Answers to tc-2020-354 RC2

June 21st, 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.*

- Note that the exact pages and line numbers in our responses are subjected to change as the revised manuscript is being prepared.

Referee's Report on: A generalized stress correction scheme for the MEB rheology: impact on the fracture angles and deformations by Mathieu Plante and L. Bruno Tremblay

This manuscript describes a modification of the return algorithm for supercritical stresses in the Maxwell-Elasto-Brittle (MEB) model for sea ice. The stated purpose of this modification is to better match simulated and observed fracture angles, and to reduce numerical growth of errors over the course of the simulation. The modification is tested on uniaxial deformation of a rectangular patch of sea ice. The modifications provide improvement over the previous approach but do not yet quite match observation. Overall, the goal and methods are clearly stated, although some notation is sloppy.

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

Trying to adjust the return algorithm to influence the failure angle is rather an indirect course of action. There does not appear to be a direct prediction of failure angle, just a demonstration through a full numerical simulation. It would be good to emphasize/clarify this point in the text (if it is in fact true), or explain how to predict the failure angle (if it is not true).

>> It is correct that we do not use the return algorithm to prescribe the fracture angle. The original goal of the study was to reduce the integration errors in the MEB rheology and study the sensitivity of the model to the return algorithm, given that the exact path along which the super-critical stresses should be returned to the yield curve is not known a priori. The fact that the fracture angle is in better agreement with observations when we use an algorithm that minimizes the error growth is a by-product of this original goal. This is clarified in the abstract at L3-5, in the introduction at L60-65 and in the discussion at L311-312 in the revised manuscript.

A more direct approach to prescribe the fracture angle would be to introduce a decohesive strain when damage increases, in the manner similar to the fracture algorithm of Schreyer et al. (2006), or Sulski and Peterson (2011). This was not included in the present parameterisation, as it would represent a significant modification of the MEB rheology, but will be considered for future model developments. These precisions added in a paragraph that we add at the end of section 2.4 in the revised manuscript.

One aspect that is lacking in the presentation is the behavior of the numerical algorithm when the mesh size is changed. Fracture models are notorious for illposedness and it would be good to illustrate that this model's predictions do not depend on the mesh size. It is also common for the failure angle to depend on the mesh aspect ratio. Both mesh refinement and aspect ratio need to be explored.

>> A more complete study of the sensitivity of the model to spatial resolution is the subject of another paper in preparation (to be submitted in the Fall). Preliminary results using a simple shear flow in a 1D channel show that the "boundary layer", or spatial scale *l* where damage occurs decreases when the spatial resolution is increased, while the number of grid points required to resolve the "boundary layer" increases. This opens the door to a series of new questions that we prefer to keep in a separate paper.

In the context of this study, we did simple uniaxial loading experiments with different spatial resolution and sample aspect ratio. We find that the simulated angle of fracture, the growth of numerical errors and the dependency of the fracture angle on the correction path are robust to the exact choice of model resolution and to the ice sample aspect ratio. This is now included in the discussion in the revised manuscript by broadening the scope of its last paragraph, previously dedicated to heterogeneity (L339-344).

We did not test the sensitivity of the results to the mesh aspect ratio. This would require significant modifications to the McGill Sea Ice Model. The code is also written using a Cartesian coordinate system and it is customary to keep dx equal to dy in such models.

I have some additional questions, comments, and suggested improvements.

1. Abstract: The VP model does not include fracture.

>> We rephrased the problematic sentence in the abstract from: "*The post-fracture deformations are shown to be dissociated from the fracture process itself, an important difference with classical Viscous Plastic (VP) models.", to:*

"The post-fracture viscous deformations in the MEB model are shown to be dissociated from the fracture process itself, an important difference with classical Viscous Plastic (VP) models where plastic deformations are uniquely defined from the state of stress on the fracture plane and a flow rule".

We also note that while the VP models do not resolve brittle fractures and the LKFs are not pre-conditioned by discontinuities in material properties, they do represent ductile fractures with simultaneous deformations that are determined by the yield stress as governed by plastic laws.

2. Page 3: You are using an Eulerian grid but I don't see equations that show advection of parameters (eg damage parameters). Are you assuming small deformations only? (See equation 12, for example.)

>> We do advect the ice thickness and concentration parameters, but neglect the advection of damage, given that the fracture process occurs in a timescale (seconds) much shorter that the advection timescale (hours). The advection of damage should be included in longer-term integration of the MEB model. Adding the advection of damage does not change the results and conclusions presented in this paper but it increases the localisation of the ice fractures. This results in higher damage values that in turn increases the rate of ridging. This has been clarified in section 2.4, L130-131 in the revised manuscript.

3. Page 4: Equation 5. Is a superposed dot the same as a partial time derivative or a material time derivative? Is this rate equation objective? Is ice deformation really rate dependent? Are there experiments about that?

>> The superposed dot is a partial time derivative. In the case of damaged ice, the large-scale sea ice deformations (especially ridging) are traditionally seen as "plastic" (see Coon et al. 1974), with stresses that are strain-rates independent that are rate-independence, in accord with laboratory experiments (Tuhkuri and Lensu, 2002). The post-fracture viscous deformations are not in accord with field observations and rather represent a simplification of the larger-scale plastic regime. These points are clarified at L112-L114 of the revised manuscript.

4. Page 4: Equations 6 and 7: You are using multiple notations for the same thing: x and y components, 1 and 2 components. In Eq. 5, C is a fourth order tenor, Eq. 6 is a 2 × 2 matrix (components of a second order tensor?).

>> We changed the indices "1" and "2" for "x" and "y" in Eq. 7, in the revised manuscript, as suggested by the reviewer.

As the reviewer points out, the tensors defined in Eq. 5 are presented in Eq. 6 and 7 using a matrix notation. This notation is often used in the literature to concisely write the components of the elastic tensor, based on the symmetry of the stress and strain tensors. It is obtained by laying out the 3 (in 2D) independent components of the 2nd order stress and strain tensors into a single row, such that the components of the 4th order tensor C are written in a 3x3 matrix (6x6 in 3D). See Rice (2010) for reference. This is clarified at L96-97 of the revised manuscript.

5. Page 5: Probably helpful to define σI and σII in terms of stress components.

>> This is added after Eq. 10 in the revised manuscript, as suggested by the reviewer.

6. Page 6: Line 150: What is the 'standard' path?

>> The "standard" path refers to the original return algorithm in the EB and MEB damage parameterization (Rampal et al. 2016, Dansereau et al. 2016), where the super-critical stresses are relaxed along a line that runs through the origin. This is now specified in a new paragraph added at the end of section 2.4 in the revised manuscript.

7. Page 6: Line 151: Change 'to for' to 'for'.

>> Corrected as suggested by the reviewer.

8. Page 6: Line 162: Schreyer et al do not use 'granular theory', assuming that means models of granular flow. It is also confusing to refer to σc as a decohesive stress tensor since it has no apparent connection to Schreyer et al.

>> We remove "granular theory" in this sentence and use the term more carefully throughout the revised manuscript.

Although our work is inspired by Schreyer et al. 2006, the name "decohesive stress tensor " is not a direct reference to their algorithm, but rather a reference to the fact that this stress is produced in association with the development of damage, hence to the decohesion of the ice material. We nonetheless note that our mention of Schreyer et al. 2006 refers to their use of a decohesive strain that is subtracted from the local elastic strain in the stress-strain relationship when the ice fractures, effectively relaxing the stress rates. This was clarified in the revised manuscript at L158-160, which now reads:

"Note that the decohesive stress tensor used in this parameterization has a similar role as the decohesive strain used in the Elastic-Decohesive (ED) model (Schreyer et al., 2006). In the ED model, a decohesive strain represents the displacement field discontinuity in a sample associated with its cracking and relaxes the effective stress rates in the constitutive equation. It is derived from a decohesion function and depends on the mode of failure. Here, we do define a strain discontinuity associated with cracking but define the decohesive stress tensor to relax the stress states back onto the yield curve at different angles in the stress invariant space."

9. Page 7: Line 177: Change 'correspond' to 'corresponding'.

>> Corrected as suggested by the reviewer.

10. Page 7: last line: What is included in the 'solution vector'?

>> The discretized set of equation corresponds to a system of N non-linear equations in the form of :

$$\mathbf{A\ x = b}\ ,$$

where **x** is a vector formed by stacking all the $u_{i,j}$ components followed by the $v_{i,j}$ components, A is a NxN matrix with components that contains coefficients for the $u_{i,j}$ and $v_{i,j}$ dependent terms and B is a vector of length N containing the other terms. Then, the solution vector **F** is written as:

$$\mathbf{F = A\ x - B}\ .$$

We chose not to include these details in the revised manuscript, as it would necessitate a lengthy numerical description, and added instead a reference to Lemieux et al., (2014) for readers in search of these precisions.

11. Page 8: Line 204: τa is a vector. I assume the scalar value you assign to it is for one component and the other is zero. (Also Page 9, Line 245.)

>> Yes. This precision is added at L204 and L245 in the revised manuscript.

12. Page 9: First line: Please give a reference showing the connection between failure in granular material and sea ice under uniaxial compression.

>> This section (4.3.3) and section 4.2 are re-written in the revised manuscript to clarify the granular character of sea ice and how it is related to fracture angles in uniaxial compression tests. A few references are added, such as Bardet et al. (1991) for granular geo-materials in uniaxial compression, Wachter et al. (2008) for fracture angles in ice samples, Wang et al. (2020) for uniaxial loading of sea ice in laboratory, Overland et al (1998) for shear bands observations in the Arctic.

13. Page 9: Line 226: I don't see a definition of δ.

>> It is defined above Eq. 29 at L226, as the angle of dilatancy.

14. Page 9: Line 228: 'In general, the fracture angle ...' is this the fracture angle for sea ice?

>> We changed "In general" for "In most materials"

15. Page 9: Line 244: Change 'waves' to 'wave'.

>> Corrected as suggested by the reviewer.

16. Page 9: Line 248: '4 cfi)' means Figure 4?

>> This was an error in the labeling, and is corrected in the revised manuscript

17. Page 10: Line 254: Change 'are' to 'is'. I do not see in Fig. 5 that large values of R is associated with growth in εsym. Can you illustrate this better?

>> We do not expect a correlation between R, the largest local error in the damage factor Psi, and ε_{sym}, which represents the domain-integrated asymmetries in the stress field. Specifically, ε_{sym} corresponds to the cumulated the far-field response to all residual errors of previous time iterates. It grows with the onset of fracture (with large R values), given that the damage parameterization is the largest source of errors. With time, however, the far-field response to previous errors become large and dominates over the new errors in the damage factor Psi. We clarified this point and these variables in section 4.3.1 and at L254-L259 in the revised manuscript.

18. Page 10: Line 255: Change 'growths R' to 'growth in R'.

>> For consistency, we changed for "error amplification ratio R", as used throughout the manuscript.

19. Page 10: Line 258: Change 'indicate' to 'indicates'.

>> Corrected as suggested by the reviewer.

20. Page 10: Line 269: Change 'depends on corrected' to 'depend on the corrected'.

>> Corrected as suggested by the reviewer.

21. Page 10: Lines 274-278: MEB and VP (and granular material models) make different predictions. Is there any evidence for your model behavior in experiments? The VP model is based on plasticity there is no fracture, so no ’post-fracture behavior.’

>> As discussed in comment #3 above, the large-scale sea-ice deformations are “plastic” (see Coon et al., 1974, Tuhkuri and Lensu, 2002). The post-fracture viscous deformations in the MEB model thus do not correspond to field observations and represent a simplification of the plastic regime. The VP model simulates the plastic deformations associated with the ductile fractures, which corresponds to the observed material behaviour at the macro-scale. The VP model however does not represent the brittle component of the fractures or discontinuities in material properties, which occur at the smaller scales but may influence the fractures orientation and other deformation statistics. These points are clarified at L276-L278 in the revised manuscript.

22. Page 11: Line 284: Change ’approaches’ to ’approach’.

>> Corrected as suggested by the reviewer.

23. Page 11: Line 286: Change ’sensitive other’ to ’sensitive to other’.

>> Corrected as suggested by the reviewer.

24. Page 11: Line 290: Change ’increase’ to ’increases’.

>> Corrected as suggested by the reviewer.

25. Page 12: Line 332: Change ’divergence’ to ’divergent’.

>> Corrected as suggested by the reviewer.

26. Page 12: Line 335: Change ’reach’ to ’reaches’. Change ’wave’ to ’waves’.

>> Corrected as suggested by the reviewer.

27. Page 13: Line 357: Change ’generalizes’ to ’generalized’.

>> Corrected as suggested by the reviewer.

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