Answers to tc-2019-210 RC2

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.*

This manuscript presents an implementation of the recently developed Maxwell Elasto-Brittle rheology for sea ice mechanics within a finite-difference scheme, and the realization of idealized simulations of ice deformation and motion within a channel. This work is quite similar with what was done in [1], however with several differences:

- A finite-difference, instead of a finite-element, numerical scheme is used - the numerical implementation of the MEB rheology is performed exploiting the code framework of a standard VP code.
- the initial condition is a purely homogeneous ice plate (constant thickness, constant elastic and strength properties), without any sort of initial disorder implemented

The effects of material properties and model parameters are then analyzed from a sensitivity analysis using strictly symmetric boundary conditions and geometry.

Overall, this manuscript is clearly written. Most of the results are consistent with [1], proving the robustness of the MEB rheology to adequately simulate sea ice damage, fracture, or strain localization. I am therefore rather favorable to a publication in The Cryosphere. There are however several mistakes, misunderstandings, or points to discuss more thoroughly (e.g., the flow rule and its relationship to the angle of fractures), which should be fixed before final acceptance. They are listed below:

We thank the referees, Dr. Weiss and Dr. Dansereau, for their thorough review of the manuscript and constructive comments.

1) Title: “The material properties of ice bridges…”. This is strange, and unclear: bridges do not have “material properties”. Maybe the authors wanted do say: “The effect of material properties on the simulation of ice bridges in the ..” ?

>> We agree. The title was rephrased as:
"Landfast sea-ice material properties derived from ice bridge simulations using the Maxwell Elasto-Brittle rheology"

2) p3, L65-73, as well as p14, L390-395: The authors argue at different places about the position of the ice bridges, up- or down-wind the channels. This argumentation is not very clear:

- to what extent the situation shown on fig. 1b is systematic ? To the best of my knowledge, ice bridges can take place at different positions along channels (as actually shown on fig. 1b), including along Nares strait (see e.g. fig. 1 of [1]), and I do not know a systematic, statistical analysis of that (but would be happy to learn about such analysis, if any).

>> On L46-53 (in the revised manuscript), we refer to the fact that ice bridges simulated by the MEB model are mostly located downstream of narrow channels, rather than upstream (i.e. as seen in Figs 4,5,7 and 9 in Dansereau et al. 2017). We do not refer to a single specific location. In the Nares strait, the (stable) ice arch does have preferred locations. Fig. 1 from Dansereau et al., 2017 shows some of these positions, usually either in the Lincoln sea (Fig. 1a and b) or in Kane basin (Fig. 1c). These positions can be seen in several landfast ice cover assessments (see for instance Tivy et al 2011, Galley et al. 2012, Yu et al 2013), and are also recently fully described in Vincent (2019).

- in [1], ice breakup occurs in several successive steps, at different locations along the Nares strait. In conclusion, I am not sure that the unique observation of fig 1b can serve as a severe constrain on model parameterizations.

>> We agree with this statement. The break-up of landfast ice is a rapid process during which the temporary ice edges are highly dependent on the pre-existing ice deformations. We cannot reproduce this in our idealized simulation. Here, we rather refer to the formation of stable ice bridges, which are less influenced by these factors. Figure 1 shows such an ice arch in Kane Basin, which remained in place from early March 2018 to mid-June 2018. We have clarified this in the revised manuscript at L46-53, L470-476 and in the figure caption. We also specify that the aim of the paper is the necessary conditions for the simulation of stable ice arches rather than the temporary ice arches formed in the process of landfast ice break-up. In Dansereau et al. 2017, the location of the stable ice arch (i.e. in Figs 4,5,7 and 9) are located downstream of the narrow channels, seldom seen in the Nares Strait (see Vincent 2019).

3) L92-93. Indeed, at the timescales involved in classical climate models (time step of several hours, as in the papers cited), the advection term can be neglected. This might not be true when considering a much smaller model time step (~s). What is the time step of the present simulations? Have you done a proper scale analysis taking that time step into account?

>> The time step used in all simulations is 0.5s (now specified at L189 of the revised manuscript and corrected in Table 2). In such a small time scale, the advection term is many orders of magnitude lower than the inertial term (the inertial term scales as 1/T, T being the time scale). The advection term can however become important at small length scales. In drift ice, it scales as $\rho_i h_i U^2/L$ □ 10^{-3} N/m^2 , where ρ_i (~900kg/m^3) is the ice density, h_i (~1m) is the ice thickness, L (~2-10 km) is the space resolution, U ~0.1m/s is a typical ice velocity. This is three orders of magnitude smaller than a characteristic surface wind or ocean stress. At the edge of an ice arch, where a discontinuity in sea ice drift is present at small scales (2 km in our case), it remains two orders of magnitude smaller than other terms in the momentum equation. This has been clarified in L80-85 of the revised manuscript.

4) In section 2.1, ice thickening through mechanical redistribution when A=1 is not considered. Was such redistribution scheme implemented? If not, this would be a problem, as such scheme, even very simple, and coupled to the MEB rheology, was found to generate realistic ITD [1]. This would therefore likely affect all the discussion about ice ridges throughout the entire manuscript. If such scheme was implemented, please detail.

>> Mechanical redistribution is taken into account in our simple 1-category model (i.e. ice or open water). When A=1 and sea ice convergence occurs, the mean ice thickness increase (see continuity Eq. 4 in the manuscript), but since A=1 is capped at one, this leads to the actual thickness of ice in a grid cell (h/A) to increase, i.e. ridging. A simple 1-category model does not resolve the ITD per se, unless the variability in ice thickness is resolved (i.e. unless the model is run at O(1m) resolution, at which sea ice no longer behaves as a 2D material.

5) Equation (1): C is not defined here, but much later (eq. (9)). Please modify.

>> Corrected as suggested by reviewer.

6) L125-127: I would not agree and there seems to be a misunderstanding here: the elasto-brittle component of the MEB rheology is, by construction, associated with small (and reversible) deformations, while the Maxwell component deals with large (and irreversible) deformations. This is therefore fundamentally different from the VP model where the plastic regime is associated with irreversible strains but strain-rate independent stresses (while the Maxwell component of the MEB model is indeed strain-rate strengthening and not strain-rate independent). Please justify in physically-sound way or remove that sentence.

>> These lines were re-written to clarify this statement. As the reviewer points out, the small deformations in the EB component are elastic or reversible, and they are not in the VP model. Here we make the observation that during the fracture process, the larger (and partly still elastic) deformations + viscous dissipation associated with the damage are analog to the plastic regime in the VP model. When a fracture is developing, the stress state is kept on the critical yield curve while the strain rates and damage increase, and the Elastic stiffness and viscosity decrease. In the VP model, the non-linear bulk and shear viscous coefficients reduce with increasing strain rates, such that the stress states (the product of the two) remain on the yield curve. Thus, in both models, the stress state is independent of the deformation rates during the fracturing. The two models do differ, as stated by the reviewer, post fracturing as the VP model does not have a memory of past deformations other than via the continuity equation and its impact on the ice thickness and concentration. In the MEB, the post-fracture elastic deformation remains important unless the damage is large (d>0.8), while deformations are viscous in fully developed fractures. The damage corresponds to a material memory of past deformation. The text in section 2.2 was heavily re-written to clarify this physics, at L119-124 and L204-219.

7) L134-137 and L222: At least in the case of (Tabata, 1955) and (Weeks and Assur, 1967), these authors discuss the creep of bulk saline ice, driven by viscoplasticity at the crystal scale. The concept introduced in the MEB rheology is fundamentally different: a linear viscous term is introduced to account for the cataclastic flow of highly damaged ice, and associated stress relaxations.

>> Here we were referring to the dissipative effect of the viscous term in undamaged ice. We agree that this effect is negligible in terms of deformation in the landfast ice (for example, a sustained stress of 50kN/m in our model results in a viscous creep deformation of the order of 10^{-5}), but is significant in

dissipating the elastic stress memory over a long time scale. This is clarified at L119-124 in the revised manuscript.

8) L153-154: This statement is wrong: plane stresses were considered in Dansereau et al., 2015, 2016, 2017 and any implementation of the MEB rheology. This is indeed the correct assumption for thin plates. Note however that the impact of such assumption (plane stress vs plane strain) has little consequence on the global behavior. Note also that the constitutive equation present in the early newsletter Dansereau et al., 2015 is that of the generic Maxwell model.

>> The paper of Dansereau et al. (2016) have a factor of 1/(1+v)(1-2v) in the stress-strain relationship, indicating that the authors have used the plane-strain assumption. Dansereau et al. (2017) however do use the plane stress assumption, as pointed out by the reviewer. We now cite the reference Dansereau et al., (2016) in the revised manuscript rather then (2015) in the original version. We state that Dansereau et al., (2017) do use the plane stress assumption for completeness.

9) Section 2.2.2. The authors propose to close the damage envelope towards large compressive stresses using eq. (11). This is another difference with the initial MEB model [2]. In principle, I would say “Why not ?”. However, several questions arise:

- What is the physical justification of such closure? At the lab scale, the failure envelope of columnar ice loaded under biaxial stresses is indeed closed towards large biaxial stresses (see e.g. [3]). However, the shape of the closure is significantly different from the one proposed on Fig 2 of this manuscript, and failure under such high confinement occurs through spalling, a failure mechanism that is not observed, to my knowledge, in the field (although out-of-plane failure mechanisms might be related). In addition, internal sea ice stresses recorded in the field never reach such strongly confined biaxial stresses, see e.g. [4].

>> See answer below.

Therefore, a second question arises:

- What can be effect of such closure on the model outputs? I would suggest the authors to compare simulations performed with and without this closure to analyze this point. If the effect is limited, as I suspect from above, then the introduction of such weakly-justified closure would represent an unnecessary complication. If some impact is observed on the formation of ice arches and/or ridges, such sensitivity analysis would be useful to understand its origin.

>> Our concern is not the bi-axial compression state (since, as stated by the reviewer, it is rarely observed) but rather the uni-axial compression which can lead to large compressive stresses – i.e. larger than the mechanical strength of sea ice. This is why we argue for the use of a capping in compressive stress: to limit the uniaxial compression. This is specified at L151 in the revised manuscript. This has the side benefit of improving the numerical stability of the model as discussed in section 4.2 in the revised manuscript. The compression capping does influence the simulation results, as discussed in details in section 3.2.4 of the original manuscript (3.1.2 in the revised manuscript): it can cause uni-axial failure along the upstream coastlines, instead of lines of shear fracture propagating at an angle from the island corners, as in the control run simulation.

10) L180-184, as well as L393. About “the lack of strain hardening in the MEB model leads to non-physical results in convergence with the absence of ridge propagation in the direction parallel to the second principal strain (maximum axial compressive strain).“ Here, the authors reference (Richter-Menge et al., 2002) on the subject of strain-hardening observed in sea ice. (Richter-Menge et al., 2002)

themselves refer to the parameterization of strain-hardening of Hibler (1979), where the maximum compressive strength of the ice is proportional to its thickness P = P*h *exp(-C(1-A)). The same proportionality is actually used in the MEB (and EB) rheology. Indeed, instead of writing E x d, η x d, σ_c x d, for the strength parameters in the constitutive equation, and writing the constitutive and momentum equations in terms of a vertically integrated stress, Dansereau et al., 2017, Bouillon et al., 2015, Rampal et al., 2016, Rampal et al., 2019, etc. all used stress, instead of the vertically integrated stress, and write the rheology term in the constitutive equation as div(hσ). This discussion about "strain-hardening" should be reconsidered in light of this.

>> This is a good point. The lack of strain hardening in our model is related to the fact that we used the vertically integrated stress definition (div(sigma) and not div(h*sigma) as in other MEB implementations), so that we keep the same numerical/model platform as our standard VP model. It the original submission, we did not adapt the yield criterion accordingly. We now include the thickness dependency in the cohesion (and compressive strength): i.e. c = c0*h *exp(-C(1-A)). This is needed for the set of equation (momentum, stress-strain relation and yield criterion) to be equivalent to the previous MEB model implementations. These clarifications are now added in the model description, in section 2. As expected, using the vertically integrated material parameters does not change the model behavior except for the strain hardening associated to increasing thickness now occurring upstream of the channel, and for a reduced stability of the model (the higher cohesion cause higher compressive stresses and increase the instability issue discussed in the paper). We modified the discussion accordingly, and the comment on strain hardening is removed. We also specify that longer time integration is required for the formation of an ice arch upstream of the channel.

In addition, strain-hardening as the result of damage is not supported by experiments on brittle or quasi-brittle materials. A classical illustration is known as the Kaiser stress-memory effect: If a material is damaged up to a given stress, unloaded, and then reloaded, damage will start again when the previous stress will be reached again (e.g., Heap 2009). In case of sea ice, and particularly in the context of ice/structures interactions, the strengthening of crushed, and then recrystallized, ice has been discussed in the literature (e.g. [5]). This process however involves various mechanisms such as sintering of crushed grains, refreezing, which are not only mechanically driven. Consequently, a change in critical stress when the material fail remains to be observed, proven or disproven in the case of sea ice, at the geophysical scale, before formulating physical parameterizations for it.

>> We agree with the reviewer. The strain hardening in our simulations is because of the h-dependency of the material strength parameters (i.e., a thickening of the ice increases the vertically integrated material strength). It is related to the use of vertically integrated stress, and not to the hardening of the ice material itself. The comment on strain hardening was removed from the analysis.

11) Section 2.2.3, and L224-225 "Note that if λ0 is sufficiently high, the MEB rheology reduces to the Elasto-Brittle rheology (Bouillon and Rampal, 2015; Rampal et al., 2015)." For the EB rheology, cite [6] rightly instead.

>> Corrected as suggested by the reviewer

12) Section 3.2.2, and L534-539. About the flow rule and Mohr-Coulomb failure criterion: "In the MEB model, the angle of fracture does not follow the theory. We speculate that the deviations are related to the absence of a flow rule linking the deformations to the yield curve and the angle of internal friction." This is confusing. Fracture occurs in an undamaged or partially damaged material.

The material "flows", or undergoes large deformation, once fractured. Therefore, why is the flow law determining the angle of the fractures that precedes the flow? Please explain the mechanism behind this.

>> Here, we use the term "fracturing" to represent the development of damage: in the MEB model, the development of the fracture is not instantaneous, and damage increases over several time steps as the deformations progress. As such, the locally increasing deformation influences the surrounding strain orientation. We speculate that this influences the stress concentration associated with the fracture that leads to yielding in neighboring cells (see Dansereau et al.,2019). The ice arch and fracture lines are a result of this propagation of local damage in space. If the orientation of the deformation rate tensor was associated with the yield criterion during this process, we speculate that the lines of fracture would follow the Mohr-Coulomb theory, as observed in other models using a flow rule (see Ringeisen et al., (2019) for instance). In the MEB model, they are not and the fracture line orientation does follow the Mohr-Coulomb theory. This result is consistent with those of Dansereau et al., (2019). We have clarified this in the new discussion section of the revised manuscript, at L477-489.

Second, please note that a flow rule is not required to close the system of equations in the case of the MEB (viscous-elastic-brittle) model.

>> We agree with this comment. The point made here is that the deformations during the development of damage might influence the orientation of the lines of fracture. This is consistent with Dansereau (2019), in which the lines of fractures are found to be determined by the stress concentration and the collective spreading of the damage along lines of damage instability.

Note also that the statements from lines 79-81 and 192 are contradictory (''We also show that the simple stress correction used in the damage parameterization corresponds to a flow rule'' "This correction does not correspond to a flow rule").

>> This was indeed not clear. We removed the 1st sentence, and kept the statement at L171-173 in the revised manuscript, i.e. that the stress correction path does not correspond to a flow-rule.

Note also that no flow rule has been determined for sea ice from in-situ observations, while the normal flow rule is not supported by lab-scale observations [4]. "In theory, the angle of internal friction governs the intersection angle between lines of fracture (Marko and Thomson, 1977; Pritchard, 1988; Wang, 2007; Ringeisen et al., 2019)": Recent and extensive work on the observation and modelling of the failure and localisation of deformation in brittle and granular materials (not just sea ice) have demonstrated that the relationship between the angle of internal friction and the intersection angle between conjugate faults is actually more subtle than predicted by the Anderson's theory of faulting: e.g., [7-12]. Initially, the Mohr-Coulomb criterion was not implemented in the MEB rheology (and similarly, the internal friction angle not tuned) in order to fit observations of conjugate faults angles in sea ice. It was rather chosen on the basis of stress measurements within sea ice (see [4]) that suggest a reasonably good fit to this criterion (see [2] on that point).
It has been recently shown that, for an elasto-brittle damageable solid, the fault orientation is not given by the Mohr-Coulomb criterion and Anderson's hypothesis, instead depends on various factors such as the nature of disorder, the Poisson's ratio, or the confinement [12]. It might be interesting in the future to better constrain the MEB parameterization on this basis, comparing simulation results with large-scale observations of leads/faults within the sea ice cover.

>> Thank you for this comment. We agree that there are many ways other than a normal flow rule to

constrain the orientation of deformations. Here, we point out that the damage parameterization should relax the elastic coefficients in a way that leads to a deformation field that is consistent with observations. This would constitute an improvement to the current damage parameterization.

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