2nd 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. The performance of their BBM implementation is then assessed first using a benchmark simulation from Mehlmann et al., 2021, then in pan-Arctic context, both compared to the same SI3 simulations repeated using the standard SI3 rheology configuration (the aEVP rheology). Results demonstrate the adequacy of the BBM implementation and its good performance in representing LKFs.

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.

I commend the work made by the authors to clarify the manuscript and genuinely address all of my (and other reviewers) comments. Many useful explanations have been added to the implementation description, and some changes to the methods largely clarify the analysis and allow for a more detailed presentation and interpretation of the results. I find this version of the manuscript to be very clear, useful and interesting, making for a strong manuscript that will open the door for a wider use of the BBM rheology in the sea ice modelling community.

I therefore recommend its publication, after addressing the following minor edits/clarifications.

Best regards,

Mathieu Plante

We appreciate Dr Plante's efforts in conducting a second review of our manuscript and for providing constructive suggestions, as well as identifying areas for improvement. Please find below our point-by-point responses to your comments.

Best regards, Laurent Brodeau

Minor comments:

- Title: perhaps indicating E-augmented C-grid?
We find that the title is already long and a bit "painful" to read, so that we would rather keep the original title. Yet, our main argument for preferring "C-grid" to "E-augmented C-grid" in the title has to do with the message we would like the title to convey. We want to make it clear, to potential readers, that the implementation we present in our paper is mainly relevant for sea-ice models that use the C-grid.

It is also clearly stated, already from the abstract (L5) or the introduction (L70), that our implementation involves augmenting the C-grid into an E-grid.

- L287: "for the term of the divergence of the stress tensor" --> either : for the stress divergence term, or : for the rheology term?
Based on the next comment of the reviewer, this sentence has been removed as it was only introducing confusion.

- L288-293: Please clarify, I have difficulty understanding what you try to say. Are you referring to the 1/h form of the last terms in Eq. 16? Also, I am not sure what errors you are referring to.
We agree with the reviewer that this part was confusing. Based on the fact that it does not bring any useful information on the cross nudging approach, we have decided to remove it. The useful information, which is that the cross-nudging treatment is performed on the vertically-integrated stresses is already stated (L277), and we have completed this statement by explicitly adding the expressions of the vertically-integrated tensors in the text (L278).

L294-295: Please clarify. Also, I may be missing something but according to the equation, the nudging intensity is modulated by gamma_ns/Ns ... So why would it be less sensitive to Ns than to gamma_cn?
As stated earlier in the text (L283), during the course of one advective time step ΔT, the cross-nudging is applied every other dynamical time steps, so N_s/2 times. Let us imagine 2 configurations: in the first one the user chooses to use N_s=100, and in the second N_s=200. The user wants to achieve the same level of cross-nudging in the two experiments, so uses the same γ_{CN} in both experiments. In the first case, each stress tensor is going to be corrected 50 times during 1 ΔT with a nudging intensity of γ_{CN}/100, in the second case it is going to be corrected 100 times with a nudging intensity of γ_{CN}/200. This, in the end, will achieve a somewhat similar level of correction during the course of 1 ΔT, hence our statement:
"The form of the term that modulates the nudging intensity, i.e. γ_{cn}/Ns , ensures that the level of cross-nudging undergone by the two tensors under one ΔT is primarily controlled by γ_{cn} and remains somewhat independent of the choice of N_s."
We don't think that we can write our original sentence in a clearer way, so it is left unchanged. (L283-L285)

L296: remove coma after (Eq.9). Also, I would introduce (CN) earlier, at L280, and use it thereafter.
We agree, coma removed (L287) and "CN" introduced earlier (L277).

L296 "before any potential upcoming correction is applied following the Mohr-Coulomb test" ::
Rewrite, as this sounds as if the nudging is made between the MC test and the correction. I believe, according to your answers to my last review, that you rather apply the nudging before applying the MC test (i.e. before calculating dcrit)?
Perhaps something along the lines of: "Due to the strong damage-stress interdependence, we

apply the CN after solving the constitutive equation but before computing dcrit and applying the stress correction".

We agree that this sentence was confusing and we thank the reviewer for his suggestion. The new sentence now reads:

"Due to the strong damage-stress interdependence (section 2.2), the CN is applied on $\boldsymbol{\sigma}^{(i)}$ rather than $\boldsymbol{\sigma}$, *i.e.* after solving the constitutive equation (Eq. 9) but before computing d_{crit} and applying the stress correction (Eq. 12). " (L286-L287)

L297-298: "then we may propagate [...] ":: Too many comas, this sentence needs to be clarified. I get that you mean that it would introduce a discrepancy between the damage and the corrected stress values. This is very helpful, thanks for adding this explanation to the manuscript.

This part was clearly painful to follow. Here are the two new sentences we have written instead (L287-L290):

"Applying the CN after the stress correction stage (rather than before) may result in the use of a mix of (i) stress values that have been corrected through a local increase in damage and (ii) uncorrected stress values (with no increase in damage). This may lead to spatial inconsistencies between the post-CN stresses and the damage field."

L303: remove come after gamma_cn
Done (L292).

L323-324: This precision may not be needed, and confuses me as "the sum of Ns successive displacement vectors" sounds too much like making a Lagrangian track out of the Eulerian vectors. But I do not think it is the case here: I assume that the mean is applied on the Eulerian U,V, hence defined at a specific grid location and not building into a track.

Yes. We agree that this sentence was confusing and completely unnecessary, we have removed it (L312).

L325: I am not sure, but I would remove the dash between order and moment.
The reviewer is right, following a small investigation this advection scheme is generally referred to as "second-order moments", we have corrected accordingly in the manuscript (L313).

L342: "yields the so-called" -> change to "is the"
Done (L330).

Add point at end of L395.
Done (L383).

Section 2.5: I find a bit awkward the miss-match between the subsections (2.5.1--2.5.4) and the use of "as a first step", "as a second step", "a final step". I think it would be clearer if you distinguished between the trajectory selection (L388-395, which could be 2.5.1), from the quadrangle selection (L403-409), instead of describing both as steps in an owerall "selection process".

We agree. We have removed all the "as a first step", "as a second step", "a final step", and instead we have followed the recommendation of the reviewer to include a new "subsubsection" (now 2.5.1) (L375).

L411-412: repetition of quadrangles: → "based on their position #1 and #2 using [...]"
Corrected accordingly (L399).

L413-414: "Similarly to [...]." → This sentence is confusing, and may not be needed, as the following sentence is clear enough by itself.
We agree, the first sentence has therefore been removed, and what the reviewer refers to as the "following" sentence has been changed from:
"Instead, we assign the time that corresponds to the center of the time interval defined by position #1 and position #2 of each quadrangle."
to:
"The time location (date) assigned to a given deformation rate corresponds to the center of the time interval defined by position
#1 and position #2 of each quadrangle." (L401-L402)

 L420: no need for indent (keep in 1 paragraph)
Done. (L406)

L442: "somewhat very different" → not clear. I suggest going directly into the differences for conciseness.
Yes, this sentence was not particularly useful, we removed it and get directly to the differences as suggested by the reviewer:
"In the solutions obtained with BBM, we note the presence of a circular network of LKFs..." (L429)

 L498: remove shown and put "figure 7" in parentheses.
Yes, done (L486).

Section 3.3.1. Looking at the curves, it looks to me that in terms of power law, the standard simulations have a similar slope in the tail of the PDFs, although with under-represented large deformations. This is worth mentioning, although I understand that it is not the focus. e.g., perhaps something on the lines of "Our results suggest a propensity for SI3-default to exhibit somewhat similar power-law behaviour (similar exponents, except in convergence) but yet to underestimate the extreme values of the deformation rates. [...]").?
Yes, it is indeed worth mentioning. We have added the following sentence, largely inspired from your suggestion, at the end of the paragraph:
"Interestingly, the underestimation of extreme deformation values set aside, SI3-default exhibits a power-law behavior similar to that of both observations and SI3-BBM, with similar exponents, except in convergence." (L502)