance (internal
strength)

>> As stated in general comments above, we agree with the reviewer that the tides and thermal stresses are important in the landfast ice break up process. In our ideal simulations, we consider our forcing to

be representative of the combined surface forces on the ice, which vary depending on the location. This was clarified at L85-96 and throughout the analysis. The forcing used to derive our cohesion estimates (0.15 N/m^2) is consistent with a typical forcing on landfast ice (10m/s winds or 0.15m/s current). This is now clarified at L407-408. We also changed the wording throughout the text to discuss the simulation in terms of surface stresses rather than wind magnitude.

Line 473 How does this compare with results from other MEB implementations.

>> To our knowledge, there are only 2 other implementations of the MEB model: The model of Dansereau et al., (2016, 2017) and NeXtSIM (Rampal et al., 2016, 2019). The angles of fracture and type of deformation associated with the damage have not been investigated in details in those studies, but was recently investigated in Dansereau et al. 2019, who demonstrated that the orientation of the lines of fracture does not follow those predicted by the Mohr Coulomb theory. Our findings are consistent to this assessment. These clarifications are included in section 4 in the revised manuscript.

Line 520: is the ":" suppose to be there?

>> Removed, as suggested.

Line 528: It would be very interesting to include this in a VP/EVP model.

>> We agree. This is something that we are currently working on.

Figure 5 text. Top panel? I can only see one panel in figure 5.

>> Corrected as suggested by the reviewer.

Figure 6. This figure should be labeled a through d instead of a1 2 3 b

>> The numbers are referring to the points in Fig 5, which correspond to the damage fields. The figure label was clarified.

Figure 7 Same as 7, Which points?

>> This error in the label is corrected as suggested by the reviewer.

Figure 8: I would say colored dashed lines

>> This error in the label is corrected as suggested by the reviewer.

Figure 10 Dots are very hard to see. It would be nice to increase the size of these.

>> This figure was removed.

Figure 11: Which colored lines? They are defined in figure 8. Are they the same?

>> Removed, as suggested by the reviewer. There are no colored lines.

Figure 12: I would still label these a, b,c and d. Then add to the text.

>> This figure was removed.

Figure 13 which colored lines? Are they the same?

>> This label error is corrected as suggested by the reviewer.

Figure 14. Please use a,b,c: : : References to the residual tolerance are not very easy. Left 10ˆ-6 and right 10ˆ-10 does not make sense.

>> Corrected, as suggested by the reviewer.

Figure 17 Arrows are very hard to see.

>> Corrected, as suggested by the reviewer.

Figures in general should be in order of them being mentioned in the text. For instance the result section seem to jump back and forth. I assume that when done with the review process they need to be inserted at appropriated places.

>> As state above, there was a duplicate figure in the manuscript. Removing this figure has resolved this issue. We also removed Figure 10 from the submitted manuscript, which ease the figure flow.

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.

Hannah, C.G., Dupont, F., Dunphy, M., 2008b. Polynyas and tidal currents in the Canadian arctic archipelago. Arctic 62, 83–95

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.

Turnbull, I. D., Torbati, R. Z., and Taylor, R. S. (2017), Relative influences of the metocean forcings on the drifting ice pack and estimation of internal ice stress gradients in the Labrador Sea, J. Geophys. Res. Oceans, 122, 5970– 5997, doi:10.1002/2017JC012805.

Vincent, R.F. A Study of the North Water Polynya Ice Arch using Four Decades of Satellite Data. Sci Rep 9, 20278 (2019). https://doi.org/10.1038/s41598-019-56780-6