Dear Editor,

We thank both reviewers for their positive assessment and constructive comments on our manuscript titled “Modelling the effects of proglacial lake filling and drainage on lake-terminating glacier dynamics”. Their feedback has substantially strengthened the revised version.

Following the major comment of Reviewer #2 on Experiment 3, we carried out an additional high-temporal-resolution (1-day) simulation of a rapid lake-level drawdown. This simulation reveals a markedly higher stress response in the glacier, with important implications for GLOF hazard assessment.

In addition, we have revised the figures as suggested, added several supporting figures to the Supplementary Material, and made corresponding clarifications throughout the text. We have addressed all reviewer comments in full, and our point-by-point responses are provided below.

We hope the revised manuscript is now suitable for publication and look forward to your decision.

Sincerely,

Ankit Pramanik

On behalf of all co-authors

Response to Reviewer #1

Summary

This very well-written and thoroughly referenced paper provides an account of modeling experiments conducting using ISSM to simulate the effects of an ice-marginal lake on the dynamics and thinning of a west Greenland glacier. The authors simulate the effect of flotation and a potential lake-level controlled subglacial water pressure field, omitting the role of frontal ablation. The authors find long-lived increases in velocity and thinning rates associated with lake presence that extend far up-glacier. The study has an interesting focus and could significantly advance our understanding of lake-terminating glacier dynamics, but I find several issues with the study’s design that have the potential to substantially alter the manuscript’s findings. These concerns are described in detail in my “major comments” section. I recommend the authors undertake a major revision to address these concerns or better argue why the results are still valuable despite these issues.

AP: *Thank you for your thoughtful review. Before addressing the comments line-by-line, we would like to clarify the aims and scope of the manuscript.*

The primary focus of this study is to understand the effect of ice-dammed lake-level changes on glacier dynamics. While glacier dynamics are influenced by several boundary factors, such as bed topography, basal hydrology, mass balance, and frontal ablation, the unique characteristic of lake-terminating glaciers is that the lake levels fluctuate over time through filling and drainage – either once or repeatedly. Assessing how these fluctuations impact glacier dynamics is the

central aim of this work. To our knowledge, this is the first study to investigate the effects of lake-level changes on lake-terminating glacier dynamics using ice flow modelling.

Our study is purely a sensitivity study on a semi-synthetic domain – we leverage a known ice sheet catchment's geometry and mass balance to test the impact of a particular process, lake level change. We do not aim to simulate "realistic" retreat of this glacier, nor do we model the processes of lake filling or drainage. Our experiments are deliberately designed to control for other influencing factors, either eliminating or holding them constant. We also analyse our results primarily in terms of the difference between scenarios, placing little emphasis on absolute magnitudes of change.

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Major comments

Eqn 3: How is this applied to land-terminating glaciers? Are they assumed to have Pw = 0? To me, it seems more appropriate to apply some assumed floatation fraction (or a range of flotation fractions) for land-terminating glaciers. For lake-terminating glaciers, you could do the maximum Pw between the constant flotation fraction and Eqn 3. While the lakes certainly induce a difference in subglacial water pressure, it seems likely to overstate this importance if you are assuming that land-terminating glaciers have Pw = 0 (which we know to be far from the truth). Accounting for a more realistic (but still simplified & tractable) water pressure field under the land-terminating glacier will likely reduce lake- vs. land-terminating glacier difference in all subsequent analyses.

AP: *The subglacial water pressure formulation is applied identically for both land- and lake-terminating glaciers, with only the lake level varying between scenarios. Since lake level is substituted for sea level, within the lake mask area (Fig 1C), "sea level" is locally assigned as 0m (land-terminating) or e.g. 100m (lake-terminating) within the lake mask, where b = -200m at the terminus, giving (sl-b) in Eqn 3 due to lake height; elsewhere in the model domain depth below sea level gives Pw. As a result, any bedrock below the lake (sea) level experiences non-zero water pressure regardless of glacier type, thereby removing any fundamental discrepancy in applied water pressure between the two glacier types. Lake level changes are applied exclusively within the lake domain; basal forcing remains the same elsewhere in all our experiments. We realise this set up was inadequately described, and we add this explanation to Section 2.1 of our revised manuscript.*

This assumption will induce Pw=0 at the lake mask region for the land-terminating glacier. In the revised manuscript, we will apply a very low lake level (<1 m) instead of 0 m lake level. However, this will not change our qualitative results.

*In the revised manuscript, we elaborate this under section 2.3 as (**Line 165**) "To investigate the dynamic response of lake-terminating glaciers to varying lake levels (lake filling and draining), we leverage the sea-level adjustment feature in ISSM (Larour et al., 2012). Within the lake mask area (Fig. 1c), we prescribe the 'sea level' (called lake level henceforth) independently, keeping actual sea level unchanged elsewhere. This has the effect of setting basal water pressure due to lake height within the mask, where the lake floor is at -200 m, and due to sea level elsewhere, where basal water pressure remains unchanged following equation 3 (Fig. 2)."*

It is important to note that our focus is on the comparison between land- and lake-terminating glaciers under otherwise identical physical conditions. Our primary aim is to highlight the difference between the two scenarios rather than quantify absolute changes. Therefore, while water pressure could be better formulated (and this would certainly be a worthwhile avenue of further exploration), any biases arising from physical assumptions are effectively cancelled out in the differential analysis. Using a constant flotation fraction would make water pressure dependent on ice thickness, which varies disproportionately between land- and lake-terminating glaciers, complicating the comparison. Instead, using water pressure dependent on lake level provides a first-order approximation that offers a clearer understanding of the effect of lake presence, which is the central focus of this manuscript.

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L140: Does the inversion already include the Pw term, or is this only applied in the forward model? If the lake's effect on upstream on Pw is substantial, shouldn't the inversion show this via lower friction coefficients? It would be good to see the friction coefficient on Figure 2 (maybe instead of surface elevation or mask), to be more explicit about when the Pw parametrization enters your workflow, and to explain why the Pw parameterization is necessary if it correct that the lake's effect on upstream Pw should show up in the friction inversion driven by the data alone. If the friction inversion does show this effect, isn't it "double counting" the role of the lake to then add a Pw parameterization on top of this?

AP: *The inversion includes the water pressure Pw term; however, it is performed only once at a constant sea level everywhere. The idea of inversion was here to find an optimum friction coefficient that was kept constant in all the experiments. While friction coefficient should be lower at the region of higher water pressure during inversion, it is optimized with the observed velocity. Further, when the water pressure changes in forward run (whether through lake level change, or through subglacial hydrology modelling), the friction coefficient is not affected, only the velocity is. This is the standard practice in ice flow modelling* (Ehrenfeucht et al., 2023; Sommers et al., 2018). *In other words, friction inversion provides the general pattern acting as the base, whereas Pw change in forward model leads to transient velocity response.*

In the lake mask area, we change the Pw formulation to make it lake level dependent, and we conduct sensitivity experiments to this lake level. We find that the glacier responds within the lake mask area, but also that responses propagate upstream out of the zone where we have prescribed that sliding should be Pw-dependent, showing that the hydrostatic effects of the lake extend beyond the area in which we force this control.

The lake extent is shown in Fig. 1c (green contour), delineating the area where lake level changes influence water pressure. This includes the tributary to the lake, where the bed lies below sea level. As shown in Fig. 2b, the bed elevation below sea level extends considerably upstream. The lake mask is restricted to the downstream part of the catchment (Fig. 1c), since the disappearance of the lake-terminating glacier outlet would either connect the lake to the ocean or cause it to drain entirely.

We agree that including a figure of the spatially distributed friction coefficient would improve clarity for the reader, and we have provided the map of friction coefficient in Fig. 2 in the revised manuscript.

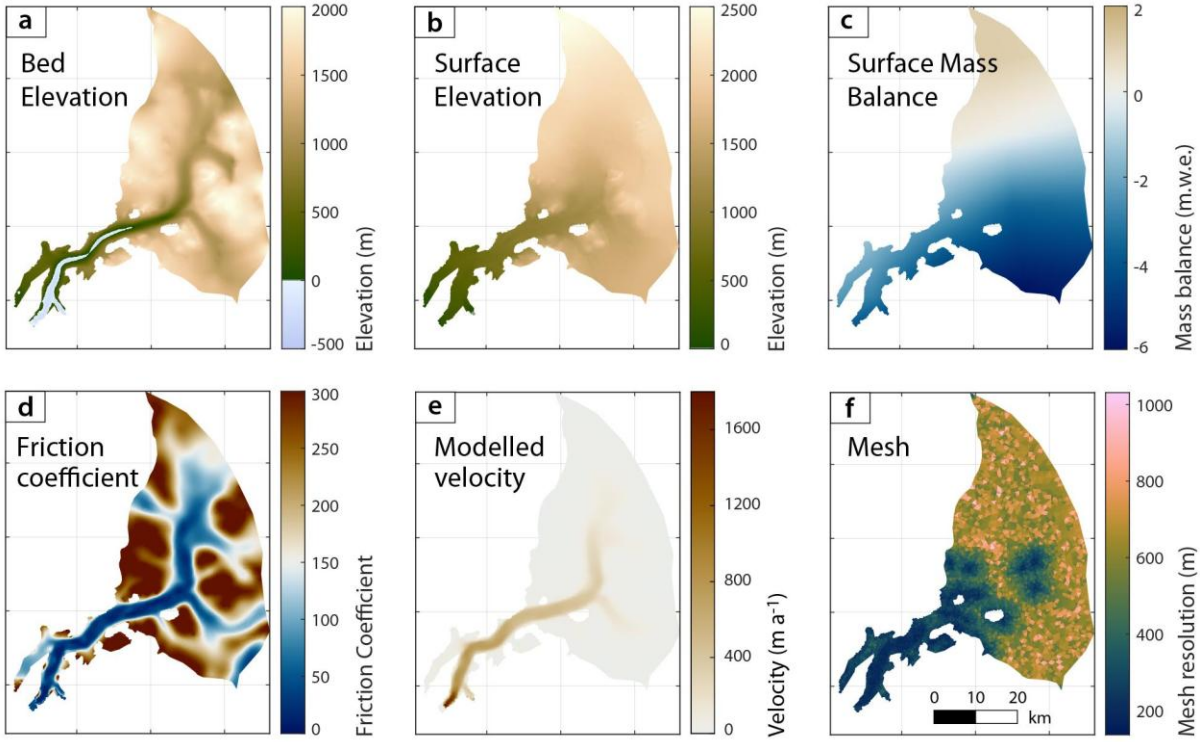


Fig. 2: Initial model conditions including bed elevation (a), surface topography (b), prescribed surface mass balance (c), friction coefficient from inversion (d), modelled velocity (e), and the model mesh resolution (f).

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L197-200 & Fig 3: The result of far-reaching lake effects seems contingent on the glacier terminus position being fixed in space and time. In reality, the glacier would likely retreat out of the lake basin before the ice surface elevation adjusted 10s of km upstream. It seems like holding the terminus fixed will result in unrealistically thin terminal ice thickness because it can only accommodate mass loss through thinning rather than retreating (and maintaining thicker ice, which will impact likelihood of flotation). This seems tied to Figure 3, which shows the greatest surface elevation change upstream, with little elevation change near the terminus. This is the opposite of what we usually see on lake- (and land-) terminating glaciers (e.g., Larsen et al; 2015; their Fig. 2; https://doi.org/10.1002/2015GL064349). How can you explain this, and how can you convince the reader than physically unintuitive results here don't cast doubt on the results that follow?

AP: *We think the reviewer may have meant to write "unrealistically thick terminus", which would fit with their comments about Fig 3 and an unintuitive response to a lake terminus. Importantly, fixing the ice front does not imply the exclusion of ice flux across it in our model. Ice flux through the front continues, allowing ice to move from upstream to downstream; the model removes ice*

from the frontal position, without a physical removal process, i.e. without explicitly modelling calving or subaqueous melting. We simply do not allow frontal retreat driven by frontal ablation, in order to eliminate the effects of calving (which is very poorly constrained, in terms of model implementation for lake settings) and isolate the role of lake-level changes – our focus for sensitivity testing. Our experiments confirm that the glacier remains dynamic and does not produce unrealistically thick or thin terminal ice. We will add an explanation of how ice flux through a "fixed front" is treated in the model.

We also note that the extent of the profile shown in Fig 3 is only the terminal 20 km of a >80 km catchment. We will draw attention to this in the caption to Fig 3, and refer the reader also to Fig 4, which shows that the greatest thinning is indeed in the terminal region. The limited thinning or inversion in elevation change in the immediate vicinity of the terminus, which the reviewer alludes to from Fig 3, is similar to an elevation change inversion shown in Fig 2 of Larsen et al. 2015.

We agree, in reality, glacier retreat would be governed by frontal ablation. However, calving laws and subaqueous melt model implementation are themselves poorly constrained in lake settings, and would require thorough sensitivity testing. Holding the ice front fixed in our experiments allows us to perform controlled sensitivity experiments to specifically investigate the role of lake level changes.

*We have stated these under the discussion section (4.4) as (**Line 525**) "Our exclusion of calving and subaqueous melting likely leads to conservative estimates of glacier sensitivity, as these processes account for a substantial proportion of mass loss in lake-terminating systems (Carrivick & Tweed, 2013; Minowa et al. 2021; Caldwell et al. 2025). Both mass loss processes influence and are influenced by lake water temperature, lake ice cover, water depth and bed topography, and their rates, distribution and calving mechanisms at ice-dammed lake margins are not well-constrained (e.g. Mallalieu et al. 2020), limited by sparse long-term observational data and uncertainties in model implementation. Incorporating calving and subaqueous melting would likely amplify the lake level effects we identify, through delivering more water into the proglacial basin, and could reveal additional instability mechanisms. Recent advances in calving law development (Benn et al., 2007, 2017) and evaluation (e.g. Barnett et al. 2026) and subaqueous melt parameterisations (O'Leary & Christoffersen, 2013; Slater et al., 2017) provide pathways for more comprehensive future modelling."*

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For the rising/lowering lake level analysis: Your discussion and overall paper framing around the rising/lowering lake level experiments seems to omit that the presence of a proglacial lake that can repeatedly fill and drain is somewhat uncommon, and requires the kind of distributary glacier geometry with a closed lake basin observed at your study site. In my experience (largely Alaska, High Mountain Asia, Patagonia focus, so maybe Greenland is different), for most sites, there is a moraine or bedrock ridge that dams a proglacial lake, which both controls the maximum height of the lake, and also means that drainage is typically a one-way process (because drainage is triggered by a dam breach). Maybe I am oversimplifying this, and I still see

a lot of value in those analyses for thinking about how the terminus responds to rising/falling lake levels, but the idea of repeated fill & drain being able to promote crevassing and destabilization of the terminus seems like it would require an uncommon kind of terminal configuration.

AP: *We clarify that our study does not consider repeated filling and drainage cycles. Each of our lake level experiments considers a single filling or drainage event, and we find that a single event is sufficient to cause responses that propagate more widely in the catchment and stimulate non-linear changes to grounding line mass flux. We also find that a single more "rapid" drainage event (though see our discussion of "rapid" with Reviewer 2) would produce a stress response that could lead to mechanical failure of or within the ice marginal zone.*

We do, however, consider in our Discussion the implications of our findings for cases where glaciers experience more than one or repeated filling/drainage events. Some settings undoubtedly favour a one-time lake drainage and transition to land-terminating (e.g. where the bed slope dips down-glacier and a proglacial basin is dammed by a moraine or bedrock ridge, such that dam-breach by erosion is one-way, as the reviewer describes). However, other settings favour repeated filling and drainage cycles. On a retrograde bed slope, in which a proglacial lake is dammed by distally rising topography and the glacier itself, retreat of the ice margin may expose new thresholds that allow for partial or complete drainage; advance of the margin may seal those thresholds and drive lake level rise. Such settings are expected to develop around the Greenland periphery (How et al., 2021) *due to the depressed bed in Greenland's interior; indeed, examples of such fill/drain cycles are already evident* (Dømgaard et al., 2023). *Repeated, partial drainage events are also widely observed in the palaeo-ice sheet record* (e.g., Regnéll et al., 2019, 2023). *These observations and prior studies motivate our work and justify our discussion of repeated events, though our experiments themselves consider only one fill or drain event at a time.*

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Minor comments

Abstract: Putting so much focus on fill-and-drain cycles seems to undersell the novelty of your study to me. Simply running a high-fidelity flow model capable of simulating lake-terminating glacier dynamics can really improve our understanding of these systems. This is related to my last major point about the prevalence of systems in which it is possible for proglacial lakes to repeatedly drain and refill.

AP: *We agree that experimenting with an ice flow model in lake-terminating cases has great power to improve our understanding of these systems. Given the variety of factors controlling behaviour of these systems (and the expected high sensitivity to factors such as basal water pressure and calving) we begin with a sensitivity study on the aspect of lake-terminating settings that is unique: that lake level can change, either once or several times. Since our manuscript has currently given the false impression we are concerned with repeated fill/drain cycles, we will make minor edits to our Abstract and Introduction in order to clarify our motivation and scope. To our knowledge, this is the first study to quantitatively investigate the impact of lake level changes on glacier dynamics, representing the novelty of the manuscript.*

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Introduction: Very well-referenced and clear flow. This is not an issue, I just wanted to note it.

AP: *Thank you*.

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L65: You state “A key distinction between ice–ocean and ice–lake systems lies in the highly variable nature of lake level”. I think this is likely less important than systematic difference in terminal slope, ice thickness, flow speed, water depth, water temperature, and density difference between subglacial outflow and ambient lake/ocean water. Caldwell et al. (2025; https://doi.org/10.1017/jog.2025.37) and Minowa et https://doi.org/10.1017/jog.2023.42) discuss these systematic differences.

AP: *We agree that terminal slope, ice thickness, and flow speed exert greater control on glacier dynamics: these factors influence glaciers similarly regardless of their type. Water depth, temperature and stratification are also likely to be important via their control on subaqueous melt rates/profiles, and potentially on calving as well as buoyancy. However, these lake characteristics are not well-constrained and their effects are not widely considered by ice sheet modelling – they would require a detailed and dedicated exploration in themselves. Given the infancy of proglacial lake / ice sheet modelling, we begin with a sensitivity study concerning a forcing variable unique to lake-terminating glaciers – short-term lake level change – in order to isolate the importance of this variable and its hydrostatic effects. Our sensitivity experiments demonstrate that it does exert a measurable influence on glacier dynamics.*

We note that Caldwell et al. (2025) inadvertently reinforce the motivation for our focused investigation of lake level. Caldwell et al. (2025) investigated glacier retreat and frontal ablation in the context of lake expansion, finding higher rates of both associated with deeper lakes — that is, higher lake levels. We argue, however, that it is not possible to disentangle the specific impact of lake level from these observations, even if that impact is relatively small. Isolating the effect of lake level requires the elimination of frontal ablation, which is precisely the approach adopted in this study. We have acknowledged the work of Caldwell and others, and its motivation for our work, in our revised Introduction and Discussion.

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L79: This is a pretty limited account of places where information on lake formation and growth exists. Table 1 in Steiner et al. (accepted: https://doi.org/10.5194/essd-2025-315) provides a decent starting point for fleshing out this collection. The Steiner paper in general may be useful for motivating why your work matters.

AP: *Thank you for alerting us to this new paper, which is very relevant to our study. We will draw on this article to further motivate our study. That said, it is not the purpose of this paragraph (nor the Introduction) to give an exhaustive account of where information about lakes exists. This paragraph points to why lake level changes (including rapid drainage events) make lake-terminating settings different to marine, and why lake level changes are a concern that motivates further understanding.*

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L141: It would be useful to see the spatially distributed model – observation velocity mismatch. You state the modeled flow speed is higher than observed here, but there is no quantitative information about the misfit, and this seems important given your study is so focused on the lake terminating region.

AP: *We agree that including a figure of the spatially distributed friction coefficient would improve clarity for the reader, and we have incorporated that in figure 2 in the revised manuscript.*

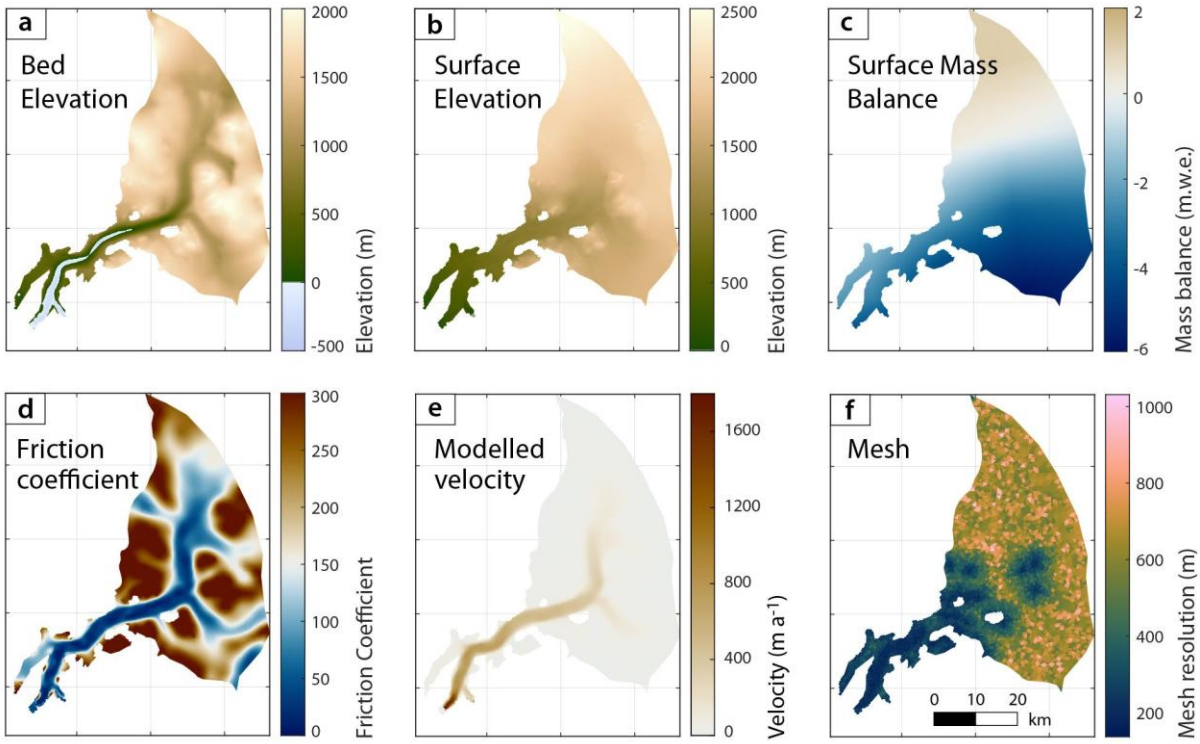


Fig. 2: Initial model conditions including bed elevation (a), surface topography (b), prescribed surface mass balance (c), friction coefficient from inversion (d), modelled velocity (e), and the model mesh resolution (f).

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L151: Worth starting that -2.65 m.w.e. per year is the glacier-wide average (I believe, given that Fig 2d shows spatially varying SMB).

AP: *Yes, we mentioned "glacier-wide average value" in the revised manuscript.*

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L170: Can you spell out a little more explicitly how you switch the model between terminus types? Is this as simple as changing the Fig 2c mask?

AP: *We do not explicitly change terminus types. We just change the lake level to 0 to represent land-terminating glacier. We only control lake level here. To make the results consistent, we first do the spin up for 0 lake level and 100 m lake level. We have rephrased this paragraph as (**L 165**) "To investigate the dynamic response of lake-terminating glaciers to varying lake levels (lake*

filling and draining), we leverage the sea-level adjustment feature in ISSM (Larour et al., 2012). Within the lake mask area (Fig. 1c), we prescribe the 'sea level' (called lake level henceforth) independently, keeping actual sea level unchanged elsewhere. This has the effect of setting basal water pressure due to lake height within the mask, where the lake floor is at -200 m, and due to sea level elsewhere, where basal water pressure remains unchanged following equation 3 (Fig. 2)."

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Experiment 2: In reality, wouldn't the max/min lake height be set by the elevation of the lake's downstream outlet? Is it a closed basin lake? After reading the paper, I think this is true, but probably worth stating clearly. Again this is somewhat tied to my last major comment.

AP: *In reality, the max/min lake levels are constrained by the elevation of the lake's outlet and the underlying bed topography. And in the case of our experimental lake, in reality this lake does occupy a closed basin. Given that this is a sensitivity study, however, whether the experimental lake levels we use represent elevations at which water would actually drain into the surrounding topography are not relevant. We use a semi-synthetic topography, designed to ensure a closed basin, ice-dammed set-up; the model does not "see" what the actual threshold of the basin is or whether any of our lake levels would drain over it or fill up to it. Our interest lies not in the maximum lake level but in the effects of lake level fluctuations, and within the scope of this manuscript, the precise mechanisms of or thresholds for lake filling and drainage are not relevant.*

*We have revised section 2.1 as (**L 116**)"Ice thickness and bed topography are obtained from the BedMachine v3 dataset (Morlighem et al., 2017; Fig. 2a, 2b). At the present-day lake-terminating outlet, we define a lake mask area (Fig. 1c) within which we locally modify the bed topography, imposing a flat lake floor behind a topographic high sufficient to allow a range of lake level experiments. This setup ensures a closed basin dammed by the ice margin, and that we exclude the influence of bathymetry on grounding line dynamics, to isolate the effects of lake level change."*

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L194: Maybe worth pointing out that this thickness pattern differs even in the absence of calving > I have always thought frontal ablation is what made water-terminating glaciers so much thicker at the terminus. You do this well throughout the paper, it just seemed like it could be more clearly stated here to emphasize this finding.

AP: *We have mentioned this fact as (**L 211**)* "*In contrast, the lake-terminating glacier develops a near-vertical terminus face, maintained by the balance between glaciostatic pressure (ice column weight) and hydrostatic pressure (water column weight)."*

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Section 3.2: This is an interesting and clearly described analysis (no concerns, just noting it).

AP: *Thank you*.

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L334: Can you provide a little more detail for why you see increase in compressive stress & reduction in driving stress away from the lake? I would think that the increase in driving stress near the lake would result in faster flow, and increase extension throughout the whole region? Although maybe what's happening is that the reduction in Pw is the biggest factor, so you're getting slowing despite an increase in driving stress?

AP: *Yes, it is happening due to the reduction in Pw, which is the biggest factor. The driving stress at the terminus is increasing due to the increase in glaciostatic pressure with the reduction of hydrostatic pressure. However, the low Pw does not lead to faster flow of ice. That is resulting in compression upstream.*

We have conducted a new set of experiments for this section and provide a revised figure explaining tensile and compressive stress in greater detail.

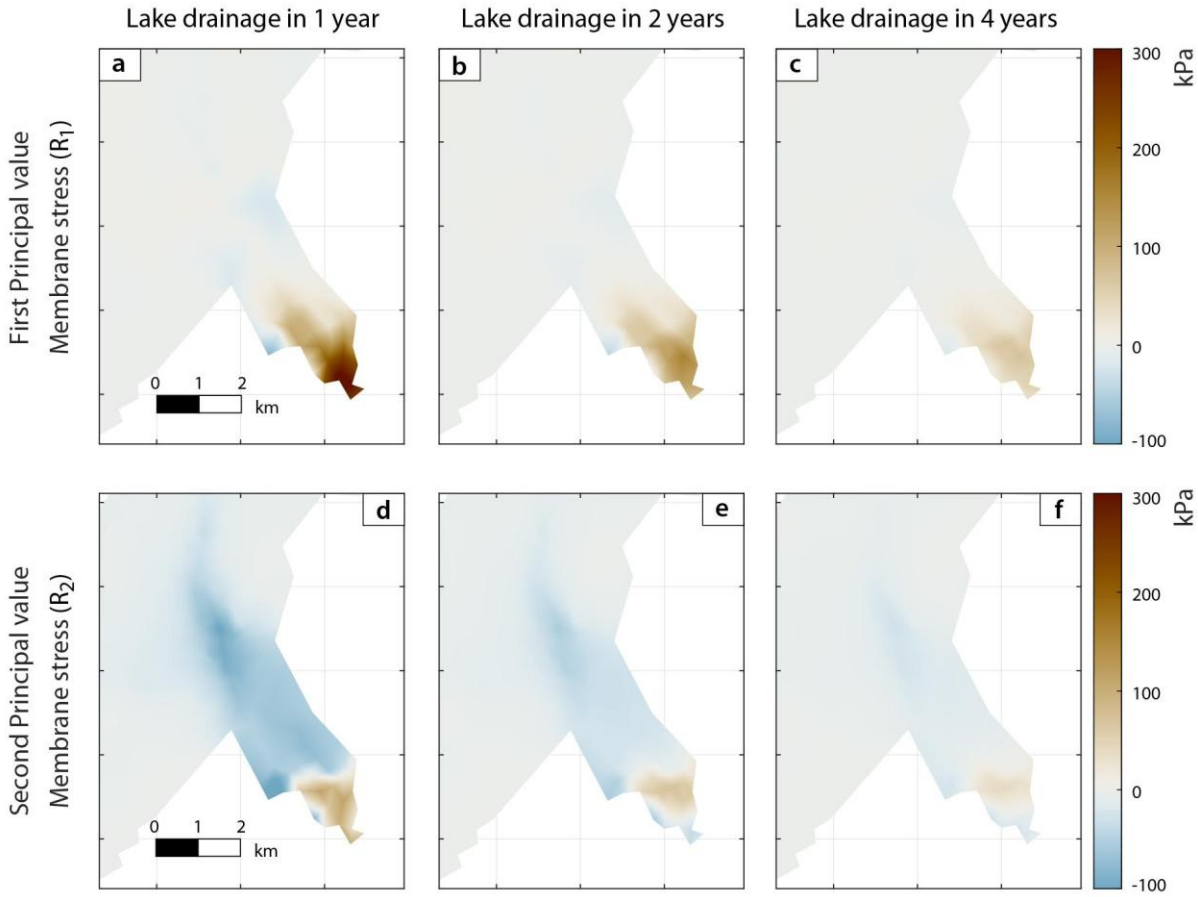


Fig. 12: Changes in first (tensile, a-c) and second (compressive, d-f) principal value of membrane stresses, in the first year after initiating a 100 m lake drainage of 1, 2 and 4-years duration, from an initially floating glacier terminus.

We have amended the new results accordingly in section 3.3. We would like to request the reviewer's attention to this section for the updated results and discussions.

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Fig 13: It is not clear where these values are calculated – are they an average over a whole region, or a specific pixel's value?

AP: *These are the maximum change in deviatoric stress at a pixel value inside the lake region. This has been stated with the revised figure.*

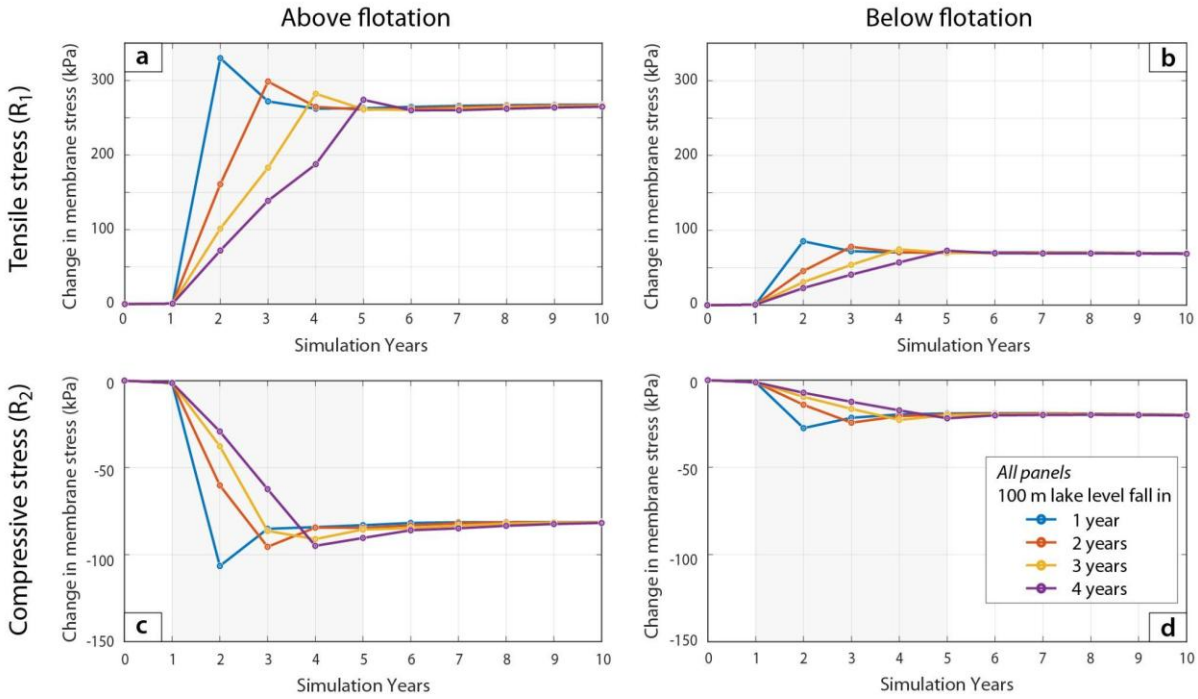


Fig. 13: Maximum change in first principal value of membrane stress (tensile) (a, b) and Minimum change in second principal value of membrane stress (compressive) (c, d) stress regimes, during rapid lake level fall from a lake level that is initially above (a, c) and below (b, d) the terminus flotation threshold. Changes are relative to the pre-drainage state. The shaded area marks the maximum duration of lake drainage (4 years).

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L380: It is worth noting in these side-by-side land vs. lake-terminating comparisons, there is often much that differs between the glaciers than just terminus condition (e.g., terminus elevation, glacier area, accumulation area, etc.). Main et al. (2022; https://doi.org/10.1017/jog.2022.114) I think shows one of the clearest studies on the influence of a proglacial lake on glacier dynamics because it is looking at one glacier over time (in which the lake drains) rather than looking across glaciers (that have many things that covary with terminus type). The Main paper is probably worth discussing at some point in your manuscript.

AP: *Thank you for pointing out the Main et al. (2022) paper. The findings of this observational study strongly support our results.*

We agree that side-by-side comparisons are likely influenced by other factors than only terminus condition, and we will amend our text to express a little caution in this regard. However, a transition from lake-to-land (or land-to-lake) is also not necessarily a controlled comparison of a lake-terminating and land-terminating state, especially if terminus change is due to ice margin retreat rather than distal dam breaching (and an otherwise unchanged glacier). The response of the glacier to lake drainage would be a useful analogue for our drainage experiments, on the other hand. Nonetheless, the Main et al. paper is extremely relevant and we

*have discussed its findings in light of ours pointing to the importance of ice margin flotation (**L 397**): "Observations suggest that buoyancy forces at the ice–lake interface are key, with pulses of rapid acceleration – and in some cases marked grounding line retreat – reported where thinning toward floating conditions has detached the glacier from bed pinning points (Holt et al., 2024; Minowa et al., 2023; Somos-Valenzuela et al., 2014; Tsutaki et al., 2013). The converse has been found to result from lake drainage and reduced buoyancy of the terminus (Main et al., 2023)."*

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L456: This seems overstated given the earlier comments about how your results with a forced fixed terminus position would generalize to a more realistic case where thinning would induce retreat (potentially out of the lake).

AP: *We have rephrased this sentence in the revised manuscript (**L 471**): "While we do not explicitly model mass loss processes at the lake-ice margin, nor their feedbacks, our results suggest that even temporary lake level increases may commit glaciers to prolonged mass loss, not only at the lake-terminating interface itself but more widely across the catchment."*

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Response to Reviewer #2

Summary

This manuscript offers a novel approach to understanding the role of proglacial lake level changes on ice dynamics under a range of different scenarios. It is generally well written and well referenced although some paragraphs require some rewording. The manuscript reveals how both long- and shorter-term increases in lake level result in increased ice velocities and higher rates of thinning compared to a land terminating scenario. The glacier response to changing water levels is shown to be non-linear with grounding line mass flux dramatically increasing once flotation occurs. The authors also show that shorter term reductions in lake level can lead to high tensile and compressive stresses at the terminus that could drive significant crevassing, however they suggest that slow drainage could act as a viable geoengineering strategy to mitigate GLOF risk and lower glacier retreat rates. At present there remain a number of limitations that require significant revision that I have immediately addressed below.

AP: *Thank you for your thoughtful review. We address our line-by-line comments below.*

Major comments

The most significant limitation, which the authors have acknowledged, is the exclusion of calving and subaqueous melting. In real lake-terminating systems, thermal undercutting and calving are primary drivers of mass loss. By omitting these to isolate hydrostatic buoyancy, the model represents a highly conservative estimate of glacier sensitivity. If there is a way to incorporate subaqueous melt, relative to changes in lake temperature (which is likely to vary significantly seasonally, with lake volume, and be influenced by the presence of icebergs and inflow from non-glacial water sources), this studies results would be greatly strengthened. The lack of removal of ice through calving is most likely resulting in unrealistically thin ice tongue formation that may presumably be acting to stabilise ice/reduce velocities behind the grounding line thus resulting in underestimations of glacier change.

AP: *We appreciate this comment and fully agree that frontal ablation — primarily calving and subaqueous melting — is a critical process that influences glacier dynamics, likely to a far greater extent than lake level alone, and that omitting these processes yields conservative estimates of glacier sensitivity. We intentionally and consciously exclude explicit treatment of these processes, in part to focus on the sensitivity to lake level and its hydrostatic effects, and in part because the processes of calving into proglacial lakes (style, geometry, rate, appropriate calving law formulation) and the subaqueous melt rates (and relationship to temperature, melting and water stratification) are poorly constrained and would require significant sensitivity testing themselves. For example, there is no consensus on an appropriate calving law for well-studied marine terminating systems (e.g., Barnett et al., 2026); applicability of a marine-suited calving law to lake cases is entirely unclear. These are obvious and important next lines of enquiry. We explicitly state our approach in the manuscript (Line 88): "**We conduct a series of controlled experiments on a semi-synthetic model domain, designed to isolate the role of lake level change by holding other parameters constant (e.g., surface mass balance, bed geometry, and calving parameterisations).**", and acknowledge the limitations (Line 524): "Our exclusion of calving and subaqueous melting likely leads to conservative estimates of glacier*

sensitivity, as these processes can account for a substantial proportion of mass loss in lake-terminating systems (Carrivick & Tweed, 2013; Minowa et al. 2021; Caldwell et al. 2025). Both mass loss processes influence and are influenced by lake water temperature, lake ice cover, water depth and bed topography, and their rates, distribution and calving mechanisms at ice-dammed lake margins are not well-constrained (e.g. Mallalieu et al. 2020), limited by sparse long-term observational data and uncertainties in model implementation. Incorporating calving and subaqueous melting would likely amplify the lake level effects we identify, through delivering more water into the proglacial basin, and could reveal additional instability mechanisms."

*We have further incorporated our comment above into the manuscript under section 2.2. Mass balance and boundary conditions (**Line 152**) "Surface mass balance is prescribed as the 1960-2000 mean, derived from regional climate model MAR (version 3.11.3; Kittel et al., 2021), which has a glacier-wide average value of -2.65 m.w.e. per year (Fig. 2c). Surface balance is held constant across simulations to isolate the impact of lake level variations on glacier dynamics. Our experiments use a fixed glacier front, while allowing the glacier to develop a floating extension, meaning that ice is lost from the frontal position as flux without explicitly modelling calving and subaqueous melting; the ice front may float if it becomes sufficiently thin. Although likely to be important components of mass loss, constraining calving and subaqueous melt processes at lake-terminating glaciers - and appropriate implementation in models - is in its infancy (e.g. Sugiyama et al. 2019; Mallalieu et al. 2020; Carrivick et al. 2020; Caldwell et al. 2025; Dye et al. 2025). Indeed, the choice of an appropriate calving law is unclear even for comparatively well-studied marine systems (Barnett et al. 2026). We therefore choose a simplified approach, without explicit consideration of frontal ablation processes, and isolate the hydrostatic effects of lake presence and lake level change in our sensitivity experiments."*

While glacier dynamics are primarily governed by geometry, mass balance, and frontal ablation — factors that are well-studied globally and similarly applicable to lake-terminating glaciers — the defining characteristic of lake-terminating glaciers is that their lake levels can fluctuate through filling and drainage events or cycles, potentially influencing glacier dynamics. Quantitatively assessing this influence is the central aim of our study.

*To isolate this effect, our experiments are deliberately designed to control for other influencing factors. However, we note that fixing the ice front position in our experimental set-up does not imply the exclusion of ice flux across it. Ice flux through the front continues, allowing ice to move from upstream to downstream, and ice is removed from the model as it passes through the frontal position. We simply do not permit frontal retreat driven by frontal ablation, such that we do not explicitly model calving or subaqueous melting. We state this in the manuscript: (**Line 155**) "Our experiments use a fixed glacier front, while allowing the glacier to develop a floating extension, meaning that ice is lost from the frontal position as flux without explicitly modelling calving and subaqueous melting; the ice front may float if it becomes sufficiently thin.**"** Our experiments confirm that the glacier remains dynamic and does not produce unrealistically thin terminal ice.*

The paper frames Experiment 3 as testing a GLOF like scenario, however the time period of drainage is on a magnitude of years rather than hours or at the most days which real life GLOF events typically occur. I think this is therefore misleading and the authors either need to cite examples of GLOFs which have undergone drainage in this style or reframe what this experiment is trying to demonstrate. It would also be great to see an experiment showing very rapid lake level changes that compare to an observed real world GLOF.

AP: *Thank you for raising this important point. Our experiments were designed through a palaeo-ice sheet lens, in which "rapid" lake drainages can typically be constrained to a season or 1-2 years. The geological record does provide evidence of incremental drainage* (Teller et al., 2002; Tweed & Russell, 1999) *and also of larger drainage events that necessarily take longer to release a larger volume of water* (Clarke et al., 2004; Glasser et al., 2016)*, but we acknowledge that the best estimates of drainage duration may also reflect the temporal resolution of the record and not the actual process. We admit that the durations of typical GLOFs in contemporary and smaller-scale settings are much faster than we have captured in our experimental design.*

We note that our model operates at a monthly temporal resolution. Thus, although a 100 m drop is applied over one year, the actual change per timestep is approximately 8 m per month; the total stress response after the end of the drainage is plotted. Nevertheless, we acknowledge that this does not fully replicate a rapid glacial lake outburst flood (GLOF) scenario.

We believe there is still value in our original experiment, which shows the qualitative and quantitative response to the rate of drainage (1-4 years) and the importance of whether the initial state is floating or grounded. These timescales may be relevant to larger, paleo-cases of lake drainage.

In the revised manuscript, we included an additional experiment (Fig. 14 and Fig. S2), designed with a higher temporal resolution (1-day) and a more realistic lake level drop to better capture GLOF-like conditions.

We have updated our existing figures, which illustrate the stress response in greater detail.

Here is the revised section 3.3 with the figures.

"Experiment 2 has shown that slow and steady lake drainage has a stabilising effect, producing a marked decline in GL flux. Yet, lakes are known to drain much more rapidly, from 1-2 years in the case of larger events in the ice sheet palaeo-record (Teller et al., 2002) to days or even hours in some settings (Tweed & Russell 1999; Veh et al. 2019; Dømgaard et al. 2023, 2024; Regnéll et al. 2025). Rapid lake drainage events represent one of the most dynamic perturbations that lake-terminating glaciers can experience.

In experiments in which we prescribe lake level fall of 100 m in 1, 2, 3, and 4 years, we find that there is a pronounced stress response in the near-marginal area (Fig. 12), in both tensile (Fig. 13 a, b) and compressive (Fig. 13 c, d) fields. Peak stress changes, relative to the pre-drainage state, occur at the culmination of drainage. The magnitude of the glacier stress response scales directly with drainage rate, with faster drainage events producing proportionally larger stress perturbations (Fig. 13). The absolute magnitude of stress changes depends also on the initial flotation state. For identical drainage magnitudes, initially floating termini (with a water depth of

200 m) experience stress changes that are about 4 times greater than when the initial terminus is grounded (lake level at 100 m).

Our annual-scale results represent the cumulative stress per year. For more realistic GLOF-like drainage events, we run a high temporal resolution Experiment 3b, with a daily model time-step and drainage duration of 1 day. In this case, we find an ~instantaneous stress response of similar distribution (Fig. S2) and magnitude (Fig. 14) to the cumulative annual drainage, linearly related to the magnitude of lake level fall.

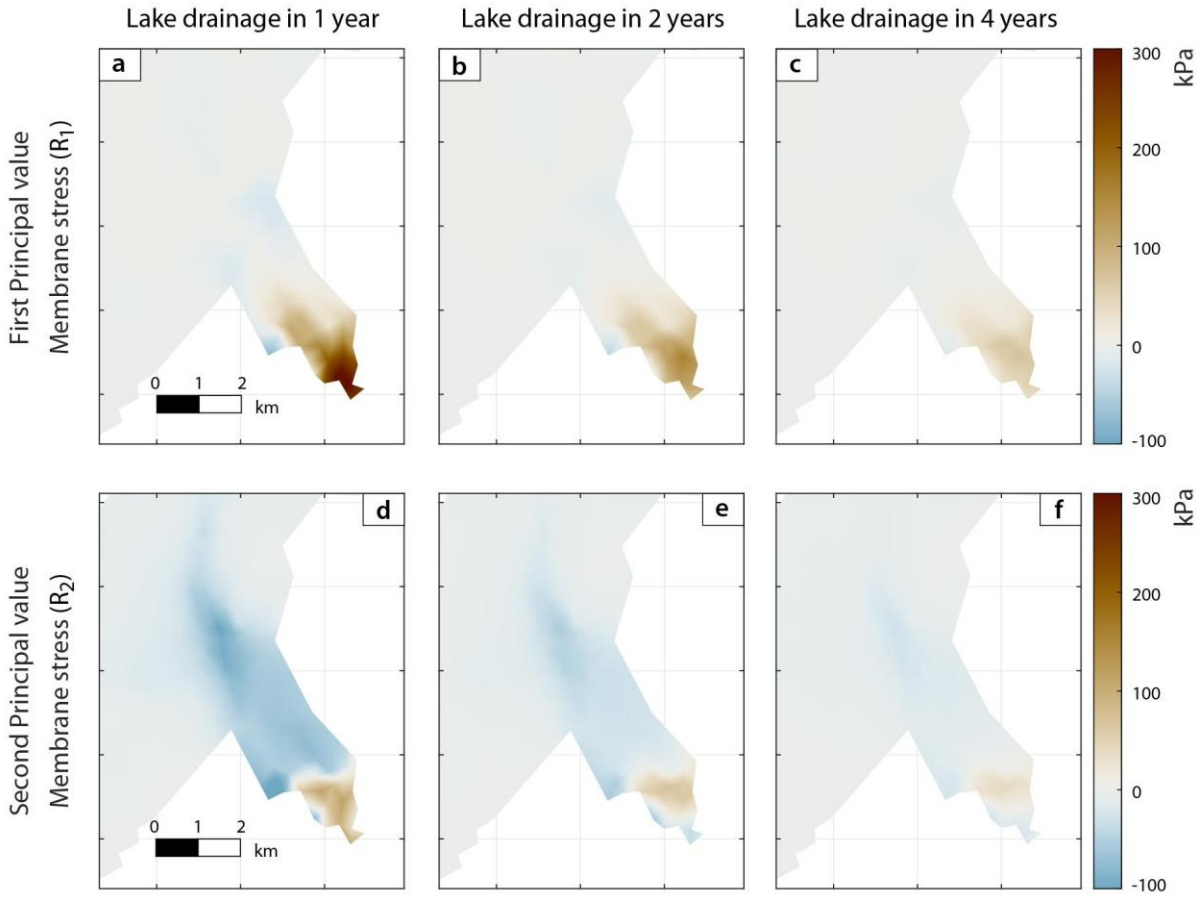


Fig. 12: Changes in first (tensile, a-c) and second (compressive, d-f) principal value of membrane stresses, in the first year after initiating a 100 m lake drainage of 1, 2 and 4-years duration, from an initially floating glacier terminus.

During rapid drainage, the sudden withdrawal of hydrostatic support creates an immediate imbalance between driving stress (gravitational forcing) and resistive stresses (basal and lateral drag, back-pressure from water). This imbalance manifests as enhanced longitudinal stretching near the terminus, and for floating termini, the loss of buoyant support effectively increases the vertical stress component, leading to enhanced basal coupling and altered stress transmission through the ice column. The resulting stress concentrations are particularly pronounced at the former waterline, where the transition from hydrostatic to atmospheric pressure boundary conditions creates sharp gradients in the stress field.

In both our annual and daily drainage experiments, membrane stresses appear to stabilise at a substantially elevated level relative to the pre-drainage state. At these stresses, equivalent in magnitude to the typical tensile strength of ice (~300 kPa), we would expect that mechanical failure and stress release would ensue; since our model does not incorporate crevassing or calving, stresses remain artificially high (Fig. 13, 14). Using established relationships between stress and crevasse depth (Nye, 1997; van der Veen, 1998; Reynolds et al., 2025), we estimate that membrane stress increases of ~250 kPa (observed for 50 m / 1 day drainage from floating conditions) could drive surface crevasses to depths of around 27 metres (Fig. 14), potentially reaching the waterline and initiating calving processes. Our experiments suggest that rapid and high magnitude lake drainage events, particularly where an ice terminus is initially floating, may reduce the structural integrity of the glacier outlet and trigger discrete calving episodes. The extent to which complete structural collapse may ensue requires explicit fracture modelling.

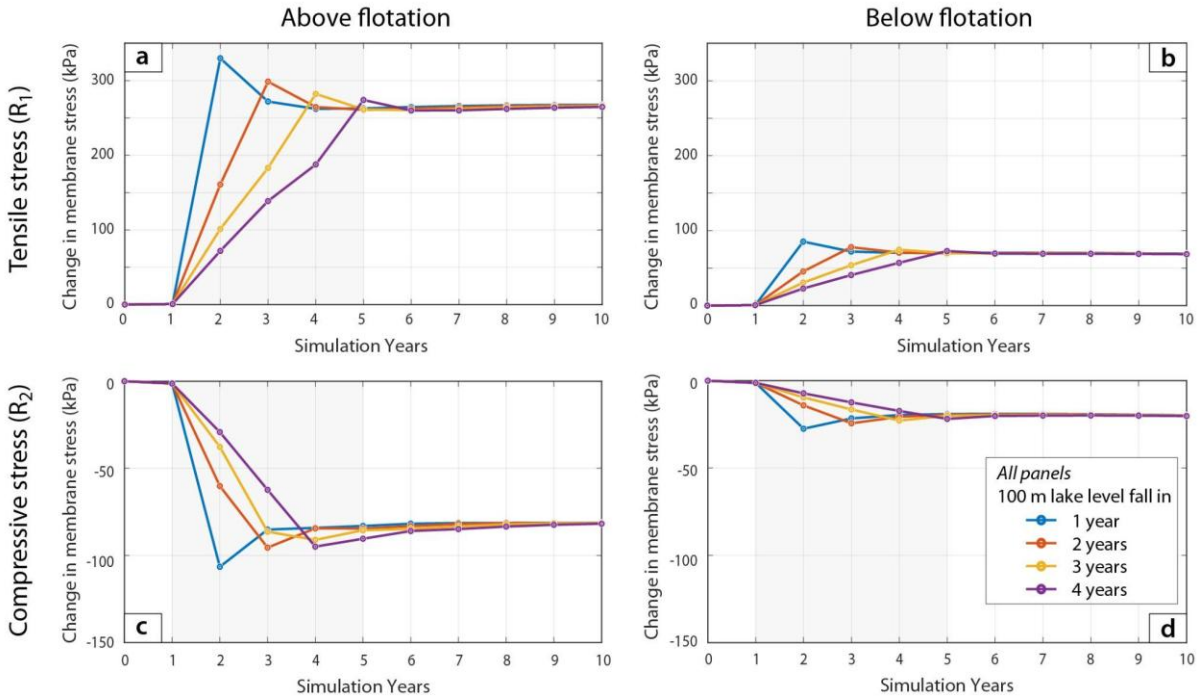


Fig. 13: Maximum change in first principal value of membrane stress (tensile) (a, b) and Minimum change in second principal value of membrane stress (compressive) (c, d) stress regimes, during rapid lake level fall from a lake level that is initially above (a, c) and below (b, d) the terminus flotation threshold. Changes are relative to the pre-drainage state. The shaded area marks the maximum duration of lake drainage (4 years).

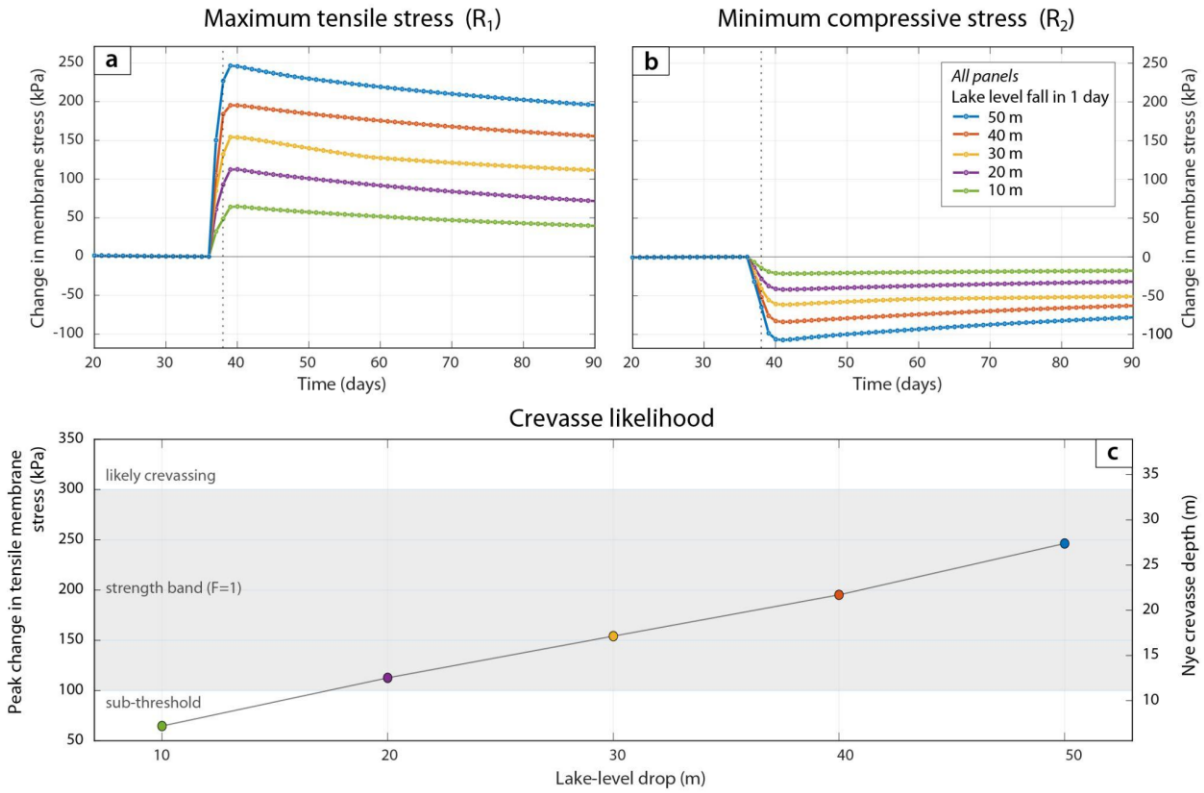


Fig. 14: Change in peak tensile (a) and compressive (b) membrane stresses during a 1-day (GLOF-like) drainage event of a lake that is initially above the terminus flotation threshold. Changes are relative to the pre-drainage state, the dashed vertical marks the hour when lake level fall stops. (c) Crevasse screening: peak change in tensile membrane stress versus drawdown magnitude (left axis) and the equivalent Nye dry-crevasse depth, d = $R_1/\rho g$ (right axis). The grey band marks the assumed tensile-strength range for crevasse initiation in ice (≈100–300 kPa; Vaughan, 1993), whose edges correspond to a fracture index F = 1 for the weak and strong strength bounds."

During Experiment 3, the model shows stress peaking and then relaxing back to equilibrium within 5 to 10 years due to the viscoelastic nature of ice. However, because the model lacks a fracture mechanics or calving law component, it assumes the ice simply "absorbs" this stress and relaxes. In reality, stresses that high (exceeding typical ice tensile strength of ~300 kPa) would result in physical breakage and therefore calving rather than just mathematical stress relaxation, this needs to be properly addressed in the manuscript.

AP: *Since fracture mechanics and calving laws are not incorporated in our model configuration, our analysis is restricted to characterizing the nature of the stress response generated. We have cautiously highlighted plausible follow-up mechanisms without drawing definitive conclusions, noting that the magnitude of the stress response may be sufficient to trigger calving and ice fracturing — processes that warrant further dedicated investigation. We note that due to the implementation of a drainage event longer in duration (e.g. 1 year) than an individual timestep (1 month), stresses have been summed, so the instantaneous stress response – which would be*

important to a fracture mechanics analysis – is not clear; this underpins our cautious interpretation.

With our higher resolution experiment, modelling a faster drainage event, the stress responses are of a similar order of magnitude. We therefore agree with the reviewer that these stresses would lead to mechanical failure of the ice front (calving). While our model (lacking fracture mechanics) relaxes after the drainage event, given the magnitude of the stress response, we interpret that calving would ensue.

We have revised this analysis with the new set of experiments, and we request the reviewer's attention to the updated section 3.3.

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Minor comments

L38: The language in this sentence could be a bit more precise; Lakes can form without having a major impact on ice dynamics, particularly when they are small. Maybe frame as → increasing lake growth can result in glaciers becoming increasingly detached from climatic drivers on ice dynamics.

AP: *Thank you for the suggestion. We rephrased the sentence in the revised manuscript as (**Line 37**) "Once a lake forms and grows at the glacier terminus, the glacier's behaviour may become increasingly influenced by interactions between the lake and the ice front, which can surpass the effects of climate conditions alone on glacier dynamics"*

--

L61: Replace semicolon with full stop.

AP: *Corrected.*

--

L75-78: The last part of this paragraph feels like a different message to the first part, maybe remove discussion of significance of glacial lake outburst floods to the following paragraph or a new paragraph as it doesn't quite fit with the discussion on ice dynamics above.

AP: *This paragraph addresses the occurrence and the impacts of proglacial lake presence and lake drainage. Lakes have impacts on the glacier system but their drainage additionally has downstream impacts, on the ocean and on the terrestrial landscape (including human infrastructure). The next paragraph then goes on to comment on the likely trajectory of change – lake damming is becoming more frequent, and the aforementioned impacts are therefore becoming a greater concern. We have rephrased the paragraph as (**Line 66**) "Rapid drainage events, triggered by moraine breaches, ice retreat and exposure of new basin drainage thresholds, or flotation-induced failure of ice dams, can release massive volumes of water in short periods (days to months). Such drainage events are a recognised and growing hazard, especially in high mountain regions, where the downstream impacts of glacial lake outburst floods (GLOFs) are amplified by steep topography, posing serious threats to lives, infrastructure, and livelihoods (Nie et al., 2017; Lützow et al., 2023; Rinzin et al., 2023; Sattar et al., 2025). Yet*

the rapid lake level changes associated with drainage events have an upstream consequence too. Lake level change has the potential to significantly modify stress regimes at glacier fronts, influencing crevasse formation, grounding line stability, ice velocity and overall glacier dynamics (Carrivick et al., 2020; King et al., 2018).
"

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L96-107: This reads well.

AP: *Thank you*

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L142: "Poorly unknown" double negative, amend to something like 'poorly quantified' or just 'unknown'.

AP: *Thank you, this should have said "poorly known"; we have corrected it.*

--

L160-162: The way this is worded makes it a little tricky to follow.

AP: *We rephrase these sentences as (**Line 165**) "To investigate the dynamic response of lake-terminating glaciers to varying lake levels (lake filling and draining), we leverage the sea-level adjustment feature in ISSM (Larour et al., 2012). Within the lake mask area (Fig. 1c), we prescribe the 'sea level' (called lake level henceforth) independently, keeping actual sea level unchanged elsewhere. This has the effect of setting basal water pressure due to lake height within the mask, where the lake floor is at -200 m, and due to sea level elsewhere, where basal water pressure remains unchanged following equation 3 (Fig. 2)."*

--

L174: Consistency with the formatting compared to the below experiments, other experiments have "and" after the semi col whereas this line doesn't.

AP: Added 'and' in the revised manuscript.

--

L206: Define extent of "lake region" how was this region determined?

AP: *The lake region is delineated using the BedMachine data. BedMachine classifies an area around and upstream of the present-day lake as "ocean", though it does not actually have a marine terminus. The original lake has two parts of glacierized basins, and we wanted to restrict the downstream extent of the lake within our study glacierized basin. The upstream extent of the lake is extended to the area where from lake-terminating tributary starts (green contour of Fig. 1). We have modified this as (**Line 117**) "At the present-day lake-terminating outlet, we define a lake mask area (Fig. 1c) within which we locally modify the bed topography, imposing a flat lake floor behind a topographic high sufficient to allow a range of lake level experiments. This setup ensures a closed basin dammed by the ice margin, and that we exclude the influence of bathymetry on grounding line dynamics, to isolate the effects of lake level change."*

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L207: A 20% difference between lake and land terminating velocities does not seem to be "modest".

AP: *This difference is not for the whole glacier, but only for the terminus part, and it happens over the period of 100 years.*

--

L215/Fig 4: This plot could be improved, the figures are quite small, and it is hard to make out the bits of the glacier we are interested in i.e. the terminus regions, maybe full glacier plots could be put in the supplementary so this plot can focus on the terminus region?

AP: *We agree that the changes in the terminal region are quite small on the page, and that it is difficult to discern changes in the upper catchment. However, these upstream changes are present and we find this a relevant result. In the revised manuscript, we have modified the colour range in these figure panels to better highlight the upstream changes, and provided an extra inset figure for each variable at year 100 focussed only on the terminal region, with appropriate colour range for the magnitude of changes there. We take the same approach to Figs 7 & 8.*

--

L230: Again, what is meant by "lake area" here, I am not sure how much glacier this refers to, could we say within x m of the terminus or a point that is fixed? A map showing this lake area could also solve this problem.

AP: *The lake extent is shown in Fig. 1c (green contour), which encompasses glacier ice where the bed elevation lies below sea level. As shown in Fig. 2b, the bed elevation below sea level extends considerably further upstream. However, we restrict the potential lake extent in our model to the lake-terminating tributary. If a "lake" extended further upstream, it would in fact connect to the ocean. The defined lake area, as in Fig 1C, is the only area in our model domain where lake level changes influence subglacial water pressure.*

*To address this confusion, we add clarification of how the lake area is defined to our Methods section 2.3 (Experimental design) as (**L 166**) "Within the lake mask area (Fig. 1c), we prescribe the 'sea level' (called lake level henceforth) independently, keeping actual sea level unchanged elsewhere. This has the effect of setting basal water pressure due to lake height within the mask, where the lake floor is at -200 m, and due to sea level elsewhere, where basal water pressure remains unchanged following equation 3 (Fig. 2)."*

--

L236-240: These sentences currently don't read so well, change "lake terminus" to something like "while the glacier was still lake terminating".

AP: *We have changed it accordingly in the revised manuscript.*

--

L251/Fig 7: Same as comment for fig 4.

AP: *We address Figs 7 & 8 in the same way as we amend Fig 4.*

--

L256-259: We could do with a plot like fig 6 to show this velocity stabilisation as this transition point is not clear from fig 7.

AP: *It is shown in Fig. 9.*

--

L274-313: This feels like a totally different experiment and is very interesting in its own right, it should therefore be described as such earlier on.

AP: *We referred to a further subset of follow-up experiments in Section 2.3 but, in hindsight, this could be profiled better. Thanks for the feedback. We have explicitly outlined these experiments in the Methods (as Experiment set 2a and set 2b) as (**L 191**) "Experiment 2b: grounding line flux sensitivity to flotation and grounding*

i) rising lake levels: increase linearly from 0 m to 100, 125, 150, 175 and 200 m over the first 50 years of the simulation, and then kept constant; and

ii) falling lake levels: decrease linearly from 100, 125, 150, 175 and 200 m to 0 m over the first 50 years.

*The lake levels selected for Experiment 2b are expected to be both above and below the threshold for flotation.", and amend the narrative in the opening to the Results sections for these Experiment sets as (**L 286**) "In Experiment 2a, lake level rise drives the ice margin to flotation (Fig. 6) and causes grounding line retreat; we therefore specifically further explore the impact of flotation in a further set of intermediate lake-rise and lake-fall experiments.".*

--

L277: What are these lake levels?

AP: *As above – we have described the different lake levels considered for this experiment in the Methods (Experimental design, 2.3).*

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L308-311: This feels better placed in the discussion.

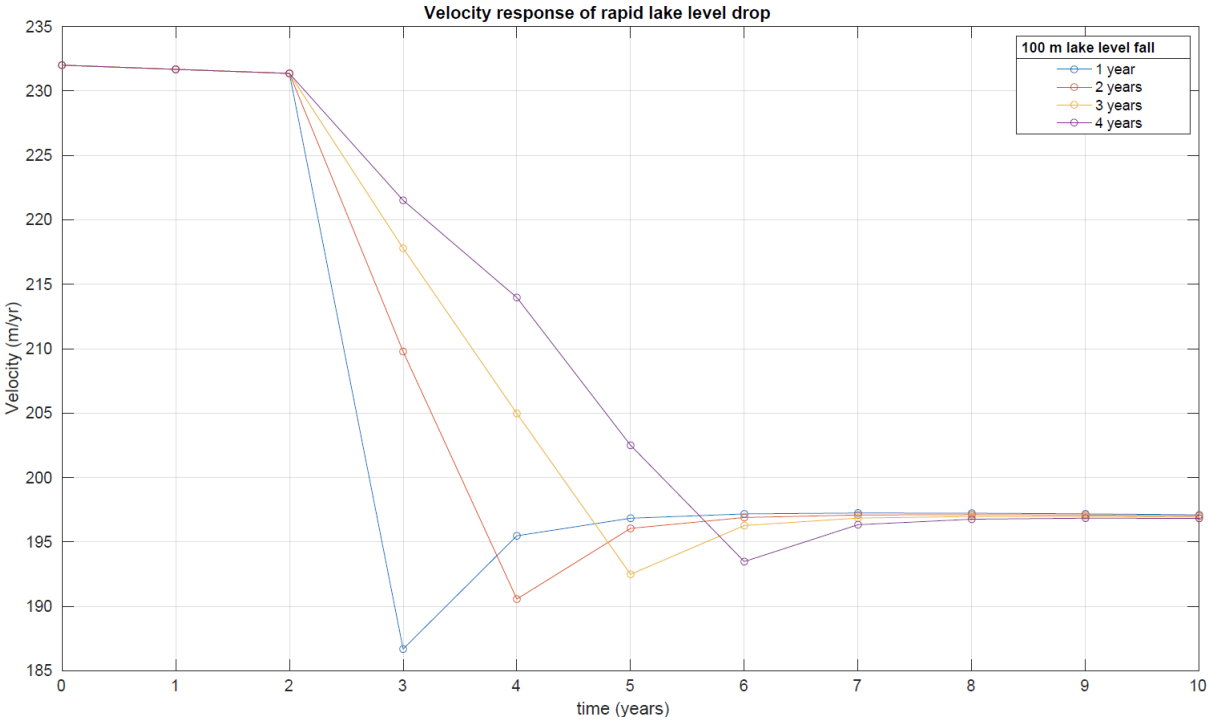
AP: *For better readability, we think it is justified to place the explanation here as a signpost to the discussion.*

--

L314: Although the focus of this section is on the stress response, it would also be interesting to see the velocity/thinning response included to allow for comparison with the other experiments.

AP: *Thank you – we agree, but the number of results figures in the manuscript is already rather high and we have included an additional figure with a higher temporal resolution rapid drainage experiment. We could add panels to Fig 12, though as the focus here is on a shorter time frame, a direct comparison with the other experiments is not applicable. Consistent with the other lake level fall experiments, the velocity is expected to decrease rapidly in response to a rapid lake level drop.*

The velocity response is shown in the attached figure; however, as Experiments 1 and 2 already cover velocity in more detail, we have chosen not to add it to the manuscript.



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L351: Please define these or make clear what these "unstable feedbacks" are here.

AP: *We rewrote this section and removed this sentence.*

--

L387-389: This opening sentence could do with improvement in readability and how it sets up the coming paragraph.

AP: These lines are not an opening sentence, but rather a closing sentence of a paragraph. Perhaps the reviewer meant 397-399?

--

L406-408: Given that ice discharge (calving?) is not quantified in this study, this sentence needs a citation.

AP: *This sentence intended to draw on the grounding line flux results, plotted in Figs 10 & 11. We changed "discharge" to "flux", and referred to the figures (10, 11). The sentence reads as (**L 421**)* *"Small changes in lake level can trigger disproportionately large changes in grounding line flux (Fig. 10, 11), but only when the ice front is close to the threshold for buoyancy."*

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L410-414: I am not sure how quickly ice advancing over the grounding line would result in a change in lake level? I imagine it wouldn't be too hard to quantify and also work out how much this would change the discharge of the lakes outlet stream. I feel that any displacement of water by ice flowing into the lake would be quickly offset by increasing drainage from the lakes outlet.

AP: *Ultimately this depends on the topography peripheral to the lake basin, and the location of the lake outlet relative to the (advancing) grounding line. If the basin is not supply-limited and is already filled to its threshold, there should be no lake level change (the displacement would be offset, as the reviewer suggests). However, if grounding line advance blocks the earlier threshold (e.g. on a retrograde slope and/or where the threshold is proximal to the ice-dam), lake level may rise to a new threshold. We agree that the analysis provided here may or may not be significant, but requires separate investigation.*

--

L412-414: Do we have any examples of such rapid lake rise?

AP: *We do not have any examples of such scenarios. This is a short-term phenomenon which may happen at the lake growth stage coincident with glacier retreat. But, this is physically possible and can vary in magnitude, but is probably difficult to capture with observation.*

--

L422-424: Describe these examples of where this has happened.

AP: *This has been observed at Russell glacier in West Greenland. We mentioned that in the revised manuscript along with the reference (**L 438**) "This finding is consistent with observations from natural drainage events where calving episodes are typically followed by restabilisation, rather than runaway retreat (e.g. Mallalieu et al. 2020)."*

--

L438-439: This sentence doesn't read well at the moment from "however" onwards, not clear what it's trying to say.

AP:

*We have rephrased it as (**L 454**) "In contrast, thinning trends of such systems appear almost universal, irrespective of the nature of the lake terminus: stable or evolving, newly formed or long-established (Tsutaki et al. 2013, 2019; Thakuri et al. 2016; Muto & Furuya 2013; King et al. 2019; Sato et al. 2022)."*

--

L456-457: This sentence is confusing, not sure where lake-contact margins come into this.

AP: We rephrase this sentence to (**L 471**): "*While we do not explicitly model mass loss from the ice front, or its feedback, our results suggest that even temporary lake level change may commit glaciers to prolonged responses not only at the lake-terminating interface itself but more widely across the catchment.*"

--

L470: "propagate also" → also propagate.

AP: *Corrected*

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L485: I would be cautious about linking these results to these poorly constrained historical events especially given that this study doesn't quantify the glacier response to sub annual changes in lake levels.

AP: *We argue that these geological events are exceptionally well-constrained in timing and magnitude, and exhibit repeated occurrence (both in the same catchment and across neighbouring catchments: Regnéll et al. 2023, 2025; Høgaas & Longva 2018 – referred to in the original manuscript). These drainage events are large, high-magnitude, and more prevalent than in many contemporary settings, and indeed motivated this work. We consider it very relevant to refer to these palaeo-examples in light of what we learn here, especially since we also now include a model experiment that addresses sub-annual lake drainage.*

--

L505-508: Could be a good point to discuss how icebergs would affect water temperature and the energy balance at the terminus and what would mean for thinning rates.

AP: *We have incorporated the following line (**L 528**) "Both mass loss processes influence and are influenced by lake water temperature, lake ice cover, water depth and bed topography, and their rates, distribution and calving mechanisms at ice-dammed lake margins are not well-constrained (e.g. Mallalieu et al. 2020), limited by sparse long-term observational data and uncertainties in model implementation."*

--

L544: Would this be considered rapid, most would assume rapid refers to a GLOF style event i.e. hours? South Lohnak could be a good case study here for examining how the stress changes at the terminus resulted in crevassing/acceleration etc if we are to discuss mountain systems.

AP: *As addressed in the major comments #2, we have redesigned our experiment 3 with shorter time-resolution to address GLOF style events.*

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