

Editor

I agree the authors have gone some way to explaining the concept of plastic damage, which is what I would call plastic weakening. The exact language is immaterial as long as there is clarity. However, since damage is generally used in sea ice dynamics to refer to reduction of elasticity and not plastic weakening/damage, I think the title and abstract of this paper will still cause serious confusion. One of the referees strongly agrees with this. I will accept this paper if the abstract clearly defines what damage means in this paper, distinguishing it from elastic damage, and if the term "Plastic damaging" rather than merely "Damaging" is used in the title. If these changes are not made, then I will reject the paper and you should seek a different journal.

We changed the title and the abstract to accurately differentiate between the two concepts. The title now reads: *"On the sensitivity of sea ice deformation statistics to plastic damage"*. The first sentence of the abstract now reads: *"We implement a plastic damage parametrization (which is different from the elastic damage in the elasto-brittle framework) in the standard viscous-plastic sea ice model..."*

Reviewer 1

In their revised manuscript, the authors now argue that they introduced “plastic damage” within the VP modelling framework. This is a clarification, as this term (and associated references) were totally absent in the initial manuscript. Such “plastic damage”, i.e., as a matter of fact a plastic weakening (as quoted by the editor in his review of the initial manuscript) or strain softening mechanism, has been indeed proposed previously, particularly in the concrete literature (e.g. Lubliner et al., 1989). However, I reiterate that this has very little to do with classical damage mechanics, introduced first by Kachanov. And, clearly, in the mechanical literature, damage, used as a single word, refers to a degradation of elastic stiffness, not to “plastic damage”. Consequently, I think that the initial ambiguity is not totally removed in the revised manuscript, especially in the title. This is unfortunate, as this could have been done easily. In addition, a more complete conceptual setting could have been proposed from a comparison with these previous works, e.g. regarding the elastic-plastic strain decomposition assumption of Lubliner and associated papers, which is by construction irrelevant in the VP framework where elasticity is absent.

To further remove the ambiguity, we changed the title to: *“On the sensitivity of sea ice deformation statistics to plastic damage”*. Furthermore, we clarified the damage type that we use in the abstract: *“We implement a plastic damage parametrization (which is different from the elastic damage in the elasto-brittle framework) in the standard viscous-plastic sea ice model...”*

Finally, I consider that the answer of the authors to my comments about timescales (and, particularly between the “plastic damage” propagation timescale) is not really satisfactory, and I still believe that this will impact the representation of intermittency of sea ice mechanics. However, I would argue that it is an inherent flaw of the VP framework, with or without “plastic damage”.

The damage timescale used in the model simulations is of the order of 1 day, in line with passive microwave satellite imagery that shows lead propagation over hundreds to a thousand kilometers over a one-day time scale. This time scale differs significantly from that used in the MEB model (1 sec, e.g. Dansereau, Weiss et al., 2016). First, Dansereau et al (2016) argue that the timestep must be *larger* than the damage advection time scale (dx/c , where c is the elastic wave speed – see section 4.1.1). We disagree; the timestep must be much smaller (~2 orders of magnitude) than the advection time scale in order to resolve the damage propagation. Second, observations suggest that stress (and crack) propagation is slower than the elastic wave speed of the order of 1 km/s. We added a sentence on line 193 of the revised manuscript: *“The damage timescale used in the model simulations is in line with passive microwave satellite imagery that shows lead propagation over hundreds to a thousand kilometers over a one-day time scale...”*.

Regarding “intermittency”. All sea ice models irrespective of the rheology show intermittency (Bouchat et al., 2023). So, clearly, the intermittency is not linked with elasticity, damage propagation and brittle fracture as argued in Weiss and Dansereau (2017). Answering this question correctly will require another study. In order to address this question, we intend to run a sea ice model in free drift, as a cavitating material (only resistance to compression), a material with resistance in compression and shear (with elastic stress included or not) and identify when intermittency appears in the sea ice deformation statistics.

Reviewer 2

Dear Antoine and Bruno,

While I disagree that using scaling information for validation of sea ice kinematics in models is a sensible tool, I feel your paper adds insight into how different models reproduce the statistical and scaling features that have been observed. For that reason, this is worth publishing. I still have reservations about encouraging the use of these metrics for evaluating models. Given the observations are not as clear cut and Marsan et al. (2004) suggests, I am not entirely convinced sea ice deformation has a well behaved multi-fractal nature. Perhaps the results in your models showing this, point towards quasi brittle systems not behaving as cleanly as Marsan et al. (2004) suggests. Also, what sets the scaling exponents is an interaction between forcing and dissipation that can be adjusted in multiple ways in models (and nature), which makes the use of this as a metric by itself for model tuning problematic. However in attempting to model the localization of deformation, this paper does add useful information to that conversation.

The goal was to show that the deformation rate metrics are sensitive to the addition of a (plastic) damage parametrization. We agree that new metrics need to be developed to better evaluate the models.

I would like to refer to reviewer 1 as to whether the discussion of the different damage parameterizations in EB and VP, and that they are not equivalent, is adequately addressed. Echoing the editor, we do not want to propagate misuse of standard terms.

Specific comments

A specific comment

In the introduction you state that models need to be both put into the same numerical framework to allow inter comparison. Do you actually achieve this? All your results are for the VP model, and not EB. So perhaps you need to reframe this to not mislead the reader you are doing this. But rather that you are focussed on the behavior of your modified VP model.

We clarified this on line 106: *"In an attempt to further disentangle the effect of elasticity, damage, and discretization, we include a plastic damage parametrization in the standard VP model, and analyze the deformation statistics of this modified VP model following recommendations from SIREx [ref], and [ref]. To this end, we compare both simulated (with and without damage in the VP model) and the RADARSAT-derived Eulerian deformation products using probability density functions (PDFs), spatiotemporal scaling laws, and multifractality."*

In reading the paper I found some consistent grammatical issues, that I suggest you proof read to make sure you catch throughout the manuscript. I point out below copy edit needs I found.

Abstract line 4: (as proposed in SIREx1) is not needed, and also SIREx should be expanded if this is kept.

Corrected as suggested: we expanded the acronym.

line 8 "damage with a healing"

Corrected as suggested by the reviewer.

line 9 "unveils"

Corrected as suggested by the reviewer.

line 10 it might help to specify "sea ice deformation statistics" ... but your choice if you want to point the reader back to your not being general here. Sea ice is only mentioned in the first sentence of the abstract, and it is inferred that you continue to talk about this.

Corrected as suggested by the reviewer. The sentence now reads: *"...plastic damage parametrization is a powerful tuning knob affecting the deformation statistics of viscous-plastic sea ice."*

line 11 The first sentence is hanging.

It is unclear to us as to why this sentence is hanging.

First paragraph of the introduction. One could argue that this whole paragraph is irrelevant to the topic of the paper. It is an interesting paragraph, and probably should be saved for the lead authors thesis. Though I would suggest adding some clarification to what you are describing.

We agree that this paragraph is not necessary for the paper in and of itself, but it eases the reader into the topic without being too long or impairing comprehension.

Line 30 you use "sea-ice" and "sea ice" as an adjective in different parts of the text. Maintain a consistency with this, choose one convention.

Corrected as suggested by the reviewer throughout the paper.

Line 43 not sure the use of hyphens here is good.

Corrected as suggested by the reviewer. We kept the hyphens but changed the phrasing such that it now reads: "*In the standard viscous-plastic (VP) rheology — with an elliptical yield curve and normal flow rule (e.g. ... and its variants) —, sea ice is...*".

Line 56 "Elasto-Brittle (EB) and modification thereof (MEB and BBM)"

Corrected as suggested by the reviewer.

Line 58 "... larger deformations occur along fault lines ..."

This sentence already has a verb after fault lines.

Line 62 check if deformation is singular or plural. This should be checked for correctness throughout.

Corrected as suggested by the reviewer throughout the manuscript.

Line 365 and other places. You have a liking of starting a sentence with "Interestingly". Excessive use of this becomes uninteresting and distracts from your use of this construct to imbibe some interest in the reader.

Corrected as suggested by the reviewer throughout the manuscript. We replaced/removed multiple instances of "interestingly"

Lines 442-444: This sentence is mangled and hard to follow.

We added some precision about what specifically is changing: "*The VPd(2, 3, 30, 27.5) configuration highlights a complex transient state in the multifractal behavior of the model when changing the parameter τ from fully damaged ice...*"

Line 468-469 remove "– also called intermittency –"

Corrected as suggested by the reviewer

Line 506 "... RGPS observations than that of ..."

Corrected as suggested by the reviewer

Code and data availability section. Citations for code and ice velocity model output not provided.

We published the code on Zenodo. The data is getting massaged to be published soon as well.