Comment on egusphere-2024-2060

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1 General Comments

This manuscript contains valuable information and impressively comprehensive experimental results for guiding future modeling, data collection, and interpretation of Vanderford Glacier. However, currently the takeaways are not sufficiently clear, certain aspects of the methodology need to be more clearly explained and defended, and in general the manuscript needs to be edited for clarity and some technical errors.

The results here substantially contribute to the general understanding of Vanderford Glacier retreat, showing that observed calving has occurred in passive, non-buttressing ice, and did not contribute to the observed retreat rates. The authors' conclusion that basal melting likely is responsible for the observed grounding line retreat at Vanderford Glacier is important and well supported by their sensitivity testing, which suggests that high but plausible basal melt rates can reproduce observed retreat.

The authors interpret that because altimetry based estimates of melt rates are too low to reproduce observed grounding line retreat in their model, these observational datasets may be incorrect in this region. This conclusion may be valid but ought to be supported and discussed more quantitatively—two ways this analysis could be further improved are suggested in Specific Comments.

As the manuscript discusses in a few places, there are other factors which play a significant role in the simulated grounding line dynamics, and additional observational constraints which would improve future modeling efforts, beyond the melt rate itself. The presentation of these other factors can be improved, as currently the emphasis on the melt rate as a source of error and uncertainty in the abstract and conclusion does not reflect the more balanced discussion section. I offer more specific comments on balancing the discussion of sources of error in Specific Comments.

Some of the scientific methods and assumptions need to be more clearly outlined and/or defended. These are also addressed in Specific Comments.

2 Specific Comments

2.1 Abstract

As written, the abstract suggests that both observed melt rates and observed calving rates are insufficient to drive observed grounding line retreat (lines 8–11), and then goes on to state that the grounding line retreat was likely driven by melting and not calving (lines 11–13). This structuring is confusing, and I think do not clearly communicate the most important findings of the paper.

The manuscript suggests/concludes later in the text (lines 421–426) that it is physically impossible for the observed calving to exert any significant effect on the grounding line, which is supported by their perturbation experiments and by their calculation of the maximum buttressing field. I think this is a much more clear finding by which to reject the influence of ice calving on the observed grounding line retreat, as it stands for itself and does not require further interpretation. In contrast, the statement in the abstract that "...calving experiments suggest that > 80% ice-front retreat—well in excess of the observed ice-front retreat since 1996—needs to occur to generate grounding line retreat similar to observations," still left me unsure whether the observed ice front retreat could account for some of the grounding line or not. This statement also sets up a dichotomy with the previous parallel statement on melt rates, which creates the impression that they may be equally insufficient to drive the observed retreat. In this context, the concluding statement (lines 12–13), that "retreat...is likely to be dominated by basal melt, with an almost negligible contribution from calving," appears arbitrary and unsubstantiated.

A re-framing of the abstract in a way that more clearly expresses the findings which support its conclusions would function as more accurate and less confusing summary of the paper.

2.2 Interpretation of "low" melt rate observations

The authors suggest that observational datasets of melt rates calculated from satellite altimetry may be incorrect in this region. Two suggestions are made to address and support this claim:

1. The primary cause of uncertainty in melt rate estimations that is cited by the authors is that portions of the ice shelf may be floating non-hydrostatically. This source of bias and uncertainty has been independently estimated in the Vincennes Bay region by Chartrand and Howat (2023, whom the authors cite) at -0.8 ± 12.7 m w.e. a^{-1} (mean and standard of deviation, which the authors do not directly cite). Whether the further end of this range, which suggests that melt could be underestimated by up to 13.5 m w.e. a^{-1} (or 14.7 m a^{-1} in terms of ice thickness), is sufficient to bridge the gap their model reveals between observed melt rates and grounding line retreat could be addressed directly by the authors.

2. In Figure 4a–b, the authors present the "Mean annual basal melt rate derived from satellite altimetry" from (Paolo et al., 2022) and (Davison et al., 2023), respectively. Given that these figures show dramatically different spatial patterns of basal melt/accumulation rates, a quantified measure of uncertainty in altimetry-observed melt rates could be estimated by calculating the variance in melt rates between these datasets which have 20 years of overlapping data. Comparing the variance in melt rate estimates between these two parallel datasets with their results could better inform their discussion of observational uncertainty, and how observed melt rates compare to their inference of very high melt rates at depth.

2.3 Other sources of uncertainty

An important finding of this manuscript is that current estimates of melt rates from the observational record are insufficient to reproduce the observed extent of grounding line retreat in the numerical simulations presented here. Extensive testing of potential sources of error in the model which could yield these results is out of the scope of the experiments which this manuscript presents, but the treatment of sources of error ought to be presented evenly in the abstract and conclusion. The authors highlight that bed topography has uncertainties which could exceed 500 meters in the region upstream of the grounding line. The observed melt rates are introduced as perturbations to a steady state spin up which may not share a similar geometry to the real historical ice sheet. If there is reason to conclude that uncertainty in melt rates could be a more important source of error than uncertainty in bed topography, or initial ice sheet state, in the grounding line dynamics of this model of Vanderford, then there should be clear evidence presented supporting that argument.

Additionally, while the authors suggest in-situ measurement of melt rates is an important future step to take (lines 363 and 484), this suggestion is not clearly supported by their results or discussion. The expense, difficulty, and lack of spatial and temporal coverage associated with direct in-situ measurements of basal melt rates make it unclear how such measurements would actually be incorporated into their model or contextualized. Simply put, a few measurements in a vast system that are very high or very low or in between do not necessarily help to characterize the behavior of the system at a useful scale. In comparison, other suggestions put forward in the discussion section are much better supported by the results and discussion of the manuscript: improved estimates of ice shelf geometry to constrain the existing, ice-shelf wide, remote sensed data which are already usable in the existing model; improved bathymetry and ocean state measurements around the ice shelf to constrain ocean circulation models and estimates of heat flux into the ice shelf cavity; improved bed topography beneath grounded and floating ice to constrain the ice sheet geometry and grounding line dynamics.

2.4 Scientific Methods and Assumptions

1. Lowering and smoothing of the bed underneath the ice shelf (lines 86–91). A more clear explanation of what happens in the models if these adjustments are not made would better allow the reader to judge the validity and possible bias introduced by this adjustment. The statement that "this approach may limit any grounding line advance across model simulations" leaves unanswered the question of what effect such a bias could have. If it does prevent grounding line advance beyond the initially imposed geometry, it may obscure a tendency of the model to advance in the spin-up stage.

2. The use of a 500 year spin-up steady state from which the perturbations are conducted could be introduced and defended much more clearly. My understanding from reading the manuscript is that there is an implicit assumption that the forcings which caused grounding line retreat over the observational record ought to be able to cause similar grounding line retreat when introduced as a perturbation from the 500 year steady state model. If this interpretation is correct, it should be much more clearly stated in the manuscript. Importantly, the state of the ice domain (i.e. thickness, velocity, strain rates) after the 500 year spin-up in comparison to the initialization fields from observations should be shown somewhere in the main text or in the supplement. Any significant differences between the model state before and after the spin up should be discussed in the manuscript as to how those differences should be interpreted, and how they may impact the results.

2.5 Other comments

line 8 "...instead, basal melt rates in excess of 50 m yr^{-1} at the grounding line...." It is unclear how much more 50m yr^{-1} is than the observed rates, making this statement vague and hard to interpret.

lines 49–50 The phrasing of this idea implies that we should not expect high local variability in melt rates, even though that is quite common. I agree that high melt rates nearby, forced by similar ocean water, supports the plausibility of similarly high melt rates under Vanderford, but only in the context of other evidence. The disparity between observed Totten and Vanderford melt rates alone, without the context of these results, is not necessarily surprising at all. This idea might be better suited for the discussion section.

lines 55–60 These sentences are not clearly connected to the introduction and seem at least partially related to or repeated in the discussion.

Figure 2 The Totten insets are blocking the Underwood ice shelf which is otherwise visible in the other figures. Because the colorbar is scaled to the higher melt rates on Totten, it is hard to actually interpret the range of observed melt rates on Vanderford. Contours, labels, or different color limits could be better. Additionally, this figure does not make clear what the spatial extent of the observed melt rates are: does the observed melt rate field evolve in time with the ice shelf cavity to follow the grounding line retreat? Or are they only provided for this spatial extent (of the unretreated grounding line)?

line 69–70 "…can current estimates of basal melt and ice-front retreat…." This sentence confused me because it sounded like the experiments would only use observed melt from a single snapshot in time (current estimates). The observed melt rate datasets and ice front retreats span at least 25 years of the observational record.

line 106–109 The quantities here should be given units similar to lines 111–112.

line 166 "…mean annual basal melt rates…." This seems to refer to an average across each year resulting in a different "two-dimensional mean annual melt rate field" (line 145) for each year. If that is the case, that should be clarified. Similarly, clarification is needed for the "Mean annual basal melt rate" fields shown in Figure 2a–b. My assumption in Figure 2 was that this is a two-dimensional field which forms the average per-year melt rate across the entire observational record (i.e. not just a single year). More explanation is needed to interpret what the mean annual basal melt rate fields actually are, and whether they are averaged across the whole record or if there is a different field for each year.

Figure 5 Elevation shown is from BedMachine—is this showing the elevation with the lowering and smoothing of the bed? It would be preferable to see the bed elevation as it is actually implemented in the model.

lines 217–218 How different is the flux across the grounding line in the spun up steady state from the actual observations used to initialize the model?

Figure 6b Why does the $M_{Davison}$–Weertman grounding line retreat so far and so suddenly? This retreat does not seem to be reflected in ΔVAF (Figure 6c) or in the map (Figure 5a).

lines 339–340 The melt rates required to drive grounding line retreat in the model are twice those of the observational data: This could be a clarifying result to add to the abstract, as it contextualizes what 50 m yr^{-1} means in relation to observations.

line 344 This struck me as a key finding: if removing the entire ice shelf relatively suddenly is actually not nearly enough to drive the rate of retreat observed, then it seems that melt rates needed to remain high at depth close to the grounding line as the ice cavity evolved.

lines 417–420 Mode-3 melting is associated with melting close to the ice fronts, generally in passive zones which contribute little to buttressing (Adusumilli et al., 2020). In this model, removing the front portions of the ice shelf entirely does almost nothing. Why would this type of melting be worse than melting at depth dominated by CDW?

line 432 "Our simulations show that the Vanderford Glacier system responds rapidly to calving events." This was confusing to me, as Figure 7 shows almost no response to most calving, and if anything a delayed, ongoing response to large calving.

lines 474–476 This could include the results that observed calving have had no effect. The phrasing here is slightly convoluted and confusing.

lines 481–488 The final paragraph of the conclusion includes vague phrasing and does not highlight a strong implication of the results. Overall the conclusion repeats a lot of the discussion. My takeaway from the manuscript overall is that there is an unresolved incompatibility between modeled dynamics, observed forcings, and observed dynamics. A clear and succinct outline of those incompatibilities, and a stance taken on what the implication of those incompatibilities are until they are resolved would be enlightening and interesting.

3 Technical Corrections

Figure 6c–d y-axes are labeled as changes in volumes with units of Gt. The labels should be corrected.

References

Adusumilli, S., Fricker, H. A., Medley, B., Padman, L., and Siegfried, M. R.: Interannual variations in meltwater input to the Southern Ocean from Antarctic ice shelves, Nature geoscience, 13, 616–620, 2020.

Chartrand, A. M. and Howat, I. M.: A comparison of contemporaneous airborne altimetry and ice-thickness measurements of Antarctic ice shelves, Journal of Glaciology, p. 1–14, https://doi.org/10.1017/jog.2023.49, 2023.