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Interactive comment on "Tidal Modulation of Antarctic Ice Shelf Melting" *by* Ole Richter et al.

Anonymous Referee #1

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General comments

The study presented in this manuscript aims to determine the impact of tidal forcing on the basal meltrates of Antarctic ice shelves, in a circum-Antarctic context. To do this the authors use an application of the ROMS modelling system, at 4-km resolution. This is the Whole Antarctic Ocean Model (WAOM), initial evaluation of a 2-km resolution version of which is in review in Geoscientific Model Development. The manuscript is broadly split into two parts, one to evaluate the effect of tides on basal meltrates, and the other to see how the tides effect that impact: the effect of tidal residual flow; changes in thermal driving at the ice base as a result various aspects of tides (mixing, residual currents etc; or changes in the mixing energy available to transfer heat and salt to the ice base.

The principal results from this study seem to be that tides are important to basal meltrates regionally, although their circum-Antarctic integrated impact is low (resulting in a 4% increase); changes to meltrates induced by tides can have a significant effect on the shelf seas and ice shelf-melt rates downstream; and the authors state that they found that “for large parts of the cold water ice shelves” tidally-induced changes in thermal driving, rather than energy from tidal currents for mixing, dominated the change in meltrates.

The rationale for the study appears to be to identify what will be missing from those model studies that do not include tides – how important the omission will be as far as the reliability of their results is concerned. This seems to me to be a useful exercise: if we know what we will be missing by not going to the computational expense of including tides, then at least we have some feeling for when it is important to include them.

Putting together a circum-Antarctic model, and running the versions with and without tides at 4-km resolution is an impressive effort. However, I do have some concerns about some of the details of the way the results of the runs have been analysed and interpreted. Before advising that the manuscript be accepted for publication I would want to be satisfied that those concerns can be met either by explaining where I’ve got my understanding wrong, or else dealt with through suitable revisions.

Specific comments

The manuscript rests heavily on another manuscript, Richter et al (2020), which is presently under review. That leads to some difficulties. It means that the reader of the present study has to read that other manuscript, and decide on whether that work is acceptable before they can be comfortable with any study that builds on it. So on the face of it, this submission is a little premature.

My main concern is in the attempts to separate out the importance of the tidal contributions in u^* and T^* to the product u^*T^*, which is used to represent the melt rate. the way the friction velocity, u^*, has been used to identify the strength of tide-induced velocities. The way the authors identify the tidal contribution in the in the friction velocity, u^*, is to look at the strength of variability in u* at sub-daily timescales by using SSA essentially as a high-pass filter.

There are three points to make. One minor one is that I don't understand why SSA was used. Surely a traditional, time-domain high pass filter would have been fine, with a suitable cut-off frequency.

The second minor point is that, formally, looking at variability at timescales longer than a day would mean that much of the diurnal tidal variation wouldn't be captured. O1, for example, has a period a little longer than one day. So the authors should make clear in the text that they captured all the tidal variability (assuming they did).

The bigger concern is that, as u* in the study is presented as a scaler, its variability will not capture the tidal contribution unless the dominant tidal ellipses are flat. In much of the central Ronne Ice Shelf, for example, both diurnal and semidiurnal constituents have ellipses that are close to circular. That area is one notable in Figure 5c as being where there is very little contribution of tides to u* variance. That doesn't mean that u* isn't strongly influenced by tides, it's just that the magnitude of the tidal currents is not varying strongly. (Interestingly, a paper from 1990 by ScheduiKat and Olbers uses a 3-layer, 1-D model set up where, for circular ellipses, there is much weaker vertical transport of heat, but I assume that the effect they modelled is not relevant here.) The authors should consider a different proxy to indicate the contribution made by shear due to tidal velocity to basal melt rates. It's likely that there will be little change in the overall picture, but what is written at the moment doesn't seem correct.

Continuing this theme, around line 208 (and elsewhere), the suggestion is made that, in many areas, the direct contribution to melting by tidal velocities (via tidal variability in u*, in this instance) does not dominate u*T*, but that it contributes indirectly, via T* as a result of mixing heat up through the water column. The important point here being that attempts in non-tide resolving models to account for tidal activity by adjusting boundary layer diffusion coefficients will in some areas be incomplete. That seems like a strange interpretation of Figures 5c and 5d, even bearing in mind the need to correct the proxy plotted in 5c. From those figures, there appear to be some areas where tidal T* plays a role, but in those areas tidal u* is also significant. There are very few areas in 5c where tidal u* doesn't make a large contribution. Most of those are under the Ronne, some of which will disappear with a correction to the proxy, and the remainder of which (far west) have very little contribution from tidal T* in any case.

Around line 155 there is a discussion about the way the location of the semidiurnal amphidromic point along Ronne Ice Front affects the position of a tidally-induced gyre over the continental shelf. Although the semi-diurnal amphidromes are centred about half way along Ronne Ice Front, there is no direct relation to the position of the gyre that forms when tides are activated.

Makinson and Nicholls (1999) applies a barotropic tidal model and found a similar, though weaker gyre over the Weddell Sea shelf that was due entirely to tidal residuals. They showed that the residual currents are a result of interactions between tides and the topography. It would be nice to have a comment about why that barotropic model resulted in a gyre so much weaker than the one found here, whether it is a result of it being only a barotropic model, for example.

The authors use the (very strong) gyre to explain the relatively high temperatures over the Ronne continental shelf, which then drive strong melting on the western side of the ice shelf. Referring to the Richter et al (2020) manuscript, those water temperatures are indeed very high in the 2 km resolution model run, very much higher than any that have been measured. Although absolute temperatures are not shown in this manuscript, this does highlight the problem of not having a fully reviewed manuscript on which to base the present work. In particular, it raises a question about the reasonableness of this very strong, tidally induced gyre, if that is indeed the cause of the modelled excessively high temperatures over the shelf.

There will always be some degree of duplication between “summary and conclusions” and the preceding sections, but this reviewer found the duplication to be excessive: I felt I was re-reading the same text. The "Summary and Conclusions" need to be thinned-down dramatically: just a review of the main points.

Technical corrections

The English needs improvement. I have attached a PDF with a large number of suggestions for re-phrasing, comments containing questions where I did not understand the authors' arguments, typographical corrections, and additional comments on content (although the principal concerns have been mentioned above).

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-169/tc-2020-169-RC1-supplement.pdf

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