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. 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 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.

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.

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.
- 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.

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 ect… 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.

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.

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.

- 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).

- 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).

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.

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.

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. visco-plastic) ''. 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.

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.

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

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.

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.

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

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, Elastic-decohesive) 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.

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.

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

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.

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

Lines 72-74: ‘’ Earlier model–observation comparison studies, aimed at defining the most appropriate rheology 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.

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.

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?

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.

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

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).

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.

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 = $1/t_d - f(zeta)/t_d - d/t_d - d/t_h$. 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, $1/t_d$, 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

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., viscous-plastic? 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?
2. You have a healing time, t_d, 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?
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.

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.

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

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.

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

References:

Brenguier, F., M. Campillo, C. Hadziioannou, N. Shapiro, R. M. Nadeau, and E. Larose (2008), Postseismic relaxation along the San Andreas fault at Parkfield from continuous seismological observations, *Science*, *321*(5895), 1478-1481.

Dansereau, V., J. Weiss, P. Saramito, and P. Lattes (2016), A Maxwell–elasto–brittle rheology for sea ice modeling, *The Cryosphere*, *10*, 1339-1359.

Eshelby, J. D. (1957), The determination of the elastic field of an ellipsoidal inclusion, and related problems, *Proc. Roy. Soc. A*, *241*, 376-396.

Kachanov, L. M. (1958), Time of the rupture process under creep conditions, *Isv. Akad. Nauk. SSR Otd Tekh. Nauk.*, *8*, 26-31.

Kachanov, M. (1994), Elastic solids with many cracks and related problems, *Advances in applied mechanics*, *30*, 259-445.

Karimi, K., D. Amitrano, and J. Weiss (2019), From plastic flow to brittle fracture: Role of microscopic friction in amorphous solids, *Phys. Rev. E*, *100*(1), 012908.

Kondo, D., H. Welemane, and F. Cormery (2007), Basic concepts and models in continuum damage mechanics, *Revue européenne de génie civil*, *11*(7-8), 927-943.

Lemaitre, J., and J. L. Chaboche (1990), *Mechanics of solid materials*, Cambridge University Press, Cambridge.

Weibull, W. (1939), A statistical theory of the strength of materials, *Proc. Royal Swedish Academy of Eng. Sci.*, *151*, 1-45.

Weiss, J. (2013), *Drift, deformation and fracture of sea ice - A perspective across scales*, Springer, Dordrecht, The Netherlands.

Weiss, J., and V. Dansereau (2017), Linking scales in sea ice mechanics, *Phil. Trans. R. Soc. A*, *375*(2086), 20150352.

Weiss, J., L. Girard, F. Gimbert, D. Amitrano, and D. Vandembroucq (2014), (Finite) size effects on compressive strength, *PNAS*, *111*(17), 6231-6236.