The comments from the reviewer are in black font and our responses are in blue font.

I would like to thank the authors for their thorough response to both my main concerns and smaller comments. I believe that the revisions made to the manuscript will help the readers better understand the proposed damage model and its implications (e.g., plotting Figure 2 for a 1000-year simulation).

We absolutely agree that the recommendations by both reviewers and the editor significantly strengthened this paper, and we appreciate all the time and effort on the part of the reviewers.

As I previously commented, I believe the SSA limits the realism of the current model since it does not account for velocity and stress distribution over the ice column, and consequently, cannot capture vertical advection of damage. These limitations are acknowledged in the discussion, and I understand that this is beyond the initial scope of this work, which aims to introduce ?a simple damage model to simple ice flow models.? Using a higher-order model and comparing the results with the SSA would be an interesting follow-up study, which the authors seem to have considered, and which I look forward to read!

We also agree that the SSA limitations do hinder a full representation of some potentially important damage physics (such as, as the reviewer mentions, vertical damage advection), and we are eager in follow-up work to explore the implications of using a model that captures more of the three-dimensional behavior.

Upon- re-reading the manuscript, I came across the following sentence, and I would be interested to hear the authors? thoughts on it:

?The loss of load-bearing capacity in a continuous region across the ice shelf means that downstream ice transmits no buttressing stress upstream, thus representing the dynamic effect of calving on the remaining ice, even in the absence of explicitly simulated iceberg detachment. Some damage also accumulates in the margins near the grounding line and in the center of the ice shelf.?

I am not suggesting that this be discussed in the manuscript, but I wonder: for a model like yours, there do you think there is little need to explicitly simulate a proper calving front (like in CalvingMIP for example)? Since most of highly damaged areas, which should calve, have almost no impact on upstream flow?

This is an interesting point, and I think to some degree is correct; in our model, the "calving front" is delineated by the boundary between ice that has lost nearly all load-bearing ability (with full-thickness damage near 1) and ice that still retains some load-bearing ability and thus ability to transmit buttressing force upstream. The key would be to ensure that $D_{\max}$ is as close to 1 as possible to ensure very little force is transmitted upstream by regions that are supposed to be maximally fractured. The other question would be how to handle the ice-ocean interface; since, in our model, the maximally-damaged ice still exists, that ice is still interacting with the ocean and the true calving front (where the ice still has some load-bearing ability) is not engaging with the ocean at its terminus in the same way it should if the maximally-damaged ice was removed. I imagine particularly in coupled ice-ocean simulations, this would have significant implications to the dynamics of the ice sheet. Therefore, I think there are situations where actually simulating a moving calving front will be important, but in some studies (if you were interested in focusing on the effect of fracture on buttressing, for example), this may be a reasonable way of handling calving.

Technical suggestions:

Line 383: typo ?cressvassing? → ?crevassing?

Line 443: ?to to?

Line 587: The ice sheet model icepack uses the Shallow-Shelf Approximation (SSA), which calculates depth-averaged flow fields [?]

Done.