Review of "Glacier damage evolution over ice flow timescales" by Meghana Ranganathan et al.

This study focuses on understanding the impact of glacier fracturing and damage on ice flow dynamics, particularly in the context of the Antarctic and Greenland ice sheets. The authors use a continuum damage mechanics (CDM) approach, representing damage as a scalar variable affected by stress and evolving over time (creation and advection of damage). They first propose a *"diagnostic damage model"* by assuming that the processes of damage creation occur on a short timescale with respect to the advection of this damage. They then validate their approach by showing that their hypothesis leads only to small error with respect to a *"transient damage model"* such as that of Pralong and Funck (2005). They then apply their model and compare it again with other implementations in the MISMIP+ glacier benchmark configuration, simulating grounding line retreat due to basal melt. Their results indicate that damage can enhance ice mass loss by 13-29% over a century (which would be equivalent to a 50% increase in basal melt rate), emphasizing the importance of including damage processes in large-scale ice sheet models for more accurate projections.

Overall, I find this paper to be well-constructed and clearly written, with figures that illustrate the main findings. The authors provide a detailed exploration of glacier damage evolution, and their model is applied thoughtfully within the MISMIP+ configuration to simulate grounding line retreat. While the quality of the work and the presentation are undeniable, I sometimes found it hard to switch from the main text to the supplement to get some specific information.

However, I have two primary concerns regarding the model implementation :

1. *Hypothesis for the diagnostic damage model, i.e., fractures accumulate much faster than they advect.*

   The choice of stress threshold in the model must have a significant impact on the results, as it directly influences where and how easily damage initiates. In the first experiment, the authors use $\sigma_t = 0.02$ MPa, a very low threshold that contrasts with higher values used in other studies (e.g., Krug et al., 2014; Sun et al., 2017), sometimes calibrated to match observed calving rates. While I understand that the model's focus is on damage accumulation rather than explicit calving, this low threshold promotes rapid damage development across broader ice regions than higher threshold values. This, combined with the feedback effect of damage on the source term (i.e., $f = f((1-D)^{-k})$, means that initial damage quickly propagates in subsequent timesteps. Given these sensitivities, a discussion of the implications for using a low σ_t, as well as the role of the damage rate factor B, would help better justifying the hypothesis.

   Without this, as a reader, I feel like the value picked for σ_t and B (I could not find the value) affect the realism of the hypothesis $\delta \gg 1$, leading to fast damage creation rate. It makes it look like the experiment is built to match the hypothesis. While I do not see any argument to revoke the hypothesis, I do not think either that the results validate the hypothesis–although later experiments seem to show little effect of σ_t which could be specific to the experiments? The sentence "*This agrees with the theory that where $\delta >$*

> 1, *fractures accumulate much more rapidly than they are advected away by ice flow*" is therefore a bit of a stretch to me. Since this hypothesis and the first experiment condition the definition of D_{acc}. I think that these limitations should be better discussed

2. *Unsymmetrical damage for a symmetrical geometry and numerical artifact:*

   Since the MISMIP+ configuration is symmetrical along the central flowline, any asymmetry in the modeled damage field (or other model output) may indicate numerical artifacts rather than genuine physical behavior. Artifacts of this nature often arise from issues like model stability (e.g., CFL condition) or other challenges in the advection scheme, particularly in handling diffusion-free processes like damage. In these cases, artificial diffusion or other stabilization techniques are often used, which can introduce numerical artifacts.

   I would suggest the authors discuss whether such factors could contribute to any asymmetry observed in their model results and clarify the type of stabilization methods employed, as well as any expected artifacts. Addressing these potential sources of numerical asymmetry (and diffusion) would strengthen numerical aspect of the study.

Regardless of these concerns, I believe this paper makes a valuable contribution to the field and certainly merits publication after what I consider to be relatively minor revisions. The study will be highly useful to the modeling community to integrate damage processes into ice flow models.

Specific comments

- Line 17: I found this first sentence a little strange, I guess that the authors refer to "viscous ice flow" as opposed to "elasto-brittle calving". Maybe consider rephrasing this.
- Line 30: the term "instability" is not really clear here. I would detail a little more the processes it refers to.
- Line 40: I would expand the ice sheet model timescale to as low as ~10^{-1} year. More and more models are interested in seasonal changes (without going to visco-elastic models interested in tidal effect, …).
- Line 60: I would change "accumulated fractures" for "accumulated damage" since CDM was used to enhance damage, until damage was deep enough to trigger a fracture over the entire column with LEFM.
- Line 72: switch the order of the two citations
- Line 102: "We evaluate the applicability"
- Line 106: I think that TC writes equations as this: "Eq. (1)". Think about correcting this for other references to your equations
- Equation 5: You therefore assume that $[t_a] = [t]$ to simplify your first term but isn't $[t]$ the resultant of the advective ($[t_a]$) and the fracture ($[t_f]$) timescale? If guess you assume $[t_f] \ll [t_a]$ to make the simplification but this relies on the hypothesis you do after. I might be missing something here.
- Line 130: how do you justify this hypothesis for a typical damage model?

- Line 135: could you precise if you use a linear or a non-linear (typically $m = 3$) Weertman friction law. I could not find the information in the supplementary material.
- Line 174: you mention $\sigma_t = 0.02$ MPa as an arbitrary value. Other studies use much higher values (at least about 0.1 MPa) (e.g., Krug et al. 2014; Sun et al., 2017). For Krug et al. (2014), these values have been calibrated with observations of calving rate. Although I understand that your model only aims to simulate damage (without going to calving or rifting), I think that the choice of σ_t is very important and will largely affects the result. Low σ_t allows to easily damage the ice in many regions, and due to the term $(1 - D)^{-k}$ initiated damage leads to even more damage at the next timestep. It also depends on the value of B in Eq. (8).
- Lines 216–224 and general statement about Section 3: I think that the limitation of your hypothesis is visible in your supplementary material where Damage seems particularly high and potentially overestimated. For example, your damage is much higher than Sun et al. (2017) but it might also be due to the difference in the criterion used to calculate the damage source () might also present biases due to other limitations in the physical model and its numerical implementation).
- You allow for your model to reach values of D up to 0.99 to avoid null denominator in the effective stress model and numerical stability and convergence issues. However, is $D = 0.99$ a realistic value for CDM. While D<1 is a numerical condition to avoid infinite ice fluidity, I think that for too high damage values, especially when over large areas and deep into the column, the CDM really shows its limitations as we continue to simulate something that is far from being continuous as continuous. Later you mention in Section 3 that you set up D_{max}~0.8. Can you precise if this is only for the MISMIP+ experiment and that you use $D_{max} = 0.99$ in the previous experiments?
- Line 230: I don't really understand the point of calculating the percentage difference in grounding line position after 1 year for different δ. You then check the same error after a longer time (e.g., 10^2– 10^4 years) for which your diagnostic model is supposed to be more valid, which to me is a much better way to look at the "error" of the diagnostic model.
- Line 266: you mention healing as a form of damage sink in the advection equation. You might consider mentioning that the right end side of Eq. (1) is a "damage source/sink" when you present the equation.
- Line 286: Could you precise the resolution of the mesh in the vicinity of the grounding line?
- Line 310: I would add a comma: "We note that in 3D, cracks are …"
- Lines 310–315 (this could actually be a third main concern):

  The model is Shallow Shelf (SSA) and therefore computes horizontal velocities considered constant over the ice column (plug flow). You can therefore assume null vertical velocities but this would greatly limit damage deep into the column (i.e., no vertical advection of the crevasses). You might also neglect "significant" vertical stress components, potentially leading to inaccurate stress calculations.

  Assuming mass conservation, you can recompute vertical velocities (based on surface and basal accumulation/melt and some assumption on the distribution) but you would need to assume a vertical distribution. Could you give more detail here?

Since a lot of the damage creates close to the grounding line where the SSA solution and the resulting stress computation is more prone to errors, what impact do you think the Stokes approximation has on the damage solution (you mention the role of longitudinal, lateral and shear deformation on crack closure later but not on the creation of crevasses/damage them/itself)? This would be interesting to discuss.

- Line 336: "SUpplement" → "Supplement"
- Line 346: I suggest "… and run the model for 100 years in two simulations".
- instead of "… and run the simulation for 100 model years in two simulations" .
- Line 515: The blurriness of sharp cracks is also due to stabilization techniques for the advection equation. Stabilization techniques often rely on artificial diffusion, even for small timesteps and or small CFL numbers.
- Line 523: From this statement, I understand that there is no advection of damage in the column except for the fact that if the ice base is melted, a larger part of the column could be affected by damage? As I said in a previous comment, a part of the "vertical advection" of the damage could therefore be due to a numerical vertical diffusion of the solution. I think this could be better discussed here.