**Reply to Reviewer 1**

"Numerical Modeling of Ice Detachment Tipping Processe: Insights from the Sedongpu Glacier, Southeastern Tibetan Plateau" by Zhang et al. (2025) uses a numerical modeling framework to demonstrate an ice weakening-basal slip feedback that results in the rapid detachment of Sedongpu Glacier which was observed in 2018.
To the authors, thank you very much for responding to all my comments in the last review. The explanation of the science you did is so much clearer now and the manuscript is much better organized. The sections flow much more effectively together. Now, readers can much better appreciate the novelty and excitement of this paper! I think the paper is well on its way to being accepted for publication, but I'd like to offer some more minor revisions.

Thank you very much for your time and efforts in improving our manuscript!

One remaining thing I would like the authors to clarify is the following. There have been some references to it in the updated manuscript, but I think it should be fleshed out a little bit more: What exactly is it that triggers the dramatic ice weakening mechanism mentioned in L213? Obviously, in real life, you cannot simply "switch on" a new piece of physics (i.e., the ice weakening-basal slip feedback) – so what natural event(s) could cause this mechanism to be activated? You have made it clear that activating this mechanism with a high initial yield strength will not trigger glacier detachment, but reasons the mechanism itself is activated are not as clear. I would love to see this a little more fleshed out in the Discussion section. It would also be nice to include a little discussion on why glacier detachment occurred at Sedongpu Valley at this particular area, yet is relatively uncommon (so far) in other glaciers of High Mountain Asia. If you don't know, it would be great to identify this as an important knowledge gap.

This is a very nice piece of advice. This is exactly why we started this project in the first place! In this paper, we know the detachment hazard could be triggered by the ice weakening-basal slip positive feedback if ice stress exceeds the yield strength. Next step, we plan to evaluate the surface and internal stress field across those glaciers in the neighboring region based on remote-sensing data and a more complicated (probably a 3D thermomechanical model) modeling approach. We could then use the tipping initial yield strength for Sedongpu to start with, and possibly adjust the tipping threshold a bit according to observations. We now add more details in the last paragraph of the Conclusion section.
"To advance this study, future efforts should extend the current two-dimensional model to three dimensions and further investigate the relationships between Iken's bound of the glacier bed and basal sliding/hydrology. By developing a more advanced numerical modeling approach, we can study glaciers in neighboring regions and estimate changes in their surface and internal ice stresses using in-situ and remote sensing observations. Subsequently, we could assess the detachment risk of surrounding glaciers, or even on a larger scale, by examining the relationships among ice stress, yield strength, surface landforms (e.g., crevasses), and basal sliding features."

Additionally, in the first set of reviews I misunderstood the authors' definition of "effective stress" because it was not defined in the text. It was not until they actually provided their definition in the second draft that I understood where this misunderstanding came from. Many glaciologists will first associate the phrase “effective stress” or “effective pressure” to be N, the overburden pressure minus water pressure (rho g h – p_w), which is directly related to ice speeds in sliding laws such as Budd’s and Schoof’s, although what you are referring to as “effective stress” is technically the correct term. I think you may want to add a sentence clarifying this by emphasizing the relationship between effective stress and deviatoric stress. Ideally, I would consider referring to deviatoric stress instead of effective stress in your discussions, as this will avoid confusing readers for whom the concept of deviatoric stress is more intuitive. Alternatively, please include a small note such as “(Note: effective pressure here is defined from effective strain rate and is not related to the effective pressure N)”.

The deviatoric stress has 6 independent stress component, as the Glen’s law shown below.

$$\sigma'_{ij} = 2\eta\dot{\epsilon}_{ij}, \quad \eta = \frac{1}{2}A^{-1/n}(\dot{\epsilon}_e + \dot{\epsilon}_0)^{(1-n)/n},$$

The effective stress is actually a nice value representing the overall contribution of the deviatoric stress. I agree with the editor’s opinion for this case, I think the current phrase/definition is clear enough.

Specific/minor comments

A small request: When adding new information (citations, clarification, new phrasings, etc.) in response to comments, please copy those changes into the response document and write what line they are at in the updated manuscript.

Thanks for this nice suggestion.

L4: “incorporating a positive feedback mechanism between ice stiffness and basal slip”

Changed.

L40: I think you can probably just cite Bassis et al. (2021)

Changed.

L42: “understandings” -> “understanding”
The introduction generally flowed so much better than it did in the first draft. I am able to understand your motivation for the science much better than before. Your explanation of glacier detachment is also much more effective than before.

Changed, and also thanks to you for your nice suggestions in the previous revision step.

L45: You can probably take out the last sentence

Removed.

L63: Delete “Glacier topography:”

Deleted.

L73: ensure that your in-text citation is formatted correctly with parentheses

Corrected.

L77: What kind of regularization does the program use? If just a simple Tikhonov, you could just say “…a linear optimization problem with a Tikhonov smoothness regularization to produce…”

Thank you for your question. We have clarified this point in the revised manuscript. The GlaTE framework applies a form of smoothness regularization consistent with the Occam's razor principle, aiming to identify the simplest solution that fits the observational and modeling constraints. This is implemented through a smoothing matrix as described by Langhammer et al. (2019), rather than explicitly using a classical Tikhonov formulation. We have revised the text to better reflect this and avoid potential confusion.

Revised text:
We estimated local ice thickness by calculating elevation differences between the pre-detachment glacier surface and the post-detachment exposed bed topography at locations where substantial ice detachment occurred. These values provided first-order estimates of ice thickness and were used as discrete constraints in the GlaTE software (Langhammer et al., 2019), which infers distributed ice thickness by optimally combining observational data with glaciological modeling in an inversion framework. The modeling component follows the method of Clarke et al. (2013), which approximates basal shear stress as a function of surface slope and apparent mass balance under a shallow-ice assumption. The inversion is formulated as a linear optimization problem with smoothness regularization, implemented via a smoothing matrix to enforce structural simplicity in the solution. We provided the estimated thickness points, a DEM, and the glacier outline as inputs to GlaTE. After obtaining the distributed ice thickness, we extracted the glacier geometry along the main centerline, which was generated following the method proposed by Kienholz et al. (2014). This flowline geometry was then used as input for the PoLIM simulations.

L80: what are the errors associated with the cross-correlation method for generating surface displacements? This could be mentioned here or in the section where you talk about other model limitations.

Thanks for your suggestions. We add below sentence in this part: “The uncertainty of surface velocity was obtained by calculating the mean displacement (5.26 m; 5.01 cm/d) from the non-glacial test areas.”

L88: Thank you for providing more details about the model! These are very useful.

Thank you for your nice review.

L90: I asked in the last review about the thermal constraints on the model. Please explain in the text that you neglected thermal evolution and kept A constant, and that there is no geothermal flux (as you wrote in your responses).

We now add more details here: "Sedongpu Glacier is a typical maritime glacier in southeastern Tibet. In this study, we assume Sedongpu Glacier is temperate and set A as a constant for ice temperature close to 0 ℃ (Cuffey and Paterson, 2010) (Table 1), i.e., we do not include a temperature solver in our model. "

Figure 1d: It would be great to overlay the outline of the glacier on this panel so we can contextualize these surface elevation changes

Thanks for the advice. Fig 1d is now improved.

L102: Assuming the definition of effective strain rate in Cuffey & Paterson Eq. 3.17, your definition of effective strain rate does not make sense. If your model is 2D, it doesn't make sense that you'd have strains in 3 directions. Also, not sure where the third term (cross term) on the right-hand side comes from. Could you please clarify why you have defined effective strain rate this way?

Thanks for pointing this out. You are right, the current one is the definition for 3D. Now we correct this with the following 2D form:

$$\dot{\epsilon}_e^2 \simeq \left(\frac{\partial u}{\partial x}\right)^2 + \left(\frac{\partial v}{\partial y}\right)^2 + \frac{\partial u}{\partial x}\frac{\partial v}{\partial y} + \frac{1}{4}\left(\frac{\partial u}{\partial y}\right)^2 + \frac{1}{4}\left(\frac{\partial u}{\partial z}\right)^2.$$

This is also mentioned in our previous paper like Zhang et al. (2013), Journal of Glaciology.

L118: Till deformation is also part of this equation – some glaciers don't slide along their beds or deform much internally, and their motion is solely due to the plastic deformation of till underneath. If you're considering till deformation to be part of basal sliding, you may want to specify this more explicitly rather than leaving it out!
How do you know that some of observed detachment is not just due to very fast till deformation? You said that glacier motion has two parts – basal sliding and internal deformation. But till deformation should be included in this. You could clarify that you are including till deformation lumped in with the friction coefficient, but you should then also address that this friction coefficient will not stay constant with time – how is this addressed?

Right. This is also a nice point. I have to agree that it is a very difficult issue. I am not sure if there is a land ice model that could properly address the problem of changing friction by coupling soft sediment. We did something similar in Greenland by using a more physical sliding law (pseudo plastic with local till model) (Zhang et al., 2024) so that we do not need to fix the basal friction parameter during the model run, but it is still a sliding law incorporated in the ice flow model, not a separate model for deformable bed or sediment. In the revised manuscript, we change this sentence to “Factors such as soft sediments and basal meltwater lubrication will reduce the basal friction, consequently accelerating ice flow, which are not considered separately but taken as a result of changing basal frictions in the sliding law in this study.” We might consider using a similar sliding law to the pseudo plastic one in the future.

Ref: Zhang, T., Colgan, W., Wansing, A., Løkkegaard, A., Leguy, G., Lipscomb, W. H., and Xiao, C.: Evaluating different geothermal heat-flow maps as basal boundary conditions during spin-up of the Greenland ice sheet, The Cryosphere, 18, 387 – 402, https://doi.org/10.5194/tc-18-387-2024, 2024.

L126: In the first review, I asked about Dirichlet and Neumann boundary conditions not because I didn’t know what they are, but because the text at this place is misleading. Arthern et al. (2015) solves the momentum equations by using both the stress-free boundary conditions (Neumann) and observed velocities (Dirichlet). Here, you have said that Dirichlet and Neumann boundary conditions are observed and modeled ice velocities, which is a little confusing, since surface velocity is a Dirichlet condition. I would suggest reframing this closer to the way Arthern et al. (2015) defines it.

OK, we change this sentence to “where u^D is the observed ice surface velocity and u^N is the ice surface velocity solved in the model by applying a stress-free surface boundary condition”.

Table 1: Could you please clarify why you used 5 MPa as the intact yield strength of ice? Most estimates of ice yield strength are nearly an order of magnitude lower (on the order of hundreds of kPa), and the Bassis et al. (2021) paper you cited uses 0.75 MPa/1.5 MPa for uni-axial tensile strength.

In Bassis et al. (2021) they did not state very clearly about the intact strength. In Table 2 they listed 1.5 Mpa and 3 Mpa for compression strength. In fact, the intact strength of ice varies pretty big. In Golovin et al. (2023), they said “The experimental strength of natural ice ... lies between 0.5 and 10 MPa usually”. So the value of 5 MPa we use in this study is a rough estimate and we also want to use some value slightly larger than that in Bassis et al. (2021) to ensure our model not to go too crazy.
Ref: Golovin, Y.I., Samodurov, A.A., Tyurin, A.I., Rodaev, V.V., Golovin, D.Y., Vasyukov, V.M., Razlivalova, S.S. and Buznik, V.M., 2023. Ice Composites Strengthened by Organic and Inorganic Nanoparticles. Journal of Composites Science, 7(8), p.304.

L140: Delete “for their calculation methods”

Deleted.

L153: “representing for interactions” -> “representing interactions”

Changed.

Fig 4: Could you comment more on how error may be introduced in/by the inversion? For example, if you compute basal friction coefficients at the start of the simulation, but basal slip is changing rapidly, that could change the friction coefficient which you're keeping constant in time.

This is another challenging question :) Honestly, it is hard to answer. In my own opinion, the error/uncertainty in model inversion for simulating abrupt ice detachment is probably not as important as "normal" land ice projections, e.g., projecting Greenland and Antarctica Ice Sheet into the next 100 years. If you can take a look at the initMIP-Antarctica/Greenland papers, there has been clear conclusions that the initialization has large impacts on final projection results. But for this detachment case, I do not think the initial status is that important as long as the detachment mechanism is triggered. It might impact the tipping point whereby we decide if the ice stress exceeds the yield strength of ice. But it is hard to estimate, and I really do not know how to put it in the manuscript.

Ref:
Goelzer, H., Nowicki, S., Edwards, T., Beckley, M., Abe-Ouchi, A., Aschwanden, A., Calov, R., Gagliardini, O., Gillet-Chaulet, F., Golledge, N. R., Gregory, J., Greve, R., Humbert, A., Huybrechts, P., Kennedy, J. H., Larour, E., Lipscomb, W. H., Le clec'h, S., Lee, V., Morlighem, M., Pattyn, F., Payne, A. J., Rodehacke, C., Rückamp, M., Saito, F., Schlegel, N., Seroussi, H., Shepherd, A., Sun, S., van de Wal, R., and Ziemen, F. A.: Design and results of the ice sheet model initialisation experiments initMIP-Greenland: an ISMIP6 intercomparison, The Cryosphere, 12, 1433–1460, https://doi.org/10.5194/tc-12-1433-2018, 2018.
Seroussi, H., Nowicki, S., Simon, E., Abe-Ouchi, A., Albrecht, T., Brondex, J., Cornford, S., Dumas, C., Gillet-Chaulet, F., Goelzer, H., Golledge, N. R., Gregory, J. M., Greve, R., Hoffman, M. J., Humbert, A., Huybrechts, P., Kleiner, T., Larour, E., Leguy, G., Lipscomb, W. H., Lowry, D., Mengel, M., Morlighem, M., Pattyn, F., Payne, A. J., Pollard, D., Price, S. F., Quiquet, A., Reerink, T. J., Reese, R., Rodehacke, C. B., Schlegel, N.-J., Shepherd, A., Sun, S., Sutter, J., Van Breedam, J., van de Wal, R. S. W., Winkelmann, R., and Zhang, T.: initMIP-Antarctica: an ice sheet model initialization experiment of ISMIP6, The Cryosphere, 13, 1441–1471, https://doi.org/10.5194/tc-13-1441-2019, 2019.

L173: Put the sentence "As shown in Figure 4, we inverted…" at the end of the paragraph to make it clearer that you are commenting on observed velocities first and foremost.

I guess you mean "at the beginning of the paragraph"? Changed.

Fig. 3: could you add a more descriptive y-axis label (e.g. "normalized ice thickness")

I guess you mean Fig. 6? Changed as suggested.

L175-180: Thanks for adding this explanation – it adds so much meaning to the manuscript!

Thanks for your suggestion.

L181: Could you be more specific about what aspects were successfully reproduced? Maybe you could say something like “our simulation successfully reproduces the decrease in ice thickness and increase in ice velocity associated with a glacier detachment.”

Great suggestion accepted.

L193: This validation section is so nice and adds so much to the paper. Thanks for including it!

Thanks for your suggestion.

L198: “Generally, the changes in englacial stress…” this sentence was already said in L188. I would also consider explaining this a little more by saying “Generally, higher ice speeds result in higher englacial stresses” or something along these lines.

Thanks for the suggestion. This sentence is changed to “Generally, higher ice flow velocities result in greater englacial stresses, increasing the vulnerability of ice regions to detachment instability.”

Fig. 4: You should clarify in the caption that this is the inversion result for a specific timestep/snapshot before the glacier detachment. Also, why does the friction coefficient get so big at the two ends of the model domain?

A sentence is added in the caption: The inversion is based on velocities observed by remote sensing from 2015 to 2018. Regarding the marginal friction, it is actually the same question asked by another reviewer in the last review. So I just copy over the reply here:

We do prescribe an initial $\beta$ = 1e3 Pa m$^{-1}$ yr. At the glacier head and terminus, we use a Dirichlet boundary condition in the velocity solver, so $\beta$ will not be updated during the initialization. Here I present three sensitivity plots for initial $\beta$ = 1e2, 1e3 and 1e4 Pa m$^{-1}$ yr. We can see clearly that the initial value has some impacts on grids close to the head and terminus, but the majority of glacier is not affected. To avoid confusion, we do not plot $\beta$ for the head and terminus grids in the revised manuscript.

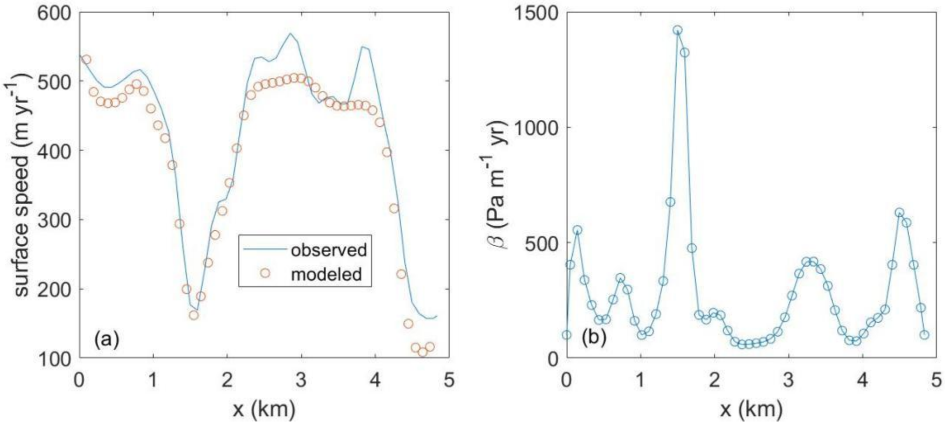



Figure 1: initial $\beta$ = 100 Pa m$^{-1}$ yr

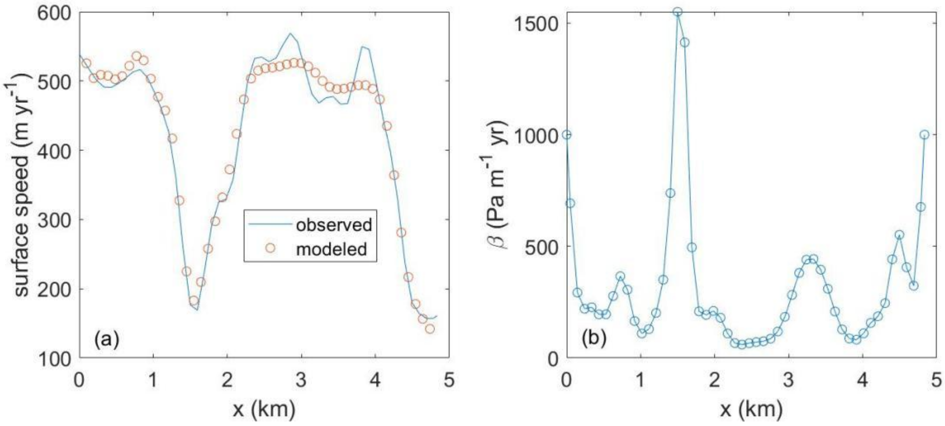



Figure 2: initial $\beta$ = 1000 Pa m$^{-1}$ yr

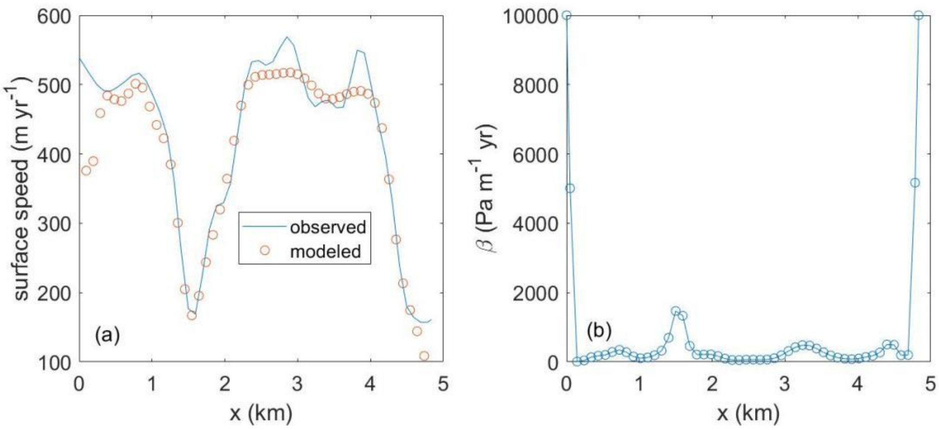



Figure 3: initial $\beta$ = 10000 Pa m$^{-1}$ yr

L215: could be worth re-defining these two parameters very briefly (maybe inside parentheses) so that readers don't have to go all the way back. Also insert "the choice of model parameters" to show that these can be chosen arbitrarily or based on a guess.

"prescribed minimum yield stress" and "prescribed minimum viscosity" are added. "the choice of model parameters" is inserted.

L225: if you're going to mention monitoring for rainfall, I think you could meat up this paragraph a little more to really convince us that rainfall could be a triggering event for these glacier detachments – for example, the fact that multiple glaciers in the Sedongpu Valley detached during this event could provide even more evidence that rainfall could help trigger this positive feedback loop.

Right. Despite some record shows that there was heavy rainfall before the Sedongpu detachment, we still lack strong evidence in between. To avoid confusion, I remove this part in the revised manuscript.

Figure 7: This is a nice figure. I would consider adding an arrow on the left side of the plot that says indicates that each row has increasing initial yield strength. That would really emphasize the visual that you only see the rapid transition to plastic flow for low initial yield strengths.

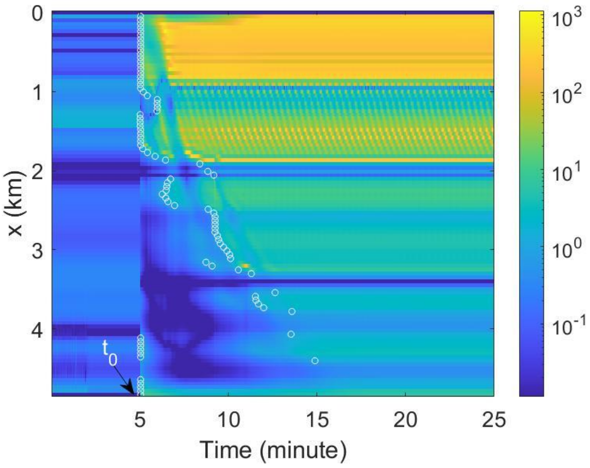
Thanks for the suggestion, but I think I would just skip this arrow idea. The current text in three rows have shown clearly the changes of initial yield strength. But I add an additional sentence in the caption to emphasize this: "The rapid transition to plastic flow occurs for low initial yield strengths."

L254: "Discussions" -> "Discussion"
- The model limitations sound way nicer at this spot in the manuscript now that so many other things have been cleared up. Thanks!

Changed, and thank you for your nice suggestions.

Fig. 8: Consider adding an arrow indicating t_0 so that readers can be sure that the abrupt transition at 5 minutes is due to your changing the model physics. I would also consider adding a demarcation for Iken's bound which shows where the ratio exceeds 1 (could be a contour line, or something like that).

Fig. 8 is improved as suggested. The white circles mark the initial occurrence of the Iken ratio along the x-axis after detachment begins




L269: Simplify sentence : "Kaab (2021) analyzed the force of balance of simplified, slab geometries…"

Changed.

**Reply to Reviewer 2**

Overview:

The authors have responded well to comments from both reviewers and correspondingly revised this manuscript. I find the paper to be much clearer and polished, and it will be a valuable contribution to the glaciological literature to further understanding of rapid glacier detachment processes. Below you will find specific comments listing a few corrections and clarifications, but overall I feel this work is in good shape and should be published following these technical corrections.

Thank you very much for your time and efforts in improving our manuscript!

Specific comments:

Line 29: criterion (singular, not plural criteria)

Changed.

Figure 1: This figure is great, nice improvements. One comment - in panel d, I think this plot shows the difference between 2018-2015 (with surface elevation lowering over the glacier and increasing downstream). This is labeled incorrectly as 2015-2018.

Thanks for the advice. Fig 1d is now improved.

Figure 2: Change punctuation to be consistent following panel labels, also "... black curve represents"

Changed. Thanks.

Line 153: "representing interactions" (remove the word "for")

Removed.

Line 185 and elsewhere: kPa is the conventional abbreviation for kilopascals (not capitalized KPa)

Corrected.

Line 191: What happens to the oscillations with a smaller (or zero) minimum ice thickness imposed instead of 1m?

We tried and the model will easily go collapse.

Line 196: suggested rephrasing: "..., which provides strong evidence for validation of our model results.”

Changed.

Paragraph beginning line 197: This appears to be a repeated but slightly different version of the preceding paragraph. Correct the manuscript to only include only one or the other.

Thanks for point this out. Corrected.

Line 240: Remove word “primarily” – this suggests one main driver, but here the point is that there are multiple factors.

Removed.

Line 266: Add a citation or multiple citations for sliding laws that depend on hydrology, rather than just “Schoof sliding law” – or else leave out any mention of specific sliding laws

Changed. We now add a “Hoffman and Price (2014)” citation, and remove the “Schoof sliding law”.

Line 269: Suggested rephrasing: “Kaab et al. (2021) analyzed…”

Changed.

Line 288: Instead of writing what you may do, I suggest using language to recommend: “To advance this research, future efforts should extend…”

Changed.

Section 6, Conclusions: You may want to consider including a bit more detailed summary of your specific findings here, as an easy reference for somebody skimming the paper to succinctly find a summary of what you did and what results you found.

We now add an additional sentence in Conclusion to give some of the details of our model findings: From the model results, we find that glacier ice detachment occurs when the initial yield strength drops to approximately 430 kPa, indicating that the ice's mechanical properties are critical in triggering abrupt collapse when mechanical stress exceeds critical failure thresholds.