Review of “Modelling GNSS-observed seasonal velocity changes of the Ross Ice Shelf, Antarctica, using the Ice-sheet and Sea-level System Model” by Baldacchino et al.

Over the last decade, several GNSS stations have been deployed across the Ross Ice Shelf, revealing strong signals with noticeable variability on intra-annual time scales, suggesting a seasonal pattern. This variability in horizontal displacement can be expressed as variations in the ice flow velocity.

Klein et al. (2020) suggested that seasonal changes in basal melt might contribute to this variability, but their modeled velocities based on realistic seasonal melt rates were much smaller than observed values (i.e. about 10% of the observed variations). Similarly, Mosbeux et al. (2023) explored how seasonal fluctuations in sea surface height could affect ice velocity through changes in driving stress over the ice shelf and changes in basal stresses at the grounding zone.

In this manuscript, the authors also investigated the effect of the seasonality in basal melt on the seasonality in ice flow velocities using ISSM. They first use Automatic Differentiation (AD) tools built in the ISSM model to pinpoint regions where changes in basal melt rate have the most influence on the ice flow.

Regions highly sensitive to seasonal melting align closely with those identified in studies focusing on the effect of buttressing on the grounded ice dynamics (Furst et al., 2016; Reese et al., 2018). Notably, the vicinity of Ross Island, known for significant buttressing effects, exhibits high sensitivity to basal melt. This same region has also been shown to exhibit large summer melt rates, as supported by both modeling (Tinto et al., 2019) and observational studies (Steward et al., 2019).

Similarly, to Klein et al. (2020), the authors then use an ocean model to explore the sensitivity of the flow to a realistic melt rate pattern. Although the authors utilize MITgcm melt rates instead of the ROMS melt rates from Klein et al. (2020), both highlight a similar melt rate pattern. However, the present study employs significantly higher melt rates by amplifying their model melt rates following a simple sinusoidal function over time in high sensitivity regions highlighted by the AD. With their method, they find that velocity variations match the observations only when basal melt rates peak at over 80 m/a on the top of the background signal of MITgcm. Such melt rates appear disproportionately high compared to the observed 3 m/a reported by Steward et al. (2019) or their own MITgcm outputs. While Klein et al. demonstrated that by increasing their melt rates by a factor of ten (reaching roughly 20 m/a) they could better match observed velocity changes at GNSS stations, they could not justify such melt rates based on current observations and ocean modelling. There is also no evident that he amplified melt rates used by the authors show realistic patterns both in time and space.

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.

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.

- 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.

- 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.

- Section 2.4: You propose a time varying melt rate $M_b(t)$ based on MITgcm output with an additional perturbation based on a sinusoidal variation $\sin 4\,\pi + 3t$ in locations where the ice flow shows a large sensitivity to melt rates variations. The amplitude of the sinusoidal perturbation is defined by a factor p. Later, we quickly realize that MITgcm variations do not affect the flow at all (variations much lower than 1 m/a in all sites, as shown in Figure 5). This means that the seasonality of MITgcm does not affect significantly the ice flow. The only way to trigger significant variations is to apply a sufficient perturbation $p \sin 4\,\pi + 3t$. However, this perturbation is not based on any realistic ocean simulation or observations and even seems to go against the model, as stated by the authors *"The raw MITgcm basal melt rates displayed a seasonal signal, however, the amplitude of this seasonal variability was not large enough and the phasing incorrect to replicate the GNSS observed velocity variability."*

  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?

- 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 non-linear 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.

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