Review of Harpur et al. (2026) ‘Calving response to lake thermal regime at an ice sheet margin in west Greenland’

Summary
This paper presents observational records of calving and lake temperature in a small ice-marginal lake and associated section of glacier terminus in western Greenland in summer 2024 and 2025. The authors show that lake temperature and thermal regime have a moderate impact on calving rates and that other meteorological and limnological variables are, at best, only very weak predictors of calving activity at the glacier. Maximum calving activity was observed when lake temperatures exceeded 4 degrees Celsius, though remained at levels that are likely fully explainable by melt undercutting, rather than there being any significant buoyancy-driven calving. They therefore posit that warming lake temperatures could be a major source of future Greenlandic ice loss, particularly at slow-flowing margins where calving events are largely determined by melt undercutting and notch formation rather than other mechanisms.

I think this paper is well-written, with well-constructed figures, and very solid methodologically, though possibly to the point of being rather long. I also do not dispute the authors’ interpretation of their results. What I struggle with are the significance of the findings. Fundamentally, the paper feels like a lot of writing to prove that – broadly speaking – warmer water melts more ice and leads to more calving, which is perhaps not the most surprising result to find. It’s also worth noting that the daily variability in calving is much higher than any temperature-induced signal, and that even the best-performing statistical models achieve a rather weak level of explanatory power, meaning that I am a little sceptical that warming lakes will be quite such a potential contributor to future mass loss as the paper would like them to be. At the same time, the field data deserve to be published, and I particularly liked the authors’ demonstration that buoyant calving was likely negligible, and their discussion of why this lake behaved differently to other ice-marginal lakes with datasets (even if I think that part is possibly tangential to the main message of the paper and could be shortened, see below). I therefore find myself slightly uncertain what to recommend. I should also say that I’m coming at this from being interested in calving, rather than being a specialist in ice-marginal lakes, so it might be that the significance of some of what is being discussed is lost on me. I think, overall, the merits of the paper carry the day, but I would urge the authors to consider if the current manuscript could be shortened. I think a couple of fairly modest changes to the introduction and discussion could also help much more effectively signpost what the paper is trying to do. So, overall, I like it, but I think a few narrative revisions might be needed to make it a bit clearer what the paper’s contributing would really help.

Page and line numbers refer to those in the clean version of the submitted manuscript.

Major Comments

- 2025 data: The 2025 data series is so short that it feels to me that it’s not adding much to the 2024 dataset, or enabling the authors to make any interpretations or conclusions that wouldn’t already be obvious from the 2024 data alone, particularly since it covers only one of the phases identified in 2024. Now, at the same time, more data is always good, but this is already quite a long paper and this maybe feels like a way to save a large amount of space without really losing anything of significance to the paper’s main arguments. I’m prepared to be argued out of this, but I would like the authors to consider how far the 2025 data is really necessary here, or whether relegating it to a separate data paper might enable the current paper to be streamlined a bit. Another avenue for reducing the length of the paper might be to shorten Section 4.1, which, while interesting, doesn’t discuss anything that relates much to the paper’s main findings about calving (and where the authors position the paper themselves in the introduction and conclusion)
- Introduction: possibly, given where the paper ends up going, it would be worth foreshadowing somewhere in the introduction that the influence of lake temperature and thermal structure on calving is expected to be strongest where the ice front is grounded (i.e. the lake is shallow) and not moving much, and that that is what this paper really wants to focus on. That then justifies the choice of field site. Otherwise, the paper is rather left open to the critique of ‘why didn’t you get data from a bigger lake where there was more going on and the thermal regime was more interesting?’, which I think, as someone primarily interested in calving, was what I found myself wondering throughout. It is covered in the discussion, but one extra sentence somewhere in the introduction would really help set the paper up

- Discussion: I think a little bit more discussion of why none of the statistical models has a high level of explanatory power might be called for here. Ultimately, even the effect of warmer water on calving is quite subtle, and I think the authors need to provide some conjecture on what their models are missing to make it a bit clearer why the future increase in water temperatures might end up being a significant source of mass loss, rather than just being lost in the noise caused by the undefined processes responsible for explaining the rest of the calving activity.

Minor Comments

- p.3, l.90: Is this more or less than the nearby non-lake-terminating fronts? The reader needs some context to understand whether this rate of retreat is meaningfully higher than would be expected for the region
- p.7, l.175-6: Can the authors say something here about the likely error manual inspection of the images might introduce? What's the minimum detectable event size? Was the entire terminus always visible or did portions of it sometimes become occluded? Any chance any events were missed?
- Figure 11: It's interesting that there's undeniably an uptick in larger events during Phase 2, but the signal in terms of number of events is much more subtle. Possibly, one point to discuss later in the paper is that, mechanically, more larger events must lead to fewer smaller events, because unstable ice that might have calved in a couple of small events is instead being removed by one big event, which would then obscure the signal in the number of events