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

Anonymous Referee #2

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This study examines the role of tides in setting the melt rates of Antarctica's ice shelves. The authors use an ocean model that includes the entire Antarctic continent, and compare the melt rates in simulations with and without tidal forcing. They relate the changes in melt rate at sub-monthly time scales to tidally-induced fluctuations in the friction velocity u* and the thermal driving T*, which jointly determine the basal melt rate, using singular spectrum analysis (SSA).

The authors find that tides change the melt rates of various Antarctic ice shelves non-negligibly, with the northwestern Ronne ice shelf being particularly sensitive to the inclusion of tides. The changes in basal melt rate are accompanied by changes in the depth-averaged potential temperature of the continental shelf, which the authors attribute to tidally-driven onshore heat transport (where the shelf warms) and downstream spread of increased meltwater input (where the shelf cools). The tides additionally increase the strength of the barotropic circulation on the continental shelf due,

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Discussion paper

which is partially responsible for the change in basal melt rate and continental shelf potential temperature. The SSA shows that stress velocity fluctuations are primarily tidal, whereas thermal driving fluctuations are typically dominated by longer time scales, and interpret the time series of u* and T* at selected locations in terms of the tidal velocities, tidally-driven mixing, and temporal variability in cavity inflows and meltwater plumes.

My overall assessment is that this study adds constructively to the existing body of scientific literature on the role of tides in governing Antarctic ice shelf melt. The novelty of the study derives primarily from its inclusion of the entire Antarctic continent at relatively high resolution, allowing a comprehensive evaluation of the role of tides to be conducted. The manuscript is certainly worthy of publication in The Cryosphere following suitable revisions. That said, I have many comments and questions on the manuscript (see below). My most major concerns are as follows:

1. The manuscript is rather light on exposition of the model setup. While I appreciate that the authors have submitted a separate model definition paper, the present study should be as self-contained as possible. I have specifically suggested that plots of the surface forcing, and in particular of the state (and perhaps circulation) of the continental shelf would substantially improve the manuscript. Such plots would allow readers to compare the simulated ocean state more directly against previous observations and model simulations, and thus judge the fitness of this model for estimating ice shelf basal melt rates.

2. While I found the SSA to be one of the most interesting parts of the paper, it is important to note that fluctuations in u* and T* do not separately have a straightforward mapping onto tidally-induced melt rates (neither instantaneously nor in the time-mean). This analysis could be more strongly linked to the rest of the paper by connecting the T* and u* fluctuations more directly to the tidally-induced melt rates, and I have included some specific suggestions in this direction below.

3. The authors' explanations for the warming/cooling signals on the continental shelf

and the causes of T* and u* variations on different time scales are plausible, but are only qualitatively inferred from the plotted maps of continental shelf temperature anomalies and u*/T* variances, respectively. I think the authors could be clearer throughout the manuscript, but particularly in these sections, in distinguishing quantitatively demonstrated findings from their own inferences/speculations.

4. There was substantial overlap in the material in sections 4 and 5, and I struggled to distinguish the purposes of these sections in general. I recommend they be combined or the material re-partitioned to clearly distinguish their content.

I expect that these comments will require major revisions of the manuscript to address fully.

Comments/questions:

L69: Please include citations for the the Mellor-Ezer-Oey algorithm and Haney factor.

L78-80: Is this shown in the companion paper that describes the development of the model?

Fig. 1: It would be helpful to show the full model domain, in addition to the study area. I appreciate that this is described in more detail in a companion paper, but the present study should be as self-contained as possible.

L85-86: At what frequency are the open boundary condition data prescribed?

L86-93: The surface fluxes of momentum, heat and salt (particularly heat) are likely to be influencing the simulated distributions of melt rates, but are not shown in any of the figures. It would be useful to see even an annual mean (or, even better, a seasonal mean) of these surface fluxes for the purpose of previous model and observational estimates, and to aid interpretation of the simulated melt rates.

Additionally, I understand that there is no dynamic sea ice in this model simulation. This is a significant caveat of the model that I think should be highlighted more clearly at this stage in the manuscript.

L95-96: Have the authors checked that the model has, in fact, equilibrated? Time series of e.g. total Antarctic ice shelf melt rates and mean cavity salinity/temperature would help to demonstrate this.

Eqn. (1): Here the authors define the tidal current speed using the time-mean barotropic flow speed. Does this not consistently overestimate the tidal current speed? At the very least I would expect the authors to subtract the time-mean velocity from the barotropic flow before computing u_tide. They could do even better by decomposing the barotropic velocity into tidal components.

Eqn. (2): Please define w_b.

L123-124: Please define the continental shelf potential temperature listed in Table 1. I think I understand what the authors are doing, but their description is very brief, and certainly not sufficient to reproduce their result.

Additionally, similar to my comment above about surface fluxes, it would be useful to see the modeled bottom temperatures and salinities everywhere on the continental shelf. A comparison (even qualitatively) with observations (e.g. Schmidtko et al. 2014) would help readers to judge how accurately the continental shelf properties are being simulated, and thus the fidelity of the modeled melt rates.

Table 1: Comparing these numbers against observational estimates, where possible, would provide a useful reference point.

L128-130: The melt rate difference discussed here and shown in Fig. 2 is somewhat misleading: shifting the locations of melt slightly can produce huge relative differences (where the denominator in the calculation is small). I would suggest computing relative differences using melt rates averaged over each ice shelf separately (i.e. compute average melt rate over each ice shelf with tides, then without tides, and then compute relative difference of the area average). This could still be displayed as a map (with each ice shelf a uniform color), and would reduce the artificially large signals due to slight shifts in melt locations.

L151-152: Is this mechanism of ice shelf frontal melt enhanced by the smoothing of the ice shelf faces that is required to avoid excessive pressure gradient errors? I would expect a sheer ice front to present more of an obstruction to tidal advection of solar-heated surface water than a smooth ice shelf front (even if it is very steep).

L177: “increase” - please specify what is increased.

L163-179: Here the authors discuss reductions in the melt under some ice shelves due to propagation of meltwater anomalies from other ice shelves upstream. I find their interpretation plausible, but the language should be softened here to make it clear that this is an inference: their interpretation is drawn from a qualitative interpretation of figures 2 and 4, rather than a quantitative attribution of the changes in melt rates.

Section 3.2: Here the authors use singular spectrum analysis to decompose variability in T* and u* into different frequency bands. I was not familiar with this technique and had to invest substantial additional time with separate sources to fully understand it. I think it would be helpful for readers to include some additional exposition of the methodology, either here or in the methods section, or even in an appendix.

While this section provides useful insights into tidal driving of the thermal driving and friction velocity, a stronger connection could be made to the resulting melt rates. For example, while Fig. 5 shows the sizes of the u* and T* variances and the fractions of those variances due to <24h period variability, they do not show how large those variances are relative to the time-mean u* and T*. The latter more closely quantifies the relative importance of tides (though my earlier comment about relative difference may apply here too - averages over each ice shelf may be necessary to avoid very large relative differences). One can judge these difference from Fig. 6, but only in a few selected locations.

Furthermore, the amplitudes of u* and T* variances are of less consequence if they are out of phase with one another, i.e. if <u*’T*’> = 0, were u*’=u*-<u*> and T*’ = T*-<T*>. Thus the importance of these fluctuations for the melt rate depends on both their amplitude relative to the means <u*> and <T*> and their phase difference. Some additional plots that convey this information would strengthen the connection between the current frequency band analysis and the diagnosed melt rate changes.

Additionally, I generally found that this section tended to “wander” somewhat between topics, and might be improved by some restructuring to improve the flow.

Fig. 5(a,b): Plotting the logarithm of the variance would help to show more of the range in these plots.

L237: “large” is subjective: a quantification would be preferable here.

L244-245: Again “large” is subjective. Based on the authors’ results, it looks to me like including a tidal velocity in the melt parameterization could do a fair job in many parts of the continent. I would suggest being more specific about what such an approach would miss, and which geographical locations would be most strongly affected.

L260: The lack of sea ice is also a significant caveat that should be discussed here in the context of previous studies that have highlighted the importance of atmosphere-ice-ocean interactions for ice shelf melt (e.g. Silvano et al. 2018).

L272: Missing word at the end of this sentence.

L314-320: I found these bullet points to be too vague, and that the bullet point structure did not convey the information more clearly. I recommend revising as a paragraph with a more specific articulation of the key take-aways from this study.

L323: Citation should not be in parentheses.

L326-327: Again, these relative melt rate differences are a little misleading, and I recommend an area-averaged quantification instead.

L331-332: Is deep water formation sensitive to these changes in the authors' model?

Sections 4-5: I did not find that these sections were very clearly distinguished - each seemed to separately discuss and conclude the paper. The authors should either clearly partition the material, or simply combine these sections and delete redundant material.

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