We thank the reviewers for their time and numerous helpful comments and suggestions. We are very pleased that the manuscript was well-received overall and that the reviewers are “very enthusiastic about the subject and overarching approach”, that “the paper addresses an interesting and important question that deserves more research attention” and that “the findings have broader applicability to proglacial lake systems globally, which makes it scientifically appealing and timely”. While the reviews were broadly positive, we also recognise that a range of concerns were raised that need to be addressed.

We understand that, at this stage, we are not supposed to prepare a final revised manuscript, but instead provide an initial response to the reviewers’ concerns. Here, we reply to the reviewers’ comments and explain how we will address these if we are invited to revise and resubmit our manuscript which we hope will be the case. In terms of minor comments regarding grammar or writing style, we would follow the vast majority of these recommendations in a revised manuscript.

We provide below our initial responses, many of which would be elaborated on in detail were we to resubmit a revised manuscript.

Summary

This study investigates the ice dynamics of a glacier transitioning from a landterminating 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 laketerminating 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 laketerminating glaciers more broadly, and would be a natural place to address limitations and future directions.

> Our intention would be to take on board the majority of these suggestions in order to improve the structure and clarity of the manuscript. In particular, we will separate out the Methods into a dedicated section. We will also develop the ‘Broader Implications’ subsection into a more extensive Discussion section in order to both explain what

these results mean for lake-terminating glaciers more broadly, and to consider the limitations of the work 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.

We agree that this could have been better explained (and that Elmer/Ice itself is not a 'simple' model!). To clarify, the Elmer/Ice SSA module was used for the simulations. The model was described as 'simple' because it is a SSA, and thus a first order model, not solving for the full-Stokes equations. Furthermore, a simple parameterization of basal water pressure was also chosen. We will modify the text to reflect these important issues and misunderstandings.

3. The finding on surface thinning vs. terminus retreat requires more careful treatment. First, the experiment comparing surface thinning and frontal retreat is wellconceived, 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.

The lake area differs in the EXP2 simulations because the position of the ice-front is different; but this experiment was designed to specifically study the effect of this frontal retreat—and as a result therefore the corresponding increase in lake area—on the dynamics of the glacier. (We also note that in Fig 11a, b, the front position is the same in the left panel (a) as in the right panel (b). They perhaps look different, but if so, this is simply an optical illusion caused by the different colours of the maps.

Regarding the two processes, we believe that the comparison is carried out over a period of time that is longer than the typical timescales of the two processes under study (i.e. we look at the difference over a decade); we therefore do believe that the comparison is still relevant and useful (since while terminus retreat may be episodic, we have averaged out individual calving events by taking terminus positions a decade apart). Nevertheless, we will now add an additional paragraph to clarify the different timescales that these processes operate on, while also highlighting why we don't believe it is an issue in this modelling framework.

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.

> We agree with the referee. Given that we will now modify the structure and include a specific section on Methods, we believe that the purpose of each of these two subsections will now be clearer.

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.

> We discuss this issue in some detail in our previous response to referee 1. However, just to reiterate here, the elevation of the surface DEM used in 2030 was linearly extrapolated based on the DEM changes between 2010 and 2020. Furthermore, the front was extrapolated to ensure that the retreat between 2020 and2030 replicated the observed retreat rate between 2010 and2020. We will modify the methods section to make the approach to the 2030 projection clearer.

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.

> We are employing Elmer/Ice with a new parameterization of water pressure and will remove the word “develop” to make this clearer.

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.

Done.

- L90-91: Belongs to Method section
- L92-94: This also belongs to Method section
- L95-97: This, to me, is the real aim.

As suggested, we will modify the final paragraph of the introduction to make it more concise. We will also move some sentences to the methods section, in line with the suggestions.

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

The lake has an outlet river, which drains from the centre of a moraine dam and keeps the water level stable. We will add this important context to the text.

- L110: "started" instead of "starting."

Will change accordingly.

- L116: Consider removing "operating."

We think “operating” provides the necessary action in the sentence.

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.

The depth measurements are referred to with sea level as a reference point; this will be made clearer in the text.

The elevation of the barrier closing off the depression will also now be mentioned in the text.

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.

A tuning approach was applied, not an inversion procedure. The free parameters were adjusted to minimize the mismatch between observed and modelled velocities and also to minimize the difference between modelled and observed accelerations. We will clarify this in the text.

- L242–251: Please provide context for the metrics used. Are these taken from cited literature or defined by the authors?

These metrics were defined by the authors, and this will be clarified in the text.

- **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.

We agree that the glacier may have experienced repeated advance and retreat cycles over this period. However, we are simply looking at the overall change in velocity over the eight-year period. However, we will change the caption to clarify that this represents the increase in velocity over this time period rather than the acceleration.

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?

We will improve the description of the model set-up as noted above. We note that Fig 7 shows both observations and model predictions used to calibrate the model. We didn't compare modelled and observed surface elevation change because the model is in steady state, so it doesn't calculate 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 laketerminating glacier dynamics is an important and underexplored topic, and the framework proposed here is valuable. It deserves more space and explanation.

We are pleased that the reviewer recognises this as the most novel part of the study. We will now expand on this section as requested, in part by providing additional context in the new Methods section and also by expanding the discussion section to provide further explanation.

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

This will be moved to the Methods section.

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

This will be moved to the Methods section.

- L367: Should this be "parallel" rather than "perpendicular"?

No, the flux gate is assumed perpendicular to the flow, so $\boldsymbol{\alpha}$_i, the angle between the velocity vector and the vector normal to the flux gate surface, is zero.

- L369: What mass balance values are used for the period beyond observations (through 2030)?

For the period beyond the observations (through to 2030), we estimate the ice discharge component of mass balance through the flux gate using from the velocities derived from of the model. In effect, the assumed surface mass is incorporated to 2030 through the extrapolated surface thinning which we use to derive the subsequent 2030 DEM.