Response to the comments of 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 a purely 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 $P_w = 0$? 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 P_w 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.*

The ultimate velocity response is governed by the effective pressure ($N = P_i - Pw$*). Greater ice thickness at the terminus of lake-terminating glaciers dampens the velocity response relative to land-terminating glaciers. We believe our assumptions adequately account for these structural differences and are appropriate for the scope of this study.*

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 (e.g, 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 will incorporate this figure in the revised manuscript.

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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.

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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 potential 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 will acknowledge 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 will incorporate this figure in the revised manuscript.*

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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 will mention "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 will clarify this under the experiment design section.*

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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.*

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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 will clarify that the difference is thickness pattern is irrespective of calving and solely due to pressure differences.*

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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 will note this in our explanation of these Results in our revision.*

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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.*

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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 will discuss its findings in light of ours pointing to the importance of ice margin flotation.

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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 will modify this sentence: "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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