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Responses to comments from Reviewer Ravindra Duddu

Damage intensity increases ice mass loss from Thwaites Glacier, Antarctica

We thank the editor and the reviewer for their constructive feedback and continued support, which has helped us improve the manuscript. In response to the comments, we have thoroughly revised the manuscript. Further details are provided in our responses below. In the following, we use "**bold text**" for the reviewer's comments, "regular" text for our responses, and "*italic*" for text extracted from the manuscript.

Comments to the Author

This is the second time I am reviewing this article. The authors have adequately responded to both reviewer comments and extensively revised the article. The figures and the text in the article are much clearer, but there are still some notation inconsistencies and a few statements that need revision. As this is first of a kind study exploring the effect of damage on ice flow of a real (Thwaites) glacier, I recommend it for publication after the following comments are addressed. However, there are discrepancies between observations and simulation results, lack of understanding of how represent basal crevasse formation, and the damage model has a few deficiencies (due to the zero-stress approximation), which requires further research. This could be elaborated in the discussion and conclusion sections better.

Response:

We sincerely thank you for your thoughtful suggestions and constructive comments, which have been highly valuable in improving the quality of this manuscript. We also appreciate your thorough re-evaluation of the revised manuscript. We have carefully revised the manuscript based on your suggestions and comments. Please find our detailed responses to each of your specific comments below.

Detailed Comments:

1) **The following two sentences in the Introduction near Line #45 need to be revised.**
   - "They have one critical limitation, i.e., being diagnostic, which means that they investigate the instantaneous effect of damage on ice dynamics, but not the evolution of damage when ice thickness is allowed to evolve according to the applied changes."
   In the references cited in this paragraph, Huth et al. (2021, 2023) and Duddu et al (2020) do consider ice thickness changes. Huth et al solve the shallow shelf equations including the ice thickness evolution and Duddu et al. use an updated

Lagrangian implementation to account for ice thickness evolution. However, because these works considered smaller time scales (months to years) there is no significant effect of damage on ice flow. Therefore, the above statement by the authors is not true in general.
- "They therefore fail to predict future ice sheet behavior or feedbacks induced by external changes, such as fracture enhancement due to atmospheric or oceanic forcing"
At least in the case of Huth et al. (2023) this is not true. Some studies just focused on the short time scales corresponding to rifting or crevasse propagation, over which ice thickness changes are not so significant.

Response:

Apologies for the inaccurate description. We have revised the relevant sentences in the **Introduction** section as follows (Italicized and underlined text highlights sentences that have been modified in the revised manuscript):

(Line 42–52 in the revised manuscript without tracks): "*These studies reveal the interaction between damage processes and observed ice flow dynamics. Several of them (e.g., Borstad et al., 2012; Albrecht and Levermann, 2014; Gerli et al., 2023; Sun and Gudmundsson, 2023) have one critical limitation, i.e., being diagnostic, which means that they investigate the instantaneous effect of damage on ice dynamics, but not the evolution of damage when ice thickness is allowed to evolve according to the applied changes. They therefore fail to predict future ice sheet behavior or feedbacks induced by external changes, such as fracture enhancement due to atmospheric or oceanic forcing. In contrast, more recent studies have integrated damage evolution into ice flow models to investigate fracture processes and their influence on ice dynamics, including the effects of ice thickness evolution (e.g., Duddu et al., 2020; Huth et al., 2021, 2023). These efforts primarily focus on relatively short time scales (months to years), during which ice thickness changes have limited influence on ice flow. As a result, while they shed light on instantaneous responses such as rifting or crevasse propagation, their ability to simulate long-term ice sheet feedbacks under sustained climate forcing remains limited.*"

2) **There are several notation inconsistencies as per the continuum mechanics. I am listing a few below that could identify:**

Response:

We have revised the relevant context and equations as you suggested.

- In Eq. 1 and thereafter \tau and \epsilon are tensors so \boldsymbol should be used in LaTeX, whereas the equivalent stress is a scalar to which the n-1 is applied as exponent, so \boldsymbol should not be used here. In general vector and tensor fields are denoted using bold letters whereas scalars are denoted by

normal letters.

$$2A\tau^{n-1}\boldsymbol{\tau} = \dot{\boldsymbol{\epsilon}} \ , \qquad (1)$$

- In Eq. (2) the notation D(\tau) implies that damage D is a function of the deviatoric stress, but this fundamentally wrong. First, the definition of this 3D damage function was never even defined as eventually the depth integrated damage d based on the zero-stress theory was used. Second, the 3D damage must be defined based on the principal stress or stress invariants and not based on a component of deviatoric stress. You can refer to Pralong and Funk (2005) or Duddu et al. (2013) for more details on this. Simple fix is to say damage D without \tau in parentheses.

$$2A\boldsymbol{\tau}^{2}\boldsymbol{\tau} = (1-D)^{3}\dot{\boldsymbol{\epsilon}} \ , \qquad (2)$$

- There are so many steps that are missing from Eq. (2) to Eq. (1) to Eq. (3), so I wonder why it is even necessary to have Eqs. (1) and (2). Perhaps, just directly start with how shallow ice flow model works and how damage is incorporated into the Kori-ULB model. Also, the parameter A is inversely related to the viscosity \mu, so I find it confusing that Eq. (3) has both A and \mu in it.

Apologies for the confusion. Damage D is incorporated into the stress balance equation through a modification to Glen's law, which described the relationship between the deviatoric stress τ and the strain rate $\dot{\epsilon}$ in the Kori-ULB model. To avoid confusion, we have removed equation (3) and revised the relevant description to clarify more logically how damage is incorporated into the Kori-ULB model.

(Line 87–118): "

The Kori-ULB ice-sheet model (Pattyn, 2017; Coulon et al., 2024) is a vertically integrated, thermomechanical finite difference model that combines shallow-ice approximation with shallow-shelf approximation (so-called hybrid model; Winkelmann et al., 2011). The Kori-ULB ice-sheet model has been used for large-scale simulation of the AIS (Seroussi et al., 2020; Coulon et al., 2024), as well as small drainage basins with different ice geometries, such as MISMIP3d (Pattyn et al., 2013) and MISMIP+ (Cornford et al., 2020) experiments, and real-world drainage basins (e.g., Thwaites Glacier basin; Kazmierczak et al., 2024).

To investigate the dynamical response of the TG basin to ice damage and damage parametric perturbations, we couple the ice-sheet model with the continuum damage mechanics (CDM) model developed by Sun et al. (2017). This model establishes a direct link between the amount of damage and ice viscosity: the propagation of damage reduces the ice viscosity through Glen's flow law, leading to faster ice.

Following Sun et al. (2017), we represent damage using a scalar variable D, which takes values from 0 (undamaged ice) to 1 (ice entirely fractured by surface *and basal crevasses). The vertically averaged damage field is defined as $\overline{D}(x, y) = d(x, y)/h \in [0,1)$, which is the closest analogue to the usual D, with $d(x, y) \in [0,h(x, y))$ represents the vertical integral of D. Damage is incorporated into the stress balance equation through a modification to Glen's constitutive flow law. In Kori-ULB, the relationship between the deviatoric stress τ and the strain rate $\dot{\epsilon}$ is described by Glen's constitutive flow law:*

$$2A\tau^{n-1}\boldsymbol{\tau} = \dot{\boldsymbol{\epsilon}}\ , \qquad (1)$$

where A is Glen's flow law factor, dependent on the ice temperature, and n is the flow rate exponent, with n =3. And the damage feedback can be described by the integration of a damage factor D in Eq. (1):

$$2A\boldsymbol{\tau}^2\boldsymbol{\tau} = (1-D)^3\dot{\boldsymbol{\epsilon}}\ , \qquad (2)$$

To determine the relationship between ice damage and the first principal stress, the CDM framework is based on two key components: a local source of damage term (d_l) that accounts for the local formation of damage, and an advection term (d_{tr}) that accounts for the transport of damage during ice flow.

In the absence of advection, ice damage is expressed as the normalized depth of the crevasses, i.e., the sum of surface crevasses d_s and basal crevasses d_b (Nick et al., 2011, 2013; Cook et al., 2014; Sun et al., 2017). Those can be calculated by the zero-stress assumption (Nye, 1957; Nick et al., 2011):

$$d_{\mathrm{s}} = \frac{\tau_1}{\rho_{\mathrm{i}} g} + \frac{\rho_{\mathrm{mw}}}{\rho_{\mathrm{i}}} d_{\mathrm{w}}, \qquad (3)$$

$$d_{\mathrm{b}} = \frac{\rho_{\mathrm{i}}}{\rho_{\mathrm{sw}} - \rho_{\mathrm{i}}}\left(\frac{\tau_1}{\rho_{\mathrm{i}} g} - H_{\mathrm{ab}}\right), \qquad (4)$$

where d_w is the water depth in the surface crevasse (here we only consider dry crevasses, so d_w is equal to 0), H_{ab} is the thickness above floatation, $g = 9.81\ m\ s^{-2}$ is the gravitational acceleration, $\rho_i = 917\ kg\ m^{-3}$ is the ice density, and ρ_{mw} and ρ_{sw} are the densities of meltwater in the surface crevasses and seawater in the basal crevasses, respectively (both set to 1028 $kg\ m^{-3}$ in this study). The first principal stress τ_1 is defined as the product of the first principal strain ($\dot{\epsilon}_1$) and the effective ice viscosity (μ):

$$\tau_1 = 2\mu\dot{\epsilon}_1, \qquad (5)$$

"

- In Eq. (4) and (5) to be consistent the density of meltwater in the surface crevasses must be denoted differently from that of seawater in the basal crevasses. Although I understand that this study did not include meltwater in surface crevasses.

We have revised Eqs. (4) and (5) (now Eqs. (3) and (4)) as you suggested. Please refer to our response to the previous comment as well as the revised manuscript for more details.

- In Eq. (6) remove the star to denote multiplication, just writing C_1 h without the star implies multiplication in standard notation.

$$d_1(\tau_1) = \min(d_\mathrm{s} + d_\mathrm{b}\,, C_1 h), \qquad (6)$$

- In Eq. (7) the velocity term \bold{u} is not defined. Is it a 3D or 2D velocity field? This is important as this the divergence of 3D velocity is zero but not the 2D field. Also, the terms max and min should not be italicized as they are text descriptors, and subscripts such as tr, ab, b, s should not be italicized in the equations as they are descriptors and not indices. Whereas, on Line 122, below Eq. (7), \dot{m} must be italicized as it is a variable denoting basal melting rate.

Thank you for pointing this out. The velocity term $\boldsymbol{u}$ is the two-dimensional horizontal velocity, and we have now clarified this in the revised manuscript. In addition, we have revised Equation (7) and ensured consistency in the formatting and notation of all relevant terms throughout the manuscript, as suggested.

(Line 126–130): "

$$\frac{\partial d_\mathrm{tr}}{\partial t} + \nabla \cdot (\boldsymbol{u} d_\mathrm{tr}) = -[\max(\dot{a}, 0) + \max(\dot{m}, 0)]\frac{d_\mathrm{tr}}{h}, \qquad (7)$$

where $\boldsymbol{u}$ is the two-dimensional horizontal velocity. The left-hand side of Eq. (7) represents the conservation of vertically integrated damage, which includes the advection of crevasses with the ice flow and the effect of stretching and compression. On the right-hand side, damage reduction is modeled through two processes: an increase in undamaged ice thickness due to surface accumulation ($\dot{a}$) and erosion of the crevassed ice bottom by basal melting ($\dot{m}$)."

3) On line 103, it is stated that "… ice damage is expressed as the total depth of crevasses …" I suggest you say – normalized depth of crevasses – as damage is non-dimensional variable so it cannot be equated to ice thickness.

Response:

We have revised the description as you suggested.

4) On lines 180 and 181, the RMSE and rRMSE are denoted. I think it is better to say difference or deviation instead of error, so RMSD and rRMSD. In numerical modeling, the term error means something specific – the difference between the exact analytical solution and the approximate numerical solution.

Unless I misunderstood, you are reporting here are the differences between different model results.

Response:

Thank you for your suggestion. In this study, we use error as the difference between simulations and satellite observations (though models are used to obtain those simulated values). In a strict sense it is the closest to an analytical solution we can get. In most publications in our field the term RMSE is more commonly used than RMSD. For these reasons, we have retained the term "error" in the revised manuscript.

5) **On line 206 – it is stated "For the period 1990–2020, the simulated mean net mass balance for Group 1 (with damage) is -26.5 Gt a-1, which is comparable to satellite-derived observations (-46.1 ± 7.2 Gt a-1 over 1992–2017; mean ± 1 s.d.)." It seems the satellite observations are two times larger than the simulated mean net mass balance for Group 1. Please clarify in the text here what are the reasons for this mismatch. Also, why wasn't the group 2 mass loss reported here.**

Response:

Thank you for your thoughtful comment. In this study, the Group1 and Group2 experiments are primarily distinguished based on their ability to match satellite-derived estimates of sea-level contribution (SLC) by the year 2020, rather than directly on net mass balance. We clarified this in the third paragraph of the **2.2 Simulation protocol** section, as well as summarized in Table 1 of the manuscript, where more detailed information is provided.

(Line 152–156, the third paragraph in the **2.2 Simulation protocol** section): *"Based on their performance during the historical simulations, ensemble members are categorized into two subgroups according to their ability to match satellite-based estimates of ice mass change in the TG basin (Shepherd et al., 2019). Simulations where the modeled ice mass change (i.e., the contribution to sea level, SLC) falls within the satellite-derived mean estimate ± two times the observed standard deviation (s.d.) are classified as Group 1 (G1). Those that significantly over- or underestimate this range (>±2 s.d.) are classified as Group 2 (G2)."*

Our results show the simulated SLC of Group 1 (with a mean of 0.24 ± 0.04 cm SLE) is comparable to the satellite-derived observations (0.24 ± 0.08 cm). In contrast, Group 2 consists of simulations that significantly overestimate or underestimate the observed range, with the mean of 0.62 ± 0.36 cm SLE—approximately 2.5 times larger. As for the net mass balance, although there remains a discrepancy between the satellite observations and the simulation results of Group 1, the mean net mass balance in Group 1 (-26.5 Gt a^{-1}) is considerably closer to satellite-derived estimates (-46.1 ± 7.2 Gt a^{-1}) than simulations that neglect the damage process (1.2 Gt a^{-1}), or those in Group 2, which can reach −169 Gt a^{-1} under extreme damage scenarios. Given the inherent uncertainties and simplifications in ice-sheet modeling, it is challenging for simulations to exactly reproduce observational data. Therefore, in this study, we focus primarily on the performance of our historical simulations that can capture the sea-level contribution rather than the net mass balance.

We have not discussed the result of Group 2 in detail in the main text. The simulated SLCs from Group 2 are outside the range of observational estimates ± 2 s.d. in the historical simulation. Therefore, we consider the parameters used to represent the damage process in Group 2 experiments to be unrealistic. Nonetheless, they are included in all relevant figures for comparison, allowing us to evaluate differences between the two groups.

6) **If Fig8. even in the extreme experiments the SLC is less than 18 cm by 2200. Thwaites is referred to as the doomsday glacier in some news articles, perhaps a comment can be added on this result in the context of how catastrophic the projected SLC of 18 – 24 cm is.**

Response:

Thank you for your comment. In our simulations, we use present-day atmospheric and oceanic forcing in our forward simulations rather than projections based on future climate scenarios. Our results show that, even under extreme damage scenarios, sea-level contribution (SLC) increases from 1–2 cm (without damage) to approximately 18 cm by 2200. Therefore, by considering the combined effect of damage and future climate change, Thwaites Glacier is likely to reach MISI if the grounding line retreats too much. It is not a matter of sea-level content, but rather, sea-level rise acceleration, and we do observe acceleration.

We have added some sentences to discuss this limitation of our study in the **Discussion** section of the revised manuscript as follows.

(Line 446–451): "*In our forward simulations, present-day atmospheric and oceanic forcing are applied, rather than projections based on future climate scenarios. Our results show that, under extreme damage scenarios, the sea-level contribution (SLC) from Thwaites Glacier increases from 1 cm (without damage) to approximately 18 cm by 2200 (Fig. 8a), indicating that damage alone can substantially accelerate mass loss leading to collapse. This suggests that the combined effects of ice damage and future climate changes could further enhance mass loss from Thwaites Glacier, reinforcing its potential role as a major contributor to future sea-level rise.*"

7) **On line 360, it is stated that "As damage is advected with the ice flow, this**

fraction increases toward the ice front, reaching 0.3 in lower-damage cases to 0.7 in higher-damage cases, with particularly high damage concentrated in the shear zone." My intuition was that the increasing strain rate toward the ice front is the major contributor to this damage increase and not the advection. Please clarify the relative contribution of advection and damage nucleation to the increase in damage downstream.

Response:

Apologies for the confusion. We agree that both advection and strain-rate-dependent damage nucleation contribute to the downstream increase in damage. In our model, damage evolves through two primary mechanisms: (1) local damage nucleation and growth, which is strongly controlled by the strain-rate regime and (2) advection of existing damage with the ice flow. While our original wording emphasized advection, we acknowledge that strain-rate-driven damage nucleation could also play a dominant role, especially near the ice front where tensile and shear strain rates are elevated—particularly within shear margins. However, due to the strong interaction between the advection and damage nucleation in our model, we currently lack an effective method to explicitly decipher their relative contributions to the increase in downstream damage in this study. For instance, damage advection not only directly affects the final damage field, but also indirectly impacts it by affecting the local strain rate (and ice velocity), and the subsequently local damage nucleation. In other words, the local damage nucleation due to high strain rate has already been affected by damage advection. Conversely, local strain rate can also affect damage advection. Therefore, it is not easy to quantify the relative contributions of local damage nucleation driven by increased strain rates and damage advection to the final damage field.

To avoid confusion, we have revised the corresponding sentence to more accurately reflect the combined influence of these two processes:

(Line 372–374): "*As ice flows toward the front, damage increases due to the combined effects of advection and local damage nucleation driven by increased strain rates. Damage fractions range from 0.3 in lower-damage cases to 0.7 in higher-damage cases, with particularly high damage concentrated in the shear zone.*"

8) **On line 409, it is stated "Instead of solely relying on ice sheet mass loss data, future efforts should incorporate observational datasets of crevasse distributions." I do not disagree with this suggestion but there is a more nuanced discussion missing here. From our modeling studies, we find that most of the damage in ice shelves must be in the form of basal crevasses, especially if there is no hydrofracture in surface crevasses. Due to the ocean water pressure at the terminus, there is simply not enough driving force in floating ice shelves to propagate surface cracks deeper into the ice below the waterline.**

Our studies on Larsen C ice shelf in Huth et al. (2023) indicate that rift propagation is almost entire driven by basal crevasses formation. Also, Clayton et al. (2024) shows that crevasses can propagate deeper in ice shelves due to the less dense firn layers near the top surface. There are no observations of basal crevasses, and the extent of firn layer is not so well quantified that can rightly inform future modeling efforts. My comment is that this requires basin/ice-shelf scale process modeling (e.g. full Stokes and phase field fracture) to better understand and represent basal crevasses evolution.

Clayton, T., Duddu, R., Hageman, T., & Martínez-Pañeda, E. (2024). The influence of firn layer material properties on surface crevasse propagation in glaciers and ice shelves. The Cryosphere, 18(12), 5573-5593.

Response:

Thank you for this constructive comment. We agreed with your important insights regarding the critical roles of basal crevasse formation and the firn layers near the top surface in driving rift and crevasses propagation within ice shelves. We have revised the relevant sentences to include this in the discussion here.

(Line 423–429): "*Instead of solely relying on ice sheet mass loss data, future efforts should incorporate observational datasets of crevasse distributions. A critical limitation remains the lack of direct observations of basal crevasses and the uncertainty in quantifying firn layer structure, both of which hinder accurate representation of damage processes. Recent studies suggest that basal crevasses may play a dominant role in damage evolution, particularly in the absence of surface hydrofracture (Huth et al., 2023), while firn properties can also influence crevasse penetration depth in ice shelves (Clayton et al., 2024). Therefore, future efforts should also prioritize high-resolution, basin- or ice-shelf-scale process modeling—such as phase-field fracture models—to better understand and represent the evolution of basal crevasses.*"

9) **On lines 412, it is stated "hindcasts for 1990–2020 (Schimdtko et al., 2014; Kittel et al., 2021) do not necessarily reflect the actual imbalance of the ice sheet during that period." I did not understand this sentence. Please explain this in more detail. What does it mean to do hindcast simulations and which figures in the paper show these results.**

Response:

Sorry for the confusion. We have deleted this sentence.

10) **On line 424, it is stated that "Our results suggest that ice damage could be a key driver of Thwaites Glacier's rapid ice loss, offering an alternative explanation to previous hypotheses." Please clarify what is meant by rapid ice**

loss. The sea level contribution is 18 - 24 cm by 2300 in the extreme scenarios, which is significant, but does it warrant the use of the term "rapid."

Response:

Thank you for pointing this out. To avoid confusion, we have deleted the term "rapid".

11) Line 440, instead of saying "increasing damage intensity …" it is clearer to say "an increase in damage intensity …"

Response:

We have revised the sentence as you suggested.

12) Overall, the figures are quite well made, and the caption are comprehensive and informative. However, I have a few minor questions or suggestions below.

- I do not understand what the length white-gray, black-gray bars in subfigures 1(a), 1(b) and 1(d). Also, what the black regions in (a) and (b), are these the ocean regions or grounded regions that are removed from the images.

Response:

Thank you for your comment. The white-gray, black-gray bars in subfigures 1(a), 1(b), and 1(d) are linear scale bars, depicted as marked line segments with numerical labels that visually convey the map's scale. For example, in Fig. 1a, the bar is labeled 0, 50 km, and 100 km, indicating that each segment represents 50 km of actual distance. We have added a sentence in the caption of Fig.1 to explain the white-gray and black-gray bars.

(Line 207): "*The black-gray bar in panel (a) and white-gray bars in panels (c) and (d) are scale bars of the corresponding maps.*"

The black regions in (a) and (b) are the ocean regions shown in the Landsat Image Mosaic of Antarctica (LIMA; Bindschadler et al., 2008).

- In Fig. 2a, I do not see the black line corresponding to observational estimates.

The observational estimate and its ±1 standard deviation range are represented by vertical black lines on the right side of Fig. 2a. We have revised the figure caption to explicitly state this.

(Line 239–241): "*The vertical black lines on the right side of panel (a) represent the observed mean value ± 1 standard deviation (Shepherd et al., 2019). The black line and shaded area in panel (b) represent the observed mean values ± 1 standard deviation (Shepherd et al., 2019).*"

- In Fig. 10 there are positive ice thickness changes (red regions in second and third figure columns) ahead of the ice shelves. Is that due to ice mélange changes or is that something that is unphysical. Please clarify in the caption.

Thank you for your comment. The positive thickness change ahead of the ice shelves is due to an advance of the calving front. Typically, in higher-damage simulations, there is a strong increase in velocity and hence a following advance of the calving front. To clarify this, we have added the following sentence to the caption of Fig. 10:

(Line 387–389): "*The positive thickness change observed ahead of the ice shelves is caused by the advance of the calving front, which results from increased ice velocity under higher-damage scenarios.*"

In the Kori-ULB ice-sheet model, calving at the ice front depends on the combined penetration depths of surface and basal crevasses relative to total ice thickness. The depths of the surface and basal crevasses are parameterized as functions of the divergence of ice velocity, the accumulated strain, the ice thickness, and (optionally) surface liquid water availability, similar to Pollard et al. (2015) and DeConto and Pollard (2016). In this study, we used this calving parameterization, but without considering hydrofracturing. A description of how calving is implemented in our model has been added to the revised manuscript for clarity:

(Line 165–168): "*Calving at the ice front is determined by the combined penetration depths of surface and basal crevasses relative to ice thickness, with crevasse depths parameterized as functions of ice velocity divergence, accumulated strain, ice thickness, and (optionally) surface liquid water availability, similar to Pollard et al. (2015) and DeConto and Pollard (2016).*"

Pollard, D., DeConto, R. M., and Alley, R. B.: Potential Antarctic Ice Sheet retreat driven by hydrofracturing and ice cliff failure, Earth and Planetary Science Letters, 412, 112–121, doi:10.1016/j.epsl.2014.12.035, 2015.

DeConto. R. M. and Pollard, D.: Contribution of Antarctica to past and future sea-level rise, Nature, 531, 591-597, doi:10.1038/nature17145, 2016.