Reviewer 1

General comments

In this manuscript, the authors propose to introduce in a viscous-plastic (VP) sea ice model a "memory" parameter that they call (inappropriately, see below) "damage", making the ice weaker (i.e. its strength P, setting the size of the yield curve, decreases) each time it yields. This generates a positive feedback leading to a weaker and weaker ice, an effect counterbalanced in their model by "healing" (eq. 15). The authors implement this scheme into their VP model and run 1-month simulations over the Arctic forced by geostrophic wind fields. They compare the results of their model with "classical" VP simulations (i.e. without this memory effect) in terms of strain localization and scaling, and argue that this new parameterization improves the modelling of strain localization (which makes sense considering the positive feedback mentioned above) and spatial scaling, but not the temporal scaling.

I have little comments about the numerical scheme, the forcing, etc... I have some about the evaluation and the comparison with RGPS data (see below). My main concern, however, is about the concepts. The authors define their memory parameter as "damage", and argue that it is equivalent to the damage parametrization in the EB/MEB family of models.

Nowhere in the paper is stated that the damage parameterization "is equivalent to the damage parameterization in the EB/MEB family of models". If anything, the exact opposite is said in one location at line 464: "This is of course expected since damage in the VPd model does not represent necessarily the same thing as damage in the MEB model". The paper simply states that damage can be parameterized in VP models, and we propose one way to accomplish this goal. The only thing that is akin to the EB-family damage is its temporal evolution equation, which is cast in a similar form. This is clarified on line 166 of the revised manuscript as: "We include damage in the VP model using a simple advection equation with source/sink terms *(akin to what is used in the MEB formulation)* of the form ...". So that it is clear that what is like the MEB is the evolution equation, not the damage per se.

Since the seminal paper of [L M Kachanov, 1958], damage mechanics became an actual branch of solid mechanics with (at least) one international journal devoted to it, many classical works (e.g. [M Kachanov, 1994; Kondo et al., 2007] among many others) and several books (e.g. see [Lemaitre and Chaboche, 1990]). In all these works, damage refers, in continuum mechanics, to a degradation of elastic properties as the result of emerging internal defects (microcracks, voids,..), an effect that is homogenized at the considered scale. Note that damage (with this true definition) can also be extended to granular media [Karimi et al., 2019], without the need to break bonds or particles, just from topological rearrangements between particles, that effectively lead to elastic softening.

The reviewer is citing literature on elastic damage. There is an equally large body of work on plastic damage describing how (plastic) damage can be included in plastic models. These models are usually elastic-plastic models, and consider both elastic and plastic damage; in the case of the VP model, only plastic damage is considered. Therefore, in reality, one usually defines damage as being in part due to elasticity (change in the Young's modulus) and in part due to plasticity (change in the yield curve) and, therefore, when the reviewer says "with this true definition", this is in fact ignoring a significant portion of the literature on damage mechanics. As Max Born said: "The belief that there is only one truth and that oneself is in possession of it seems to me the root of all the evil that is in the world."

The memory parameter introduced by the authors has therefore nothing to do with damage, which, by construction, cannot be distinguished implemented in a model that ignores elasticity. This is not just a matter of semantics: See e.g. L61-65 "Damage parametrizations — first developed in rock mechanics — are ad-hoc in that they are not derived from observations and/or from first physics principle". This is a very strong, yet false statement. Damage has actually been formulated from first physical principles, observed, measured in many communities (mechanics, geomechanics, geophysics (e.g. from a decrease of elastic wave velocities around major faults after an earthquake [Brenguier et al., 2008]), etc,see the literature suggested above). Consequently, in its present form, this manuscript would falsely perpetuate the idea within the sea ice and perhaps other communities that damage is an "ad-hoc" parameterization, and doing so, would ignore only about 65 years of work.

The reviewer is arguing that damage mechanics comes from first physical principles. This is incorrect as some form of smoothing has to be performed to extract continuum-scale variables out of the damaged system. The modeling of damage through the variation of elastic modulus is one of many ways of doing this. One could also model it by reducing the effective area of the sample, or by looking at void volume fraction for instance. In the paper cited by the reviewer [Brenguier et al., 2008], no mechanism is provided as to why an initial abrupt increase in elastic wave speed is followed by a slow decrease, *in seismic activity*. They hypothesize that the observed behavior is governed by postseismic stress relaxation within deeper parts of the fault zone. It could be due to an increase in elastic modulus, followed by a decrease (in the upper crustal layers), but the observed behavior could also be due to a change in density (elastic wave speed is a function of density) associated with the rearrangements of relatively high-density pockets (phase transition) in deeper parts of the mantle (Ringwood, 1972). Moreover, it is not because something is true in rocks mechanics that is true for sea ice. The damage in the context of sea ice modeling is formulated as a simple relaxation equation with one free parameter α applied to the viscosity and relaxation time. If it came from first physics principles, the parameter α would have been given a name other than a "constant" (e.g. Dansereau et al., 2016).

On the reverse, the memory effect introduced here is indeed an ad-hoc parameterization. Note that in the MEB framework, the dependence of the viscosity on damage is partly empirical as well, within some physical bounds.

The reviewer states that the damage parameterization is partially ad hoc or empirical. This is precisely the point we are making. The damage parametrized in the MEB comes from the definition of elastic damage, which locally changes the Young's modulus. In a complete elastic-plastic model, one also has to consider plastic damage, which modifies the yield curve of the material. In our case, we parametrize the plastic damage.

In the EB/MEB modelling framework, damage was introduced with two objectives:

- introduce indeed a memory of past loading history. The difference with the present work is that the elastic properties are evolving through time, i.e. following damage mechanics, not the strength.

Exactly, this is how plastic damage works.

- even more importantly, the objective was to propagate elastic stresses within the elastic medium following a damage/fracturing local event, this way allowing fracturing itself to propagate in an appropriate way. This comes back to the historical work of [Eshelby, 1957]: an inclusion within an elastic body, with elastic constants within the inclusion different from the surrounding medium, generates an elastic stress redistribution kernel. This is exactly what is modelled within the EB/MEB framework, and obviously not within the VP framework, with or without a memory parametrization. This fundamental difference should be clearly stated in the manuscript to avoid confusion.

As stated by the reviewer in the comment below, the VP assumes steady state between stresses and strain, and this redistribution of stress is taking place within the outer loop solver (at one given time step). The sentence (on line 171): "In contrast with the MEB model, damage is not bound by the propagation speed of elastic waves." was changed to "In contrast with the MEB model, damage is not bound by the propagation speed of elastic waves, *and damage will propagate within the outer loop solver, as opposed to being resolved with a finite elastic wave speed.*"

Still on concepts: to imagine that a material previously damaged will yield/fail more easily than an intact material seems to make sense at first glance. However, the relation between damage and strength is far to be trivial, certainly non-linear, most likely strongly dependent on the material, the loading geometry etc... Actually, progressive damage models have been used to explore this complex physics, and showed that strength generally results from complex (including long-ranged elastic) interactions between defects, resulting in non-trivial size effects [Weiss et al., 2014]. Even if ignoring the interactions between defects, the ultimate strength of a solid will depend on the (extreme) statistics of the population of defects, actually on the "largest" flaw in the [Weibull, 1939] classical approach, and not on homogenized "damage". Regarding the parameterization of memory implemented in this paper, it also poses some contradictions with the VP constitutive equation. One way to understand this contradiction and the incompatibility of the

parameterization with actual damage is that the authors implement a time-evolving "damage" evolution equation (with a given propagation time scale). Damage, by definition, measures the effective stress and its propagation is therefore an image of stress redistribution. However, the viscous(-plastic) constitutive equation intrinsically assumes a steady state between stress and deformation rate. In other words, it does not account for stress redistribution: there is no $d\sigma/dt$ (propagation, or elastic) term, only a σ (diffusion, or viscous) term.

The reviewer is again referring to elastic damage. Plastic damage is simulated as a change in the plastic flow vector. The definition of elastic damage provided by the reviewer does not apply to our parametrization. In this context, it is true that what we are doing is not *elastic-based* damage. We now introduce at line 162 the term as "plastic damage (hereafter referred to as damage for simplicity)", followed by the sentence: "In the context of sea ice and plasticity theory, plastic damage (hereafter referred to as damage for simplicity) can be accomplished by including strain weakening in the model that is independent of subsequent divergence and reduction of ice thickness and/or concentration (see for example Lubliner et al., 1989 for a simple model of plastic degradation). In recent years, more complex models have been developed that include (elastic-)plastic damage, notably in concrete (see for instance Luccioni et al., 1995; Jason et al., 2006; Voyiadjis and Taqieddin, 2008; Parisio and Laloui, 2017; Hafezolghorani et al., 2017; Friedlein et al., 2023; etc.) in which plasticity is taken into account in the damage variables, and the yield curve changes accordingly. Since elasticity is not taken into account in the VP model, we use a parametrization of damage that changes the yield curve of sea ice depending on its damage level extracted from its viscosity rather than its elasticity. Therefore, the damage that we present here, while intuitively based on previous literature, remains a parametrization of a more complex mechanism."

On the more specific question of time scales, the authors propose a time scale for the propagation of "damage" without justifying it on a physical basis, or specifying the associated space scale considered. This is missing from the paper and has repercussion on the interpretation of the appropriate, nominal healing time scale (see specific comments below). As in previous works on visco-elastic-brittle-type models, the conclusions of the authors however support the requirement of a clear separation between the damaging and healing time scales. The fundamental underlying contradiction arising from the absence of such separation (healing almost as fast as fracturing) should be introduced much earlier in the text.

The reviewer is referring to the following sentence: "The choice of a small damage timescale comes from the synoptic timescale at which fractures develop, while a large healing timescale comes from the thermodynamic growth of one meter of ice." The revised sentence now references the spatial scale and reads (see line 173 of the original manuscript): "The choice of a small damage timescale comes from the synoptic timescale at which fractures develop *over tens of kilometers*, while a large healing timescale comes from the thermodynamic growth of one meter of ice."

Concerning parts 4 and 5 and the comparison of model results to RGPS data in terms of strain localization as well as space and time scaling:

- I do not understand the interest to compare models outputs to observation in terms of PDFs (5.2) and then CDFs (5.3). The very same information is contained in both.

We keep both the PDFs for comparisons with existing literature and the CCDF (see below) because it supports the discussion better.

- On figure 4, the data seem to have been plotted (or accumulated) in a wrong way, i.e. $P(<X)$ is shown, not $P(>X)$. This should not be done that way, as small strain-rate values (at least, in the observed dataset) suffer from signal/noise biases, so we are more interested by the upper tail of the distribution. Consequently, I do not understand the associated evaluation (section 5.3).

Good point, thanks for noticing this. We are plotting the CCDF ($P(X > x)$). The figures are now flipped vertically (removing the confusion). The caption now correctly refers to CCDF, instead of CDF, and the y-axis was changed for a log scale to highlight the differences between the various configurations in the tail of their distribution (see above).

- Overall, the authors report some "scaling", or at least a scale-dependence of strain-rates. This, in my sense, is not completely enough to argue that the model incorporates the right physics (or not). Indeed, the models

(VP or VPd) are here forced using geostrophic wind fields which are themselves characterized by some complexity. And so spatial gradients in the forcing fields will inevitably induce some strain localization. The memory effect introduced here seems to reinforce this, which appears reasonable indeed. A real test would be to force such model with a homogeneous field. In that case, spatial scaling naturally emerges using progressive damage models, as the result of elastic long-ranged interactions and threshold mechanics (so, the introduction of damage).

We cannot find this study showing that the spatial scaling emerges naturally in progressive damage models in the peer-reviewed literature. Please give the reference.

The shear strain in the geostrophic wind field scales like $[U]/L = 10\,\text{m/s} / 500\text{km} = 2 \times 10^{-5}\,\text{sec}^{-1}$, where U is a characteristic wind speed and L is the synoptic length scale. Applying a 2% transfer function between wind speed and free drift ice velocity (Brunette et al. 2023), this gives a shear strain rate in the sea ice of 4×10^{-7} which is to the left of the tail end of the PDF associated with LKFs.

Nevertheless, some important differences between sea ice scaling properties and fluid turbulence exist, such as a space/time scaling symmetry, which is not discussed here. See e.g. [Weiss, 2013] for a discussion on this topic.

Indeed, these are important considerations for sea ice but are treated in great detail in other works, as the reviewer pointed out. We include a brief discussion on this topic after line 30: "As discussed in Weiss, (2013), the scaling properties of sea ice, contrary to those of turbulence in fluid, have been shown to be coupled in space and time such that the value of one is influenced by the sampling resolution of the other, and also by its spacetime localization."

In conclusion, to the question "Can we introduce a memory effect in VP sea-ice models from an empirical parametrization of the strength evolution?", I would say, why not ? , and it could indeed improve, empirically, the representation of sea ice deformation fields to some extent. However, the manuscript in its present form cannot be recommended for publication, for all the conceptual reasons given above. First, the term "damage" should be removed everywhere from the text, including in the title, as this manuscript is not dealing with this concept (see above). Then, a clear distinction between the concepts used here and in EB/MEB should be stated clearly, following the comments above.

The term damage was used in previous sea ice modeling work, following rock mechanics conventions, to mean "elastic damage", i.e. a reduction of the elastic modulus as opposed to a reduction in strength. Damage was also used in the simulation of plastic deformation in concrete and other materials, and referred to as "plastic damage". We now, clearly define the terms elastic and plastic damage up front and state that the term "damage" used in the text refers to "plastic damage", akin to published articles on the EB/MEB/BBM models that refer to "elastic damage" as damage.

Specific comments

Abstract: "We implement a damage parametrization in the standard viscous-plastic sea ice model to disentangle its effect from model physics (visco-elastic or elasto-brittle vs. viscoplastic)". This sentence is misleading. First, how is damage not part of the model physics? Second, you do not compare a visco-plastic model with a visco-elastic model here that includes or not damage. Please rephrase.

A parameterization is not part of the model physics (rheology), by definition. It is a process that is not resolved by the model that is implemented in terms of variables that are resolved by the model. A parameterization can be inspired from observations (e.g., heat conduction) or physics (e.g., damage), but it is not model physics. We are comparing two VP models one with and one without damage. We have removed "visco-elastic or elasto-brittle" in the parenthesis and the revised text now reads: "(visco-plastic with and without damage)".

Line 21: "Internal stresses rapidly redistribute these forces from ice–ice interactions over long distances." This is exactly what an elasto-brittle model (based on elasticity and progressive damage) is built to represent, and what a VP model does not consider, either with or without a memory effect included.

Here, we are describing the behavior of real-world sea ice, independently of the model physics. As to the above comment, stresses are redistributed rapidly by elastic waves in an elasto-brittle rheology and instantly in a VP rheology, i.e., within the outer loop solver.

Line 35: "sea ice motion". And deformation.

Corrected as suggested by the reviewer.

Lines 36-40: "sea ice is considered as a highly-viscous fluid for small deformations. In this case, sea ice deforms as a creeping material." For small "stresses", instead. Please clarify.

Sea ice behaves as a viscous material for any stress that lies within the yield curve. These can be equally large as the plastic stress on the yield (to within a small epsilon). The viscous regime is really applicable to small deformation not just small stresses. This sentence is standard in all publications on the VP model.

Line 48: "the inclusion of deformation on discontinuities": not clear. What do you mean? Deformation or discontinuities should not be introduced in an ad-hoc manner, but result from the modelled rheology.

We were referring to Coon et al. 2007 statement: "Displacement data show that discontinuities in velocity caused by lead opening, closing, and shear must be accounted for in the representation of deformation." But we agree it is unclear. The revised sentence now reads: "For instance, ice would be better described using a rheology that accounts for discontinuities in the velocity field, and an anisotropic yield curve that allows for tensile stresses \citep{coon2007}".

Line 49: "anisotropic yield curve *that allows* tensile stresses".

Corrected as suggested by the reviewer.

Line 51: "Finite element models...". This should be taken as another main comment: Another source of confusion in the paper is the association of certain rheologies (EB-type, Elasticdecohesive) but not others (VP) to the numerical schemes that have been employed in models implementing these rheologies. This gives the wrong impression that there is some causal relation between the numerics and the model equations. These are actually two distinctive things: you first formulate a system of equations and if consistent, it is independent from the choice of spatio-temporal discretization scheme you then apply. In the discussion, you even extend your supposition of good/bad agreement with observations to the Eulerian vs Lagrangian framework in which the equations are cast. However, as for numerical schemes, you never offer and explanation as to why. These suppositions add up, so much so that the reader is lost: what is best between damage or not damage, memory or not, rheology vs numerical scheme. The comparison you make here allows distinguishing only between a VP model with and without a memory effect and that should be made clear.

The first implementations of the EB model family were done using a finite element approach with a Lagrangian advection scheme. The authors concluded that the better realism of the MEB was due to the elasto-brittle rheology but without justification. A justification of this statement would have included a comparison of the MEB rheology using a finite difference and a finite element model with a Lagrangian scheme and identify what part of the "better realism" is due to numerics (FEM and FD) and what part is due to physics. Plante et al. 2021 coded the MEB model in finite difference to answer that question. The sentence was changed to: "Models that incorporate some of these recommendations include the Elasto-Brittle and modification thereof (EB, MEB, and BBM) [references] *originally cast in* Finite Element numerical schemes (FEM) [...]".

Line 53: .."that damage associated with (prior) fractures also affects ice strength". No ! You are here making a confusion between elastic modulus and strength (stress at failure). If both are expressed in Pa, they are completely different concepts – see also general comments above.

Corrected as suggested by the reviewer to: "These [EB] models include a damage parametrization that accounts for the fact that damage associated with (prior) fractures also affects the elastic modulus of the ice [...]".

Line 61-65. Please remove this wrong statement (see above).

The reviewer is referring to: "Damage parametrizations — first developed in rock mechanics — are ad-hoc in that they are not derived from observations and/or from first physics principle. For instance, a damage parameter can be quantitatively expressed as a scalar relationship between the elastic modulus of a material before and after fracture `\citep{amitrano1999diffuse}`. In this model, the ice strength does not decrease when damage is present; instead, it is the Young's modulus that decreases, resulting in larger deformation for the same stress state." The first sentence now reads (modified text in italic), "Damage parametrizations — first developed in rock mechanics — are ad-hoc *in that they are inspired by observations* and not from first physics principles."

The reviewer makes contradictory statements. In the first reviewer's paragraph on page 4: "On the reverse, ...", the reviewer states that "the dependence of the viscosity on damage is partly empirical". Then the reviewer wants us to remove a "false statement" (lines 61-65), even though these lines are saying what the reviewer said in his previous statement.

Line 65: "the damage is expressed as a function of the stress overshoot". Not clear: The (temporal) evolution of damage, or damage increment, is defined as a function of the stress overshoot.

Corrected as suggested by the reviewer.

Lines: 69-70: Again, you introduce some confusion between the definition of damage itself and its incre-mentation or propagation, or between damage and fracture mechanics. Please clarify.

The reviewer is correct. We clarify in the following way: "Other more complex descriptions of damage parameterizations (both in its definition and evolution) — such as fracture initiation around elliptical flaws [ref in the paper] — have been used in rock mechanics and could in principle be implemented in sea ice models."

Lines 72-74: "Earlier model–observation comparison studies, aimed at defining the most appropriate rheol-ogy for sea ice, found that any rheological model that includes compressive and shear strength reproduces observed sea ice drift, thickness, and concentration equally well (e.g. Flato and Hibler, 1992; Kreyscher et al., 2000; Ungermann et al., 2017)". It seems a little odd to cite here these early studies of rheological model comparisons, which do not include any elasto-brittle (i.e., damage) or memory-effect models, if the focus of your paper is to support or not the inclusion of these memory effects.

This sentence is there to introduce the reader to the necessity of looking at deformation statistics to differ-entiate rheologies.

Line 76: The reference to Marsan et al., 2004 is misplaced: they did not discriminate between different sea ice rheologies, they analyzed observations of deformation statistically.

Corrected as suggested by the reviewer.

Lines 78-83: Do the Bouchat et al. 2022 and Hutters et al, 2021 papers really conclude that the differences in the capability of these models to reproduce observations is due to their spatial discretization, i.e., FEM vs FD, or is this an interpretation? Then, what is the explanation for the factor of 5 between the resolutions required for FEM vs FD?

They suggest that the spatial discretization explains the differences and that more work is required to ad-dress this. A finite difference model needs 5-7 grid cells to spatially resolve discontinuities. The effective resolution of a finite element discretization with a Lagrangian advection scheme is therefore comparable to that of a finite difference model with a spatial resolution roughly an order of magnitude higher.

Line 87: A clarification here: "elastic deformations prior to fracture". In all EB-type models, the material is always elastic, prior to and after damage. In the MEB and BBM case, the material is visco-elastic and it becomes predominantly viscous, i.e. the relaxation time decreases after extensive damage.

Corrected as suggested by the reviewer: "[...] consideration of elastic deformations prior to and subsequent to fracture allowing the material to retain a memory of past deformations."

Line 105: About neglecting the advection term because of spatial resolution: isn't it a question of temporal resolution?

It is a function of both length scale and time step (temporal resolution). Advection scales like $u^2/L \approx 0.05\,\mathrm{m/s^2}/10^4\,\mathrm{m} \approx 10^{-7}$, which is 5 order of magnitude smaller than other terms in the momentum equation (see Zhang and Hibler, 1997)

Line 138: "and viscous relaxation time η and λ". η is the viscosity, λ the viscous relaxation time, defined as the ratio of the viscosity and elastic modulus. They are not both relaxation times (and only two variables are independent).

Corrected as suggested by the reviewer. It now reads: "[...] (e.g., elastic stiffness E, viscosity η, and relaxation time λ) [...]".

Line 154-155: "Its purpose is to convert the excess stress into damage (d)". See general comments: its physical "purpose" is to redistribute stresses within the material, i.e., there is a propagation of stress perturbations.

In this sentence, we are simply reviewing the MEB framework and its numerical implementation; i.e. the temporal evolution of the stress state is solved using a finite time step, and when it lies outside of the yield curve, the stresses are brought back to the yield along a line that passes through the origin using a parameter Ψ $(= \sigma_f/\sigma')$, where σ_f is the corrected stress state lying on the yield curve and σ' is the uncorrected stress state lying outside of the yield curve. Ψ in turn is a source in the damage equation (see for example, Dansereau et al., 2016: "In the Maxwell-EB model, the change in level of damage corresponding to a local damage event is determined as a function of the distance of the damaged model element to the yield criterion."; or Rampal et al., 2016).

Equation 16: I am a bit confused here: is there a parenthesis missing on the first term of the RHS of this equation? Otherwise, you could write the RHS as = 1/td – f(zeta)/td – d/td – d/th. Hence you have two constant (or at least damage-independent) terms, one damage-dependant damage rate term and one damage-dependant healing rate term. But these two last terms are of the same sign, whereas healing should in principle offset damaging. In your equation, the only positive term, incrementing damage, would be the first one, 1/td, a constant. Can you clarify or perhaps expend on the meaning of each term in the equation? In any case, "d" in this equation is not damage, but a memory parameter

There are no parentheses missing. In the plastic regime, when $\zeta \approx 0$, $d(d)/dt = (1 - d)/t_d$, and damage increases asymptotically to a maximum value of 1. In the viscous regime, when $\zeta \approx \zeta_{max}$, $d(d)/dt = -d/t_h$, and damage asymptotes to zero with a time scale t_h. The two sentences above were added in the paragraph after Equation 16 of the revised manuscript.

Line 171-173: See former comments about time scales in your model.

1. You choose a damage time scale that is "not bounded by the propagation speed of elastic waves", which in a brittle material that breaks, sets the upper bound for the speed of propagation of fractures. However, on what physical basis do you choose 1 day? How is this time linked to the mechanical behavior you assume in your model, i.e., viscousplastic? You state that it is a "typical time scale for fracture propagation". Are there any observations or references supporting that? The information is also incomplete: if you give a time scale, you need to give an associated space scale over which propagation is happening, i.e., it takes a fracture one day to travel what distance?

We specify the spatial scale associated with damage. Line 173 of the original manuscript now reads: "The choice of a small damage timescale comes from the synoptic timescale at which fractures develop *over tens of kilometers*, while a large healing timescale comes from the thermodynamic growth of one meter of ice."

2. You have a healing time, td, that varies between 2 days and 30 days. However, according to your equation 16 (last term on the right-hand-side), the healing rate in your model is not a constant but is damage-dependent (more intensively-damaged elements heal faster), making the healing rate space and time-dependent. This is an important point, that is not but should be mentioned in the text. Also, can you provide a physical explanation for the damage-dependance you are proposing for your healing rate?

The reviewer is correct. We add at line 171: "Note also that the healing term in Equation 16 depends on the level of damage." A heavily damaged piece of ice would heal faster due to (but not limited to) having more

surface area exposed to water, and therefore, refreezing more easily.

3. Assuming that your healing rate does vary between 2 days and 30 days (eg., if d = 1): this is only 2 to 30 times slower than the damage propagation time scale. Recall that the healing time is the time it takes for ice over a completely damaged model element or grid cell (d = 1) to recover completely its undamaged (d = 0) state. One of the key points in the previous MEB (or BBM) framework is that there is a very large separation of time scales between damaging and healing (ex., 10^5–10^6, see [Dansereau et al., 2016; Weiss and Dansereau, 2017]). If it is not met, damage does not "have time" to propagate before elements heal again, interactions between damaged parts of the ice are hindered, and the memory effect vanishes, obviously. You mention that in lines 457-459, but it would facilitate the interpretation of your results if this mention come earlier in the text.

We now include a discussion of this earlier in the text (after line 173): "The healing timescale should be much larger than the damaging timescale [as pointed out in, Dansereau et al., 2016; Weiss and Dansereau, 2017]."

Lines 175-179: "Note that a VP model is a nearly ideal plastic material, i.e. it can be considered as an elastic-plastic material with an infinite elastic wave speed; therefore, the fracture propagation is instantaneous (i.e., it is resolved with the outer loop solver of an implicit solver or the sub-cycling of an EVP model). In the above equation, n is a free parameter setting the steady-state damage for a given deformation state." See general comments above about distinguishing between the equations and the numerics and about the contradiction between including a damage evolution equation together with a steady-state stress equation, and actually including the concept of damage within a VP framework.

It is only a contradiction when we think about damage as a decrease in elastic modulus. In this sentence, we are simply stating that the damage is controlled by the viscosity.

Line 316: "soup-like": fluid-like?

Corrected as suggested by the reviewer.

Line 317: "except when th ≈ td when fewer extreme deformation events are present." See previous comment: This is rather trivial. At each time step, over completely-damaged elements (d = 1), you then offset damage by the same amount of healing: there is no net effect of damage nor healing.

This was clarified on line 317 of the revised manuscript: "In this case, the damage increment over partly damaged cells is compensated by the healing term."

Lines 392-400: Interesting. See general comments above for possible insights about time scales and temporal behavior.

We added this sentence after line 400: "The proposed damage parametrization simulates plastic weakening along LKFs and increases the memory of the system – something that the sea ice concentration and thickness do not accomplish when a lead opens and closes. It differs from the (elastic) damage definition introduced in earlier work (e.g., Girard et al., 2011; Rampal et al., 2016; Dansereau, 2016; Plante et al., 2020), where damage reduces the elasticity of the material, and therefore increases deformation for a fixed stress state. In the case of plastic damage, it is the effective viscosities in shear and divergence that are reduced, leading to larger strain rates for the same stress states. The VP sea ice rheology is one of many rheologies that simulates many climate-relevant sea ice properties satisfactorily (drift, deformation statistics) despite all of its flaws (yield curve, flow rule, elasticity simulated as viscous creep)."