Response to reviews: "Simulating jökulhlaups from an ice-marginal lake within a 2D model of subglacial drainage and basal sliding"

Reviewer 2

We thank Reviewer 2 for their positive review and their helpful comments. Below, we detail our response to each comment. Where relevant, we also include the nature (in blue) and location of our manuscript change.

Specific comments

Comment 2.1: Model setup: I looked at your Github and found to my delight that you have used the Python ISSM interface (rather than Matlab). It would be great for people to know that it's up and running well; maybe you can specify the ISSM version, interface language, and operating system in your model setup section.

Reply: We actually generated results using both MATLAB and Python on a variety of operating systems. We chose to carry out synthetic experiments in Python so as to remove the potential cost-barrier to MATLAB implementation. In the Greenland case, MATLAB remained simpler to implement because of pre-existing routines for L-Curve inversion. Implementing those routines in Python is an ongoing effort. We have added text as suggested to the model formulation section, with a new sentence on lines 64–66 which reads:

"The underlying numerical solvers in ISSM are implemented in C++, and model runs are configured in either a MATLAB or Python interface. Below, we present results generated using both the Python and MATLAB ISSM interface."

Comment 2.2: Model setup line 256-60: is the mesh refinement according to flux, effective pressure etc., or is it manually refined based on where you think it should be refined? Can you talk more about the adaptive mesh refinement and how you decided on which parts of the mesh should have finer resolution? Does this resolution influence the results?

Reply: Thank you for raising this point. We manually refined the mesh at the glacier outlet and close to the lake, choosing a finer mesh to faithfully represent the glacier geometry and the connection between the lake and the subglacial drainage system. Elsewhere, a coarser resolution was used to limit the overall computational cost. To clarify this, we have added the following text on lines 265–266:

"The mesh is further manually refined to a typical edge length of 50 m close to the ice-marginal lake (Fig. 5c) in order to faithfully represent glacier geometry close to the ice margin"

We did not perform a formal mesh-sensitivity or convergence analysis, so we have stopped short of claiming our results are fully mesh-independent. The original GlaDS was tested for mesh sensitivity (see Appendix A in Werder et al., 2013) and they found that the mean channel frequency, max discharge, and effective pressure does not change significantly once edge length is below 1.5 km.

Comment 2.3: Can you explain why you use Budd sliding in the synthetic case and Schoof for the application to Isunnguata Sermia? Why can't you just use Schoof for both or Budd for both? Or try both sliding laws in both cases? Does choice of sliding law change the results for either case?

Reply: We used the Budd friction law in the synthetic case in order to facilitate comparison with the Kingslake and Ng (2013) model, and the Schoof friction in the Greenland case as it has been demonstrated to give a more faithful representation of ice dynamics when coupled to GlaDS (see McArthur et al., 2023)

We have updated the relevant section in the methods, moving text from Section 4 to here to avoid repetition and consolidate the explanation of our choices. Lines 120–124 now read:

"In our experiments on a synthetic domain we use the Budd-style friction law (Eq. 1, Section 3) to facilitate comparison to the Kingslake and Ng (2013) model. In the Greenland experiment (Section 4), with an observational constraint on $\mathbf{U}_\mathrm{b}$, we use the Schoof-style friction law (Eq. 2). A Schoof-style friction law has been shown to more faithfully represent observed ice-dynamics with a GlaDS derived N than the Budd-style friction law (McArthur et al., 2023)."

We have not carried out additional experiments to systematically isolate the influence of friction law on our results. Such a comparison is complicated by the different friction coefficients in each law, such that changing the friction law also requires defining an appropriate set of coefficients for comparison.

As raised in the comment below (2.4), the synthetic case does not surpass floation fraction, so with a Schoof friction law we would not expect to approach Coulomb-like behaviour. We therefore anticipate that the qualitative behaviour of the system under the two friction laws would be similar. However, we consider systematic investigation of the interaction between friction laws and coupled hydrological models to be beyond the scope of the present study (see lines 107–110).

McArthur, K., McCormack, F. S., and Dow, C. F.: Basal conditions of Denman Glacier from glacier hydrology and ice dynamics modeling, *The Cryosphere*, 17, 4705–4727, 2023.

Comment 2.4: Why does changing the englacial void ratio change the Greenland case's behavior so much, but not the synthetic case? Why does the synthetic case never surpass flotation pressure, while the Greenland case consistently does? You are making the argument that the long flood durations are partially because GlaDS can't deal with uplift, but the synthetic case seems to still be showing monthslong flood durations without ever going near flotation pressures (Fig. 2b) – so this argument may not hold. I would also love to see another sensitivity test where you change the englacial void ratio (Fig. 3)

Reply: Thank you for noting this, which is an important observation that we had not previously described. In an effort to address each of these comments we have made a number of changes in order to better emphasise that we believe uplift is an important missing phenomena from our model but is not the only one.

On lines 306–307 we have added:

"Compared to the synthetic case, we also increase the englacial void ratio e_v to 0.01. This has little influence on our synthetic case when tested to the same limits (Fig. D1) because the model does not reach flotation pressures (Section 3.1)".

In support of this change, we have added an additional figure to the Appendix (Fig. D1) showing the minimal influence e_v has on the synthetic case.

To strengthen our discussion of why we consider the lake water temperature to also be an important reason as to why the floods remain too long, we have updated the discussion section 5.3. On lines 513–526 we have added:

"...with no physically-informed representation of rapid cavity expansion, our model is unable to reproduce this specific mode of fast flood onset and instead floods primarily evolve as a result of channel expansion.

However, uplift alone is unlikely to account for our excessive flood durations because our model also underpredicts the rate of channel expansion. In our synthetic experiments on a relatively small glacier resting on a sloping bed (Section 3), the steep hydraulic gradient means that water is efficiently evacuated from the system and water pressure does not exceed flotation at the outlet (Fig. 2f), precluding uplift triggered rapid drainage. Even with a model of elastic uplift, floods in our synthetic experiment would therefore remain unreasonably long.

In our model, we assume water in the subglacial system is always at pressure melting point (Section 2.1). In turn, we effectively assume that the temperature of water entering the subglacial drainage system from the lake is also at the pressure melting point. Observations indicate ice-marginal lakes can exceed this temperature (e.g., Clarke, 2003; Ng et al., 2007; Carrivick et al., 2017) and that high lake water temperature increases the heat energy available for channel expansion via wall melting, resulting in higher peak discharge and shorter duration floods (see Fig. 6A in Carrivick et al., 2017). Inclusion of this factor in our model may simultaneously resolve our underprediction of flood peak discharge and our overprediction of flood duration (discussed above). On an alpine glacier with an analogous supraglacial lake, and using the Spring and Hutter (1981) equations for channel enlargement, Werder et al. (2010) found that increasing the lake temperature from 1°C to 4°C increases peak discharge from 380 $m^3 s^{-1}$ to 960 $m^3 s^{-1}$."

Comment 2.5: Fig. 5: it would be helpful to show f and g on the same axes (modeled & observed) to understand the difference between them

Reply: We did display the data this way originally. However, because there is uncertainty displayed the observed record, the clarity of the figure is improved by plotting the two records on independent axes. The data themselves are also entirely independent, with the measured lake record not driving the model in any way. We have not made a change in response to this comment.

Comment 2.6: I like that you tried changing the channel and sheet conductivity. Can you provide some additional comments about some real-world cases where you might see higher or lower sheet or channel conductivity? What kind of bed types or drainage configurations would lead to seeing these conditions in real life?

Reply: We have added some additional text and changed the structure of lines 169–174 to add:

"These include the channel and sheet conductivity terms k_c and k_s, which govern how easily water is transmitted through the system (i.e., a hard-bedded glacier might be expected to have a higher conductivity than a soft-bedded glacier, Dow 2022); and the terms controlling the geometry of linked cavities such as the basal bump height, h_b, the channel sheet width, l_c, and the cavity spacing, l_r. We also vary the terms controlling water input to the subglacial system, via the basal melt rate M_r, and to the lake, via the lake refill rate, Q_{in}, which is held constant through time in the synthetic experiments."

Comment 2.7: L394: Why do you think your assumption of a continuous sheet to be more physically plausible than the isolated cavity assumption? Wouldn't this change from glacier to glacier? Can you try running the jokulhlaup with the lake NOT connected directly to the sheet, and would this affect the results?

Reply: Thank you for raising this. We recognise that our assumption of continuous hydraulic connection between lake and the distributed cavity system might be more accurately described as a modelling idealisation. Thin, actively deforming ice close to a repeatedly draining ice-marginal lake seems to favour a situation in which the lake is able to impart hydraulic influence on the adjacent cavity system. However, as pointed out, the nature of this connection is likely to vary glacier to glacier, and is likely to vary even within individual glaciers throughout lake drainage cycles, something our model is unable to represent. In recognition, we have changed the relevant text, which on lines 401–406 now reads:

"This approach has been shown to match well to observations of jökulhlaups elsewhere, when flux is prescribed across the ice–lake contact (e.g., Flowers et al., 2004). However, as with the assumption Kingslake and Ng (2013) that the distributed system is entirely isolated from the lake, we recognise that our opposing assumption of continuous exchange between the lake and adjacent sheet is also an idealisation arising from our model formulation. In reality, it is likely that the ice–lake contact boundary is characterised by complex and highly variable conductivity in both space and time, complexity which is not possible to resolve within our current model."

Because of the formulation of GlaDS, and the continuous dependence of both sheet and channel discharge on the hydraulic potential gradient (see Appendix Equations B6 and B12) it is difficult to meaningfully isolate the ice-marginal lake from the sheet as proposed. While it is possible to isolate the lake from the sheet (in the sense that sheet flux does not alter the lake level), the changing hydraulic potential at the boundary would still modify sheet discharge close to the lake–glacier contact. In effect, the influence of the lake would still drive the glacier hydrology but the lake itself would not be lowering at the physically correct rate. As a result, we do not consider this to be a worthwhile sensitivity test. However, we do acknowledge that the specific assumption itself may not be true at all glaciers and may vary seasonally at specific sites.

Comment 2.8: L400 ish: Can you comment more on why the response to basal melt is so different with GlaDS and the Kingslake and Ng (2013) result?

Reply: To address this comment, we have added additional text to the paragraph explaining why the response to basal melt is so different between two. This text can be found on lines 410–414:

"In the Kingslake and Ng (2013) model, basal melt is fed directly to the linked-cavity system. With independent effective pressures in the channel and the linked cavity system, higher basal melt lowers the effective pressure in the linked cavities only. Because sheet to channel exchange occurs at a rate proportional to the effective pressure difference, meltwater input accelerates cavity–channel exchanges, promotes earlier channel growth and triggers lake drainage at lower lake levels, with a lower peak Q_r"

Comment 2.9: Figure 7: why is v_b much larger for the modelled than the observed? Why are observed flood anomalies so much larger than modelled flood anomalies? Later in the discussion you say the model is an underestimate but it seems the other way around, based on model/observation comparison. Can you account for this?

Reply: We expect that the mean v_b is greater in our model than observed (in a limited portion close to the lake margin) because of our free boundary condition in the stress balance solution along the lake boundary, which effectively corresponds to a permanently empty lake. We describe this on line 484 (and expand upon on line 540). As to why observed flood anomalies are so much larger than modelled flood anomalies, we expect that this reflects both the duration of observed (days) and modelled floods (months) and also our fixed boundary conditions. We have added text to this effect on line 476–484

"Livingstone et al., (2026) attributed the observed acceleration to i) ice inflow into the lake as a result of a sudden drop in water level and change in force balance at the ice front, and ii) a transient reduction in basal friction as the sudden influx of flood water overpressurises remaining channels and the nascent distributed system becoming re-established at the end of the melt season. In our model, flood duration is longer than observed (see Section 5.3). The resulting change in effective pressure during a flood occurs over a longer duration in our model, which in turn leads to a more muted velocity response than is observed (Fig. 7). In addition, the boundary conditions in our ice-dynamic solution remain fixed through time and we do not simulate the change in frontal forces as the lake drops during a flood..."

We are unclear on the third point of this comment, as we currently state that our model both under- and overestimates velocity compared to observation. We overestimate background v_b within a limited distance from the lake (due to simplified boundary conditions) and underestimate the magnitude of velocity anomalies during floods because of flood duration and our simplified boundary conditions. We hope that by addressing the first and second comment we have addressed the third.

Comment 2.10: L450: what about the other two subglacial lakes? Although the flowpath passes through anomaly 1, do the model results support the formation of a subglacial lake there?

Reply: We are not able to comment on the formation of subglacial lakes elsewhere in our model, which exist further upglacier than the drainage pathway of our modelled ice-marginal lake. Anecdotally, one subglacial lake does seem to drain coincident with an observed flood in 2015 (see Livingstone et al., 2019), but the hydrology of the three lakes likely represents a complex mix of glacier-wide hydrology that is beyond the scope of this paper.

Comment 2.11: L476: can you comment more specifically on the limitations of your model setup? What aspects of the model setup could improve your approach? You did a nice job explaining the limitations of GlaDS but would love to hear more about the continuity eqns, boundary conditions, etc that was added to the default GlaDS setup.

Reply: We do dedicate a section of our limitations to those linked to our model itself and not inherited from GlaDS (see lines 527–544). To make this more obvious, we have changed the opening of that paragraph to be:

"Besides the limitations of GlaDS inherited by our model, we also make other assumptions which influence our Greenland model outcomes and likely dampen the peak flood discharge."

In addition, we have added a line 531–533 which reads

"We assume a perfect connection between the ice-marginal lake and the distributed sheet, which may dampen the flood hydrograph by allowing the lake to constantly leak into the subglacial system." to reflect the limitation raised in comment 2.7.

Minor comments

Comment 2.12: L174: Can you provide a citation or justification for initializing phi at 90% of the overburden pressure? Same thing with the initial sheet thickness.

Reply: There is not an agreed-upon best-practice for initialising GlaDS models. However, in initialising ϕ at 90% of overburden and sheet thickness at half of bump height, we are following Hill et al., 2024 and we have added this reference.

Comment 2.13: L176: Please specify what you mean by "the other three boundaries"

Reply: We have changed the sentences prior to be clearer so the relevant text on lines 182–184 now reads:

"At the terminus, we prescribe ϕ to be $\approx 1\times10^4$ Pa as our GlaDS Dirichlet boundary condition. Though arbitrary, this does not qualitatively affect the results. We impose zero water inflow as our Neumann boundary condition along the other three upglacier boundaries"

Comment 2.14: L227: What do you mean by "vigorous"? I would use a more quantitative phrasing here.

Reply: We have changed this to "higher amplitude"

Comment 2.15: Line 256 – typo in "anisotropic"

Reply: Thank you for noting this, we have corrected it.

Comment 2.16: Figure 4 – graphically specify high k_c case and low k_c case for additional clarity (rather than just high case, low case)

Reply: The high case and low case labels apply to the entire row, rather than channel conductivity specifically. We have left the figure as is.

Comment 2.17: L405: Since not all readers will be doing a detailed read of the full manuscript, I wonder if this discussion could use the full words rather than the variable names. Otherwise it's a bit challenging to go back and forth to understand what each variable is.

Reply: Rather than only using the full words at this point of the discussion, we have gone through the discussion and re-established the full-variable names on first use.

Comment 2.18: L437: what happened to the final flood that was not hindcasted by the model? You discuss this later (that your model does not account for glacier thinning which could help trigger an additional jokulhlaup) but could be good to mention either that you will discuss it later or provide some quick explanation here before moving on.

Reply: We have added text to line 450–451 which reads:

"The eighth flood (in 2023), not hindcasted by our model, is characterised by a short return interval relative to previous floods (see Section 5.3.)"