Review of:

“*Implementation of a brittle sea-ice rheology in an Eulerian, finite-difference, C-grid modeling framework: Impact on the simulated deformation of sea-ice in the Arctic*” by Laurent Brodeau, Pierre Rampal, Einar Ólason, and Véronique Dansereau.

This manuscript presents the implementation of the Brittle Bringham Maxwell (BBM) rheology in the SI3 community sea ice model, which uses a C-Grid Finite Difference framework. In particular, the authors present their use of the E-Grid discretization scheme to avoid difficulties with the staggered velocity and stress components in the C-Grid. They also present their use of an upper-convected time derivative approach to advect the stress components. The performance of their BBM implementation is then assessed by comparing the sea ice deformation statistics in SI3 simulations using the implemented BBM and the aEVP rheology.

I find that the manuscript is interesting, important and has potential for publication in GMD. In particular, it provides detailed discussions on difficulties implementing the BBM rheology in the SI3 sea ice model, and presents the numerical tools used by the authors to overcome them. This is an important step for the rheology to be more thoroughly investigated and used by the community. However, the methods used to validate the implementation is not adequate, and leaves doubts on the realism of the implemented BBM. In particular (among others listed below): why is SI3 initialised from neXtSIM fields? This seems to imply that the implemented BBM is not performing well unless the damage is restarted from the other model. This method also makes the comparison between the implemented BBM and the aEVP difficult to interpret, as the BBM benefits from the initial neXtSIM damage, and thus likely has an inherited heterogeneity.

As such, while the implementation itself makes for an interesting publication, significant work needs to be done to address these validation method weaknesses. This may require changing the methods, and thus significant time to be adequately addressed.

For these reasons, I recommend this manuscript to be send back to the authors for major revisions.

Best regards,

Mathieu Plante

Major points:

- In the methods, it is indicated that the sea ice model is initialized with fields from neXtSIM simulations. This choice rise doubts on whether this BBM implementation is usable on its own, or if it is it only realistic if initialized from neXtSIM fields. Also, and most importantly, this method brings a significant heterogeneity advantage for the BBM in the results, as it is additionally inheriting the damage from neXtSIM. It could very well be that the differences discussed in the result section is only a reflection of this difference. This is an important validation weakness and needs to be addressed.

- It is difficult to assess the performance of the rheology implementation without any confirmation that the sea ice drift, thickness and concentration are realistic. Also, it is mentioned in the methods (L340-342) that the simulations are tuned to yield similar mean sea ice deformation values. While this method may make sense for the presented metrics, I expect that this tuning comes with significant (and possibly unrealistic) differences in sea ice drift, transport and thickness. Yet, this is not investigated. Furthermore, the difference in drag coefficient means that there are larger internal forces in the BBM simulation than in the aEVP simulation, which likely largely impacts the PDF of sea ice deformations. This tuning method is thus also likely advantaging the BBM simulation, and may not be well suited for this validation. It should thus be revised / discussed more in-depth.

- Results from the aEVP simulation seem particularly bad, even for an EVP model at 10km resolution. Is this using the standard aEVP model parameters in SI3? Please provide more information in that regard. While I understand that tuning the aEVP parameters might be out of the scope of this paper, this will nonetheless likely be picked by the community as being an unfair comparison, especially that we know some model parameters that are better at representing LKFs (i.e., decreasing the ellipse aspect ratio). This is especially noteworthy given that the BBM parameters are themselves tuned for a better performance (e.g. in section 2.3.3). A similar effort should be done for the aEVP parameters, or at least the authors should indicate and justify the choice of not doing so in the analysis.

- It is often implied, as motivation for this implementation, that it is not possible to implement the brittle models with the staggered components in the C-grid, citing Plante at al., (2020) as an example. This is inaccurate and misleading: Plante et al., (2020) in truth demonstrated how to implement the MEB with staggered components without yielding numerical problems. The numerical inaccuracies assessed in Plante et al., (2020) were not related to the staggered stress components, but to the mathematical expression of the prognostic damage equation under large convergence (i.e., their paper demonstrated the same instabilities mentioned at L131 in the current manuscript, with a follow-up, Plante and Tremblay, 2021, offering a solution). The proposed method here should thus not be presented as a solution to a numerical instability problem, but as a novel approach to avoid the treatment of staggered stress components.

- While the text is mostly clear, some sentences are a bit wordy and tedious. This is throughout the text but most particularly in the introduction and section 3.

Specific comments (some repeating the comments above):

L8-12: This sentence is tedious; I am not sure what you are trying to say. Do you mean results show that the BBM in SI3 is suitable for climate simulations? If so, I think that this is far-fetch, only based on the localisation of sea ice deformations. I don’t think that this is appropriate for this manuscript.

Introduction: This section needs some work: it is a bit “wordy” and it feels more drafty then the rest of the manuscript.

L15: remove “all”

L42: “as an upgrade of MEB”: this is a non-physical way to put it. The two rheology represent different physics, and we cannot call the BBM as an “upgrade” of the MEB. Please rephrase more objectively.

L45: “preserving […] the thickness pattern of the sea-ice cover consistent with observations”, preserving here does not work. Rephrase.

L46-47: If it is proven, add the associated references. Otherwise, rephrase.

L64-65: This should be re-worded, as the McGill (and MITgcm) model is capable of producing pan-Arctic simulations (i.e., the models are not limited to the experiments presented in specific papers). The idealised experiments in Plante et al., (2020) were simply not designed to compare against sea ice drift and deformation observations. What you mean here is rather that the other implementations have not yet been assessed in a pan-Arctic context against sea ice drift and deformation observations? This might not be the right focus for this manuscript.

L69-75: Here it is implied that the problem has no solution yet, although it was overcome in Plante et al., (2020). You should rephrase to reflect that here you propose a different approach, one that avoids interpolation.

L70: “is not well-suited for brittle rheologies”: This is too vague and feels subjective. Do you mean that “it makes for a difficult implementation of brittle rheologies, in which any error in the prognostic stress variables are also integrated in the damage parameter”?

L74: Perhaps: → we propose a novel approach to address this difficulty ?

Eq. 7: I see that the exponential SIC term is inside the parenthesis, and thus also has the dependency on alpha? This is different from other brittle implementations, and the rationale behind this change should be provided. Note also that this parameter is indicated as beta in Eq. B6, please rectify.

Eq. 13: This formulation (Plante and Tremblay, 2021) miss-matches the comment at L121-122, as the stress state is only going back directly to the yield curve if $\Delta t = T_d$.

L139-141 : “that of a viscous-plastic one such as VP” → that of a viscous-plastic (VP) rheology?

L143: This is not exact: the ice is always viscoelastic in the MEB and BBM, not only when damaged. Only, the viscous dissipation acts on a (much) longer time-scale in the undamaged case.

L144: Watch for VP vs. EVP (here and through the text). Here I believe that you mean the EVP.

L150-151: I am not sure that this is a real distinction between MEB and VP models. I.e., regardless of the mechanism to represent “discontinuities” (damage in the brittle models, rate-invariance in the VP), it remains that a continuum model does not resolve discontinuities, which are thus only represented by Finite Difference gradients.

L158: Rephrase: a rheology is not bound to a numerical scheme. For instance, the MEB in the McGill model also uses an implicit iterative approach. The EVP uses explicit formulation.

L161-163: It could be noted that this small time step is also necessary to resolve the elastic waves, otherwise the elastic component (and the damage) is no-longer physically meaningful.

L166: In the case of the stress component, from a scale analysis, the advection is likely very negligible: the time-derivative term is the elastic term, acting at very short time scales, at which the advection is indeed very insignificant, especially at a 10km grid scale.

L170: “tracers are defined at the point located at the center of each cell” → tracers are defined at the cell centers.

L183-188 (suggestion): it could also be noted that in the VP formulation, the stress components are only diagnostics, and this staggering of the strain rate components only affects the computing of the Delta parameter, which itself is not prognostic (not integrated). However, in the brittle models, the staggering affects a larger number of prognostic variables (d, sigma), each of which are integrated and inter-dependent. This make the implementation unforgiving for any incoherence in the treatment of the staggered components.

L194-195: “in addition to being conceptually debatable in the context of a brittle model, which is expected to simulate very sharp spatial gradients”, again, I find this a bit misleading, as this points to a caveat of using a continuum model, not the choice of rheology. You never truly resolve discontinuity in FD continuum approach. This sentence thus suggests that the brittle models should not be used in a continuum context to begin with.

L197-198: This is false and inacceptable: Plante et al., (2020) actually demonstrated how to implement the MEB with the staggered components without having these errors, while it is written that Plante et al. (2020) implemented the MEB with has all these errors and inaccurate solutions.

L199-202: This is quite vague, subjective yet strongly worded. What do you mean exactly by “consistent”? Do you mean that one needs interpolation but not the other? How much is it an issue?

L221-222: For consistency, wouldn't it be better to also have A and h defined also at the f-points? So that all tracers defining the E and lambda parameters are treated the same way?

Eq. 16: I am not sure if this is an error (?): I would expect the stresses to be corrected (and not defined) by the right hand side, which represents a (weighted) difference between the two offset grids. Am I missing something? More precisely, I would expect an equation on the form of sigma_new = sigma_old + weight * Delta-Sigma, but here we have sigma_new = weight* Delta-Sigma. Please clarify.

L271: Each time step, to me, seem to refer to the smaller time step, but the thermodynamics is the longer time step. Please clarify. It would also be useful to define the small and large time steps as the “dynamical” and “advective” time steps.

L307: remove “so-called” and “which is”.

L333-334: Can you clarify here that this initialization from OCE-neXtSIM is made for both the BBM and the aEVP simulations? Also, please give more insight on why the sea ice model needs to be initialized from neXtSIM. Are results similar if the model is used without this neXtSIM dependency? I would argue that the analysis would be most meaningful and convincing if the simulations were ran without this neXtSIM initialization.

L333: “and damage (only for SI3-BBM)”: this is a very important difference in the method between the simulations, yet this effect is not at all discussed or investigated. It means you initialize the BBM with heterogeneity imported from neXtSIM, but less so for the aEVP case.

L342: This tuning of the sea ice deformation could be interesting, as opposed to tuning the sea ice drift, but we need to understand how else it impact the simulations. How is it impacting the sea ice drift and thicknesses? Are they very different between the simulations? Are they all realistic, or is this tuning made at the cost of the large-scale dynamics? This should be shown and discussed.

L354-355: How does your method compares with the SIREx methods?

L368: This to me looks like a particularly bad result even for the aEVP at 10km: we indeed do not expect the EVP to produce much LKF at this resolution, but this result is particularly smooth... Is this using the default aEVP parameters in SI3, or can it be a result of the drag coefficient tuning? This should be discussed, and add some notes here to show awareness that the aEVP results could be improved by simple modifications to the model parameters, and justify the choice of not doing so for the comparison.

Section 3.4.2: This feels a bit strange as you demonstrate just above that the aEVP simulation does not have a tail. So, it is unclear what is the purpose of this analysis, as it only repeats what is already demonstrated in section 3.4.1.

L398: “a transition” : Please clarify what transition this refers to.

L403: “Interestingly”, do you mean “In particular”? It sounds as if this result is not expected.

L408: These c_{da} values should be provided in the method section, at L342. Also, this is a surprisingly wide difference between the simulations, and thus suggest that the internal stresses would be much smaller in the aEVP simulations. This surely impacts the production of large deformation rates and LKFs. This needs to be investigated as to show how much this impacts the difference in representing the tail of the sea ice deformation PDFs.

L417-426: What is the purpose of this analysis? It is unclear yet what the structure functions tell us, in terms of model performance (Bouchat et al., 2022).

L429-431 Again, this is a strong comment that is not demonstrated. It also does not reflect that there have been successful implementations of the MEB on the staggered C-grid.

L472-474: A note could be added here that the fully-converged EVP likely influences the production of LKFs.

L483-485: This investigation is interesting and should be shown.

L487-489: This has not been assessed here, there are no comparisons with neXtSIM.

Conclusion: This section includes several strongly worded but subjective statements that do not serve well the actual contribution of the manuscript. In the context of a scientific manuscript, such subjective statements have a history of distracting readers from the actual analysis and to raise doubt on the transparency. This should be revised to be more nuanced and to keep the focus on the presented analysis.

L499-501: This is not accurate and too strongly worded. As the authors acknowledge in the introduction, the MEB has been implemented in the McGill sea ice model, and the MITgcm, which are both capable of producing pan-Arctic simulations.

L501-502: "has proven to be poorly fitted for brittle rheologies." Again, it is not demonstrated. This is a rather subjective statements not reflecting that the MEB was successfully implemented on staggered C-grids in other models.

L520-523: This last paragraph needs to be removed. It has not even been assessed here: there are no comparison with neXtSIM, only against aEVP. It is particularly blind to the listed numerical sensitivities associated with this implementation, which likely strongly affect the results (e.g., just looking at Figure 5).

L571: If I understand well, you get the stress state as a function of the (d, u, v, h, A) from the previous iteration (i.e. from the constitutive equation), but then apply the cross-nudging before computing the new damage. One issue I see with this is that the propagation of damage is supposed stem from the stress concentration around the previous damage. By applying the cross-nudging between these steps, I fear that the cross-nudging would affects the development of damage and the orientation of its propagation. As this method is a main contribution of this paper, I believe it would be worth providing some analysis and discussion on this regard. Also, to me, the cross-nudging is not very different, in terms of numerical effect, than using interpolation: you are smoothing between two solutions instead of between staggered components. Not that it is an issue to me, but as the text highlights that interpolating is not appropriate, it then raises the question: how is this cross-nudging more appropriate than interpolating?

References:

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