Summary

This study investigates the ice dynamics of a glacier transitioning from a land-terminating to a lake-terminating system, using a specific glacier in Iceland as the study site. Despite being site-specific, the findings have broader applicability to proglacial lake systems globally, which makes it scientifically appealing and timely. The paper proposes and tests a new parameterization scheme for basal water pressure at lake-terminating glaciers, uses observational data and sensitivity analysis to model surface velocity, and derives a velocity–water pressure relationship for the study glacier — the most important and novel contribution of the work. The experiment designed to isolate the effects of surface thinning and frontal retreat is also thoughtful and effective.

That said, the manuscript in its current form has some serious issues that require major revision, both technical and structural, which I outline below.

Major Comments

1. Structure of the manuscript needs substantial work. As currently written, the Results and Discussion section reads more like a Methods and Results section. I would strongly recommend separating out a dedicated Methods section where the experiments are properly described. Many sentences currently scattered across the Introduction and Results belong there. As it stands, it is difficult to understand what the paper is trying to do until well into the Results. Similarly, the discussion is minimal. A separate Discussion section would help explain what these results mean for lake-terminating glaciers more broadly, and would be a natural place to address limitations and future directions.

2. The use of the model is unclear. It is not clear whether the full Elmer/Ice model is being used or only selected equations from it. The manuscript refers to a "simple glacier dynamics model," but if Elmer/Ice is being used in its entirety, calling it "simple" is an understatement. The abstract mentions both "develop a simple model" and "Elmer," and it is not clear what was developed by the authors and what was directly employed from Elmer. This needs to be clarified.

3. The finding on surface thinning vs. terminus retreat requires more careful treatment. First, the experiment comparing surface thinning and frontal retreat is well-conceived, but it remains incomplete in its current form. A notable issue is that the lake area differs between the two scenarios being compared. How do you eliminate the effect of varying lake area in the comparison? In the left panel (Fig 11a, b) the lake is visibly larger than in the right, which could by itself drive differences in acceleration. To get a cleaner picture, I would suggest adding comparisons such as S20F10 vs. S10F10, and S20F20 vs. S30F20, to isolate the effect of lake geometry.

Secondly, the comparison is between two processes that operate on fundamentally different timescales: surface thinning is gradual and continuous under a warming climate, while terminus retreat is episodic. A few rapid, high-intensity frontal ablation events can produce far more acceleration over a short period than the equivalent amount of surface thinning. Given this, comparing them directly against each other is somewhat misleading. I would suggest presenting them as standalone results rather than ranking one against the other.

4. Sections 3.3 and 3.4 appear contradictory on first reading. I understand these sections serve different experimental purposes, but the way they are currently presented leads to an apparent contradiction that is confusing before the reader appreciates the distinction. The presentation needs to be restructured so this distinction is clear upfront.

5. The 2030 projection lacks sufficient context. The manuscript presents results for a 2030 glacier geometry, but there is very little explanation of how this geometry was obtained. Was it derived through forward modelling? If so, what mass balance forcing was used? What is the geometry of the lake at that point? I would assume the lake is considerably larger by 2030 — what water pressure value (Pwp) is applied in that case? Without these details, the 2030 analysis feels underdeveloped and weakens what could otherwise be a compelling projection.

Specific Comments

Abstract: Well-written overall. The main question is whether the authors are developing a new model or employing Elmer/Ice with a new parameterization of water pressure — this distinction should be made explicit.

Introduction: Well-written and builds the story nicely, though it is somewhat long and could be made more concise. The final paragraph lists three different aims, which dilutes the manuscript's focus; consider consolidating into a single clear aim or combining them more tightly. A few sentences in the final para also belong in the Methods section.

- L63–64: Please add a reference.
- L90-91: Belongs to Method section
- L92-94: This also belongs to Method section
- L95-97: This, to me, is the real aim.

Study Site: Please clarify whether the lake has an outlet or is a closed basin.

- L110: "started" instead of "starting."
- L116: Consider removing "operating."

L165–167: The depression depth should be referenced from a clearly stated datum. Please mention the elevation of the moraine and clarify what reference point is used for depth measurement.

Section 2.3: The general approach is sound, but a few things need more detail.

- L233–241: Is this similar to inversion procedure standard within the Elmer/Ice framework, or was it implemented by the authors? If the former, please provide a reference; if the latter, please elaborate.
- L242–251: Please provide context for the metrics used. Are these taken from cited literature or defined by the authors?
- **Acceleration and Fig.8:** I don't think acceleration is appropriate to use here, just between two years of observation. Glacier may have gone through repeated advance and retreat cycle in this period.

Section 3.1: The velocity matching with observations is good work. However, the 2030 projection is too vague in its current form (see Major Comment 5). Also, this section largely demonstrates model skill in reproducing observations, which is essentially model calibration. Fig. 7 shows observations, not model predictions. It may be worth presenting it as such.

Additionally, did you compare modelled and observed surface elevation change?

Section 3.2: This is the most novel part of the study in my view, but it feels somewhat narrow and underexplored in the manuscript. The impact of water pressure on lake-terminating glacier dynamics is an important and underexplored topic, and the framework proposed here is valuable. It deserves more space and explanation.

Section 3.3: The experiment description belongs in the Methods section. See also Major Comment 3.

Section 3.4: The first three paragraphs should be moved to the Methods section.

- L367: Should this be "parallel" rather than "perpendicular"?
- L369: What mass balance values are used for the period beyond observations (through 2030)?