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

We thank Dr Plante for conducting a thorough review of our paper and for raising important points that have helped improve the scientific quality of the manuscript.

Before moving to a point-by-point reply format, here are our answers and comments to the major concern raised.

Based on the major concern expressed by Dr Plante, and also by Reviewer #1, on our initialization/spinup strategy, we have decided to use a new and simpler spinup strategy. As such, our simulations have been performed again, initialized with the new strategy, and consequently, all diagnostics and figures have been processed and generated again, based on these new simulations.

But before providing the details on the new strategy, let us first rapidly justify the rationale behind our initial choice of initialization/spinup.
In our old ice-reinitialization spinup strategy, we used A, h and d taken from a neXtSIM simulation to reinitialize these fields, into SI3, a couple of months before the end of the spinup. We should have better emphasized that the sole purpose of this "mid-way sea-ice re-initialization" was solely to prevent the production segment of our two simulations to be initialized with an initial sea-ice state that has become too unrealistic in terms of sea-ice thickness and extent (due to the unavoidable accumulation of errors / drift during the first phase of the spinup), while conserving a "somewhat" spun-up 3D ocean state.
We thought that using A, h and d from the well-documented simulation of Boutin et al. 2023, was a good choice because their simulation was run on the same numerical grid and has been shown to be quite realistic when compared to observations.
However, we now understand that the fact that it is a BBM-driven simulation can be understood as unfair towards the aEVP simulations. However, we are completely convinced that it is not. This is because the main source of heterogeneity in the BBM-driven simulations comes from heterogeneity in the damage parameter, which will be spun up in a matter of days (Bouillon and Rampal, 2015; Rampal et al., 2016). Some ice thickness heterogeneity is inherited from the old spinup strategy though, but in our experience this is much less important than the damage evolution. This is borne out by the results of our revised initialisation strategy (using fields of the GLORYS2v4 reanalysis, performed with LIM using EVP) , which yields essentially the same results as the previous one.
For us, the main weaknesses of the old approach is that the surface properties of the ocean, such as temperature and salinity, are not updated the same way as the sea-ice properties are, creating a (very) temporary inconsistency between the ice-cover and the surface properties of the ocean.
But again, the main purpose of our old approach was to start the production simulation, December 15th 1996, with a sea-ice thickness the most consistent possible with respect to observations, and with a "lightly" spun-up 3D ocean state.

Conceptually, we face the following dilemma for the spinup strategy:

- A: Perform a very short spin-up and conserve a "rather realistic" sea-ice cover and thickness but potentially leave the 3D ocean in an adjustment phase
- B: Perform a long spinup, and risk initiating winter 1996-1997 with a sea-ice cover and thickness that has evolved quite differently from their observed counterparts, due to the imperfections of the model

Based on the main focus of our study, and as briefly discussed in the new version of the manuscript, we think that option A is completely acceptable, and this is what we have chosen for our new "simpler" initialization/spinup strategy.
As detailed in section 3.2 of the new version of the manuscript, we only perform a two-month long spinup, itself initialized October 1st 1996 with A and h, and the 3D state of the ocean,

taken from the GLORYS2V4 ocean reanalysis (NEMO + LIM using EVP, run on the same horizontal grid as our two Pan-Arctic experiments with data assimilation).

Actions performed :

- New spinup approach chosen
- The 2 simulation re-run following new spin-up approach
- Section about simulation description updated accordingly
- Diagnostics, figures, tables, updated using new simulation outputs
- Our results are not affected at all by this, result section almost not affected

Also based on concerns raised by the reviewers, on what has been wrongly interpreted as unfairness towards the aEVP rheology, we now make it very clear, all over the paper, that what we use as the reference SI3 simulation is the default workhorse setup of SI3 as provided in the current version of NEMO. As such, for instance, experiment “SI3-aEVP” has been renamed to “SI3-default”, and it is clearly stated in the paper that a better-tuned “aEVP” would likely perform better (L513-516).

Furthermore, following the suggestion of reviewer #2, we have included a new section (3.1) that presents results of idealized SI3 simulations run on the “cyclone” test-case of Mehlmann et al. 2021, using both our BBM implementation and the default aEVP setup of SI3 (new figure 5).

Another important information regarding the new simulations:
In the first version of the manuscript, our developments and simulations were based on the version 4.2.1 of NEMO. At the time, it was the current stable release of NEMO. But during the course of the review process of this paper, a bug related to the ocean-ice drag parameterization has been identified and fixed by the NEMO team. This bug has been judged sufficiently severe by the NEMO team to justify the release of a new stable version: the 4.2.2.
Link to release note: https://forge.nemo-ocean.eu/nemo/nemo/-/releases/4.2.2
Consequently we have switched to version 4.2.2 of NEMO for all the simulations presented in the new version of the manuscript.
The bug fix has significantly impacted the values of the simulated deformations and has therefore required a new tuning of the air-ice drag coefficients.

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 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.

Please see our reply to this major point above.

- 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.

Indeed, we do not include a thorough evaluation of the drift or thickness at large scale in the paper, first because it would be a full and dedicated study on its own to do it properly, and second it is not the scope of the present paper.

However, we can tell you that from the numerous simulations we performed during this tuning process, we have not noticed any significant qualitative or quantitative changes on the above-mentioned fields, that would let us revise our conclusions about the overall good performance obtained on the deformation metrics. In this regard, let us remind you that just as all our source code files and scripts, the output (netCDF) files of our simulations are made freely available to the community, see the Code and data availability, allowing anyone to control by themselve the aspects of the fields you mention.

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.

Based on your comment, and those of the reviewer #1, we do not "compare" anymore the skills of the BBM and the aEVP simulations in our paper. We realized that attempting such a comparison brings too much confusion on (and distraction from) the main message, which is: "a brittle rheology has been successfully implemented in a staggered-grid Eulerian framework, namely the SI3 model of NEMO, through a new approach based on a E-grid discretization. It allows realistic simulation of sea ice deformation across scales and may offer a new option to the large community of SI3 users to use a brittle rheology for sea ice and explore its potential impacts in coupled simulations."

Moreover, as you will see in the new version of the manuscript, and based on the suggestion of Reviewer #2, we have included the results of the "cyclone" idealized test-case of Mehlmann et al. 2021 for both rheologies. In this idealized experiment, all the relevant parameters are set to the values defined by the authors for this test-case, as such both SI3-defaut (the new name for SI3-aEVP) and SI3-BBM use the exact same $C_D^{(a)}$ of 1.2 10^{-3}, the same is true for other parameters used by both rheologies. Please see figure 5 in the new version of the manuscript.

- 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.

The paper has undergone some serious rewriting in this regard, also based on comments from Reviewer #1.

Yet, it (still) looks "bad" as you say, but we can assure you that we use the exact default and recommended setup of SI3, which is the result of years of collaboration between the NEMO development team and the Sea-ice Working Group of NEMO. As such, we cannot do better than moderating some of our conclusions, which is what we have done.

We now make it very clear, all over the paper, that what we use as the reference SI3 simulation is the default workhorse setup of SI3 as provided in the current version of NEMO (4.2.2), as such, for instance, experiment "SI3-aEVP" has been renamed to "SI3-default", and it is clearly stated in the paper that a better-tuned "aEVP" would likely perform better (L513-516).

- 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.

We agree, all this was awkwardly introduced in the first version of the manuscript. And in this regard as well, the manuscript has undergone some substantial rewriting.

There is now more information about the solution that you have introduced to prevent instabilities in your 2020 paper in the introduction (L59). We also make it clear, in different parts of the manuscript, that the motivation for the new E-grid-based approach we present is driven by: (i) the requirement to fully advect the stress tensor along with the damage tracer (something important for a model that is used primarily to perform coupled ocean/sea-ice simulations in realistic global or regional configurations); and (ii) the fact that we want to avoid to use a smoothed damage scalar at the corner point of the grid (which requires a damage variable to be both defined at the corner point and also "advectable").

- 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.

The abstract has been largely rewritten based on your comments and those of RW1.

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"

Removed (L14).

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.

This sentence has been completely rewritten. (L41)

→ "Recently, Ólason et al. (2022) introduced the Brittle Bingham Maxwell rheology (hereafter BBM) as an effort to address the incomplete treatment of the convergence of highly damaged sea-ice in MEB."

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

This sentence has been rewritten and uses the verb "reproduce" in place of "preserve". (L44)

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

References to Ólason et al., 2022 and Boutin et al., 2023 have been added. (L46)

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.

We have completely rewritten this part, it now reads:

"As of today, a few efforts have been made to implement MEB in sea-ice models comparable to SI3 in terms of discretization method and grid, such as the MIT general circulation model (Losch et al., 2010), or LIM, the former sea-ice component of the NEMO modeling system (Rousset et al., 2015). And more recently, Plante et al. (2020) have successfully implemented MEB in the McGill sea-ice model (Tremblay and Mysak, 1997; Lemieux et al., 2008, 2014). Overall, the work of these modeling groups have highlighted some challenging aspects that are specific to the implementation of a brittle rheology in a realistic Eulerian model that uses the finite-difference method on a staggered grid. As suggested by the work of Plante et al. (2020), when discretized on the Arakawa C-grid (Arakawa and Lamb, 1977), the same grid as used by SI3 (Vancoppenolle et al., 2023), brittle rheologies seem to be more prone to numerical instabilities than their viscous-plastic counterparts. In particular, they report that the stability of their MEB implementation is sensitive to the resort to spatial averaging, an interpolation technique that is traditionally used to relocate certain fields between the staggered points of the grid.” (L57)

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.
Based on your comments and those of RW1 this paragraph has been completely rewritten, and no such statements are made.

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”?
Same here, this statement is gone.

L74: Perhaps: we propose a novel approach to address this difficulty ?
We would rather use “new” instead of “novel”, as our approach is not really ground-breaking, therefore our new sentence reads:
“In this paper, we propose a new discretization approach adapted to the numerical implementation of a brittle rheology in an Eulerian finite-difference-, C-grid-based sea-ice model.” (L69)

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.
Note: it is now Eq. 6.
Yes, this form is specific to BBM, see Eq. 10 in Ólason et al., 2022. The rationale behind this is to give more realistic ice drift at low concentration.
Thank you for spotting this typo, it has been corrected \beta → \alpha. (L746)

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 Δt = Td .
You are right, we were wrong, the new sentence now reads:
“As discussed in Dansereau et al. (2016); Plante and Tremblay (2021), d_{crit} is used to scale overcritical stresses back towards the *Mohr-Coulomb* damage criterion, …” (L123)

L139-141 : “that of a viscous-plastic one such as VP” that of a viscous-plastic (VP) rheology?
Yes, better this way, corrected. (L139)

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.

You are right for MEB, but not for BBM. Under compression, BBM is purely elastic until a certain normal stress threshold, P_{max} , is reached. And beyond, *i.e.* $\sigma_I < -P_{max}$, then viscosity is activated. Our original sentence, which has been written by Dr Dansereau herself, remains valid. Even if MEB, as opposed to BBM, always includes a viscosity, it is not wrong to state that "it considers unfragmented sea-ice as an elastic and damageable solid", as the viscosity tends towards infinity for no damage. Besides, it is stated, at the beginning of the sentence, that these are "elasto-visco-brittle" rheologies (L143).

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

We meant "VP" in a broad sense, referring to viscous-plastic rheologies in general, including EVP, which despite its name is a viscous-plastic rheology, hence the use of the "s" at the end of "frameworks" in "As opposed to the VP frameworks". To prevent further confusion we now use "viscous-plastic" in place of VP when we imply "viscous-plastic".

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.

We are not sure to understand the meaning of the reviewer's comment. We thus apologize in advance if this is the case.

We agree that none of the MEB and VP models actually "resolve" discontinuities. Both try to "simulate" them, in their own ways. And these ways are simply different in the two rheologies, because they are built on different physical hypotheses/constitutive (mechanical) models. So we think that this is a distinction between MEB and VP models that is worth mentioning . And we by no means claim that VP models are not simulating "near-discontinuities", only that they do not originate from the same mechanism.

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.

We agree, we have rephrased the phrase as follows:

"Finally, note that in their numerical implementation of BBM, Ólason et al. (2022) chose to solve the dynamics explicitly using a time-step sufficiently small to account for the propagation of damage in the ice in a physically realistic manner." (L169)

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.

It is mentioned in the new version of the text in the new part that details the choice of the small time step. (below new Eq. 15, L176)

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.
Yes it is, but the damage is a scalar that can live on for days, if not weeks depending on the temperature conditions. As such, it has to be advected like any other tracer of the model, and based on the strong interdependence between the damage and the internal stresses (through the elasticity, and viscosity of damaged ice) it is more consistent to also advect the stress tensor so that no spatial inconsistencies occurs between the damage and the resulting stress when considering simulations longer than say a couple of days.
We have added some extra discussion about this point in section 2.2 (L159-L168).

L170: "tracers are defined at the point located at the center of each cell" tracers are defined at the cell centers.
Yes, better indeed, thank you. Corrected (L186).

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 makes the implementation unforgiving for any incoherence in the treatment of the staggered components.
We have slightly improved this part, using some bits of your suggestion, but it remains mostly similar to our initial version.

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.
This part has been completely rewritten, and this statement is no longer present.

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.
We never intended to convey this message, the idea was simply to mention that, similarly to what we experienced, you also experienced checkerboard instabilities as a consequence of the spatial averaging, but BEFORE implementing your method to overcome them. And after having read countless time the section 2.3.2 of your 2020 paper this is still what we understand when you write:
"Averaging the shear stress components from the neighboring nodes (as in Eq. 37 for the scalars) causes a checkerboard instability in the solution because of the staggered shear stress corrections and memories."

Our wording was clearly awkward and confusing, apologies for this. So we have completely rewritten this part, in a more accurate way, providing more details on how you overcome the checkerboard instabilities. Hence, the new version of this whole paragraph is:
"On the C-grid, a common way to interpolate a scalar defined at F-points onto T-points is to simply use the average of this scalar on the four surrounding F- points, and conversely to interpolate from T- to F-points. In the aEVP implementation of SI3 (Kimmritz et al., 2016), the problem posed by the staggering of tensor elements is overcome by using this averaging approach to interpolate the square of the shear rate $\dot{\varepsilon}_{12}$ from F- to T-points . Later on, the term P /Δ is also interpolated from T- to F-points in order to estimate σ_{12} . In their implementation of MEB, Plante et al. (2020) also use this approach to interpolate the damage tracer at F-points. However, they report that using the same approach to estimate σ_{12} , and hence σ_{II} , a T-points when performing the Mohr-Coulomb test (Eq.13), results in checkerboard instabilities. The solution they propose to prevent the occurrence of these instabilities is to introduce an additional σ_{12} that is defined at T-points. This additional σ_{12} is updated at each time step using – as an increment – the average of the four σ_{12} increments computed at the surrounding F-points." (L201-210)

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?
We have rephrased this sentence, making it less strongly-worded and more accurate ("consistent" is in terms of advection scheme used for the advection).
The new sentence reads:
"Finally, with the C-grid, the implementation of the advection of σ12 (F-point) in a way consistent (in terms of the advection scheme used) with that used for σ11 and σ22 (T-point) is somewhat challenging. That is because the advection of a scalar defined at the F-point, using the same scheme as that used for the advection of scalars at T-points, requires the existence of a u and a v at V- and U-points, respectively." (L118-222)
We have also added the discussion about why we think that despite involving very small terms it is important to advect the stress tensor along with the damage-scalar in section 2.2. See our point above (about L166) on this matter.

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?
Absolutely! We have considered this option since day 1 of the E-grid idea, but this would require to port the thermodynamics module of SI3 onto the F-points of the grid as well. This is something that may sound straightforward to achieve considering that the thermodynamics is mostly based on column/1D processes, but we can assure you that it is way more tricky to implement given the complexity of the thermodynamics module of SI3…
Nevertheless, this is definitely something that we plan to implement in the near future, together with the NEMO/SI3 developers, if our implementation is deemed promising by the NEMO team of course…

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.

You are right, our cross-nudging equation was full of typos in the first version of the manuscript. This has been corrected. And more generally, all parts dealing with the cross-nudging have been significantly re-worked and enhanced, based on some of your comments and those of RW1. See new section 2.3.2.

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.

In this sentence, we are referring to the advective time step, we have added “advective”. (L316)

We have followed your suggestion, and now “small/big time step” has been replaced by “dynamical/advective time step” throughout the manuscript.

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

This sentence no longer exists in the current version of the manuscript.

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.

Based on your major concern, the strategy has been completely changed (see our reply at the very beginning of this document). Hence the text is also changed and we hope that it is clear in this new version.

We performed a single 2-month long spinup (October 1st to November 30th 1996) using the SI3-default setup initialized with A, h & the 3D ocean state taken from the GLORYS2v4 reanalysis. Then, both SI3-default and SI3-BBM were started December 1st 1996 with the same restart generated at the end of the spinup. (see new section 3.2)

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.

Same here, everything has been changed, see our previous and introductory reply. Personally we tend to believe that it might be the opposite: that the heterogeneities imported from neXtSIM in our old spinup strategy, mainly in terms of ice thickness, could have actually helped to trigger the formation of LKFs in the aEVP run. The BBM simulation produces new LKFs in a matter of a few hours, regardless of the aspect/state of the initial sea-ice cover, *e.g.* including if concentration and thickness are set to constant over the entire Arctic basin.

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.
Please see our previous and upcoming replies on this drag coefficient matter. The focus of the paper is on the numerical implementation of BBM into SI3 with an assessment of simulated sea-ice deformations against those obtained from satellite data. The Journal is called "Geoscientific Model Development" and we believe that, as such, the work presented in this paper is very relevant and substantial . Plus we are no longer comparing the BBM to the aEVP simulation anymore, because apparently, too many remarks from the reviewers seem to imply that we have purposely made aEVP look bad.

L354-355: How does your method compares with the SIREx methods?
We believe our method is very similar to that used for SIREx (Bouchat et al. 2022). However, the scripts used by Bouchat et al. to compute deformations and scaling are still not available, and so despite having asked the lead author of that paper to share them with us a very long time ago. We thus cannot claim full equivalence between our method and theirs, which is why we refer to the method presented in the paper of Ólason et al. 2022 from which three of us are the authors. Besides, a lot of details regarding the approach and methods we use is given in the manuscript, now in section 2.5, and all our scripts are available online so that anyone can use/check/improve our method.

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.
See our previous reply to the similar point made in the major concerns section.
What's new in this current version of the paper:

- We stress that aEVP could be better tuned
- We use "SI3-default" in place of the old "SI3-aEVP"
- All aEVP-related parameters are shown in new Table 1
- Total deformation simulated by aEVP at different resolutions is assessed via the Mehlmann cyclone test-case, which produces results very similar to those of Mehlmann et al 2021, which tend to suggest that, after all, the aEVP of SI3 is not so badly tuned.
- Same test-case results are also shown in Annex C with aEVP using 1000 iterations instead of 100 in the rest of the paper.
- Based on your remarks and those of RW1 we have also performed a full Pan-Arctic simulation with aEVP using 1000 iterations instead of 100 (figures included at the end of this document).

- And we have also performed the same aEVP simulation with an air(-ice) drag coefficient of 2 10-3 in place of 1.4 10-3 as used in the default setup (see figure 3 at the end of this document).

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.
This section has been heavily rewritten, and we barely mention the results for the aEVP driven simulation. Note: it's now section 3.3.2.
But we would be criticized for not including SI3-default in this diagnostic.
Again our main interest is to see how SI3-BBM behaves with respect to RGPS, SI3-default only serves the purpose of showing what to expect with the workhorse setup of NEMO.

L398: "a transition" : Please clarify what transition this refers to.
This refers to the transition when the moment q of a random variable's distribution diverges/is not finite. Here by "divergence" we mean that the law of large number does not hold, or in other words, that we cannot find a μ such that $\sum_i X_i/n \rightarrow \mu$
Usually, this happens for $q > \beta+1$, with β being the exponent of the power-law tail of the pdf of that random variable. In order to satisfy the curiosity of an eventual reader we've added the reference to a classic book on statistics theory from the 50's, in which this point is discussed (Savage, 1954).

L403: "Interestingly", do you mean "In particular"? It sounds as if this result is not expected.
We have changed to "In particular" (L543).

L408: These cda 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.
Note: these values of $C_D^{(a)}$ have changed with the NEMO bug fix mentioned in the intro, but this does not change the fact that BBM requires a larger $C_D^{(a)}$ to be in agreement with RGPS at the 10 km scale.
We have added these values in the method section just as you advise (L490-495).
In order to address your point, we have performed a clone of simulation SI3-default using $C_D^{(a)} = 2\ 10^{-3}$, the same value as used in SI3-BBM, instead of $1.4\ 10^{-3}$. As shown in Figure 3 at the end of this document, if we increase $C_D^{(a)}$ as such in the SI3-default simulation, apart from obtaining amplified deformations everywhere (which will yield a PDF of the total deformation way above that of RGPS, not shown), almost no additional LKFs are created (we just see one about north of the Komsomolets Island).
Also, note that in the new figure 5 (manuscript) showing the solution in cyclone test-case,

both SI3-BBM and SI3-default use the same $C_D^{(a)} = 1.2\ 10^{-3}$ as recommended by Melhmann et al. 2021.

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).
We think the structure function is still an insightful diagnostic to evaluate to which degree a sea ice model is capable of localizing e.g. the simulated sea ice deformation, and more specifically how the degree of localization of deformation rates depends on their actual intensity (i.e. small deformation events being less localized than the larger ones). The higher the curvature of the structure function, the higher the degree of multifractality (the smaller the fractal dimension of the deformations distribution over space), hence more localized are the largest deformation events compared to the smaller ones. This approach is very much used to characterize e.g. the patterns/distribution of atmospheric precipitations that look typically very heterogeneously distributed over space.
Yet, the question whether this metric can be used (solely) to discriminate between models may be subject for debate. And if this is what the reviewer meant with his question, then we would tend to agree.

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.
This sentence is no longer present.

L472-474: A note could be added here that the fully-converged EVP likely influences the production of LKFs.
Well it turns out that, at least with the SI3 implementation of aEVP, it does not. As mentioned in a previous reply, we have performed a clone of simulation SI3-default with N_{EPV}=500 instead of the default N_{EPV}=100 (Figure 2, at the end of this document) and you will see that it barely changes anything. Same with the new figure X included in the new Appendix C that compares the cyclone test-case solution with N_{EPV}=1000 instead of the default N_{EPV}=100. Again, in the new version of the manuscript we stress that aEVP could be better tuned (L513-516), but we were unable to get different results by changing the number of iterations.

L483-485: This investigation is interesting and should be shown.
We think our paper is already very dense.We have spent a lot of energy adding some extra material like the cyclone test-case, we have performed new sensitivity experiments with SI3-default to back our honesty. We are sorry but will not carry out this investigation in this paper.

L487-489: This has not been assessed here, there are no comparisons with neXtSIM.
We refer to the exact same figure as in Ólason et al. 2022, so we think it is fine to mention it this way.

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.
About our transparency: we would like to bring to your attention the fact that contrary to a lot of recent papers published in this field of sea-ice rheology, we are openly sharing all our source codes, scripts, and the simulated data we have produced…
Having said that, we agree on the rest of your comment and you will see that our conclusion has undergone a serious rewriting as well and is more moderate than this old version.

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.
We have removed this statement and rephrased as follows instead:
"We have shown that our implementation, which features a prognostic ice damage tracer and a prognostic stress tensor, is able to realistically simulate sea-ice deformation statistics on a pan-Arctic scale when compared to satellite observations." (L633-635)
We indeed probably expressed our point inaccurately, and we apologize. The important part of our statement was "... a realistic solution on a pan-Arctic scale…". Our message was certainly not to contradict what we ourselves wrote in the introduction about an existing implementation of MEB into the McGill model, and even less to pretend that this model is not capable of producing pan-Arctic simulations! It would actually be very interesting to see such simulations and their evaluation with respect to sea ice drift, thickness-volume and deformation in a publication...
As for the MITgcm though, we are indeed aware of the recent work of the reviewer together with colleagues from AWI/Germany to implement the MEB in this model, but in this case (i) there is no published publication presenting this implementation as far as we know, and (ii) there is no paper presenting pan-Arctic simulations from this model with this rheology switched on either (some of us have seen a single poster with preliminary results, but again of idealized simulations only). And so no comparison to observations at the Pan-Arctic scale that we had a chance to see.

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.
This was clearly an awkward statement that has now disappeared from the manuscript.

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).
PAragraph removed.

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?

It is a good point, yet we are not sure to fully understand your reasoning.

We favor the option to apply the cross-nudging (CN) right before the Mohr-Coulomb (MC) test for the exact same reason we think it is important to advect the stress tensor: the strong interdependence, and hence spatial correlation, between the stresses and the damage.

If we apply the CN right after the MC test, then we may propagate to neighbor points (via the averaging process at play in the CN) stress values that have been corrected without propagating the associated increase in damage (we don't want to have to apply a CN on the damage!). And we want the damage and the stresses to remain fully spatially consistent.

But as suggested by sensitivity experiments we have performed, comparing the two options, in the end, the contribution of the cross-nudging correction at a given sub-time-step is so weak that it does not really matter if it is done before or after the MC test. It has been impossible to notice significant differences between simulated fields obtained using one or the other options.

You are right, this point is worth mentioning in the paper, so we have added some discussion about this aspect in the text (L296-301).

Then, to reply to your second point, we now clearly state in the conclusion (L646-644), that the use of the cross-nudging, required by the use of the E-grid, introduces some smoothing of the solution, and is therefore in contradiction with one of the reason/motivation for choosing the E-grid in the first place: namely to avoid averaging some fields. However, we also recall the reader, that despite the resort to this cross-nudging, the use of the E-grid allows to have a native prognostic damage tracer at the corner points that is never smoothed (beneficial for the estimate of σ_{12}), and also allows to advect σ_{12} using the same advection scheme a that used to advect σ_{11} and σ_{22}.

References:

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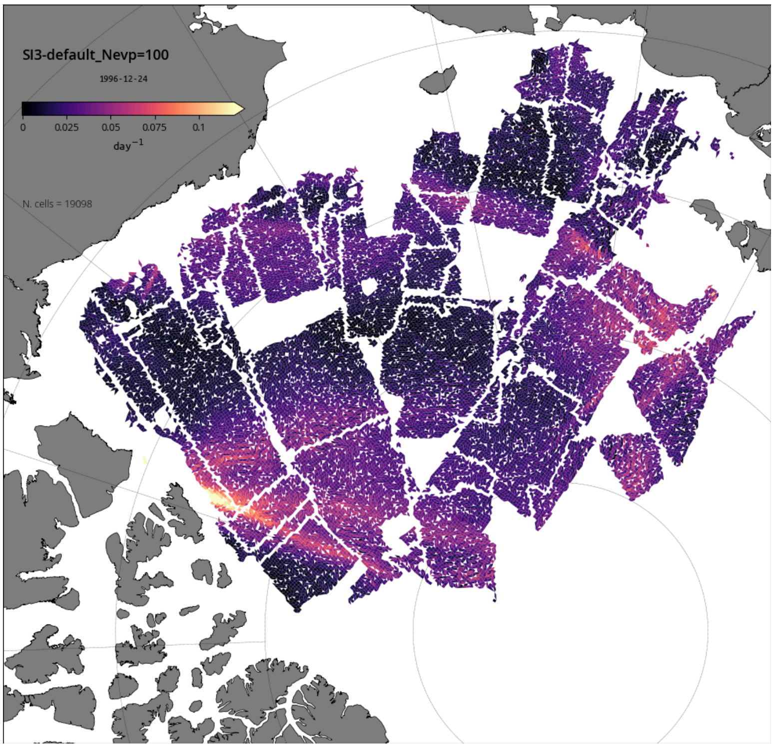

Fig.1/ Figure 7.c of manuscript, total deformation for experiment SI3-default, (N_{EVP}=100, and $C_D^{(a)}$ = 1.4 10^{-3}).

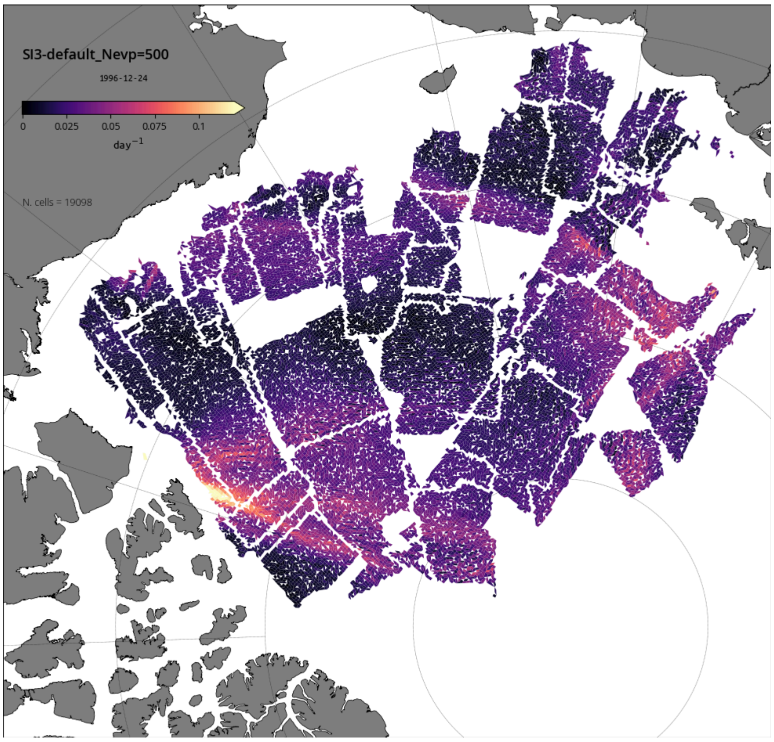

Fig.2/ Same as Figure 7.c of manuscript, but for SI3-default using N_{EVP}=500 instead of N_{EVP}=100.

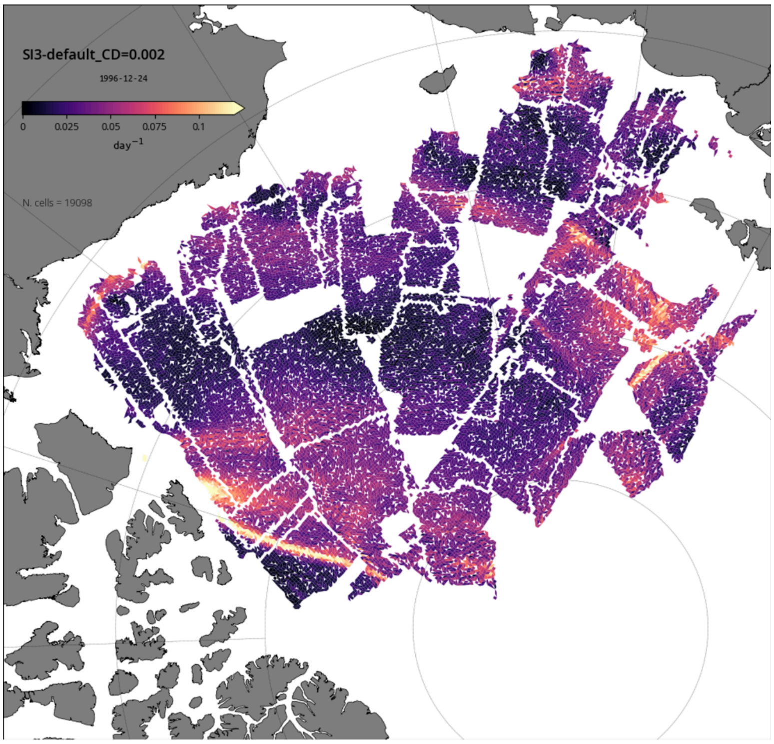

Fig.3/ Same as Figure 7.c of manuscript, but for SI3-default using $C_D^{(a)}=2\ 10^{-3}$ instead of $C_D^{(a)}=1.4\ 10^{-3}$.