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:

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 .." ?

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).
- 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.

2) 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?

3) 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.

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

5) 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.

6) 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.

7) 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.

8) 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]. 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.

9) 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 $E \times d, \eta \times d, \sigma_c \times d$ 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 $\nabla \cdot (h\sigma)$. This discussion about "strain-hardening" should be reconsidered in light of this.
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.

9) Section 2.2.3, and L224-225 "Note that if λ_0 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.

10) 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.

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. 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"). 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.

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