“Numerical Modeling of Ice Detachment Tipping Processes: Insights from the Sedongpu Glacier, Southeastern Tibetan Plateau” by T. Zhang et al. (2025)

<u>Major comments</u>

“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.

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.

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)".

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.

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

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

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.

L45: You can probably take out the last sentence

L63: Delete "Glacier topography:"

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

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..."

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.

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

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).

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

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?

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?

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.

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.

L140: Delete "for their calculation methods"

L153: "representing for interactions" -> "representing interactions"

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.

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.

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

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

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."

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

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.

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?

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.

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.

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.

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!

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).

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