

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 2 –

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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 2

1.1 General comments

The findings of Klein et al., 2020 and Mosbeux et al., 2023 are referenced but then appear to be largely disregarded until the discussion. In particular, the potential influence of changes in sea surface height and ocean tides is completely overlooked. Tides are known to cause substantial variations in velocity over short periods (e.g. Anandakrishnan et al., 2003; Doake et al., 2002), and also potentially over long periods of up to a year (e.g. Murray et al., 2007). The GNSS processing smooths out short-term tidal effects, but I expect daily variability is large, being previously observed nearby at up to 100 percent of the mean (e.g. Brunt et al., 2010). It is not inconceivable that small, solar annual or semi-annual tides could drive the remaining 1 percent semi-annual variations in velocity shown in Figure 3 and it needs to be explained why they can be ignored..

Hopefully our summary above addresses this important and fair comment. In the revision, we will re-run our model experiments with the seasonal variability in SSHs.

Secondly, it is not clear to me why a more realistic melt forcing was not used? The forcing used here (and required to match behaviour at sites 1 and 2) is symmetric with two peaks, when observations from Stewart et

al., 2019 show only one dominant peak in the northwest region near Ross Island. Also, the peak melting observed by Stewart et al., 2019 occurs consistently in February while here melt peaks are applied in April and October, and the magnitude of the melt perturbations appears to be substantially higher than observations, with 20 m/a additional melt required, presumably on top of a baseline rate?

Again, this is a fair point. We realize that we have not made the objective of our study clear. Our experiments try to quantify how high the melt rates need to be to explain the variations in ice speed. We highlight that these perturbed basal melt rates are unrealistic for Sites 3 and 4, and suggest that they may be more realistic for Sites 1 and 2 and are potentially likely to occur in the future due to global warming. Stewart et al., 2019 found maximum summer basal melt rates of 52 m/a at the calving front near Site 2, highlighting that our basal melt magnitudes are not that unrealistic compared to the current maximum summer basal melt rates. Please refer to the summary provided above for a more detailed response to this comment. We will also clarify that these perturbed basal melt rates are on top of the baseline rate provided by MITgcm, and we will provide a figure of these baseline basal melt rates in the Appendix.

My fundamental issue with the paper is that without an understanding or attempt to account for other more dominant factors that are driving variability, I don't think it can be concluded with any confidence that seasonal melt is driving (or even influencing) seasonal velocity variability at any of the sites.

Hopefully, our responses above clarify this point, we realise now that we did not take into account other studies in our current manuscript, and this will be updated significantly. Additionally, other possible dominant factors will be discussed in more detail in the updated manuscript and we will make sure to account for changes in SSH, following *Mosbeux et al.* [2023].

1.2 Specific comments

Abstract: 'sensitive regions' are mentioned several times before it can be determined that this means regions where ice velocities are sensitive to basal melt

The sensitive regions being discussed here are related to *Baldacchino et al.* [2022]; *Fürst et al.* [2016]; *Gudmundsson et al.* [2019]; *Reese et al.* [2018]. We will clarify this in the updated manuscript.

Line 12: "We suggest that... velocity variations... could be partly... driven by melt". This is a very tentative conclusion and implies that you don't believe this to be the case either.

This is a sentence where we tried to clarify the scope of the study, which is to investigate whether basal melt CAN explain GNSS' data, but we are not providing a definite answer.

Line 55: sp. Siple Coast ice streams

Done.

Line 70: I would argue that GNSS doesn't provide a unique opportunity to measure seasonal variations in velocity – satellite methods can also do this – although GNSS does have better resolution and accuracy.

We would argue that the high temporal resolution of the GNSS instruments that are deployed for multiple years on the ice shelf still provide this unique opportunity. Furthermore, GNSS allows us to capture wintertime velocities, when visible-band remote sensing is not possible (due to 24hr darkness in Antarctica). This alone makes the data unique.

Line 70: sp. MacAyeal

Done.

Line 80: While you discuss the work of Klein et al., and Mosbeux et al. in the previous paragraph, the aims and approach taken in this paper do not follow from this discussion.

This section will be edited so that the aims and approach in this manuscript follow on from the discussion.

Line 101-104: These two sentences contradict each other.

This sentence has been removed "However, thinning of this region or changes in the ice-front location (i.e., via iceberg calving) will alter the stress balance and velocities of the ice shelf (Gudmundsson et al., 2019; Klein et al., 2020)."

Line 129: What is meant by 'data processing was iterated'? How are initial positions updated and until what criteria are met?

Because initial position precision wraps into final position precision, updating the initial position in a GNSS Rinex file with an estimate closer to the actual initial position improves the final estimate. To achieve this, the data is processed the first time, this first estimate of position is then used as the initial position for a second processing round. This is accomplished by creating a new rinex GNSS data file with the updated position and processing. This is typically only done once and removes unrealistic steps between position solutions.

Line 132: What is the 'reported processing uncertainty'?

The formal position uncertainty is estimated by the NRCAN CSRS-PPP service.

Line 133: More detail is required here to understand the processing steps and error propagation.

We think this is clear. The gradient in the x direction, dx/dt , provides the velocity in the x direction. The uncertainty in this gradient provides the uncertainty in v_x . The same goes for the gradient in the y direction. We propagate the uncertainties by adding percentage uncertainties when determining the total velocity magnitude and direction by adding the percentage uncertainties when multiplying and dividing.

Line 136: Is aliasing the correct word here?

We think aliasing is fine here.

Line 141: Where is this presented?

Figures 3 and 5, this has been edited in the updated manuscript.

Line 159: Is melt varied across the whole domain or locally? Assuming these units are m/a (velocity) per m³/a (melt), it is not clear over what area of the ice shelf the melt is integrated.

Yes the melt perturbation $\delta\dot{M}_b$ is spatially variable (hence the use of a directional derivative, and Automatic Differentiation), and is expressed in m/a. The integral is over Ω the entire model domain (although the gradient will be 0 over grounded ice since melt is not applied there).

Line 181: It would be useful to know what the baseline MITgcm basal melt rates are and how / if they vary seasonally before the perturbation is applied.

A figure showing the baseline MITgcm basal melt rates will be added to the Appendix of the updated manuscript. In this figure, we can see that the basal melt rates do vary seasonally, with a peak observed in the austral summer. However, we also see that there is a lot of noise in the MITgcm basal melt rates throughout the year, with multiple smaller peaks shown in the austral winter.

Fig 2 caption: The whole of the RIS and islands are also outlined in black so it is not clear which bit is 'passive ice'.

Passive ice will be identified using a different colour in the updated manuscript. The caption will be edited accordingly.

Fig 3. What is meant by 'errors'?

The errors are described in Section 2.2 as uncertainties. This will be edited in the Figure 3 caption.

Line 247: I don't see evidence that 'local changes in basal melt influence the velocities at Site 4'? This also seems to contradict the conclusions (Line 486).

In Figure 4 Site 4 we see high sensitivity at the KIS grounding zone to changes to basal melt. Here we are highlighting that local changes in basal melt *AT* the KIS grounding zone impact the velocities at Site 4. In the conclusions, we suggest that these local changes in perturbed basal melt rates *AT* the KIS grounding zone are unrealistic and thus changes in basal melt do not influence the velocities at Site 4. We can clarify this in the text in the updated manuscript.

Line 265: What is the difference? It could be important to note here.

The aim of this paper is not to try and match the GNSS and modelled velocities, but rather we are trying to determine whether it is possible to reproduce similar velocity variations from basal melt. We want to provide a proof of concept and test our hypothesis that perturbations (both in magnitude and phase) in basal melting at the identified sensitive regions could reproduce a similar behaviour in velocities to the GNSS observations. Therefore we will not quantify the difference, as we deem this is outside our aims of the study.

Figure 6: Is this the baseline dataset (i.e. perturbation of 0 m/a)? In which case why is there no velocity variability in the 0 m/a perturbation scenario (Fig 5). The mean and max imply seasonality in this baseline dataset.

Yes, this is the baseline dataset, and a time series of the baseline dataset will be included in the updated manuscript. We observed no velocity variability in the 0 m/a perturbation scenario, and we suggest this is due to the high seasonal basal melt values shown in Figure 5 not occurring at the GNSS sites locations, but close by (especially for Site 2). Hence, why the perturbations in basal melt rates were needed in our experiments.

Line 422: I don't think it is justified to say that "the majority of Site 2's intra-annual velocity variability is driven by seasonal changes in melting".

This has been changed to "Site 2's intra-annual velocity variability could partly be driven by seasonal changes in melting".

Line 449: Duplicate reference of Liu and Miller, 1979.

This has been removed.

Line 456: What is the mechanism for a short period of surface melting to lead to a velocity increase in the centre of the ice shelf, far from any shear zone?

This paragraph will be reworded, and sentence removed, as it does seem unlikely that a short period of surface melting in the centre of the ice shelf can lead to a velocity increase. However, Site 3 is located near one of the major rifts on the RIS, where shear stresses could be impacted by surface melting leading to velocity variations. The impact of the El Nino event on the basal melt rates will instead be focused on. [Klein *et al.*, 2020] state that the surface heat fluxes over the ocean during this surface melt event in January 2016 may have been substantially different than those used to drive the ocean model that provided the basal melt rates used to force their ice-flow model. This will also be further discussed in our updated manuscript.

Line 460: This is a misrepresentation of what is stated in Mosbeux *et al.*, (2023) who do tentatively attribute the 6-monthly signal to tides.

This sentence will be edited, and further discussion will be provided on the findings of [Mosbeux *et al.*, 2023] in the updated version of the manuscript.

References

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