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. The referees’ original comments are provided below in black text and our responses are given in blue with new text in the manuscript in italics.

Reviewer 1 response

Review of “Investigating the transition in ice-dynamics from a land- to lake-terminating glacier using a simple glacier dynamics model”

By Otero et al.
Submitted to *The Cryosphere*

Overview
This manuscript presents a modeling study focused on an Icelandic glacier that has transitioned in recent years into a lake-terminating glacier. The topic is of broad relevance to glaciated regions worldwide, and much remains to be understood about how changes in terminus boundary conditions affect different glaciers in different environmental settings. A relatively simple and computationally efficient model is set up to simulate the glacier at different stages, relying on a tuning exercise to find model parameters that reasonably reproduce observed velocity. Subsequent experiments are conducted to attempt to isolate the influence of various processes.

Overall, the paper addresses an interesting and important question that deserves more research attention. I am very enthusiastic about the subject and overarching approach, and I also think that there are some aspects that need to be clarified to strengthen the work to help make it a more impactful contribution to the glaciological literature. Please see below for general and specific comments.

General comments

1. The usual convention for surface mass balance is positive for accumulation and negative for ablation. At some places in the manuscript, it becomes confusing whether surface mass balance is instead referring to surface ablation. I recommend carefully checking the signs to make sure that you are being consistent with what you intend to indicate. I have highlighted some of these instances below in the specific comments.

Thank you for highlighting this; we have now checked the signs throughout the document and modified them following the usual convention.

2. Assuming a uniform P_wp factor is a major assumption in the way the model considers changes in the subglacial hydrologic system. Why is it a justifiable to assume “perfect hydrologic connectivity” between the lake and further up-glacier? Simply pointing toward what has been done in the past in modeling marine terminating glaciers (lines 222-223) is not a compelling justification, and also a grand simplification and potentially problematic assumption in that case as well. The sensitivity study in section 3.2 is a nice gesture toward exploring the influence of variations in basal water pressure, but the underlying assumption of uniform P_wp is a big limitation on the usefulness of these results. In the discussion of broader implications, the inferred increase in basal water pressure and corresponding retreat might not necessarily always be true for glaciers that become lake-terminating. This will depend on the ice and bed geometry, environmental conditions, and how the configuration and efficiency of the drainage system evolves. The simplified approach to representing basal hydrology used in this model does not provide compelling evidence for the universal conclusion as presented. I suggest that it would be more accurate to suggest that more research is needed to investigate the evolution of subglacial drainage in different lake-terminating glaciers.

We agree with the referee that there are limitations with our simplification of assuming a uniform P_wp factor and we absolutely agree with the suggestion that “more research is needed to investigate the evolution of subglacial drainage in different lake-terminating glaciers” and we now clarify this in the discussion (Sections 4.5 and 4.6). Regarding the assumption of a minimum level of water pressure corresponding to a water height equal to the lake level, we do believe it can be justified as a simple principle within the model. While tidewater glaciers’ near terminus ‘*minimum*’ water pressures must be constrained by sea level, we believe the near terminus basal water pressure in a lake-terminating glacier is similarly constrained by the water level of the proglacial lake. Furthermore, while we agree that there will likely not be “perfect hydrologic connectivity” between the proglacial lake and the immediate near terminus upglacier drainage system, we do believe it is a reasonable simplification for the model given the way the water pressure will be conveyed beneath the terminus via any hydraulic connections (channels or films/cavities). This of course reflects the assumption that the hydrostatic pressure exerted by the lake water against the calving face prevents the subglacial water level from dropping below the lake's surface level. In reality, the limited evidence available from borehole water pressure measurements suggests that the hydraulic head within a calving glacier are in fact higher than the surface level of the proglacial water body, both in marine- and in lake-terminating glaciers (e.g. Sugiyama et al., 2011, their Fig 1 below).

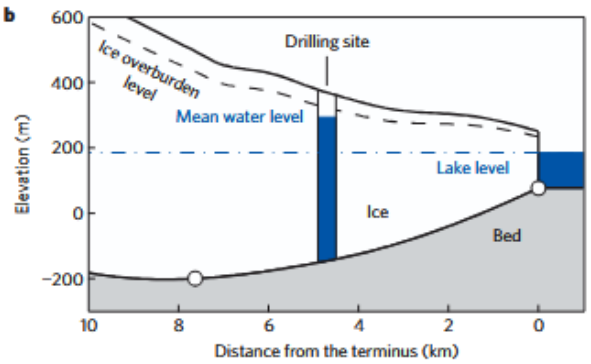


Fig. 1 from Sugiyama et al, 2011 (their Fig 1b) where the mean upglacier water level, as measured in a borehole, was considerably higher than the 'minimum' base-level set by the proglacial lake level.

We have now added a more detailed explanation of our assumption, including the addition of references relevant to lake-terminating glaciers, so that the paragraph now reads (lines 227-234):

"This minimum level of water pressure is the pressure which corresponds with the water height equal to the elevation of the lake surface, if we assume a 'perfect' hydrological connection between the lake and the upglacier subglacial hydrological system. This idealised assumption is based on the consideration that the hydrostatic pressure exerted by the lake water against the calving face prevents the subglacial water level from dropping below the lake's surface level. This assumption is commonly used when modelling both marine- (e.g., Bondzio et al., 2016; Cook et al., 2014) and lake-terminating glaciers (e.g., Carrivick et al., 2020). Furthermore, the limited evidence available from observations of subglacial water pressures, derived from borehole measurements at a lake-terminating glacier, suggest that the hydraulic head was in fact higher than the surface level of the proglacial water body (Sugiyama et al, 2011)."

Regarding the limitation of assuming a uniform P_wp and the broader implications, new paragraphs were added, one in the methods section and the other in the broader implications, that read (lines 241-246 and 472-475):

"We additionally note that, in using P_{wp}, we are in no way suggesting that the complexity of subglacial hydrology can be represented by a single scalar parameter. However, a more physical model, such as one that represents evolving subglacial cavities and channels (Gagliardini and Werder, 2018), would be computationally expensive and involve a large number of unknown parameters, which would be very difficult to constrain due to the lack of direct observations. The use of a single scalar parameter which represents broad-scale effects on basal pressure, and which can be estimated through simple tuning, allows more extensive investigation of the effect of lakes on near-terminus velocities.

However, as this model represents basal water pressure in a very simple way, with a constant parameter value across the entire glacier, further research is needed, using models that

represent basal hydrology in a more complex manner, to investigate subglacial hydrology in lake-terminating glaciers and determine the extent to which our simplification is problematic."

3. SSA is explained to be a reasonable assumption in the fast-moving portion of the glacier near the terminus. Further up the glacier, this justification does not apply. I don't think you necessarily need to do a sensitivity demonstration comparing to a higher-order ice-flow approximation (although that would certainly be interesting and strengthen your results, especially if you could demonstrate that SSA produces the important relevant behavior in the area of interest), but it would be helpful to consider the implications of the SSA assumption on your results.

We agree with the referee that the SSA approach is not fully justified further upglacier. The SSA approach does reproduce well the acceleration at the tongue between 2010 and 2018, and this is the specific behaviour we aimed to reproduce, using an efficient model, and is why SSA is used.

A new paragraph was added to the Results and discussion section addressing this concern, that reads (lines 455-461):

"Finally, the use of the SSA implies that vertical stresses are being neglected in favour of horizontal ones, which means that ice deformation is not being taken into account and the only movement considered is sliding. This could lead to significant errors in a glacier with a large area of slow flow, dominated by deformation, as might be expected in the accumulation zone of Skaftafellsjökull. However, this cannot be confirmed by the observed velocities, which are not well resolved in this area (Figure 5), likely because of their slow flow and lack of surface features. Nevertheless, in the fast-flowing areas of the glacier and particularly in the lower glacier below the icefall, which is the region of most importance to this study, the glacier's movement is indeed well modelled by the SSA velocities (Figure 8)."

4. The model tuning exercise described in lines 233-241 would benefit from some more explanation. In tuning three different parameters to match observed velocities, how do you separate out the influences of each of the free parameters to be confident in the physics being represented in a plausible way? Each of the three tuning parameters in reality likely varies a lot over the glacier, yet the optimization exercise finds uniform values for each to apply over the entire domain. How might that assumption influence your findings? These limitations should be more clearly considered and acknowledged.

We agree with the referee and seek to give more detail how the derivation of uniform values might influence our findings. Importantly however, we also highlight in more detail that many (probably most) modelling approaches adopt tuned "global" parameters (see, for example, Zekollari et al (2022) for a review); furthermore, our domain is very small compared with a lot of

modelling studies so parameters might be expected to vary less than in more climatically complex and spatially extensive model domains.

In order to address this issue in more detail, the possible limitations of the model associated with the constant parameter values are discussed in a new paragraph in the Results and Discussion section, which reads (lines 439-448):

"One possible limitation of the model concerns the fact that the free parameters have been treated as constant across the domain, even though they are known not to be. This could mean that spatial variations in velocity are not adequately represented in the model. This may explain, for example, why the more complex spatial variations in the observed velocities shown in Figure 7 are not reflected in the modelled velocities, which are smoothed and more uniform. We would contend however that this limitation does not significantly affect the key findings of the model which are focussed more on the broad velocity trends in the near terminus region and along the glacier centreline (e.g. Figure 8)."

5. I think more detail is needed to describe how you extrapolate your future forcing out to 2030. How exactly is this extrapolated – linearly continuing the pattern from 2010-2020, or some other way of projecting forward? It is also not clear to me whether dynamic thinning and thickening are calculated within the model or if the terminus position and ice surface elevation are prescribed. What about the glaciated area and lateral boundaries? Do those change? And how is the lake depth projected? What about changes in the subglacial hydrology?

The surface DEM was linearly extrapolated out to 2030 using the changes observed in the glacier surface between the 2010 and 2020 DEMs. In a similar simple manner, the front retreat was extrapolated out to 2030 using the retreat rates observed between 2010-2020.
The subsequent 2030 terminus position and surface elevation were thus prescribed and not calculated within the model. We assume that the lateral boundaries remain unchanged across all models while the glaciated area changes due to the retreat of the front.
The newly extended lake depth in 2030 is determined by the basal geometry, derived from the previous radar surveys, while the water pressure parameter, P_wp, is prescribed in the same way that we chose in the tuning process, hence the subglacial water pressure (and thus 'hydrology') is dependent on basal geometry.

We have added more details to the new methods section in order reflect the details requested by the referee and thus to better describe the extrapolations used in the future forcings. The new paragraph now reads (lines 280-286):

"*In a final experimental projection, the geometry, surface elevation and front position of the glacier was extrapolated to 2030 using the data and trends observed between 2010 and 2018 or 2020, in order to predict how the glacier's dynamics might evolve over the coming decade (Figure 9a). The surface DEM for 2030 is estimated assuming a linear extrapolation of elevation change from the 2010 and 2020 DEMs. To estimate the 2030 front position, the 2010-2020 width averaged retreat rate (Figure 3b) is used to derive the front position for 2030. The lateral*

boundaries were kept fixed in the projection, and the depth of the lake was estimated from the basal geometry (Figure 2). The model with the 'best-fit' set of parameters was thus applied to this extrapolated geometry to estimate glacier velocities in 2030."

6. The experiments designed to isolate the influence of thinning versus terminus retreat (described in section 3.3) involve a neat experimental setup to try to disentangle these forcings. I think some careful attention to the wording about what is dynamic thinning as opposed to prescribed surface elevation change would be helpful to clarify the procedure and results. Typically, I would think of dynamic thinning as a result of mass transport due to changes in ice velocity patterns, something that would be modeled instead of prescribed as a change in surface elevation based on observations.

We agree with the referee and in the modified manuscript, we focused on our wording to try and ensure that any aspects of thinning were clearly distinguished be they due to 'dynamic' thinning or for example a simple prescription of surface elevation change. Furthermore, regarding the prescribed surface elevation change in our experimental projection to 2030, this is due to both dynamic thinning and surface mass balance, and we have tried to make this clearer.

Specific comments

Lines 71-72: Velocities used are from the August-September period (late summer). How would considering earlier in the melt season or winter velocity impact results? What kind of seasonal dynamics over the melt season does this glacier experience? How do those seasonal dynamic change with the shift to lake-terminating?

We addressed this query in the Results and discussion section by considering, where possible, the significance or otherwise of subseasonal dynamics data. A new paragraph reads (lines449-454):

"Another limitation concerns the timing of the derived ice velocities, and their uncertain degree of representation of annual means. The available ice velocities were from August and September, that is, from late summer. These late-summer velocities can be considered a reasonable approximation of annual velocities, because the high velocities typical of early summer have already passed, but they have not yet dropped to the low velocities typical of autumn and winter (Davison et al., 2019; Yang et al., 2025). It is clear however that the seasonal variability of velocities could limit the applicability of the velocity data used."

Lines 75-78: This description can be clarified – I'm not sure how to understand "decreasing down-tongue" – this is explained better later on in the paper, so it would be good to do so here as well.

We agree and have now modified the new paragraph as follows (lines 177-180):

"Velocities decrease along the 5 km glacier tongue from the base of the ice fall to the terminus in 2010, reaching ~18 m yr-1 near the front (Figure 5a and Figure 7a). However, in 2018, while velocities decrease initially downglacier from the ice fall, they remained higher than in 2010 and in the terminus region, velocities actually started to increase ~1km upglacier from the terminus, reaching ~78 m yr-1 (Figure 5b, Figure 7b and Figure 8) near the glacier front (Baurley et al., 2025)."

Figure 5: The difference in velocity between 2010 and 2018 shows spotty patterning – this might be realistic, or could it be related to any differences in observation resolution and processing between the two years? Some acknowledgement of the spotty pattern and discussion of why it arises would be good to include.

We added a new paragraph highlighting the "spotty patterning" and considering whether or not it is realistic (and in short here, we expect it is a processing artefact, and is one of the reasons we use averaged 'swaths' of velocity rather than specific at a point observations). The new paragraph reads (lines 184-188):

"The difference in velocity between 2010 and 2018 suggests a 'spotty' patterning (Figure 5c) in the lower glacier tongue; this pattern might be realistic but given its structure is perhaps more likely to be a processing artefact. As a result, we use averaged swaths of velocity, at a given distance upglacier, in our analyses rather than using specific at-a-point observations. Importantly, both the differenced velocity fields (Fig. 5c) and averaged swaths confirm the pronounced increase in velocity, and thus velocity difference between 2010 and 2018, as one approaches the glacier terminus."

Line 86: Mean annual SMB? Might be worth clarifying.

Thank you, we have clarified this at it was a mistake.

Lines 86-87: I typically think of negative SMB as meaning more melt than accumulation, but this seems to suggest the opposite, that most years have positive SMB – which would imply net growth rather than net loss (from the surface component of mass balance). Is this correct? Figure 6 also shows positive SMB as described, in all years except 2010, but should this actually be labeled as ablation or melt instead of SMB? Although the last sentence makes it seem that the overall positive accumulation is correct, dominated by the large accumulation area. But then the sentence about slowing mass loss with the volcanic eruption doesn't make sense. Please see my general comment above.

> Thank you for these observations. The SMB is in fact positive for this glacier over the 2011-2019 period due its hypsometry (large accumulation area), and we point this out in the last sentence of the paragraph. We have however modified the sentence about the slowing in rate of mass loss to make it clearer and the line now reads (lines 195-197):
>
> *"The 2011-2019 period represents an anomalous period of surface mass balance where the glaciers across Iceland experienced a slowdown in their rates of mass loss that coincided with regional cooling across the North Atlantic Ocean (Noël et al, 2022).*"

Line 208: What is the range of element edge length or area?

> The typical edge length of the triangles in the grid is 150 m, except in the front area, where it is 50 m. This was added to the text (lines 215-216).

Lines 218-219, Eq. 2: What is the datum for the bed elevation and lake surface elevation? Is this relative to sea level (probably yes, but it's not stated) or some other reference point?

> The datum for the bed and lake surface elevation are relative to sea level in this work which we have now clarified in the text (line 226).

Line 229: A more realistic way of doing this is to couple with a hydrology model that solves for spatially variable pressure and subglacial flow, rather than relying on assumptions of uniform overburden or water pressure driven only by ice geometry. For example, this recent paper does this for a lake-terminating Himalayan glacier:
Thota VK, Vijay S, Sommers AN, Banerjee A, Mey J, Motagh M. Seasonal variability in ice velocity driven by subglacial hydrology of Drang Drung Glacier, Western Himalayas. Journal of Glaciology. 2026;72:e44. doi:10.1017/jog.2026.10150

> We agree, in principle, that a more realistic way of doing this '*might*' be "to couple with a hydrology model that solves for spatially variable pressure and subglacial flow, rather than relying on assumptions of uniform overburden or water pressure driven only by ice geometry". However, we would argue that in reality, such a coupling would be extremely complicated and not justified for the aims of this paper. In particular, given the limitations of the constraints available regarding the subglacial drainage system structure, we don't actually believe it would be more realistic but instead, would simply be a different and more complicated model (with no constraints on boundary conditions, drainage system structure, transitions between flow conditions etc). As a result, and for this study, we have opted for a simple parameterization of the basal water pressure, making it dependent on the depth of the glacier bed below the lake level via a single parameter.

Line 287: In the experiment out to 2030, my understanding is that the surface elevation is prescribed and velocity is calculated using SSA in a stress balance without modeling dynamic thinning and thickening. Also, how is the glacier outline assumed in this projection? No change to the lateral borders? See the general comment above.

We refer to the response above but note here that the only change to the glacier outline was the retreat of the ice-front and there is no change to the lateral borders.

Lines 302-304: It's well established that the relationship between subglacial water pressure and ice velocity is nonlinear, although a universal form of that relation remains elusive. And evidence suggests that water under glaciers is usually found to be at high pressure, except near edges where it is open to the atmosphere and transitions to a free surface flow, meaning that widespread assumption of low water pressure if probably unrealistic.

We are somewhat unsure if we have fully understood the concern raised here as we do not have a "widespread assumption of low water pressure" which we would also agree would probably be unrealistic.
In effect, the minimum water pressure considered in this experiment is greater than the pressure produced by a basal water height equal to the lake level, hence we believe we have not assumed a widespread low basal water pressure.

Line 376: Surface ablation is conventionally considered as negative surface mass balance (see general comment about this)

We agree and the whole section was modified following the usual convention as noted above.

Line 384: Why define a new abbreviation SAbl for surface ablation here?

Sorry – we removed this.

Table 3: Similarly to the previous comment, I don't think it's necessary to introduce acronyms for the different quantities included in this table, as they are not used elsewhere.

The acronyms used in equation 5 remain while the others were removed.

Lines 395-397: "The proposed model highlights how the hydraulic connection between the proglacial lake and the subglacial drainage system significantly reduces basal friction." This may or may not be true, and is not modeled in convincing detail to justify this broad claim. The impact on the subglacial system is a boundary condition change that will impact the pressure distribution upstream (so it is a connection in the sense of pressure influence, but not necessarily water exchange from the lake to the subglacial system, at least not extending very far up from the terminus).

We agree with the referee and our intention was not to suggest "water exchange from the lake to the subglacial system". but instead to reflect the fact that the lake level imposes a (minimum) boundary condition on upglacier water pressure as you suggest. The sentence has been modified to clarify and reflect this and now reads (lines 464-467):

“The proposed model highlights how the hydraulic connection, understood as a connection in the sense of a pressure influence rather than water exchange, between the proglacial lake and the subglacial drainage system significantly reduces basal friction in the near-terminus region when compared to a land-terminating glacier.”

Lines 413 and 415: ice sheet’s (missing apostrophe)

Added.

Lines 417-420: This sentence reads a bit long and difficult to follow. I suggest a slight rewording to something like this: “Our study demonstrates that lake-terminating glaciers experience distinct boundary conditions that impact glacier dynamic and associated mass loss, highlighting the need to consider the influence of proglacial lakes in future projections of Greenland’s contribution to global sea level.”

We have reworded following the suggestion.

Line 423: Consider mentioning the tuning exercise here: “By tuning three uncertain parameters to match observed ice velocity, the model successfully reproduces…”

The sentence has been modified to highlight the tuning exercise and the three parameters.

Lines 425-427: Please see general comments above about strong conclusions being drawn about uniform variations in effective pressure.

We agree and have modified the conclusions accordingly to be less strong.

Line 434: Eventually, many lake-terminating mountain glaciers will retreat up and out of their lakes to no longer be connected, as well as thin to an extent where they no longer accelerate. Consider mentioning this longer-term behavior here – this is in contrast to the Antarctic Ice Sheet as referenced here.

We agree and have now added a new sentence to the Conclusions stating (lines 516-518):

“Conversely, considering behaviour over longer timeframes, many lake-terminating mountain glaciers, as well as those in Greenland, will eventually, as they continue to retreat, become disconnected from their proglacial lakes and once again terminate on land, with consequences for ice dynamics.”

References:

Zekollari, H., Huss, M., Farinotti, D., & Lhermitte, S. (2022). Ice-dynamical glacier evolution modeling—A review. Reviews of Geophysics, 60, e2021RG000754. https://doi.org/10.1029/2021RG000754

Reviewer 2 response

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

6. **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.

> Thank you for these suggestions, many of which we have adopted in order to improve the structure and clarity of the manuscript. In particular, we have now separated out the Methods into a more detailed dedicated section (now covered in Lines 202-334). We have also developed the ‘Broader Implications’ subsection into a more extensive Discussion section in order to explain what our results mean for lake-terminating glaciers more broadly, to consider in a new section the limitations of the modelling protocol (Section 4.5) and to highlight potentially valuable future directions to advance the work.

7. **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 have now modified the text in order to better reflect these important issues and misunderstandings.

8. **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; however, 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 different processes, we believe that the comparison is carried out over a period of time that is longer than the typical 'operating' timescales of the two processes under consideration (because we look at the changes and differences over a decade). As a result, 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 have now added an additional paragraph to clarify the different timescales that these processes operate on, while also highlighting why we don't believe it is a significant issue in this modelling framework. This paragraph now reads (lines 400-405):
>
> *"One potential issue with these experiments, which merits comment, is the fact that the timescales on which these two processes operate can be different. Whilst thinning is a more gradual sustained process that operates most intensely during the summer*

months, the retreat of the glacier front is an episodic process often characterised by brief periods of intense iceberg calving. Nevertheless, we suggest these differences do not significantly affect the experiment as prescribed, because the modelled time scales under consideration, span a decade, and are therefore integrating change over a much longer timeframe than is typical of either of these processes."

9. **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. As a result and in light of the comments about structure above, we have now modified the structure and include a specific section on Methods. Our intention is that the purpose of each of these two subsections are clearer now.

10. **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 who also requested clarification on the 2030 projections. However, and just to reiterate briefly here, the elevation of the surface DEM used in 2030 was linearly extrapolated based on the DEM changes observed between 2010 and 2020. Furthermore, the front was extrapolated to ensure that the retreat between 2020 and 2030 replicated the observed retreat rate between 2010 and 2020. We have now modified the methods section to make the approach to the 2030 projection clearer (Sec. 3.2).

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 have removed 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 have modified the final paragraph of the introduction to make it more concise. We have also moved some of the 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 have added this important context to the text (lines 107-108).

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

Changed accordingly.

- L116: Consider removing "operating."

We de think that “operating” provides the necessary action in the sentence and would therefore prefer to keep it.

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 has now been made clearer in the text (line 106).
> The elevation of the topographic barrier closing off the lake-filled depression is also now mentioned in the text (line 106).

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 have now clarified this in the text (lines 250-258).

- 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 has now been clarified in the text (line 259).

- **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. In that context, and as shown by the orange lines in Figure 8, the glacier has undergone a clear acceleration over that 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?

> We have now endeavoured to improve the description of the model set-up as noted above. We note that Fig 7 shows both observations and model predictions which were used to calibrate the model for the 2030 projections. 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 have now expanded on this section as requested, in part by providing additional context in the new Methods section (Sec. 3.3) and also by expanding the discussion section (Sec. 4.2) to provide further explanation.

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

> This has now been moved to the Methods section.

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

These have been moved to the Methods section (Sec. 3.1).

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

No, the flux gate is assumed perpendicular to the flow, so α_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 the ice-flow 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 (as described above and in response also to Referee 1).

List of key relevant changes made

- The last paragraph of the introduction was rewritten.
- A new sentence with relevant information concerning controls on the lake drainage was added (Section 2.1).
- A new section presenting the observational data was added (Section 2.2).
- A new short paragraph was added explaining the 'spotty' pattern of different velocities between 2010 and 2018 (Section 2.2.4).
- A completely revised new Methods section was added (Section 3).
- New explanations about the water pressure parameter and the control that the lake provides in terms of a minimum water level were added (Section 3.1).
- An expanded explanation of the 2030 modelled projection was provided (Section 3.2).
- Expanded explanations of the experiments investigating the influence of front retreat and surface thinning on ice-flow velocities were added (Sections 3.4 and 4.3).
- An expanded explanation of the sensitivity analysis regarding the water pressure parameter was added (Section 4.2).
- A new section was added highlighting the limitations of our model protocol (Section 4.5)
- A new expanded section considering the 'Broader implications' of the work was added (Section 4.6).
- Some changes were made to the 'Conclusions', primarily concerning the longer-term dynamic evolution of current lake terminating glaciers (Sec. 5).