Response to the third revision of “Tidal Modulation of Antarctic Ice Shelf Melting” by Richter et al.

We thank the editor and reviewer for their remarks. Reviewer and editor words are in blue text (including manuscript excerpts with highlights), while our response is in black. Major comments have been addressed below. Typographical slips and technical remarks we agree with have been included in the updated manuscript without re-stating them here.

Response to Review #1

My only significant remark is that I'm unsure how much Appendix C adds to the manuscript, but I don't feel strongly either way and leave it as an editorial decision.

-> Editor: Appendix C describes a quite nuanced choice of the reference state, which might be different in other studies. Since there are no issues with limited space, I suggest leaving Appendix C as it is after incorporating slight modifications suggested by the Referee.

We agree with the editor and have incorporated all modifications. We note that the reviewer questioned our recommendation for future work and we have decided to remove it (L346-351):

“*However, the other cases might be more useful in other studies. For example, when developing a tidal-melt parameterisation that is applied to non-tidal models (as done by Jourdain et al. 2019), the non-tidal case as the reference state might be the most straightforward approach. Similarly, to understand the effects of excluding tides from a model, choosing the tidal state as reference seems logical.* ~~*We encourage future studies to pick up on these findings and lead a comprehensive discussion about the limitations of Perturbation experiments using models of highly non-linear systems. For example, the realism of the Non-linear influence of tidal parameterisations may be assessed with a similar perturbation approach.”*~~

Pdf annotations:

L102 f.: “*Annual average and decomposition results were derived from the final year of the 4 km simulations, while mean tidal current speed was based on an additional subsequent 30-day integration (January) of the tidal case.*”

I don't understand why this was needed. Couldn't the mean tidal current speed be calculated from the last year of the 4 km tidal run, or even the last month of the last year. Why was an additional month needed? No problem, of course, but it makes me think I've missed something. Was it just a convenience, for some reason? On the same subject, in your response to Reviewer 2 you mention that you looked at the mean tidal speed from July and that this showed little seasonal variation (cf January). To a reader, it seems odd that you have done the year of tidal run, but need an extra month for the tidal speeds. Is it because of

the large size of the u,v dataset? Without this information a reader will assume that it was the year of data that showed a lack of seasonal signal. None of this is a problem, except that the reader will wonder if they have misunderstood what has been done.

We found it more efficient to simulate 4 years with monthly output and then add a month with hourly output compared to changing the output frequency during the long simulation. Our choice here has no influence on the results, as the model is close to equilibrium and tidal currents in year 4 will be very similar to tidal currents in the subsequent month. As we use a repeat-year forcing, tidal current speed is also derived for 2007 (January and July). We agree that this detail might cause confusion, but including it seems necessary to be clear. We have added a clarification.

We have changed the text to:
"*Annual average and decomposition results were derived from the final year of the 4 km simulations from a relatively low frequency output (monthly). Mean tidal current speed was based on an additional subsequent high frequency output (hourly) of a 30-day integration (January) of the tidal case. The different output frequency was used to make the most efficient use of available storage on the supercomputer.*"

Eqn. 6 L133 ff.:

$$w_{b,T} - w_{b,NT} \propto$$

$$(u^*_m + \Delta u^*/2)(T^*_m + \Delta T^*/2) - (u^*_m - \Delta u^*/2)(T^*_m - \Delta T^*/2) =$$

$$\overline{u^*_m \Delta T^*} \qquad \text{(thermodynamical)}$$

$$+ \overline{T^*_m \Delta u^*} \qquad \text{(dynamical).}$$

Would it be pedantic to suggest that the left hand side should also be the temporal average? Similar in Appendix C.

The reviewer is correct. These are all temporal averages. We have corrected this typographical mistake in Eqn 6 and Appendix C.

L194 f.: *"We have decomposed the mean impact of tides on ice shelf basal melting into dynamical, thermodynamical ~~and covariational parts~~ (see Sec. 2.3)."*

Left over from previous version?

Yes, we have corrected this and again checked for other mistakes of this kind.

L197 f.:*"In most regions, dynamical effects have a major positive contribution to melting or refreezing. We associated these changes with tidal currents, as these regions also feature elevated tidal current speed (Fig. 3b). Tidal current induced friction, for example […]."*

But what else could they be associated with? I thought that if this was a "dynamic effect" then it had to be a result of tidal currents. Can this sentence be deleted?

The reviewer is right. This is a leftover from a previous version of the manuscript, when residual flow had not been ruled out as contributor to dynamically-induced melting at this point in the manuscript.

We have removed this sentence.

L198: *"Tidal current induced friction, for example, increases melting at shallow grounding zones of cold water ice shelves, in agreement with earlier arguments around the ice pump amplification (e.g. under Filchner-Ronne Ice Shelf, along the Siple Coast under Ross Ice Shelf, under Larsen C Ice Shelf, Fig. 5b, e and h, and under the Amery Ice Shelf, Fig. B1b)."*

The use of the word "friction" seems strange, since the authors are referring to the results of model in which the melting is parameterized. I agree that the parameterization assumes that vertical current shear is driving the melting by enhancing turbulence and therefore increasing vertical diffusivity of heat and salt. But the model isn't modeling the shear and friction. I think it should just be "Tidal currents, for example, increase..."

We agree with the reviewer and have adapted their suggestion.

L285 f.: "*In the light of these limitations, this study should be seen as a first large scale investigation into a process potentially important for sea level rise and global climate. We encourage research groups using other pan-Antarctic ocean-ice shelf applications to implement tides and repeat the experiments of this study."*

Unsure if this is required. I would delete it, but I leave it to the authors.

We agree that the previous sentence already includes this information. We have deleted the sentence in question.

L292:"*Tidal mixing is sensitive to the choice of the vertical mixing parameterisation in ROMS and, while the configuration used by WAOM v1.0 has been established in several regional studies (Galton-Fenzi et al., 2012; Cougnon et al., 2013; Gwyther et al., 2014), there is evidence that the applied mixing scheme (KPP) overestimates tidal vertical mixing (Robertson and Dong, 2019; Robertson, 2006)."*

Does this mean "validated"? Or just "used"? A bit ambiguous at the moment

(Galton-Fenzi 2009) offers some evaluation for the chosen mixing scheme (see p. 111). However, we feel that the word "*used*" is more appropriate than *"validated*" here. We have corrected this and included the new reference. The text has been changed to:

"*Tidal mixing is sensitive to the choice of the vertical mixing parameterisation in ROMS. Our configuration has been tuned by Galton-Fenzi (2009) and used in several regional studies (Galton-Fenzi et al., 2012; Cougnon et al., 2013; Gwyther et al., 2014). However, there is evidence that the applied mixing scheme (KPP) overestimates tidal vertical mixing (Robertson and Dong, 2019; Robertson, 2006)."*

Response to Review #2

L53: *"We derive estimates of ice shelf-ocean interaction using the Whole Antarctic Ocean Model (WAOM) at 4 km horizontal resolution (Richter et al., in press)"*

The authors will likely remember this anyway, but the reference to Richter et al. should be updated to reflect that the study has now been published.

We have updated the reference.

L158: "*Melt rate changes at model resolution (4km) are larger, with a standard deviation of 352 % (not shown).*"

I do not understand this sentence - isn't 4km the resolution of the model used in this study? Please clarify.

We structured this paragraph by spatial scale. First we talk about how tides impact the total Antarctic mass balance, then ice shelf averages, and finally differences at kilometre scale. The information in parenthesis was intended to remind the reader that our model resolution is 4 km (the smallest possible scale to analyse). We acknowledge that the sentence might cause confusion and we have changed the text to be more explicit to:

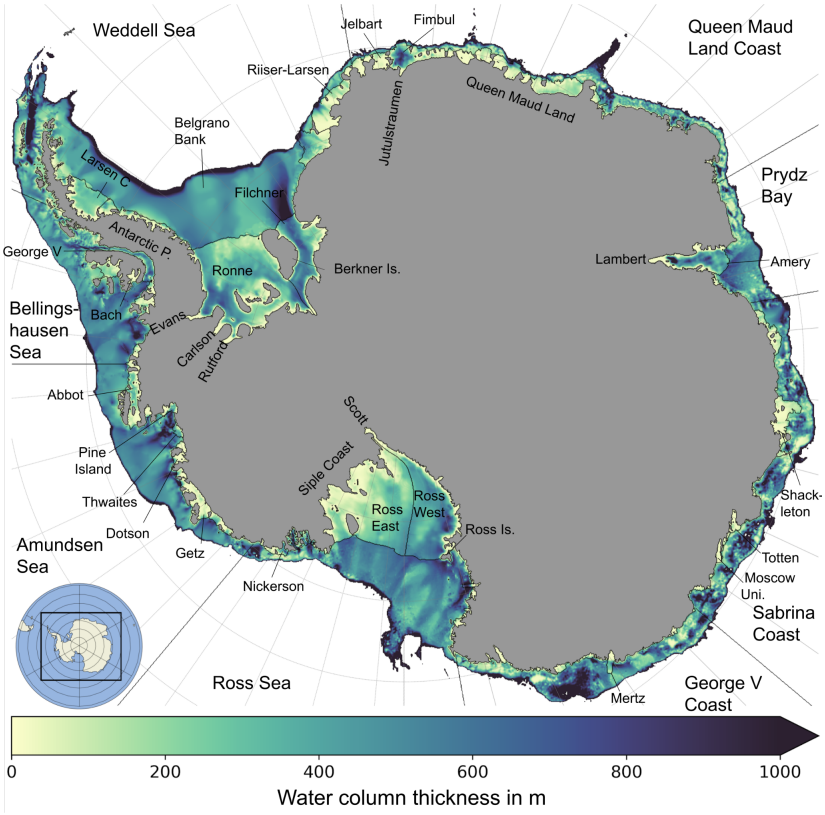
"Melt rate differences within ice shelves are larger. The standard deviation at model resolution, for example, is 352 % (not shown)."

Response to editor remarks

Fig 1. [western] East Antarctica

is it common to refer this way? should be just East Antarctica?

We agree that *western East Antarctica* is not very common. *East Antarctica,* however, is often used to refer to everything to the right of the Transantarctic Mountains (right with reference to Fig. 1). We believe that *Queen Maud Land Coast* is most appropriate and have changed the label in Fig. 1 and all occurrences in the text accordingly.



Line 195: *" Figure 5 and 6, respectively, show the results of this decomposition for some key regions, organised into cold and warm regimes."*

I am not sure it is obvious what is meant by "cold and warm regimes". Perhaps, some definition or clarification would be helpful.

We agree and have added a definition. The new text reads:

"*Figure 5 and 6, respectively, show the results of this decomposition for some key regions, organised into regimes with and without signs of relatively warm circumpolar deep water (CDW) intrusions (hereafter referred to as warm and cold regimes)*."

Lines 304, 308

Since this is a summary, for a non-specialist I suggest avoiding acronyms and writing in full "TBL" and "AABW".

We have now spelt out all abbreviations in the summary. As AABW only appears one more time in the manuscript, we don't introduce this abbreviation at all.

References

Galton-Fenzi, B. K. 2009. “Modelling Ice-Shelf/Ocean Interaction.” Phd, University of Tasmania. https://eprints.utas.edu.au/19882/.