Editor

Originality (Novelty): 2. In recent years there has been considerable interest in the scaling properties of sea ice fracture and deformation and the ability of models to reproduce this. While I find the premise of this paper, the introduction of a damage parameter into the VP rheology, to be somewhat poorly motivated from the perspective of the proximate physical processes, the VP model itself is not argued closely from proximate physics in any case. The inclusion of damage therefore seems a legitimate extension for the purpose of generating improved understanding of the links between rheological assumptions, resolution, and potentially other factors in generating multifractality.

Scientific Quality (Rigour): 2. My rapid review suggests that a methodical approach has been taken with due care and attention. However, I shall rely on detailed reviews to assess the scientific rigour of the manuscript.

Significance (Impact): 3. While there is legitimate academic interest in the scaling properties of fracture, it is not convincingly explained why these are of practical interest, e.g. to climate modelling or operational needs. The non sequitur at the end of the second paragraph ("As such..") in the introduction does not, in of itself, bear up to critical analysis, though the argument could be made more convincing and would likely open up interest more broadly.

We agree. We added, before the last sentence of the paragraph at line 28, the following: "These physical processes must be adequately represented in both global climate models and ice–ocean prediction systems either through added physics or parameterizations. The ability of a sea-ice model to reproduce Linear Kinematic Features includes length, angle between conjugate pairs of LKFs, lifetime, and statistical spatiotemporal properties such as scaling, multifractality, coupling, etc."

Presentation Quality: 2. The paper seemed generally well written, nicely dissecting the relevant issues, and is logically organised. The opening sentences seem more appropriate for a general audience than the readership of The Cryosphere but are more amusing than distracting.

The manuscript proposes a simple, but ad hoc and poorly physically justified, approach to modification of the ice strength in a popular sea ice rheology scheme. The purpose of this is to create a low-computational cost way to better simulate some of the scaling properties of deformation. It is interesting, though not especially surprising (as the authors note), that this enables capture of some of these scaling relations with such a simple treatment.

I am sympathy with the general approach taken and there is much in this paper that is of value. The ad hoc approach is obviously not ideal and raises important concerns over appropriate choice of timescales, but then the rheology that is being modified is also exceedingly ad hoc and has been in use for over 40 years. Clearly the authors are aiming for a no-cost, practical approach that can be easily implemented into existing modelling architectures.

The development of the elasto-brittle (EB) and modification thereof (MEB and BBM) raised several interesting questions that have not been addressed by the developers of the EB family, namely, what part of the simulation results are due to the numerical implementation (finite element and Lagrangian advection scheme vs finite difference, fixed grid and eulerian advection scheme in the traditional VP approach)? What part of the results are due to resolved model physics (elasticity in this case) and ad hoc parameterization (the damage)? The current paper is the third of a series of three papers trying to address these questions. In the first one, Bouchat et al (2022) corrected non-reproducible results or unsubstantiated statements reported in Girard et al (2011) and Weiss and Dansereau et al. (2016), namely: "In particular, the VP rheology, currently used in most sea-ice models, has been shown to be unable to capture the properties of ice strain rates." and "... [the VP model] fails at reproducing the observed properties of sea ice deformation ...". In the second paper by Plante et al, (2022), the MEB was coded in finite difference on a fixed grid with an Eulerian advection scheme as a platform to address the first question. The main goal of the current paper is to disentangle the effect of resolved physics (plasticity in this case) from parameterized physics (damage) in the context of the VP model where both can be turned on and off independently. The no-cost, practical approach that can be coded easily in existing architectures is a secondary goal/outcome of the current work. This was stated in the abstract at line 1, in the introduction at lines 88–92, and has been reworded to convey this message more clearly. In the abstract at line 1 we have: ”We implement a damage parametrization in the standard viscous-plastic sea ice model to disentangle its effect from resolved model physics (visco-plastic with and without damage) on its ability to reproduce observed scaling laws of deformation.”

Nonetheless, one of the reviewers raised very important points that must be taken into account. Particularly damage refers to reduction of the elasticity, not plastic ice strength. As an Editor, I view it as my job to prevent the propagation of terms that are likely to cause confusion and are at odds with existing and mature bodies of work. As a result, I will not accept this paper if the word damage is used in its current form.

We agree. The distinction between the commonly accepted definition of damage and the definition used in the manuscript was not clearly spelled out. The goal of damage in models is to simulate large deformation for a given stress level when bounds between molecules are broken. In the context of linear elasticity theory, this can only be accomplished by reducing the shear or young modulus of the material. In the context of sea ice and plasticity theory, this can only be accomplished by including strain weakening (independently from divergence and a subsequent reduction of ice thickness and concentration). The term damage however is used in both communities and referred to as elastic and plastic damage, respectively (see for instance Lubliner et al., 1989; Friedlein et al., 2023; Jason et al., 2006; Voyiadjis and Taqieddin, 2008; etc. for (elastic-)plastic damage literature). 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.”

If the authors wish to proceed with revisions then careful account should be taken of all the comments by the reviewers, with particularly full responses given if a recommendation is not followed. But I reiterate that an alternative language must be found to express the reduction in ice strength and the word damage removed from the paper and title. The obvious term that springs to my mind is plastic weakening, which is what I believe it actually is.