

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

Reviewer 1

We thank Tómas Jóhannesson (Reviewer 1) for their positive and helpful reviews. Below, we detail our response to each comment. Where relevant, we also include the nature (in blue) and location of our manuscript change.

General comments

Comment 1.1: There is a brief model description on p. 5–6 and in Appendix B. This includes explanations of model parameters and variables that are studied later in the paper. However, the variable l_c (channel sheet width) and M_r (basal melt rate) are not explained although they are part of the sensitivity analysis shown in Figure 3. Consider adding a short description of these parameters/variables.

Reply: Thank you for identifying this oversight on our part. We have added relevant text and five additional equations (B4 and B11–14) to our appendix describing the key GlaDS equations. These can be found on lines 590–593 (B4) and lines 616–627 (B11–14)

Comment 1.2: In l. 102/103 you say “As in the model of Kingslake and Ng (2013), we do not account for frictional heating in our solution.” If you are just explaining that the model assumes the water to be at the pressure melting point, it would be better to just say that. The assumption that the water is maintained at the pressure melting point means that potential energy is dissipated through friction in the water flow so that frictional “heating” is in fact represented in the model although the water cannot be said to be heated. Perhaps clarify this statement?

Reply: Thank you for pointing this out, we have amended the text on line 106 to read:

“we assume water exists at the pressure melting point”

Comment 1.3: In Eq. (2), the y-component of the basal sliding velocity (v_b) appears in the numerator but the x-component does not. This seems strange. Consider whether this is as intended. Also, you might briefly explain the difference between the formulation of the Schoof-type sliding law in ISSM and the more well-known original formulation by Schoof.

Reply: Thank you for identifying this mistake, it should be U_b in the numerator so we have corrected this in Eq. 2.

ISSM’s Schoof-style friction is in effect identical to the original Schoof (2005) implementation, it only differs in writing the drag coefficient as C_s^2 and the exponent as $m = 1/n$. This ensures positivity during inversion for C_s but has no bearing on the law’s behaviour. We have added text to this effect on line 119–120 so that it reads:

“(note that C_s^2 , rather than C_s itself, is used to ensure a positive number when inverting for basal friction)”.

Comment 1.4: In lines 375–377 you state that “Ice near to the lake accelerates markedly” but in fact the acceleration affects a much larger area. Clarify. You also mention that “Such a velocity pattern has been observed during jökulhlaups at ... Skaftá by Magnússon et al. (2007). The velocity pattern observed by Magnússon affects almost the entire Skaftárjökull outlet glacier, not just an area near the subglacial lake. Clarify this also. You might call this location “Skaftá cauldrons” as Skaftá is the name of the river outside

the glacier, whereas the jökulhlaups in question are released from cauldrons in W-Vatnajökull called Skaftá cauldrons.

Reply: Thank you for noting this, which we agree is imprecise. We have corrected to Skaftá cauldrons and we have changed lines 382–383 so that they now read:

“The glacier accelerates during flood growth, with the increase in U_b propagating downglacier from the lake outlet, before abruptly slowing down during flood recession after peak flood discharge Q_r ”

this in turn changes the meaning of the subsequent lines (383–385) on the Skaftá cauldrons to be more faithful to the observations of Magnússon et al (2007).

Comment 1.5: In l. 399 you mention melt rates of 1 m a^{-1} to 5 m a^{-1} . These seem very high. Explain

Reply: These are arbitrary melt rates in a synthetic model and fall within the range of melt rates tested elsewhere. For example, in the Subglacial Hydrology Model Intercomparison Project (de Fleurian et al., 2018) *Suite A* tests a range of melt input values up to 50 mm d^{-1} , or $\approx 18 \text{ m a}^{-1}$. We have not made a text change in response to this comment.

De Fleurian, B., Werder, M.A., Beyer, S., Brinkerhoff, D.J., Delaney, I.A.N., Dow, C.F., Downs, J., Gagliardini, O., Hoffman, M.J., Hooke, R.L. and Seguinot, J., 2018. SHMIP The subglacial hydrology model intercomparison Project. *Journal of Glaciology*, 64(248), pp.897-916.

Comment 1.6: In l. 437 you explain that “Of these floods, seven are hindcasted by our model (Fig. 5f), closely agreeing with observations in terms of timing and the amplitude of change in l_h .” This is impressive but you might explain that this success is mainly a consequence of the fitting of the simulated inflow to the lake with a degree-day model and the capacity of the lake which depends on easily measured quantities and continuity (assuming that the lake will eventually drain at a high stand for more or less any subglacial hydrology model configuration). The success of the impressive fitting to the observed variations in the lake level is only indirectly caused by the coupled subglacial hydrology - ice flow model, which is the main subject of the paper. Explain briefly how the successful fitting to the lake level variation is achieved and that it does not necessarily reflect successful simulations of the subglacial hydrology.

Reply: While we acknowledge that an accurate inflow is important, we do disagree with the assertion that the lake drains mainly as a consequence of the simulated inflow being tied to a realistic climatic forcing. While continuity can explain why accumulated inflow eventually produces drainage, our model reproduces the timing of drainage, the amplitude of lake level, and controls the level at which the lake stops draining, rather than simply eventually draining at a reasonable high stand. This reflects the configuration and hydraulic potential of the wider subglacial system and its coupling to the lake. Nonetheless, in order to address this comment and highlight the importance of an accurate inflow in our solution, we have modified the paragraph (lines 453–464) which directly follows on from the above quote to now read:

“We caution that the successful hindcasting of l_h here is not in and of itself validation of our modelling approach or parameter choice. The nonlinear evolution of l_h and Q_r through time (Fig. 5f) is driven in part by forcing our model with a realistic, time-varying climate, which drives both the lake input Q_{in} and $\nabla\phi$ through the glacier-wide melt input to the bed (see Section 4). In contrast, flood cycles are cyclical in our synthetic model when the system is forced with constant basal melt and lake input (see Section 3). Elsewhere, nonlinear variability in flood discharge has been shown to arise when a transient climate is imposed (Kingslake and Ng., 2013, Ng et al., 2009). Given a fixed lake area and realistic inflow, it is reasonable to expect that the lake will eventually drain once it reaches a highstand, irrespective of the state of the subglacial system and the nature of lake–glacier coupling. However, the timing, magnitude and variability of drainage depend on the evolving state of the subglacial system adjacent to the lake. In our model, drainage occurs when the hydraulic potential gradient across the ice–lake contact becomes sufficiently large to drive substantial water flux from the lake into the subglacial drainage system. The resulting reduction

or reversal in hydraulic potential gradient across the ice–lake contact then promotes flood termination. Thus, while the imposed climate and lake geometry contribute to the successful hindcasting of l_h , the timing and character of individual drainage events emerge from the coupled lake–subglacial hydrology.”

Comment 1.7: Did Flowers et al. (2004) really work with sheet thicknesses exceeding tens of metres (l. 486) (?). This seems excessive. In the 2004 GRL paper, she mentions 7–11 m sheet thickness. If you are referring to this range you should say “sheet thicknesses on the order of ten metres” or something like that. This sheet thickness magnitude refers to Björnsson’s (1997) interpretation of observations of the catastrophic 1996 jökulhlaup from Grímsvötn, which you might find worth referencing in this context.

In the following sentence (l. 486–488) you state “Recent repeat satellite observations identified metres of uplift along a jökulhlaup pathway ...” quoting Magnússon et al. (2026). The lifting reported by Magnússon et al. was close 1 m. Please clarify. Maybe say “identified approximately 1 metre of uplift” or “identified 1 metre of uplift” about Magnússon’s observations.

Reply: Thank you for noting the lack of accuracy in both of these statements. We have corrected each as suggested and we have added the reference to Björnsson (1997). Lines 505–509 now read:

“Work by Flowers et al. (2004) on subglacial lake Grímsvötn, albeit with a prescribed rate of flux from the lake, found that an elastic model able to represent sheet thicknesses on the order of ten metres can explain rapid escalations in jökulhlaup discharge observed over hours to days at the site (Björnsson, 1997). Recent repeat satellite observations identified ~ 1 m of uplift along a jökulhlaup pathway, suggesting that instantaneous ice uplift in a high water-pressure regime is crucial to enabling high jökulhlaup discharge (Magnússon et al., 2026)”

Comment 1.8: In l. 518–519, you state: “... and recognizing that our model successfully reproduces flood timing across a multidecadal record without tuning to individual events, we view the discrepancies in duration and peak as a tractable modelling challenge rather than a fundamental limitation.” The successful reproduction of the flood timing is interesting, but as I mention above does not, in my opinion, have much to do with the coupled subglacial hydrology - ice flow model developed in the paper. It seems to me that in order to reproduce the short flood duration and higher maximum discharge, a substantial improvement in the model is necessary, such as the implementation of large-scale (elastic) lifting of the ice from the bed, as stated in the paper at several places. This would be a fundamental improvement, so the sentence that I quote above is perhaps too optimistic or describes this too lightly, although I hope this problem is indeed “tractable”.

Reply: We agree that we have framed the necessary improvements too lightly. Including a description of elastic uplift and treatment of water temperature within the model would indeed require significant development. However, we do note that since submission of our manuscript, Wells et al., 2026 have described an initial version of GlaDS-2 which includes a representation of uplift once water pressure exceeds overburden pressure. While that preprint is based upon a finite-difference model with explicit timestepping (requiring significant changes before it can be incorporated into a finite element model like ISSM), it does offer a promising approach and suggests that the necessary improvements, at least the elastic deformation, are indeed tractable.

In recognition of this comment, we have adjusted the relevant text on line 545–549 to read:

“Notwithstanding these limitations, and recognising that our model successfully reproduces flood timing across a multi-decadal record without tuning to individual events, we view the discrepancies in duration and peak as a significant but tractable modelling challenge, rather than a fundamental limitation. The recently described finite-difference formulation of Wells et al., (2026) provides a promising avenue for future development, incorporating an implicit representation of ice uplift, pressurised flow, and free-surface flow”

Wells, S., Utkin, I., Hewitt, I.J., Farinotti, D. and Werder, M.A., 2026. GlaDS-2: modeling subglacial drainage including ice uplift and free-surface flow in two dimensions. EGU sphere, 2026, pp.1–31.

Minor comments

Comment 1.9: l. 3: “these models are 1D representations” to “these models are typically 1D representations”

Reply: We have adopted this suggestion.

Comment 1.10: l. 18: “drain beneath glaciers in flood events called jökulhlaups” to “drain beneath glaciers in floods called jökulhlaups”

Reply: We have deleted events as suggested.

Comment 1.11: l. 18: “jökulh-laup” to “jökul-hlaups” (plural and ugly hyphenation)

Reply: We have pluralised jökulhlaup but the hyphenation is a consequence of LaTeX’s typesetting and a desire to maintain even margins. This is unlikely to be an issue upon typesetting of the final manuscript.

Comment 1.12: l. 27 and 39: “of a flood hydrograph” to “of the flood hydrograph”

Reply: In each case we are referring to a generic concept rather than a specific flood hydrograph. We have left the text unmodified.

Comment 1.13: l. 68: “ISSM-GlaDS” to “ISSM–GlaDS” (long dash)

Reply: We have corrected ISSM-GlaDS to ISSM–GlaDS with an en dash throughout.

Comment 1.14: l. 72: maybe reference Werder et al. (2013) here too?

Reply: As suggested, we have added a citation to Werder et al., (2013) here.

Comment 1.15: l. 72: “is assumed to exist at the pressure melting point” to “is assumed to be at the pressure melting point”

Reply: We have made this change

Comment 1.16: l. 137: “length either side” to “length on either side”

Reply: Neither are incorrect so we have left this as is.

Comment 1.17: l. 172: “ $\mathrm{mm}, \mathrm{a}^{-1}$ ” to “ $\mathrm{mm} \mathrm{a}^{-1}$ ” (no comma)

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

Comment 1.18: l. 189: “and into the following is” to “and the following one is”

Reply: As suggested, we have made this change.

Comment 1.19: l. 191: “and $\nabla\phi$ is generally perpendicular to the ice surface slope”. Is this “generally” the case. The contours of the potential are not always parallel with the contours of the ice surface elevation but they do not seem “generally” perpendicular to each other. Maybe reword? Again in l. 352, I do not understand what is meant by “perpendicular to $\nabla\phi$ ”.

Reply: We have reworded lines 197–198 to read:

“channelised drainage is limited and $\nabla\phi$ is controlled by the ice surface slope”.

We have deleted the text in question on line 352, since the contours themselves are not shown in the figures.

Comment 1.20: l. 230: “Fig. 3b[i]”) to “Fig. 3c[i]”) ?

Reply: We have corrected this mistake, thanks for pointing it out.

Comment 1.21: l. 244: “filling-drainage” to “filling–drainage”

Reply: We have corrected this.

Comment 1.22: p. 15: Add period at the end of the caption of Table 2.

Reply: Thanks, we have corrected this.

Comment 1.23: l. 313: “lake level” to “lake-level”

Reply: We have made this change here and elsewhere

Comment 1.24: l. 328: “had an estimated minimum mean discharge”. Strange wording. Shouldn’t you compare with the observed peak discharge?

Reply: We agree that the wording is strange, but we do not have an observed peak discharge so it remains correct. We have updated the text on lines 334–335 to read:

“which had an estimated mean discharge of at least $480 \text{ m}^3 \text{ s}^{-1}$ and a likely considerably higher instantaneous peak”

to clarify our meaning.

Comment 1.25: p. 19: add period after “Fig” (two instances)

Reply: Thank you, we have corrected these and all other instances

Comment 1.26: l. 412: “is strongly controlled” to “are strongly controlled”

Reply: We have corrected this.

Comment 1.27: l. 483: You might add a reference to Magnússon et al. (2026) here.

Reply: We have added this as suggested.

Comment 1.28: l. 492: Add a period after “(Section 2.1)”.

Reply: We have corrected this.

Comment 1.29: l. 513: “in our ice dynamic solution” to “in our ice-dynamics solution”

Reply: We have corrected this.

Comment 1.30: l. 527: Regarding borehole and moulin datasets, you might refer to some of the papers by Meierbachtol, Harper and colleagues, such as DOI: 10.1126/science.1235905, who have produced a lot of interesting borehole data about subglacial hydrology in Greenland.

Reply: We have added the suggested reference.

Comment 1.31: l. 527: “emulators” to “models”?

Reply: We are referring to emulators specifically, so we have left this as is.

Comment 1.32: l. 537: “jökulhlaup events” to “jökulhlaups”

Reply: We have made this change.

Comment 1.33: l. 538: “lake water temperatures” to “lake-water temperature”

Reply: We have made this change.

Comment 1.34: l. 545: missing period at the end of the sentence.

Reply: We have corrected this.

Comment 1.35: l. 600: “Og” to “og”

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

Comment 1.36: l. 781: “Journal of glaciology” to “Journal of Glaciology”

Reply: We have also corrected this.

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 U_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 flotation 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 $380 \text{ m}^3 \text{ s}^{-1}$ to $960 \text{ m}^3 \text{ s}^{-1}$.”

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 of 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 ϕ 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 $\approx 1 \times 10^4$ 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.)”