Review of *Seasonal variability in Antarctic ice shelf velocities forced by sea surface height variations* by Mosbeux et al. (2022)

This study uses an ice sheet model to examine the impact of seasonal sea surface height (SSH) anomalies on ice flow of the Ross Ice Shelf. The SSH anomalies that are used to force the ice sheet model are derived from an ocean model that is verified against satellite altimetry measurements from 2018. Simulated changes in ice flow are compared with GNSS time series from various locations on the Ross Ice Shelf (RIS). The ice flow anomalies in response to SSH anomalies on seasonal timescales are on the order of metres per year, and the effect of grounding line migration is found to be the dominant process by which SSH anomalies induce changes in the ice flow response. The study argues that examining the impact of such short-term changes in SSH is important in being able to interpret the impact of future sea level rise on ice sheet dynamic response.

I found the manuscript an enjoyable read: it is well written, the figures are appropriate, and the methodology is sound and generally well-described. My main comment is on the implications and significance of the findings, which I detail below, followed by minor comments.

Main comment

Figures 7 and 8 show that the changes in ice flow in response to seasonal SSH changes are quite low ±3 m a^{-1}. From this, I wonder about how significant the impact of seasonal changes in SSH is? L25-26 of the abstract says: "... will provide further insights into longer term ice shelf and ice sheet response to future changes in sea level." It'd be great to see the significance of the findings discussed in the context of future sea level rise. Do you think including this SSH response at seasonal timescales is necessary to delineate between the ice flow response due to climate change compared with natural variability, for example? What kinds of uncertainties would we introduce by ignoring this process? On what timescales (e.g. for future ice sheet scenarios) is it important to consider these seasonal SSH processes? It'd be helpful to see a discussion of this in the manuscript, perhaps in Section 5.

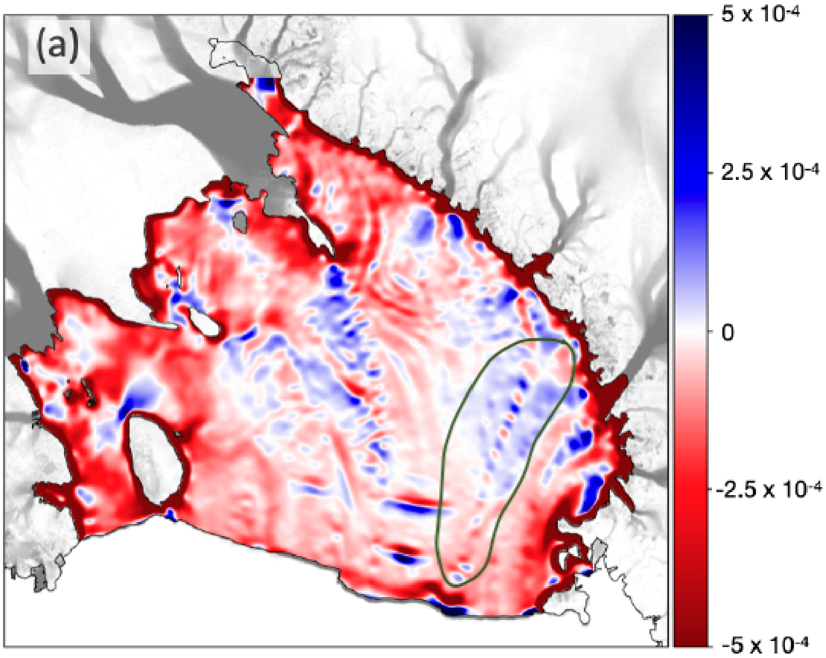
To answer this, it could be helpful to consider whether/to what degree it's possible to scale the findings here to SSH variability on longer timescales? Without introducing new simulations, I'd be interested to see whether a relation could be found between the percentage changes in SSH to the percentage changes in driving stress to the percentage changes in ice flow (and similarly for the grounding line migrations). That is, is it possible to say for every XX% change in SSH, there is up to XX% ice flow change and XX km of grounding line migration? Would we expect these numbers to scale (and if so, linearly? Exponentially?) if we were, say, looking at interannual versus seasonal variability?

Although I'm not recommending this for the current study, it also might be interesting to test this by forcing the ice sheet model with sinusoidal variations in SSH, of different periodicities and amplitudes (e.g. that roughly approximate the seasonal cycle, ENSO, IPO, etc.), and see if a relationship does come out of it. This could be helpful in drawing implications for the impact of different modes of forcing compared with climate change-induced speed-up.

Minor comments

- L13-15: Sentence starting with "Ice shelf response…" could be made a bit more clear – it seemed to me to say the same thing twice – or even deleted.
- L14: provide → provides
- L24-end: Perhaps one more idea could be added here, which is how might we expect ice shelf velocity to respond with further percentage increases in SSH? Do we expect increases in SSH anomalies as the sea level rises?
- L42: "a few longer GNSS records and satellite-based estimates…show variability…" Would be good to include a citation here.
- L43-48: do you mean here that we could essentially scale-up what happens over the seasonal cycle to longer modes of climate variability? If so, it'd be good to explain this in a bit more detail.
- L62: "." → ","
- L68-69: "several GNSS records" → would be good to see citation
- L68: missing)
- L70: seasonal, annual, and intra-annual are used in different parts of the manuscript. Do these all refer to the same periodicity? Would be good to clarify.
- L84: suggest deleting "long" and just clarifying the time span
- Figure 1: would be great to see the domain of the ice sheet model. Would it be possible to include it on this figure?
- All figures: it would be great if the fontsize of all the text in the figures was at least the same size as that of the manuscript text. I found it a bit difficult to read some of the labels in the figures.
- L136: ", using" → ". We used" (break up long sentence)
- L137: "to determine the most realistic". What metric did you use to determine what is the most realistic?
- L148: modeling framework. I understand that the model setup and initialization is discussed at length in Klein et al. (2020). However, it would be very helpful to see a few more details here, either in the main body or in the supplementary. For example: the area that comprises the model domain, initialization datasets (ice surface elevation, topography), what grounding line is used, flow law and basal friction law (and example outputs of these fields).
- L149-150: I don't think I fully understood how the SSH variability is used to force the model. The SSH time series (over the full RIS spatial domain) is derived from the Tinto et al. (2019) model output, but which particular timestamps, and how many of them, are used to force the ice sheet model? Also, are these SSH anomalies applied as an increase/decrease in the ice shelf elevation (surface and draft)? A short description of that in the SI would be helpful.
- L153-154: It'd be good to include a discussion on the implications of using the SSA model here. As discussed in Rosier and Gudmundsson (2016), full Stokes is necessary to capture the flexural stresses associated with tidal motion, which are similar to the stresses induced in this model from seasonal SSH variability. Do you think this could have a significant impact on the results, including the extent of grounding line migration?
- L177: increase in driving → decrease in driving
- L178: an ice flow slowdown → ice deceleration (here and elsewhere – just a suggestion!)
- L179: an acceleration of the ice flow → ice acceleration (here and elsewhere – just a suggestion!)
- L207-208: some more info on these 100 inversions would be great. What parameter values did you vary? How did you choose how they should vary?
- L221: products → product

- L245: “shows a similar range of variability” to the DRRIS station?
- L261: is the underline on range a typo?
- L264: what filter did you use? Would be good to have a brief description here.
- Figure 5: Interested to hear your interpretation of the DRS data. How much does it impact the RIS signal? I imagine there’s a much heavier weighting for the agreement between the RIS and OCS in determining which model is most realistic, but how does the DRS data come into the analysis or decision-making framework?
- L302-303: but in the same region where the absolute changes in SSH are larger? I.e. the yellow-er areas in figure 6c corresponding to the purple-er areas in figure 6b?
- L310: does “regionally-averaged” here refer to the whole ice shelf? Also, do certain regions have more weighting in this regional average, and if so which regions (presumably close to the ice-ocean front?)
- Figure 6: This is a small detail, but there’s a line of bulls-eye like points across the RIS that is particularly evident in figure 6a and I’ve highlighted below from figure S4a (green). Is this something to do with the baseline surface elevation used in the model or something dynamical?



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- Figure 7: Can we know from the simulations about how changes in which region of the ice shelf have the greatest impact on the ice surface speed? It’d be possible to test this with new simulations. E.g. if there was a series of experiments where the driving stress and grounding line migration processes were isolated to see how they each impact the ice surface speeds, and if so by what magnitude, for different regions of the shelf. These are probably outside the scope of the current study, but I can imagine it might be helpful to know that kind of information so that we can target specific regions for longer-term monitoring (e.g. deploy GNSS).

  Also, these speed anomalies are very small! How much should we care about these changes? Or how much would SSH have to vary before we cared? I can imagine small changes could be particularly important for some ice streams where migrations of the grounding line, even minor ones, could lead to more marked retreat. Is that the case for many of the ice streams that feed the RIS?

- L433: Do you have any thoughts on why the modeled range in ΔU is much less than the estimate from satellite altimetry?
- Figure 10: I found it a bit hard to tell the difference between some of the colors in this panel, particularly the blues. Would it be possible to use a greater contrast in the colors used? Another option would be to increase the thickness of the lines to minimize the white space and better distinguish between the different hues.
- L463-464: is there a relationship between the magnitude of the variance and particular features of the friction coefficients from the different inversions? How much do the different friction coefficients vary in the critical regions (e.g. near the grounding line of the ice stream?). Also, what values do you give the friction coefficient in regions that were floating during the inversion that become grounded due to grounding line advance?
- L487: decelerate or accelerate the flow by ±10 m a^{-1}? (add the ±)
- L490-492: also, we do not expect that the ice is in hydrostatic equilibrium at the grounding line
- Section 5 Conclusions: overall I found the conclusions long, and could be made a bit more concise by removing some of the summary of the results that is a repetition from the results section (e.g. sentence over line 597-600), and instead a stronger focus could be made on the implications
- L618-632: As per main comment, it would be great to hear more here about how we can use the findings of this study to better understand the differences between the ice flow response to variability and climate change. Also, how much do you think it matters that we capture the impact of the seasonal cycle of SSH on ice shelves? How much uncertainty does its neglect introduce into model simulations, e.g. of future sea level rise? With increasing SSH, do we expect to see an increase in the magnitude of the SSH changes, and hence an increase in the significance of the processes examined in this study?
- L653: how did you choose appropriate ranges for the regularization parameters?
- L683: “are fairly small” → would be helpful to add an order of magnitude?
- Appendix B2. Subgrid-scale parameterization: I was a bit confused by this description, although I’m not very familiar with subgrid-scale parameterizations! It would be helpful for all the terms to be labeled. For example, what does the i subscript, and the Δx refer to?
- L734: fowline vue → flowline view?
- L954: the word Filchner-Ronne has some extra unintended characters in there! This occurs elsewhere in the references when special characters are being used, including letters with accents, and the years are missing from the references.

New reference

Rosier, S. H. R. and Gudmundsson, G. H. (2015): Tidal controls on the flow of ice streams, Geophysical Research Letters, 43 (9), 4433-4440, https://doi.org/10.1002/2016GL068220