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.

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*

<u>Overview</u>
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 will clarify this in the discussion. Regarding the assumption of a uniform P_wp factor, while we recognise it is a substantial oversimplification, 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).

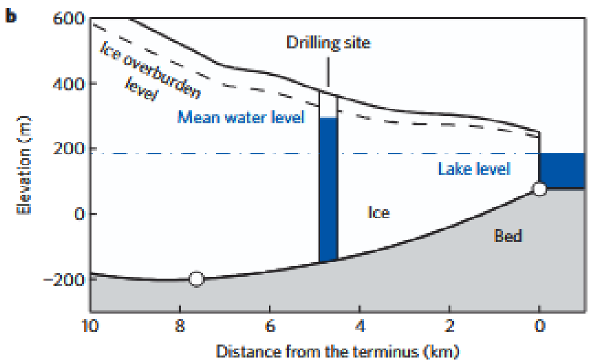

Fig. 1 from Sugiyama et al, 2011 (their Fig 1b) where the mean upglacier water level, as measured in a borehole, was considerably higher than the ‘minimum’ base-level set by the proglacial lake level.

We now suggest adding a more detailed explanation of our assumption, including the addition of references to lake-terminating glaciers, so that the paragraph would now read:

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 assumee 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, a new sentence would be added, that reads:

However, as this model represents basal water pressure in a very simple way, with a constant parameter across the entire glacier, further research is needed, using models that represent basal hydrology in a more complex manner, to investigate the extent to which variations in subglacial hydrology may impact lake-terminating glacier dynamics.

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. We will however add extra detail in the manuscript as requested that considers the implications of using the SSA assumption on our results.

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 in a response, we would discuss in more detail how the derivation of uniform values might influence our findings. Importantly however, we would also highlight in more detail that many (probably most) modelling approaches adopt tuned "global" parameters; 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.

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 DEMs between 2010 and 2020 DEMs. In a similar simple manner, the front retreat was extrapolated out to 2030 using the retreat rates observed between the 2010-2020 period.
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 will add 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.

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 a modified manuscript, we would focus on our wording to 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, that will be due to both dynamic thinning and surface mass balance, which we will need to ensure is made clear.

<u>Specific comments</u>

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 will address this query in both the results and discussion section by considering, where possible, the significance or otherwise of subseasonal dynamics data. We will also highlight that the seasonal pattern of velocities varies substantially between years which could be a source of uncertainty for the model results.

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 would intend to modify the new paragraph as follows:
>
> *Velocities decrease along the 5 km glacier tongue from 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 will add 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).

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

> We will clarify as this 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 will however modify the sentence about slowing mass loss to make it clearer which would now read:
>
> *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 will be added to the text.

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 will clarify.

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, 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 will remove 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 will remain while the others will be 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". The sentence would be modified to read:

The proposed model highlights how the hydraulic connection, understood as a connection in the sense of pressure influence rather than water exchange, between the proglacial lake and the subglacial drainage system, significantly reduces basal friction 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 will reword 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…"

This will be done.

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

We agree and will modify 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 would add a new sentence to an additional sentence to the Conclusions stating:

Conversely, considering behaviour over the long term, many lake-terminating mountain glaciers, as well as those in Greenland that will terminate in lakes in the near future, will, 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