Modelling GNSS-observed seasonal velocity changes of the Ross Ice Shelf, Antarctica, using the Ice-sheet and Sea-level System Model (ISSM)
– Response to Reviewer 3 –

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Firstly, we would like to thank all three reviewers for their constructive and detailed comments. We agree with many points that were raised, especially the lack of discussion of our results in the context of recently published papers (e.g., *Klein et al.* [2020] & *Mosbeux et al.* [2023]). We have responded to each reviewer's comments below. All three reviewers' main comments included the need for additional discussion and consideration of *Klein et al.* [2020] and *Mosbeux et al.* [2023]. This is a good point that we will address throughout a revised manuscript, as we realize that we did not adequately motivate and contextualise our study, and these previous works deserved more recognition.

Novel contributions

Several reviewers questioned the novelty of aspects of our study. Here we briefly summarise aspects of our study which we believe are novel contributions.

- We present new GNSS time series from the Ross Ice Shelf that have not previously been published. These include sites near the calving front, near a significant pinning point, and in the deep interior of the ice shelf near the grounding line.
- Notably, our Site 2 is close to the calving front near the Ross Island region, which has been identified as observing high basal melt rates on a seasonal timescale (Stewart et al., 2019).
- We show that these measurements consistently show 2 peaks in ice shelf velocity every year (for Sites 1, 2 and 4, the newly collected datasets), contrary to previous measurements presented in *Klein et al.* [2020] and *Mosbeux et al.* [2023].
- We suggest that the seasonal variability of SSH (i.e., yearly cycle) may not be able to reproduce our GNSS seasonal velocity variability (i.e., semi-annual).
- We therefore turned to the potential role of basal melt and wanted to test *what it would take* to match velocity variations by changing the forcing as little as possible.
- Our approach of combining Automatic Differentiation and weekly MITgcm basal melt rates ([*Klein et al.*, 2020] used monthly basal melt rates) is also novel.

Sea surface height (SSH)

An area where the two previous studies should be discussed more in our manuscript is in regards to what other factors that could be driving the observed velocity variations on the ice shelf. [*Mosbeux et al.*, 2023] nicely shows that the seasonal variability of SSH can explain their observed seasonal variability of ice velocity.

To take this into account, we will rerun our simulations with the same SSH forcings implemented in [*Mosbeux et al.*, 2023] to consider this factor. However, we expect that the seasonal variability of SSH cannot explain our two-peaked seasonal velocity variability, as mentioned above. In our revised manuscript, we will also discuss other possible factors (tides, sea ice buttressing etc), that may also be good candidates to explain our new GNSS observations.

Basal melt rates

Melt rates are difficult to model and properly constrain, especially close to grounding lines, despite their critical role on ice dynamics. All reviewers commented on the realism of the basal melt rates. We agree that these basal melt rate perturbations we use are extremely high for the Ross Ice Shelf, today and in the future. However, this paper focuses on asking whether perturbations in basal melt rates *can* reproduce a similar velocity variability as observed by the GNSS units. We acknowledge that our contribution is a proof of concept, not a definitive answer to the question, and we will do our best to make this clear in the revision.

Multiple peaks in melt rate perturbation

Several reviewers questioned our use of multiple peaks in melt rate perturbation. Here we clarify our motivation for doing so. The baseline weekly MITgcm basal melt rates include a clear peak in the austral summer, and multiple other (much smaller) peaks throughout the year, highlighting that the basal melt rates have more variability than presented in [*Klein et al.*, 2020]. We also refer to [*Stewart et al.*, 2019] basal melt observations in our discussion, highlighting that they observe the largest peak in the austral summer, but also smaller peaks in the austral winter.

Klein et al. [2020] suggest that the actual total summer increase in the heat content of the AASW layer near the ice front is likely to be larger than the modelled increase, and the seasonal enhancement of the basal melting will continue further into autumn than in their model. [*Klein et al.*, 2020] extended the late melt period to April and found that it also shifted the timing of maximum velocity a month later, showing that a longer or later melt period at the front could align the modelled and observed velocity phases.

Our approach is to use multiple basal melt peaks as the basis for our phasing of the basal melt forcing, and we apply perturbations on this forcing until we reproduce a similar velocity variability to the GNSS observations. Through this, we can highlight that seasonal basal melt rates can reproduce the GNSS velocity variability on an interannual timescale for XX of the sites. We do not state or intend to imply that these perturbed basal melt rates are realistic for the Ross Ice Shelf. Our study instead serves as a proof of concept, motivated by Klein et al. "as-yet-unidentified seasonal processes". This overall aim will be clarified in the revised manuscript.

1 Reviewer 3

1.1 General comments

I therefore have several concerns regarding the realism of the modeled melt rates and the conclusions of the paper. Furthermore, the paper overlooks the potential influence of other factors such as sea surface height variations and tidal effects, which have been shown to significantly impact ice flow dynamics in previous research. Even focusing solely on basal melt rates, seasonal melt close to the grounding line where ocean models usually struggle to correctly model high melt rates (e.g., the melt under Pine Island ice shelf in Dutrieux et al., 2013) and their effect on the grounding zone, could have been explored by the authors

Thank you for this comment. Firstly, we are aware now that we did not discuss the potential influence of other factors in enough detail. This will be added in the updated manuscript, as well as discussing the [*Klein et al.*, 2020] and [*Mosbeux et al.*, 2023] studies in more detail. As detailed in our summary response above, we will take into account sea surface height variations and discuss tidal effects, when concluding the influence of basal melting on the observed velocity variations. Regarding the realism of the modelled melt rates and the conclusion of the paper, please also refer to our summary response above.

1.2 Specific comments

Figure 1: To me, this figure could be reworked and made cleaner. Why drawing null velocities in the ocean? It only decreases the readability

Figure 1 will be reworked and made cleaner in the updated manuscript.

On site 3, which is the main site used by [*Klein et al.*, 2020], the data derivation from displacement to velocities gives you a minimum in April.

Yes, this is correct. These are the velocities we obtained using our processing steps as outlined in the manuscript, and our results compare well with [*Klein et al.*, 2020]. We show similar seasonal variability in the GNSS-derived velocities at Site 3, however, we do observe a minimum in April and a maximum in August, which are offset by 1 month compared to results presented in [*Klein et al.*, 2020] (minimum in March and maximum in July).

Figure 2. The figure really looks like a draft and not a publishable figure. The grounding line and the safety bands are both plotted in black. There is no metrics on the x and y that are used and written. The southern part of the grounding line is cutoff without specific reasons

We agree with the reviewer that this figure needs work, but following the recommendation from reviewer #1, we will instead remove this figure in the updated manuscript as suggested by Reviewer 1.

To me, if the MITgcm modelling shows a seasonality in melt rates, this seasonality should be explored, even if it does not give the correct phasing on the ice flow velocities. The MITgcm melt rates should be shown with maps of melt rates at different period of the years, or at least with a timeseries of the integrated melt rates over the ice shelf. For example, the model melt rates in [*Klein et al.*, 2020] shows only one peak melt rate in February (see their Figure 7a or the maps in Fig. 8). Why building a twice peaking melt rate if it is not realistic or backed by any modelling or observation?

A figure showing the timeseries of the baseline MITgcm basal melt rates will be added to the updated manuscript. As detailed in our above responses, we are using these perturbations in basal melt rates to understand whether basal melting *CAN* reproduce the GNSS observed velocity variations, assuming that MITgcm basal melt rates are imperfect and may not include all possible variability (especially in the vicinity of grounding lines).

Figure 5: Looking at the pattern of your observed velocity variations, it seems that ice flow reaches a minimum velocity in March and a second one in August. My understanding is that this is the reason why the authors apply two peak melt rates in your idealized sinusoidal melt. However, such a semi-annual cycle caused by something different like a semi-annual variability in tidal amplitudes and affecting the grounding zone of the ice shelf, as suggested in [*Mosbeux et al.*, 2023] conclusions. This could be seen as a process similar to the nonlinear response of the ice shelf (and the ice sheet) to the diurnal tide (e.g. Gudmnundsson, 2011; Rosier et al., 2020). Site 3 semi-annual cycle does not seem as clean as on other sites but still visible with a sharp drop in velocity in November followed by plateau from early January to March, a second drop in March-April before a reversal with a speed up until August, ending with a second Plateau from August to November. From the detrended displacement in Figure A4, we do not see any sharp change in displacement in November. How do you explain such result? Also, the strong direction changes before January 2016, does not really reflect in the detrended x and y displacement. Looking at [*Klein et al.*, 2020], the velocity trend looks a bit different. It would be good to investigate the reasons for this.

This is an interesting point, we will include further discussion about other potential factors (SSH and tides) that could be driving the observed variability in velocities in the updated manuscript. The differences between our velocity trend and [*Klein et al.*, 2020] velocities will be discussed in more detail in the updated manuscript. The difference could be due to [*Klein et al.*, 2020] using T-TIDE analyses to remove the tidal signals in the dataset, and could also be due to the time window used. In this paper, we use a time window of 8 weeks to smooth the short-term tidal effects and to identify seasonal changes.

References

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