Reply to reviewer 1

GENERAL COMMENTS

The authors present a study of glacier detachment at Sedongpu Glacier from a numerical modeling perspective that attempts to recreate conditions of glacier attachment by simulating changes in glacier viscosity and feedbacks with basal sliding. The topic and motivation is really important, as glacier detachments deserve more scientific attention and can be destructive to downstream communities and infrastructure (as the authors note as well). I think the topic of the manuscript in itself has a lot of merit, and it's cool that the authors have been able to reproduce catastrophic collapse of the glacier. Because of this, I think that this paper is a good candidate for publication. However, I have serious concerns about the approach the authors take as well as the structure and presentation of the manuscript. These can be categorized in the following areas.

We thank the reviewer for the thorough review and also all of these great comments and suggestions! They significantly help in improving the manuscript!

1. I am somewhat unconvinced about the authors' choice to model glacier detachment from the perspective of a rate-weakening basal slip and variable ice viscosity. The authors claim that the feedback between ice stiffness and basal slip plays a major role in the instability that causes the glacier to detach within minutes in their simulations. However, there is very little discussion of how this feedback mechanism or its numerical implementation works. Neither have the authors cited any works that discuss this feedback. I remain unconvinced as to why this mechanism is responsible for the detachment both from a conceptual perspective and from their model results. The authors need to strengthen their explanation of this feedback and how/why they are modeling it. This could include adding a literature review section about damage mechanics and more explanation about why they decided to structure their coupled model the way they did.

Thanks for raising the questions. Here we present more details.
The glacier movement consists of two parts: basal sliding and internal deformation. For abrupt changes like glacier detachments, the basal sliding must experience a violent acceleration. According to Helanow et al. (2021), the universal basal sliding law with or without cavity probably has the same form as the Schoof sliding law:

$$\frac{\tau_b}{N} = C\left(\frac{u_b}{u_b + A_s C^n N^n}\right)^{1/n}$$

we can see that the basal sliding is affected by the basal effective pressure (N) that is related to basal water pressure (P_w),

$$N = \rho g H - P_w$$

where $\rho g H$ is the overburden ice pressure.

It means that as basal water pressure increases, the effective pressure will decrease and then helps accelerating ice flow. Thus, ideally, as P_w approaches very closely to $\rho g H$, N will be close to zero, and we imagine a very rapid increase in ice velocity. But things are a bit different than we thought. In the following two figures, we set $P_w = 0.01\rho g H$ and $P_w = 0.99\rho g H$, and we can clearly see the velocity increase.

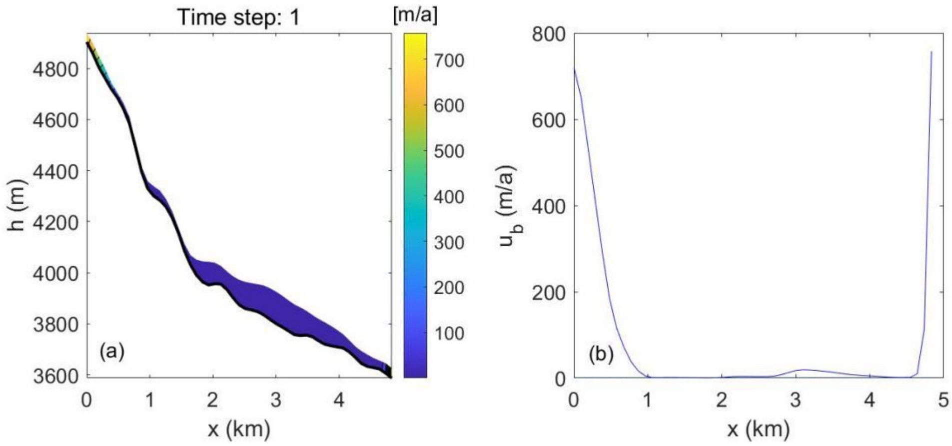


Figure 1: The case of $P_w = 0.01\rho g H$

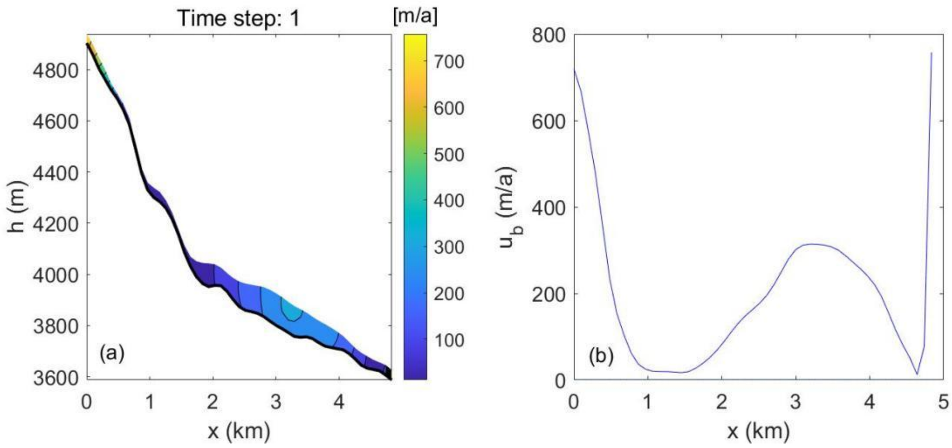


Figure 2: The case of $P_w = 0.99\rho g H$

However, it is still quite far away from the magnitude of ice detachment. We can get the reason by looking at the Schoof sliding law. For a given τ_b, the basal velocity increases with a power of 1/n. Therefore, it is not very likely to have an abrupt ice detachment hazard by solely increasing basal water pressure in the Schoof sliding law.

After all, the Schoof sliding law is found under the traditional slow-moving dynamic framework (e.g., Gagliardini et al., 2007). As a result, the interaction between basal water pressure and slip is able to explain the mechanism of glacier surge (Thøgersen et al., 2019), but still hard to simulate ice detachment. In fact, if we take a look at some recent videos of ice detachment / avalanche, e.g., the recent Swiss glacier collapse (https://www.youtube.com/watch?v=KHXBVAnKDDY), we can see that glacier ice body no longer remain intact during the movement, i.e., it is highly damaged. Note that our Sedongpu case is a low-angle detachment, probably not as fractured as the Swiss one. But it indicates clearly that the traditional constitutive law probably does not hold any more!

As we still lack a discrete element model for this problem, we hope to study the detachment events in the current continuum model framework. That is the reason we choose to use the constitutive law considering yield strength and plastic deformation in Bassis et al. (2021), which works for the damage mechanics for those highly crevassed regions in Antarctica ice shelves. This new constitutive law not only include the traditional elastic deformation, but also include the plastic deformation which occurs when stress exceeds yield strength of the material. This introduces a positive feedback - if stress exceeds yield strength, ice gets yielded, and a slow, elastic deformation becomes a fast, plastic deformation. In addition, the yield strength will also decrease as ice gets failure, leading to more easily yielded region. That will in turn get more ice yielded and eventually most of the glacier region becomes highly fractured.

Another important point is that, based on previous studies (Kääb et al., 2021), there are thick, soft till at the base of detached glaciers. For the Sedongpu Glacier in this study, the basal sediment is so soft that the subsequent erosion easily incised the bed up to several hundreds of meters. A soft bed helps accelerating ice flow and then make more ice regions get yielded, which will then result in more decreased basal frictions and accelerate more of ice flow.

In the revised manuscript, we add a new paragraph with more previous studies relevant to damage work and explanations.

"Previously, a well-studied fracture criteria that defines relationships between material strength and applied stresses has been widely applied in glaciology to model ice fracture and iceberg calving, as well as in studies of ice flow mechanics (Pralong and Funk, 2005; Albrecht and Levermann, 2012; Duddu and Waisman, 2012). Most numerical ice flow models adopt a stress threshold approach, where fracture occurs when stresses exceed a critical value (Hulbe et al., 2010; Borstad et al., 2016; Jiménez et al., 2017), though alternative methods like pressure or strain thresholds (Duddu et al., 2020) remain less utilized. Despite laboratory benchmarks, natural system observations to validate fracture criteria and stress thresholds remain scarce."

2. I am somewhat skeptical that the instability modeled in this manuscript (the viscosity-basal slip coupling) is truly a physical instability and not a numerical instability caused by the choice of model parameters. For example, when the coupling mechanism is activated, the viscosity of the ice goes from 60 to 0 Pa yr, which then triggers the rest of the glacier collapse. A more rigorous stability analysis is needed to ensure that this is a real physical phenomenon.

Thanks for the questioning. This is exactly what we've shown and discussed in Figure 3 (Figure 6 in the original manuscript). We agree that the modeled detachment results are dependent on the choices of model parameters, $\eta_{\min}$ (minimum viscosity) and $\tau_{\min}$ (minimum stress), in the following two equations.

$$\eta = \eta_{min} + \left[\frac{1}{\eta_1} + \frac{1}{\eta_2} + \frac{1}{\eta_3}\right]^{-1}$$

$$\tau_y = \max\left(\tau_c - (\tau_c - \tau_{min})\epsilon_p/\epsilon_c, \tau_{min}\right)$$

Both η_{min} and τ_{min} are for numerical stability, but their real values are hard to know.

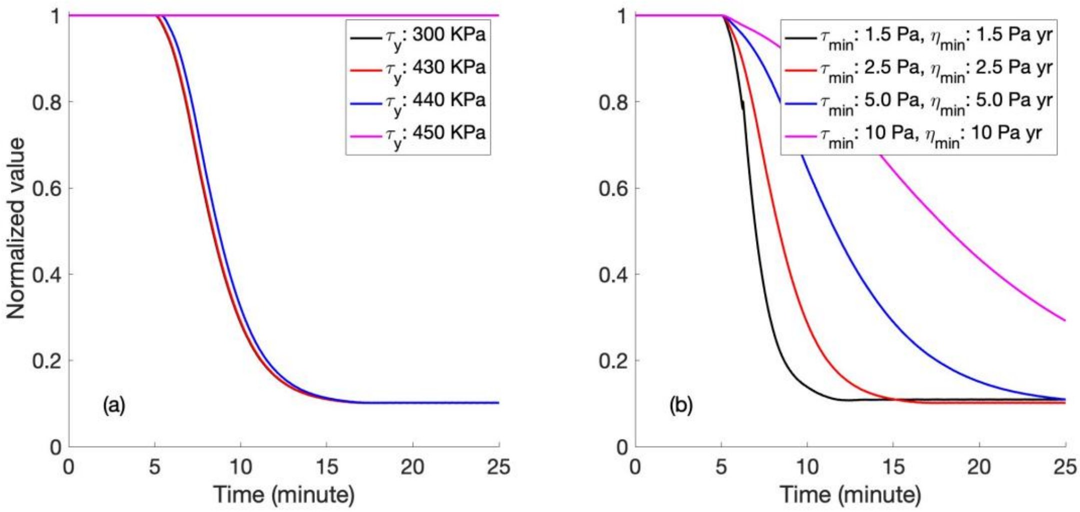


Figure 3: The sensitivity of mean ice thickness changes of Sedongpu to different initial yield stress values (a), model parameters τ_{min} and η_{min} (b) during the 25 minute model time span. The detachment begins at minute 5.

From Figure 3b we can see that the detachment speed dependents on the values of τ_{min} and η_{min}, but the choices of τ_{min} and η_{min} do not change the fact of ice detachment occurrence - a result of plastic deformation due to ice strength decreases after reaching the yield strength. For example, if the yield strength of ice is pretty big, say, 1e7 Pa, then basically there will be no regions where ice stress exceeds yield strength and gets yielded. In this case, the glacier will stay stable (Figure 3a). But if the yield strength is small, say, 1e5 Pa, then during the ice movement, some regions on the glacier might be relatively easily to get yielded. After it becomes yielded, the yield strength will then further decrease and let it be even more vulnerable to get fractured. As a result, such regional, plastic, accelerating ice flow will quickly affect the whole glacier and lead to a detachment. We now add more discussions and explanations at Section “ Model sensitivity”.

3. The authors should provide some sort of validation or provide insights into the plausibility of their results. They repeatedly claim that the viscosity-basal slip feedback is more important than subglacial hydrology or till failure, but they do not provide any evidence to support this, either in the form of citing previous works or providing any sort of validation for their model. In addition, the authors do not show the misfit for their basal friction inversion, so we don’t know whether or not their sliding parametrization is able to reproduce realistic glacier velocities.I think the authors should either include this evidence or scale back their claim that this feedback is more important than other mechanisms which have been more extensively proven to play a role in detachments

and surges (e.g., subglacial hydrology and geothermal heat).

We agree that model validation is an issue that needs more consideration. The ice detachment happened so quick that there was basically no time for in-situ observations. The satellite image data has been used in this study for reconstructing geometry and inversion of basal sliding parameters. The most reliable validation data is the duration time of ice detachment. According to "Scientific Assessment Report on the Ice Avalanche Dammed Lake Event at the Great Bend of the Yarlung Zangbo River" (in Chinese) published in 2022, the estimated duration time was around 300 seconds (Figure 4).

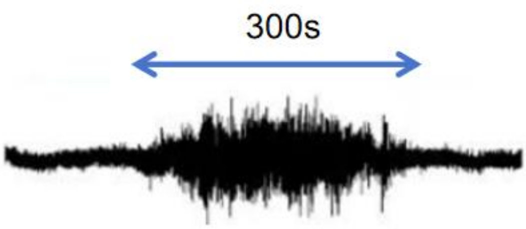


Figure 4: Seismic signal for 2018 Sedongpu detachment

In our model study, as shown in the black curve (Figure 3b), most of the ice volume is lost within around 5-6 minutes. As discussed in the previous bullet, $\tau_{\min}$ and $\eta_{\min}$ are not physical, but numerical parameters. However, these two parameters only affect the magnitude of ice detachment. Thus, we believe that our model mechanism is capable of simulating the abrupt changes of ice flow in this case. In the revised manuscript, we add some additional details about this issue.

Regarding the validation for inversion, we change the original Figure 4 to this one in below so that we can clearly see the performance of the inversion results.

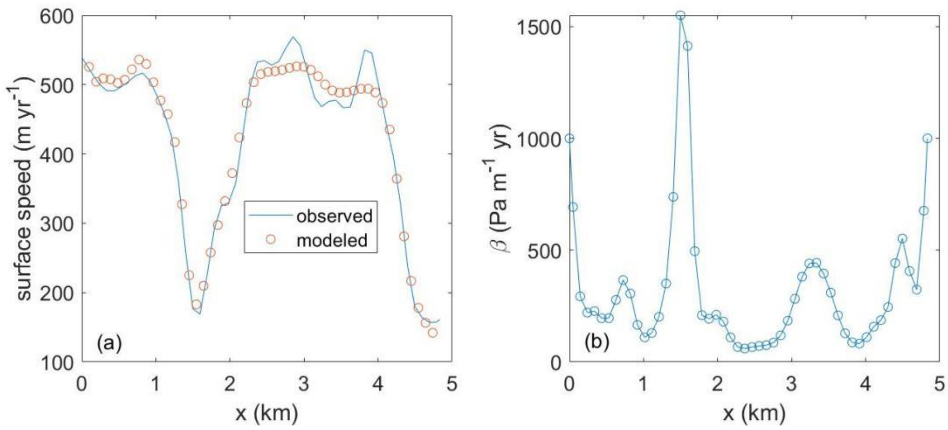


Figure 4: The comparison of observed and modeled surface speed after inversion (a), and the inverted basal sliding parameter using the Robin inversion algorithm (b)

4. The authors don't include any sort of hydrology (subglacial or surface) in their modeling even though surface meltwater-driven hydrofracture and subglacial hydrology play a strong role in bulk

ice toughness and basal slip. I get that this would be difficult to include in a model like this, but it would add a lot to the manuscript if the authors could at least address this in the text. They also talk about effective stress but have not defined it any of their model equations, and claim that high effective stress corresponds to ice acceleration which goes against the traditional glaciology definition of effective stress.

Thanks for the suggestions. We agree that the discussions about subglacial hydrology is important. It is true that for now it is difficult to include basal hydrology module. We've discussed this issue in the section "Model limitations" (now changed to "Discussions"):
"Furthermore, our model does not incorporate thermal coupling or basal hydrology schemes, potentially neglecting key physical mechanisms involved in glacier detachment. For instance, Thøgersen et al. (2019) identified a velocity-strengthening-weakening transition that governs surge initiation, though their framework assumes intact ice and slow movement–conditions that may not adequately capture rapid detachment dynamics. While we acknowledge the importance of ice-bed interactions with basal hydrology (e.g., Schoof sliding law), our current implementation employs a simplified sliding law (Eqn 13) to couple basal till strength with ice flow. This approach, though computationally efficient, could be enhanced in future work to better represent these complex processes."
We also add the definition of effective stress at Section 3.1 "Ice flow model". The definition of effective stress is still traditional. During model runs, we calculate effective stress and use it to judge if it exceeds yield strength. If yes, then ice will go with a plastic deformation, which will then probably lead to a positive feedback of failure.

5. I would like a lot more information on the implementation of the model or finite element scheme. What mesh resolution is used, what meshing software was used, and how the spatial and temporal resolution was determined. In addition, there should be more discussion of the implicit assumptions that go into the model and what effects these have on the results.

The details of our model can be found in Wang et al. (2020) "A two-dimensional, higher-order, enthalpy-based thermomechanical ice flow model for mountain glaciers and its benchmark experiments". In the revised version of this manuscript, we add a bit more numerical descriptions, but we prefer not to repeat most of the numerical details here. The mesh resolution is clearly stated in Section 3.1. The choice of temporal resolution should at least satisfy the CFL condition. We use a very small dt (0.5 second) so that our model can remain stable in the forward runs. As we use a finite difference method, we do not use a specific meshing software as normally needed by a finite element method.

6. The manuscript doesn't follow as much of a logical flow as I would prefer. There is insufficient explanation of the model and its assumptions at the beginning of the manuscript before the equations are presented. I go more in detail about this in the specific comments below.

The structure of the paper is now adjusted according to the reviewer's suggestion.

7. I also believe it could be helpful for a native English speaker to read through the next iteration

of the manuscript as the language is often imprecise and not entirely scientific.

Language is improved in the revised manuscript.

SPECIFIC COMMENTS

L5: "Yield strength of the glacier" is very general. As the authors mentioned later in the manuscript, the yield strength of the glacier is extremely heterogeneous. I would recommend changing this to something like "initial yield strength of glacier ice".

Thanks. Changed.

L8: "just several model time steps" is pretty vague

Changed to "The transition from slow to abrupt flow occurs after most regions of the glacier reach a plastic state.".

L9-10: How would these results be used for early warnings? Maybe you can talk about this later in the discussion.

This sentence is removed and some discussions are added in the "Conclusion" section.

L16: Merge this with the previous paragraph

Merged.

L22: The "largest ever recorded event" of what? Of surges, of a glacier detachment? The language could be a bit more precise.

The context is now updated.

L24: The authors mention that remote sensing advances allow us to detect changes in surface features like crevasses and fractures but the authors did not use any of these data to validate the model results

Right. Thanks for pointing this out. The detachment occurred so quick that we basically do not have any remote sensing data for validation. As we replied earlier, we used the duration of detachment for validation. We now remove this paragraph.

L38: This is the first time the authors mention the ice stiffness-basal slip positive feedback. It would be great to have an additional few paragraphs explaining this feedback and motivating why the authors choose to model it. It would also be great to see some previous examples where this mechanism was suggested or modeled successfully before.

We add a new paragraph in the revised manuscript:

Glacier fracture and damage significantly accelerate ice flow by structurally weakening ice and reducing its effective bulk viscosity, as observed in Pine Island and Thwaites Glaciers where upstream fracturing correlates with flow acceleration (Lhermitte et al., 2020; Sun and Gudmundsson, 2023; Surawy-Stepney et al., 2023). This damage interacts with basal slip—where ice slides over bedrock—through stress redistribution that enhances basal crevassing (Bassis and Ma, 2015) and by facilitating meltwater penetration, which reduces basal friction and further accelerates slip (Sun et al., 2021; Clayton et al., 2022). Consequently, damage evolution is critical for projecting long-term ice flow changes and land ice stability (Albrecht and Levermann, 2014; Bassis et al., 2024), though model uncertainties persist regarding damage parameters and feedback mechanisms.

L41: Specify the glacier name again

Specified.

L44: What is Medog? Is it a village, a weather station?

It is the name of a county. This information is added.

L48: Some information about the quality/texture of the Quaternary deposits would be useful here. Is it soft sediment or more coarse till? Also, how thick is the debris cover on the glacier?

We add a new sentence here: The Sedongpu Glacier was underlain by a thick sediment/moraine layer which was eroded during the 2018 detachment event, forming a canyon up to 300 meters deep (Kaab, 2021; Kaab and Girod, 2023).

Figure 1: (a) It would be great to highlight the glacier that is being modeled. (b) and (c) the text is a little blurry - would be better to have higher resolution. Please also show the outline of the glacier in (b) and (c) so we can get a better idea of where the glacier is inside the valley. Plotting the difference between the two DEMs would also probably be more useful (i.e., 2018 DEM minus 2015 DEM).

Thank you for your suggestions. We have emphasized the location of Sedongpu in figure a, upgraded the resolution of figures b and c, and also added glacier contour information to these two figures. In addition, we have added the elevation difference (d) for 2015-2018.

L56: I think this whole section (ice flow model) should go after Figure 2.

Thanks for the suggestion, but we prefer the current way. This figure is a overall workflow, so it might be a bit easier for people to get the idea after we introduce the model.

L57: What is the dimensionality of this model? It would be good to specify.

We stated very clearly that it is a two dimensional model.

L57: PoLIM is a thermomechanical model. What are the thermal parameters specified here? What assumptions are made about heat flow? Is there any geothermal heat flux?

Right, PoLIM is a thermomechanical model, which means the velocity and temperature solver could be coupled together. We can also choose not to solve the temperature field by assuming a constant flow rate parameter (A) for temperate glaciers. Sedongpu Glacier is a typical temperate glacier. So we just need to use a constant A. In this case, we do not have to prescribe geothermal heat flux, which is a boundary constraint for temperature solver.

L70: Reference to Table 1 would be useful here.

Add the reference to Table 1.

L71: Justify why you are using a Weertman-type sliding law here, and why effective pressure (N) isn't important.

We use a linear Weertman sliding law prior to detachment. The main reasons are (i) we do not have any data of basal water pressure and (iii) we just need an initialization that captures the ice flow features prior to detachment so that we can well simulate the subsequent dynamics, which is based on a good model initialization.

L83: There is also another model initialization section in the "results and discussion" (Section 5.1). Consider merging these two?

We merged the subsection 5.1 "Model initialization" into 5.2 "Sedongpu detachment simulation" as initialization is also part of our simulations so that we can avoid this confusion and also.

Table 1: What is the geothermal heat flux? Also, consider converting the rate factor A to Pa^-n s^-1 for easier comparison with other papers. It would also be good to include citations for the choices of critical strain and intact strength of the ice.

The unit of A is changed. We do not really need geothermal heat flux in this case, as Sedongpu is a typical temperate glacier and we can use a constant A, i.e., we do not really need to solve the temperature field. As a result, we do not need to use geothermal heat flux as a boundary thermal condition. The citation for critical and intact strength is also added.

L90: Where do you get the Dirichlet and Neumann velocities from? What does it mean for velocity fields to be Neumann/Dirichlet? Also, what is the misfit from this inversion, and is the model able to reproduce realistic velocities?

Great you ask this! To fully understand these questions, I have to refer the Arthern and

Gudmunsson (2010) paper. Here are some brief and simplified explanations:

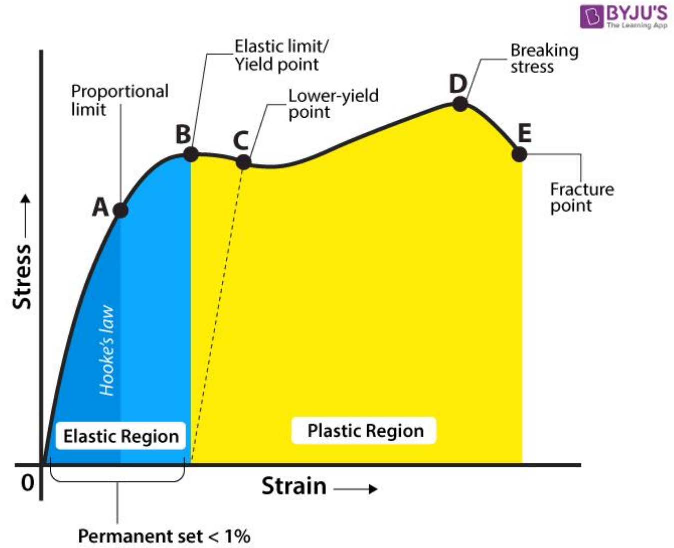
The basic idea of the Robin inversion is to compare the observed and modeled ice surface velocity by changing basal frictions each step. For example, at some location marked as x_i, we can first set an initial basal friction β, and then we run the model. If we see the modeled surface velocity is larger than the observed value. Then β is probably too small, and we need to adjust its value by increasing it a bit, and then we run the model again. There are chances that this time we get a smaller surface velocity than observed, and then we need to reduce β a bit. We will repeat such iterations until we have modeled velocities that are closed to observations. So now we understand that there are two different surface velocity values involved here, modeled and observed surface velocity. For Dirichlet boundary condition, we apply observational velocities from remote sensing data and apply them as surface boundary condition, $u_s = u_{obs}$. For Neumann boundary condition, we apply a normal stress free condition at ice surface, just as a normal ice flow model does, $\sigma \cdot n = 0$. So we know that our model can reproduce realistic velocities if modeled velocities match observations after initialization.

L101: There needs to be an explanation of how you obtain eta_1 and eta_2 and what each of the three viscosities mean. Also, how is eta_min determined and what is the model's sensitivity to choice of eta_min?

We now change the description way of this equation. η_{min} is a tuning parameter and its impact on model results is shown in Figure 6b. Generally speaking, η_{min} is the minimum viscosity, so the modeled ice velocity will increase if we set a small η_{min}.

L106: When and how does the viscosity transition to plastic viscosity?

The ice will become "plastic" if / when the stress exceeds yield strength. See this diagram.



Source: https://byjus.com/physics/yield-strength/

L108: It would be helpful to cite previous works that have demonstrated good results with a model like this

Citation added.

L113: Are you using the linear Weertman-type or this rate limited one?

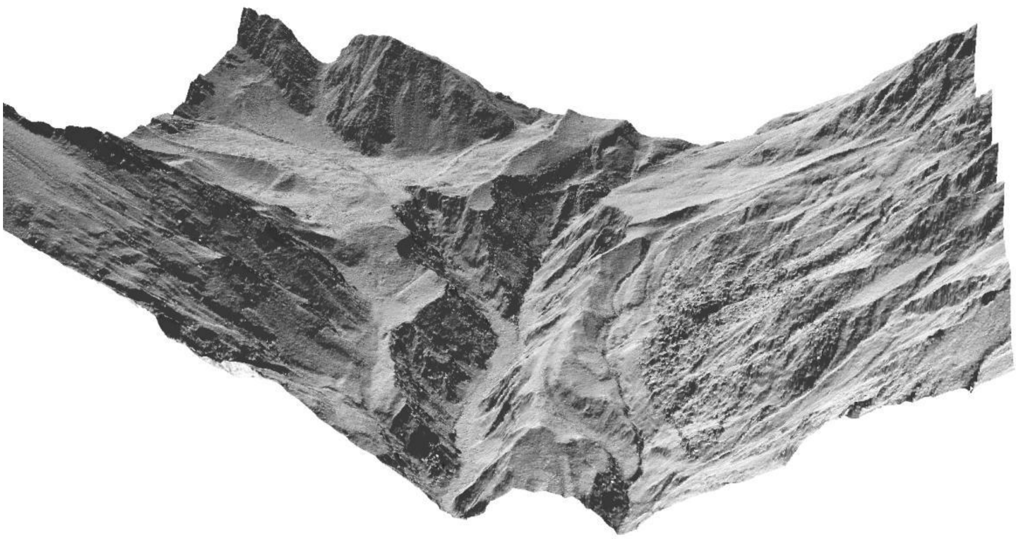
We use the linear Weertman sliding law just for model initialization. To simulate ice detachment, we use this sliding law instead in order to consider the impact of stress transitioning.

L117: Include a reference to Figure 2 before presenting the model equations. It would also be great to expand more on Figure 2 in the text.

Manuscript is changed here according to these suggestions.

Equation 13: Why is the ice yield strength in the expression for basal shear stress? In Bassis et al.'s supplement, the second term inside the brackets has tau_c (the intact strength) in the denominator, but here you have tau_y (yield strength).

Right. For the equation in Bassis et al. (2021), the bed is probably not as soft as in our Sedongpu case. See the following figure. The bed of Sedongpu Glacier is deposited by up to 300 m soft tills. Thus, we here use \tau_y to better represent the coupling of ice stiffness and basal slip. We now put some more sentences here in the manuscript.



L126: The "Datasets" section should be before the model equations.

Changed.

L138: Need more information on what the "glaciological modeling results" are. Are they using a flow approximation? If so, what is being used? Some more details on how the glacier geometry was determined would be helpful here.

Thank you for this helpful comment. In the revised manuscript, we have clarified the method used to obtain the distributed ice thickness of Sedongpu Glacier. Specifically, we used the GlaTE software (Langhammer et al., 2019), which derives glacier-wide ice thickness by optimally combining discrete thickness estimates with glaciological modeling constraints through an inversion framework. The modeling component is based on the method of Clarke et al. (2013), which uses a shallow-ice approximation to relate basal shear stress to surface slope and apparent mass balance. This formulation is cast as a linear optimization problem with smoothness regularization, resulting in a physically plausible thickness field.

In our case, the discrete thickness estimates were derived from elevation differences between pre-detachment glacier surfaces and post-detachment exposed bed topography, providing first-order constraints on ice thickness. These thickness points, together with a DEM and glacier outline, were used as input for GlaTE. The resulting distributed ice thickness field was then sampled along a centerline generated using the method of Kienholz et al. (2014), and this flowline geometry was subsequently used in the PoLIM simulations. These clarifications have been incorporated into the revised manuscript.

L139: What are some limitations to using a flowline model as you are doing, compared to a 2D model? How would this change the results?

I guess you'd like to know the comparison with a 3D model. Our flowline model is 2D in x-z dimensions. I would say that using a 3D model could be more exciting, since it uses a 3D geometry and possibly more accurate than a 2D flowline model. But it will also bring more difficulties in numerical implementation and modeling, for example, meshing (finite element) and accurate data inputs. The major limitation of using a 2D model is that it does not fully account for the impact of lateral drag of ice flow. But the main dynamic features should be similar between a 2D and 3D model, if we implement the same instability physics in the model.

L144: Why were higher velocities filtered out? It seems like a threshold of 400 cm/day could exclude important data if the glacier was moving faster.

At the initial velocity calculation completion, some areas of extreme values (greater than 400 cm/day) were identified: these results were generally concentrated in the upper part of the glacier (cloud cover); there were also some anomalies in the lower and middle part of the glacier with a very small area range, which were found to be practically not undergoing rapid displacements by comparing the two-phase optical images of this location. Therefore, we consider results greater than 400 cm/day as anomalies.

Figure 3: Move the panel labels (a and b) to above the figures so they are more visible. Including a hillshade contour around the glacier domain would also be useful. In Panel A, you should use a diverging colormap so it's easier to see where the glacier thickness increased and decreased. Also,

why is there data missing in the upper tributary in panel B?

We have moved the panel labels (a and b) to above the figures and added the hillshade layer in this figure. Due to cloud interference in the upper glacier region, we unable to calculate the surface velocity of this region, resulting in no valid output for this region.

L148: There was already a model initialization section. This should be merged with that. It doesn't make sense to have a model initialization in the Results/Discussion section.

This section is now merged into its following model results section.

L153: Why did you use the mean velocity from 2015-2018 instead of the velocities just prior to the detachment? Does Sedongpu exhibit seasonal variations, and how would that affect your calculation of the friction coefficient at the moment of detachment?

It is a nice question. The model initialization mainly account for an average status of land ice dynamics. Please see Seroussi et al. (2020) "initMIP-Antarctica: an ice sheet model initialization experiment of ISMIP6" for more details. The inputs (geometry, velocity) for model initialization are data obtained across multi-year time spans. As we explained the Robin inversion at L90, the model initialization is basically a numerical optimization approach that could tune basal friction parameters to match observations. It is hard for an inversion algorithm to capture fast and transient processes. Glacier movements all have seasonal variations, but the remote sensing data can only get the mean velocity value for a given period of time. Thus, it is also pretty hard to accurately simulate the high-res seasonal changes before the detachment. This means the stress regime across the glacier before the detachment probably also represents an average condition. It might impact the threshold of ice stress that exceeds yield strength. But in this study, we think the mechanism explanation is a bit more important than model accuracy.

Figure 4: What is the resolution of this flow line model? How do these velocities compare to observations?

Please see our descriptions in the model description section: We use a Δx = 48 m in x and 20 vertical layers in z with a terrain-following coordinate. Now we change Figure 4 so that we can see a direct comparison with surface velocity.

L159: The authors claim that external environmental forcings could be responsible for the detachment, but these forcings are not included in the model.

Right. We now change this sentence to "Environmental forcings may have acted as external triggers for the 2018 Sedongpu detachment". So it is clear that they might induce the occurrence of detachment, but they are not the responsible mechanism.

L164: Why is the coupling mechanism activated at t=5 minutes? What physical event does this represent? Does the model reach any sort of equilibrium prior to t_0? And why does the glacier instantaneously collapse when you activate the coupling mechanism - can you be sure that it isn't a numerical instability?

Before we simulate ice detachment, we need to confirm our model is stable. We explain this a bit more in the manuscript. But 5 min is an arbitrary choice. It should be long enough for the model to get stabilized before the detachment. The instantaneous collapse after we activate the instability mechanism is exactly what we'd like to see and is the goal of this paper. The real Sedongpu glacier detachment just happened this way. We explained in the major comment responses why it is a physical result, not numerical instability.

L167: 90,000 m/hour seems unrealistic. Can you comment on why it's so high, and what this could be representing?

Unfortunately, it is real. The whole detachment finished in 300 seconds. In a warming world, some glaciers may not move slowly any more. There truly exists some tipping points that those glaciers may just disappear in a sudden. I am not sure so far if we will have similar events in Antarctica, but these detachments are real warnings for the whole human society.

L170: This is the first mention of effective stress in the entire paper. There is no mathematical or conceptual definition of effective pressure anywhere prior to this, and effective stress was not a parameter in any of the model equations shown. How is effective stress being calculated?

The definition of effective stress is now added in the model description section.

L171: An increase in effective stress should not result in ice acceleration, unless you are defining effective stress differently from the traditional glaciological definition. Higher effective stresses are associated with higher frictional contact between the ice and bed, and so a higher effective stress should correspond with the glacier slowing down. Effective stress needs to be defined explicitly in the ice flow model section and maybe again here.

You might understand it in a reversed way. The increase of effective stress is a result of ice acceleration. If we look at the equation of stress, $\tau = \eta\dot{\epsilon}$, stress will increase if strain rate increases when ice flow speeds up if viscosity keeps stable. But the viscosity decreases in our cases. The changes of stress is a combination result of the changes of viscosity and strain rate. Clearly, in our case, the increase of strain rate overwhelms the decrease of viscosity, and after the increased stress exceeds the yield strength if ice, the detachment instability mechanism is triggered.

L173: Is the 6.3 minutes from the beginning of the simulation or 6.3 minutes after t_0 the activation of the coupling mechanism?

It is 6.3 mins after t_0 when we activate the coupling mechanism. We now add "after t_0" at the end of this sentence.

Figure 5: It would be great if the authors could comment on the sudden change in viscosity from 60 to 0 Pa yr. It seems unrealistic to me that the viscosity would go to 0 within less than a minute.

The viscosity does not reduce to 0, but to $\eta_{\min}$. Perhaps you could take a look at this recent Swiss glacier collapse (https://www.youtube.com/watch?v=KHXBVAnKDDY). The glacier became highly fractured during the high-speed movement. In such cases, ice viscosity can no longer remain a big value. We can also look at Figure 6b. If we increase $\eta_{\min}$, the glacier collapse will also take a longer time. I agree that this kind of events are a bit beyond our imagination. They are quite different than traditional slow-moving glacier dynamics.

L177: Nice relating with previous work

Thanks. There are very few previous studies about glacier detachment, unfortunately.

L184: If yield strength varies from 100-1000 kPa, why did you only test between 300-500 kPa?

Right. This sentence is not accurate. We change it to “Figure 6a shows cases with initial yield strengths between 300 and 500 kPa”. We’ve tested more cases than we showed here. But as the glacier detachment occurs at an initial yield strength of around 430 KPa, it is not very necessary to show other cases like 100 KPa and 200 KPa, etc...

L190: What is the sensitivity of the model to A and to the temperature of the ice?

The A value (3.17e-24 Pa^{-3} s^{-1}) we use in this study is for ice temperature around 0 degree C. See the following table from Cuffey and Pateson (2010) “The Physics of Glaciers”. We can get some slower collapses if we apply smaller A values, but I am not sure if it is necessary to do that.

Table 3.3: Measured and inferred values of creep parameter A at different temperatures, for $n = 3$.

T (°C)	A (10^{-25} s^{-1} Pa^{-3})	Method	Reference
0	24	Mean of 5 calibrated models	See below†
	38	Mean of 5 borehole tilt values◇	Raymond 1980
	55	Closure of tunnels	Nye 1953
0	24	**Recommended base value**	
	93	Various lab tests	Budd and Jacka 1989
-2	27	Various lab tests	Budd and Jacka 1989
-10	3.9	Ice shelf spreading	Jezek et al. 1985
	5.3	Ice shelf spreading	Thomas 1973b
	2.5–4.3	Ross Ice Shelf flow‡	MacAyeal et al. 1996
	1.8–3.2	Filchner-Ronne Ice Shelf flow‡	MacAyeal et al. 1998
-10	7.6	Borehole tilting	Fisher and Koerner 1986
	6.7	Flow-line with borehole	Reeh and Paterson 1988
	8.7	Borehole tilting	Dahl-Jensen and Gundestrup 1987
-10	3.8	Mean of ice shelf values	
	7.7	Mean of simple shear values	
-10	3.5	Various lab tests	Budd and Jacka 1989
	3.5	**Recommended base value**	

† Hubbard et al. 1998; Gudmundsson 1999; Adalgeirsdottir et al. 2000; Albrecht et al. 2000; Truffer et al. 2001.

◇ We have calculated A for $n = 3$ using $AR^3 = A_o\tau_o^n$, where A_o, τ_o, and n are values given by original authors, and R is Raymond’s corrected stress value. Stresses are for the greatest depth of reported measurements.

‡ Calibrated model for flow of entire ice shelf (Ross) or part of ice shelf (Filchner-Ronne). Low and high values are for effective temperatures of −15 and −20 °C, respectively.

L199: It would be great to include a conceptual explanation or discussion of why the coupling feedback mechanism almost instantaneously results in failure and rapid acceleration of the ice. This is the crux of the manuscript and should be explained in more detail.

The section “The tipping processes of Sedongpu detachment” presents details of this coupling feedback. We now put more information to make it more clear.

L200: Earlier the authors suggested that hydrology doesn’t play as much of a role as the viscosity-slip feedback mechanism. It is unclear to me then why rainfall events should be monitored as opposed to crevassing or softening of the glacier ice.

Here we mean external factors like heavy rainfall could be incentives for glacier detachment to happen. It could lubricate ice bed, accelerate ice flow and soften glacier ice. There are chances that, during these processes, the ice stress at some locations might exceeds yield strength and then turn on the instability mechanism. But yes, we believe in this case water (hydrology) is not as important as the yield-slip coupling mechanism.

Figure 6: What is the normalized value? I don’t think the normalization was defined clearly in the text or in the caption for the figures. Is it a spatial average? Maybe I missed it, but I don’t understand the y-axis for these plots.

We add an additional sentence in the caption: “The y-axis indicates the normalized value of mean ice thickness along the flowline.” We use normalized value here in order to better see how much ice mass is lost during the model run.

L206: What does ice becoming “yielded” mean?

It means the stress goes beyond the yield strength of ice. But we change it to some other expressions like “exceeds yield strength” to avoid potential confusion.

L218-220: This sentence would make more sense if it were in the introduction. Or, if the authors wish to claim that the elastic-plastic transition is important, it would be helpful to include a scatter plot of stress vs. strain rate or something like that to show the glacier viscosity transitioning from elastic to plastic.

Now we add a new figure showing the relationship between basal drag, effective pressure and basal geometry (Iken’s bound). Also see the reply to L237 below.

L224-225: “This has remarkable scientific and engineering applications for large infrastructures in the local regions” - I think it would be good to be more specific about which aspects of your results would be helpful for science/engineering applications. Otherwise, this sentence may make more sense if it were in the introduction or conclusion.

Nice point (reminder). We now remove this sentence and replace it with another one “To do that, we need to conduct detailed and accurate investigation of glacier geometry for building a reliable ice flow model and early warning system”. This is a sensitive region for big infrastructures, and we better just discuss science here.

Figure 7: The panel labels are hard to see - maybe put them on top of each plot instead of at the bottom. Also the colorbars need units.

Figure 7 is now improved and the caption is also changed accordingly.

L226: This section should go closer to the beginning when you are introducing the model - it would be helpful to understand more of the model limitations from the outset.

Thanks for the suggestion. We prefer to let this section stay here, as we feel it is more like a supplement to both ice flow model and model results.

L228: The assumption of constant ice density wasn’t included in the model description at the beginning. It would be helpful to mention this in the “ice flow model” section.

Added.

L237: Could you write briefly about how including some of these mechanisms could affect the results? E.g., if you used Schoof’s sliding law or Iken’s bound, how would that change the results?

We now add a new paragraph in the “Model limitations” (now changed to “Discussions”) section regarding the Iken’s bound:
In kaab et al. (2021), they analyzed the force balance of simplified, slab geometries and marked Sedongpu Glacier as stable. In fact, for stable glaciers with basal cavities, basal drag is constrained by an upper limit known as Iken's bound,

$$\tau_b/N \leq m_{\max}$$

where τ_b is the basal shear stress, N is the effective pressure and $m_{\max}$ is the maximum value of the up-glacier-facing slopes of obstacles. Here we assume N is the overburden ice pressure (no basal water pressure) and set $m_{\max}$ to the maximum bed slope. Figure 8 shows that once the detachment instability mechanism is triggered at t_0, the ratio $\tau_b/(N m_{\max})$ in the upstream region of Sedongpu Glacier rapidly exceeds 1 (violating Iken's bound), which aligns closely well with the timing of the detachment event.

The advantage of the Schoof law is that we can include the basal hydrology. In the Sedongpu case, it is hard to do as we do not consider the effect of basal water, which is clearly stated in the context.

L248-249: What are the early warning signals of glacier detachment? These weren’t discussed at all in the manuscript.

We revised the “Conclusion” section a bit and put some more explanations of the earning warning signals.

Technical corrections and typos

L38: “novel” -> “novel”, “postive” -> “positive”

Corrected.

L101: “Glen’s pow law” -> “Glen’s power law”

Corrected.

L89: change “According” to “Following”

Corrected.

L163: “simulated” -> “simulate”

Corrected.

L183: “destablizing” -> “destabilizing”

Corrected.

L230: “descrete” -> “discrete”

Corrected.

L234: “hydrolodry” -> “hydrology”

Corrected.