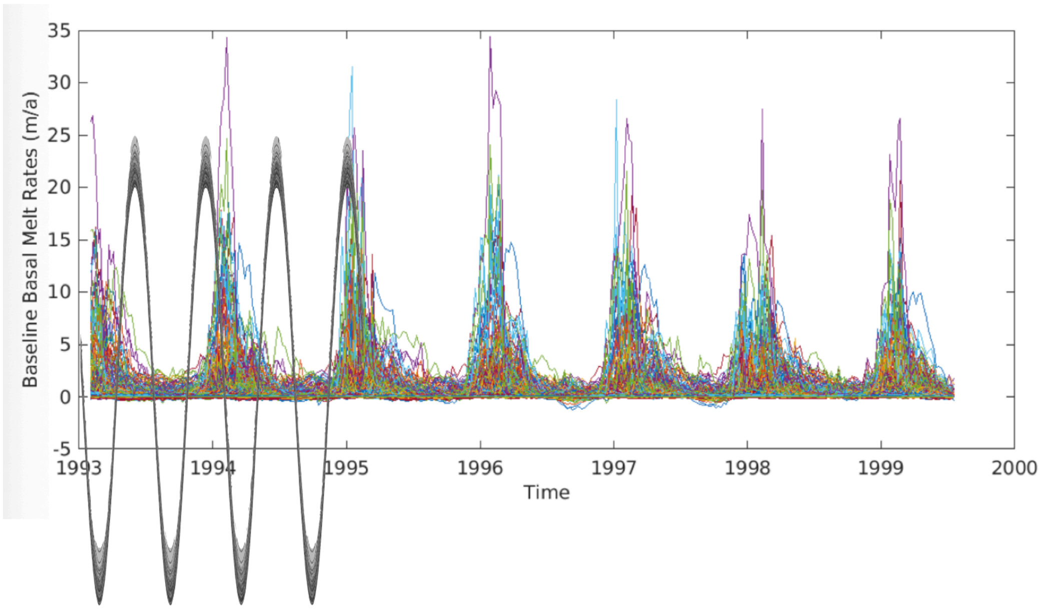
**Modelling GNSS-observed seasonal velocity changes of the Ross Ice Shelf, Antarctica, using the Ice-sheet and Sea-level System Model (ISSM)**

I first want to apologize for the delay in this second review. I partially delayed it because I wanted to make sure it was fair and right. As I explain it hereafter in greater details, I found this work interesting and definitely useful to the community but I think that additional warnings are needed when it comes to the conclusions.

I thank the authors for their answer to most of my questions about the manuscript and their considerations for my suggestions and suggestions by other reviewers, especially the new simulations including SSH perturbations. I think that the manuscript has largely improved although, in my opinion, the conclusions on the melt-induced seasonal velocity changes remain too strong and misleading. The authors should only claim what it can: they apply a synthetical basal melt to get the desired ice flow response and show that they succeed… but that the necessary synthetical basal melt is very far from realistic, given state of the art ocean modelling and subsequent basal melt estimations. Here after, I expose my main arguments (given my understanding of the paper).

The sinusoidal perturbation applied to the basal melt have been argued be too strong during the previous review. Such perturbation was indeed surprising given the low melt rates typically observed over the region, as rightly reminded by the authors: *"The RIS basal melt rates are relatively low due to the cold dense water masses formed on the continental shelf [...]".* The authors justify this choice based on observations by Steward et al. (2019): "*Recently, high basal melt rates have been observed at the calving front near Ross Island due to the seasonal inflow of summer-warmed AASW from the adjacent Ross Sea Polynya downwelling into the ice shelf cavity*". However, this high seasonal melt has been observed close the calving front and Ross Island. Previous modelling has shown that this intense melt rate is seasonal (e.g., Tinto et al., 2019) and occurs over January-March, which coincides with observations (e.g., Steward et al., 2019).

My issue, as I stated in my first review, is that the authors choose to (1) synthetically introduce melt at specific spots guided by the sensitivity map to maximize the impact of the forcing with (2) a timing guided by a sinusoid specifically phased to match the velocity change observed, and (3) set the amplitude of the sinusoid to values largely exceeding MITgcm values. I quickly tried to compare the MITgcm melt to the sinusoid used for perturbation–notice that I could be wrong since I did this by hand (to me, this figure combination would be a very nice addition to the paper)–and I get the following figure, where we see a very different phasing of the two components:




I agree that ocean models have their limitations but correctly modelling melt rates have been one of the main goals in the community interested in ice-ocean interaction over the last decades, with great improvement and melt rates matching observations to a great extent. MITgcm is one of the leading ocean models in this matter and is, to me, unlikely to be off by such an extent. When it comes to Ross Ice Shelf, the comparison of the modelling by Tinto et al. (2019) and the matching with observations by Steward et al. (2019) is a striking example of the current capacity of state-of-the-art models. In any case, the speed changes induced by the MITgcm melt rates are neglectable (blue line in Figure 4) with respect to simulations including the sinusoidal perturbation, meaning that either MITgcm is significantly wrong or that melt rates are not a key factor until we apply perturbations that do not look at all like MITgcm. In this regard, the phasing of SSH-induced speed change seems to be off too (I understand that you apply the same forcing as Mosbeux et al. (2023) but only with a hydrostatic grounding line migration) but the amplitude of the changes looks better than the melt rate induced perturbation without requiring a synthetical tuning. The comparison is also not entirely fair since you compare melt rates applied to specifically sensitive regions to realistic SSH over the entire domain.

In conclusion, at the reading of the paper, it seems that the authors pushed a bit too far their storyline, i.e., “melt-induced seasonal speed change”. The conclusions of the paper should be tempered before publication and the synthetical approach applied to build the ocean forcing should be better emphasized in the discussion. I strongly believe that the future of ice sheet modelling relies in matching such seasonal variations and that this manuscript shows valuable results in this context, i.e., melt rates is a factor but maybe not the main factor to model such speed changes if we trust the ocean model (which I personally do more than a forcing tuned to match a target). This conclusion aligns well with Klein et al. (2020) and I think it is a good thing to have two studies using slightly different approaches to get to a similar conclusion. The number of adjustments on the model melt rates required to reproduce the data is just too high. *“When a measure becomes a target, it ceases to be a good measure” (Goodhart’s Law).*

I also have a few specific questions concerning the revised version.

**Specific comments**

In the abstract, we can read: "*While this study does not bring a definitive answer to the question of what the drivers of seasonality in ice flow are, this study shows that seasonal basal melt rates could explain the GNSS velocity variability on ~~an~~* (typo) *seasonal timescale for two of the four GNSS sites".* This is an example of what I would call "too strong" or "misleading" conclusions due to the forcing needed.

I thank the authors for reworking their figures based on some comments, especially Figure 1. However, to me, Figure 3 does not look good enough. I do not understand why the grounding line is randomly cut-off on the left and on the top of the panels (same for Figure A5, A6 and A7). Figure 5, suffers the same issue and personally think that melt on the grounded ice and especially on the open ocean should not be displayed as a null value but just left "blank".

Figure A3: the y axis for the direction has a strange unit system (e.g., 0.1 +1.896e2 degrees is not very reader friendly)

Figure A9: Why is the time going from 2040 to 2042? It would be interesting to align A8 and A9 to compare MITgcm melt rates in sensitive regions (see my earlier suggestion). Also, what is the meaning of the different colors and lines?

Line 669: "exploring" instead of "exploration of" to stay consistent with (1) and (2)?

Line 673: I think that this correction, explaining that the perturbations are large (maybe add that the design of the experiment leads to make the perturbation in very sensitive spot, which is not necessarily the case with real melting). I think that you should ensure that this level of scientific restraint is maintained throughout the entire manuscript.