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.

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.

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?

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?

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.

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.

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

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 al., (2023; https://doi.org/10.1017/jog.2023.42) discuss these systematic differences.

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.

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.

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).

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?

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.

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.

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

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?

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?

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.

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).

Review by William Armstorng and Carly Kammerdiener