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

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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.
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.

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.
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.
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?
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.

<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?

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.

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.

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

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.

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

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?

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

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.

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.

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

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

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.

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).

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

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. "

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

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

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.