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 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 $\mathbf{U}_\mathrm{b}$ 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 $\mathrm{m\,a^{-1}}$ to 5 $\mathrm{m\,a^{-1}}$. 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 $\mathrm{mm\,d^{-1}}$, or $\approx$18$\mathrm{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 appoximately 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 $\sim$ 1m 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 to 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. EGUsphere, 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 pluralsied 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: "$mathrmm\,,\mathrm{a}^{-1}$" to "$mathrmm\,\mathrm{a}^{-1}$" (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 $m^3\ s^{-1}$ 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

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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.