A review of "Damage strength increases ice mass loss from Thwaites Glacier, Antarctica" by Y. Li et al.

This manuscript aims to address an important question in ice sheet modeling studies, i.e., if and how much the ice damage affects the ice flow in some vunerable regions like WAIS. Thus, it is no doubt a valuable study and lies perfectly in the scope of TC.

General remarks:

- Despite the damage method has been used in some previous numerical studies, it is still necessary to compare the modeled damage field with observed crevass and rift images - I think it is critical to convince us how much we can trust the damage model results.
- If the authors aim to give a plausible projection of GMSL contribution from Thwaites Glacier, then it is necessary to use CMIP forcing data, from both atmosphere and ocean.
- And more details of forcing data and model configurations are needed. See the following details:

Details:

L10: damage is a result (or metric) of crevasses, not the reason.

L17: GMSL instead of sea-level rise

L21: again, damage is the result of rifts and crevasses

L29-39: the review has not included other studies, e.g., Duddu et al. (2020) and Kachuck et al. (2022), and probably many others, a big improvemt of this paragraph is highly necessary.

L40-54: It would be very helpful if you can provide an image showing the crevass distribution across Thwaites Glacier along with the current Fig 1.

L55: Here I think there probably lacks a paragraph describing what diagnostic and prognostic modeling studies we currently have, and what kind of problems in those studies have by not including the damage mechanics in their models, before you move on to this paragraph introducing your solutions.

L76: "zero-stress assumption" might be better

L79: do not understand how you get d1(tau1) even after looking at Appendix 1. Intuitively, it looks like to be min((ds+db), C1*h), i.e., the min value between the total crevass depth and the limit you set. Can you provide more explanations?

L80: remove the comma after where

L83: remove extra () for d(tau1)

L84: I need more details of d1(tau1) to understand this equation

L85: remove the comma after where

L92: change "steady state" to "steady-state", same for other places

L102-109: have you compared the modeled damage results to the observed crevasse distribution from satellite image? I think this is also important.

L110-116: What are the forcings for the experiments Ctrl and Ctrl_cal? Do you calibrate the forcing data for Ctrl_cal in order to reproduce the historical trend of ice mass change? If so, how do you do the calibration? What is the time span for the historical runs?

L119: what is the RMSE for grounding line position?

L120: I do not follow the sentence "At the start of the historical run, the present-day SMB is reinstated without the additional mass-change term". Can you explain it a bit more?
L124: So no CMIP projection forcing data? Then we should be careful to conclude a GMSL contribution from this study, as it is more like a comparison (damage v.s. no damage) study.
L129: how do you do with the basal melt rates for previously grounded the regions after they become floating as GL retreats? Do you couple the PICO model with the ice sheet model?
L141: 43-member?
Section 3.1: There are something I do not understand in this part. For Ctrl_cal, you can actually calibrate the forcing and let the modeled and observed mass change match each other, even you do not turn on the damage mechanism, correct? But from Fig 2a, clearly there is still some disagreement between Ctrl_cal and the observations. Why is that? For G1 and G2, basically what you do is damage parameter calibration, and you can find some parameter combinations that can give a good model output. But how can you tell the difference of model and observations is not from the bias of the forcing data you use, but is due to the damage mechanism? That is the point I am still confused. That is another reason that I think a comparison between modeled damage value and obserbed crevasse distribution is necessary. The current form of Fig 2 needs improvements too. It is hard to tell those curves for G1 and G2. I would suggest to keep only several curves that are close to observations, and put the whole ensemble somewhere in the Appendix.
L176-192 and Fig 3: So this part explains again my concern for Section 3.1. The RMSE of Ctrl_cal is even smaller than G1-G15. Does that mean we can calibrate the forcing data, e.g., basal melt rates, to get a better hindcast modeling result than tuning the damage parameters, or can we say that forcing is more important than damage? In Fig 3, I would also like to see the comparison of modeled and observed ice thickness data, which is a also very important information, or you might consider to add an additional figure for thickness.
Section 3.2: I'll hold my opinions for this section for now, as I do not see much information about SMB and basal melt forcings, e.g., if you use a coupling scheme between ice sheet and ocean model or you use a some parameterization approach. Before I have these information, I can't tell how meanful the projection numbers are in this section.
Figure 6: you do have the current damage field. Then I think you probably want to compare them with some satellite images of crevasses. I think it is important for us to understand if the damage method you use is valid or not.

References:

Kachuck SB, Whitcomb M, Bassis JN, Martin DF, Price SF. Simulating ice-shelf extent using damage mechanics. Journal of Glaciology. 2022;68(271):987-998. doi:10.1017/jog.2022.12,

Duddu R, Jiménez S, Bassis J. A non-local continuum poro-damage mechanics model for hydrofracturing of surface crevasses in grounded glaciers. Journal of Glaciology. 2020;66(257):415-429. doi:10.1017/jog.2020.16