Response to reviewer remarks on “Tidal Modulation of Antarctic Ice Shelf Melting” by Richter et al.

We thank the editor and reviewers for their remarks. Our response is in blue text. For cross referencing we have labelled each comment. R1C2, for example, refers to Reviewer 1, Comment 2.

Response to Review #1

General comments

R1C1: 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.

We thank the reviewer for this overall positive feedback. We have addressed all concerns below.

Specific comments

R1C2: 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.

The accompanying model development paper submitted to GMD is now in its second round of revisions[1]. All reviewers of the original GMD paper see the development step that WAOM v1.0 represents worthy of publication. All reviewer concerns have been addressed and re-submitted for consideration. Two reviewers have been invited to provide further reviews of the revised manuscript. The review process is open access. Reviewers of this manuscript can inspect the entire process.

R1C3: 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.

We will revisit and substantially expand our analysis in respect to the tidal melt mechanisms following the comments from both reviewers (also see R2C2). How these changes will address each of the specific comments from this reviewer is outlined below.

R1C4: 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.

SSA decomposition is superior to time domain filters, as it allows for precise cut-off frequencies. However, we do acknowledge that we tried to open a walnut with a sledgehammer here. In our case the main purpose of the filter is to effectively remove high frequency variability associated with tidal currents. This criterium has been used before by Stewart et al. (2018) to separate shoreward heat flux driven by tidal currents. Similar to Stewart et al. (2018) we now use a traditional time domain filter and demonstrate that we effectively remove most of the high frequency variability associated with tidal currents in the low pass filtered signals of T* and u*. Figure R1 demonstrates this point for T* in a region with strong tides north of Henry Ice Rise (Ronne Ice Shelf, single grid cell).

[1] https://doi.org/10.5194/gmd-2020-164

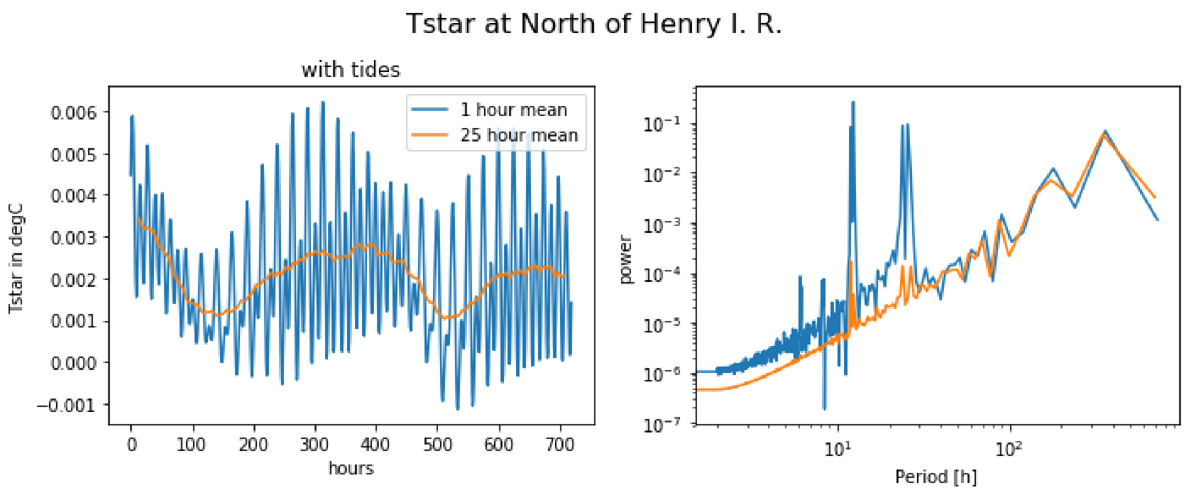

Figure R1: Temporal evolution (left) and power spectral density (right) of T* at 1 h and 25 h resolution for 30 days of a single grid cell north of Henry Ice Rise (Ronne Ice Shelf). The rolling mean effectively removes most of the high frequency variability associated with tides.

R1C5: 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).

This is a fair point. We now have experimented with different cut-off frequencies and found 25 h to be the best compromise between capturing most of the tidal current variability and avoiding contamination from other high frequency phenomena associated with, e.g., eddies and daily surface forcing. We now demonstrate that the filter effectively removes most of the high frequency variability associated with tides (see Fig. R1).

R1C6: 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.

We thank the reviewer for pointing this mistake in our methodology out. We now apply our filter on the two orthogonal surface stress components first and then calculate u* from these filtered components (instead of calculating u* first and then applying the filter). This way we now account for current variability in magnitude and direction and, thus, accurately capture the contribution of tidal velocity to shear.

The suggested reference is very relevant. ScheduiKat and Olbers (1990) suggest that tidal strength variability, rather than tidal strength, governs ice shelf melting driven by tidal vertical mixing. With the corrected filtering approach for u* (filtering vector components) we capture tidal strength, that is the area of the tidal ellipses. To capture tidal strength variability, that is the eccentricity of the tidal ellipses, a different filtering method would need to be applied.

Final attribution of melting to individual tidal mechanisms (such as vertical mixing) is out of the scope of this study. However, we will discuss this issue in the revised manuscript to inspire future work. In detail we will more clearly communicate the scope of our analysis regarding tidal melt mechanisms upfront. We will refer to the evidence by Scheduikat and Olbers (1990) (that tidal strength variability, rather than tidal strength, drives tidal vertical mixing) and point out that our approach does not capture tidal strength variability. Finally, we will suggest that future work aiming to attribute tidal melting to vertical mixing, will need to apply a filtering method that extracts tidal ellipse eccentricity.

R1C7: 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.

The overall aim is to get insights into the governing mechanisms that drive tidal melting. Our approach so far has been to infer insights through the variabilities in u* and T*. However, we acknowledge that there is no direct mapping from spectral analysis to mean melting and, hence, the informative value of this approach is limited. We now have found a more direct way to derive insights, consisting of two parts: dynamical/thermodynamical decomposition and spectral decomposition.

Tidal melting can be decomposed into its dynamical (associated with u*) and thermodynamical (associated with T*) components. Jourdain et al. (2019) defines a direct dynamical/thermodynamical decomposition of tidal melting (their Eqn. 5), which is similar to:

$$\begin{aligned} m_T - m_{NT} &\propto \overline{u^*_T T^*_T} - \overline{u^*_{NT} T^*_{NT}} \\ &= \overline{(u^*_{NT} + \Delta u^*)(T^*_{NT} + \Delta T^*)} - \overline{u^*_{NT} T^*_{NT}} \\ &= \overline{u^*_{NT} \Delta T^*} + \overline{\Delta u^* T^*_{NT}} + \overline{\Delta u^* \Delta T^*} \end{aligned} \quad \text{(R1).}$$

Here *m* denotes mean melt rate, the subscripts T and NT denote tidal and non-tidal, the overbar is temporal averaging and the deltas describe the differences between the tidal and non-tidal run. For u*, e.g., that is:

$$\Delta u^* = u^*_T - u^*_{NT} \quad \text{(R2).}$$

For the revised manuscript, we will apply this analysis to our pan-Antarctic results. This will generate direct estimates of tidal melting due to the dynamical, thermodynamical and covariational terms (similar to Jourdain et al., 2019, their Fig. 6) and, hence, give insights into the driving mechanisms. This would also directly inform about the prospect of applying traditional tidal melt parameterisations that manipulate shear at the ice shelf base alone (the dynamical term) on pan-Antarctic domains.

In addition, a spectral decomposition of the covariation of u*T* will, first, inform about the temporal resolution necessary for the dynamical/thermodynamical decomposition and, second, provide further insights into the underlying mechanisms. We define the spectral decomposition as:

$$\begin{aligned}
m_T - m_{NT} &\propto \overline{u^*_T T^*_T} - \overline{u^*_{NT} T^*_{NT}} \\
&= \overline{(u^*_{T,HP} + u^*_{T,LP})(T^*_{T,HP} + T^*_{T,LP})} - \overline{(u^*_{NT,HP} + u^*_{NT,LP})(T^*_{NT,HP} + T^*_{NT,LP})} \\
&= \overline{u^*_{T,HP} T^*_{T,HP}} - \overline{u^*_{NT,HP} T^*_{NT,HP}} + \overline{u^*_{T,HP} T^*_{T,LP}} - \overline{u^*_{NT,HP} T^*_{NT,LP}} \\
&\quad + \overline{u^*_{T,LP} T^*_{T,HP}} - \overline{u^*_{NT,LP} T^*_{NT,HP}} + \overline{u^*_{T,LP} T^*_{T,LP}} - \overline{u^*_{LT,HP} T^*_{NT,LP}} \\
&= \Delta(u^*_{HP} T^*_{HP}) + \Delta(u^*_{HP} T^*_{LP}) + \Delta(u^*_{LP} T^*_{HP}) + \Delta(u^*_{LP} T^*_{LP})
\end{aligned}$$

(R3).

Here the subscripts HP and LP denote Low-Pass and High-Pass and the Deltas describe the differences between the tidal and non-tidal run. For the purely high frequency component, e.g., this is:

$$\Delta(u^*_{HP} T^*_{HP}) = \overline{u^*_{T,HP} T^*_{T,HP}} - \overline{u^*_{NT,HP} T^*_{NT,HP}} \quad \text{(R4).}$$

If the spectral components that contain high frequency u* and/or T* (periods less than 25 h) contribute little to the mean melt rates of 30 days, the dynamical/thermodynamical decomposition for the entire year can be done with 25 h means. Further, different tidal melt mechanisms act on different time scales. Tidal residual flow, e.g., is expected to only affect melting at timescales slower than 25 h.

Based on these two analysis, we will be able to more directly narrow down on the governing mechanism responsible for the mean melt change in our model. The original approach will be redundant.

R1C8: 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.

This is a good point. We have drawn the connection between the amphidromic point and the gyre too carelessly. The gyre is caused by tide-topography interaction at Belgrano Bank (Makinson and Nicholls 1999), which is more than 200 km away from the amphidromic point (as predicted by different barotropic tide models, see Rosier et al. (2014) their Fig. 1a). We will remove any arguments based on this connection from the revised manuscript.

R1C9: 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.

We thank the reviewer for pointing us towards this reference. The gyre in our model is indeed an order of magnitude stronger than in the simulation by Makinson and Nicholls (1999, from now on called MN99). We agree that what is causing the difference in strength, is an interesting question, in particular, in the light of the strong tidal melting under the North-west Ronne Ice Shelf. We do not know the answer to this question. The following points might play a role:

1. The gyre strength in NM99 is very sensitive to the topography gradient and depth estimates over Belgrano Bank range from 400m to 10m (discussed in Makinson & Nicholls, 1999). Therefore, a differences in bathymetry might cause a stronger gyre in our simulation.
2. NM99 does not include thermohaline or wind driven circulation on the continental shelf, we do. This circulation can interact with tidal residual flow (also stated by Makinson & Nicholls, 1999).
3. NM99 does not include ice shelf interaction. Melt water plumes in our simulation might strengthen the gyre (and, potentially, drive stronger melting in turn).

To investigate this topic, additional experiments are needed, e.g. deactivating ice shelf interaction or a tides only run. A regional configuration will likely be sufficient (e.g. similar to Mueller et al., 2018, but with an extended domain including the continental slope). In the revised manuscript, we will add a discussion around this point and propose future work.

R1C10: 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.

That is a fair point. We now have identified a warm bias in this region (for the 2 km version of the model, see Fig. 6 of the GMD paper revision[2]). We do not know if this bias is related to tides. We are only speculating. In the GMD paper, we attribute the warm bias to missing HSSW formation, possibly caused by overly mixed conditions. Tidal residual flow as well as tidal currents cause mixing due to shear at the bottom and ice draft. Tidal mixing in NM99 is strong where the temperature bias in our simulation is strongest (at the Ronne Ice Shelf front). In NM99, as well as our simulation, tidal current speed at the Ronne Ice Front is an order of magnitude stronger than tidal residual flow. Investigating the impact of tidal mixing on HSSW formation would be a great follow-up study, probably best done with a regional configuration. In the revised manuscript, we will add a discussion about this point and communicate more clearly that we are in the speculative realm here.
We will also note that accurate shelf temperatures are likely to diminish the importance of the gyre for melting.

R1C11: 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.

We will tailor the summary accordingly (details are described later under R2C35).

Technical corrections

R1C12: 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

All of these minor concerns will be addressed in the revised manuscript.

[2] https://gmd.copernicus.org/preprints/gmd-2020-164/gmd-2020-164-AC1-supplement.pdf

Response to Review #2

R2C1: 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 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.

We thank the reviewer for this positive feedback.

R2C2: 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.

The question of evaluation has already been addressed under R1C2. Evaluation of pan-Antarctic continental shelf conditions is a complex issue and we have decided to do this for WAOM v1.0 in a separate paper[3]. We have chosen an interactive discussion journal for the model development paper so that the review process is fully accessible to the reviewers

[3] https://doi.org/10.5194/gmd-2020-164

of this paper. Having said so, we will expand on the model description to enhance the readability.

R2C3: 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.

Under R1C7 we now propose to apply a different analysis that yields a direct decomposition of mean melting into dynamical (u*) and thermodynamical (T*) components. We thank the reviewer for their suggestions, which have helped to formulate this proposition.

R2C4: 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.

We believe the new analysis (described under R1C7) will substantially strengthen some of our arguments in this section. In the revised manuscript we will also draw the line between results and speculations more clearly.

R2C5: 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.

We will edit the text to remove overlap and more carefully partition the text.

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

Comments/questions:

R2C6: L69: Please include citations for the Mellor-Ezer-Oey algorithm and Haney factor.

We will include the respective citations. The revised sentence will be (additions in bold):
"*To minimise pressure gradient errors in WAOM, we smooth the ice draft and bottom topography using the Mellor-Ezer-Oey algorithm* ***(Mellor, Ezer, and Oey 1994)*** *until a maximum Haney factor of 0.3 is reached* ***(Haney 1991)****.*"

R2C7: L78-80: Is this shown in the companion paper that describes the development of the model?

Yes, in Table 2.

R2C8: 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.

Regarding Figure 1, we will include an inset showing the full model domain.
R2C9: L85-86: At what frequency are the open boundary condition data prescribed?

The open boundary conditions are monthly averages. We will include this information in the revised manuscript.

R2C10: 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.

Interpretation of the melt rates as predicted by WAOM v1.0 is subject to a different manuscript[4]. The evaluation of the ocean conditions, including the surface ocean, has been done in the accompanying model development paper[5]. This development paper shows that WAOM v1.0 (with tides) captures the main characteristics of Antarctic ice shelf melting. WAOM v1.0 (with tides) should be seen as the control run for the experiments described in the present manuscript.

R2C11: 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.

Advantages and disadvantages of prescribing surface fluxes instead of running a sea-ice model has been discussed in the revision of the model development paper[6] (see R1C12 and R3C20). Prescribing surface buoyancy fluxes has the advantage of accurate surface salt flux location and strength from sea ice polynyas. We will also clarify this point in the revised manuscript of this paper.

R2C12: 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.

Antarctic mean ice shelf melting is a strong proxy for continental shelf conditions and ice shelf melting is the main quantity of interest for this paper. We have shown the equilibration

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[4] 10.31223/osf.io/stcgq
[5] https://doi.org/10.5194/gmd-2020-164
[6] https://gmd.copernicus.org/preprints/gmd-2020-164/gmd-2020-164-AC1-supplement.pdf

of mean ice shelf melting in the companion paper (see Fig. 2). In the revised manuscript we will highlight this point and include a reference to Figure 2 of the companion paper.

R2C13: 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.

We thank the reviewer for pointing this mistake in our methodology out. We now apply a high pass filter (with a cut-off frequency of 25 h) on the two orthogonal velocity components first and then calculate the magnitude from these filtered components. The revised equation is as follows:

$$|u|_{tide} = \left\langle \sqrt{u_{b,HP}^2 + v_{b,HP}^2} \right\rangle_t \qquad [\mathrm{m\,s^{-1}}]\,, \tag{R6}$$

whereby *u* and *v* are orthogonal velocity components at an hourly resolution and the subscript *b* denotes barotropic and *HP* High-Pass filtered. The temporal average (subscript *t*) is taken over 30 days. This way we capture the velocity magnitude associated with tidal currents alone more accurately. Here is the revised figure:

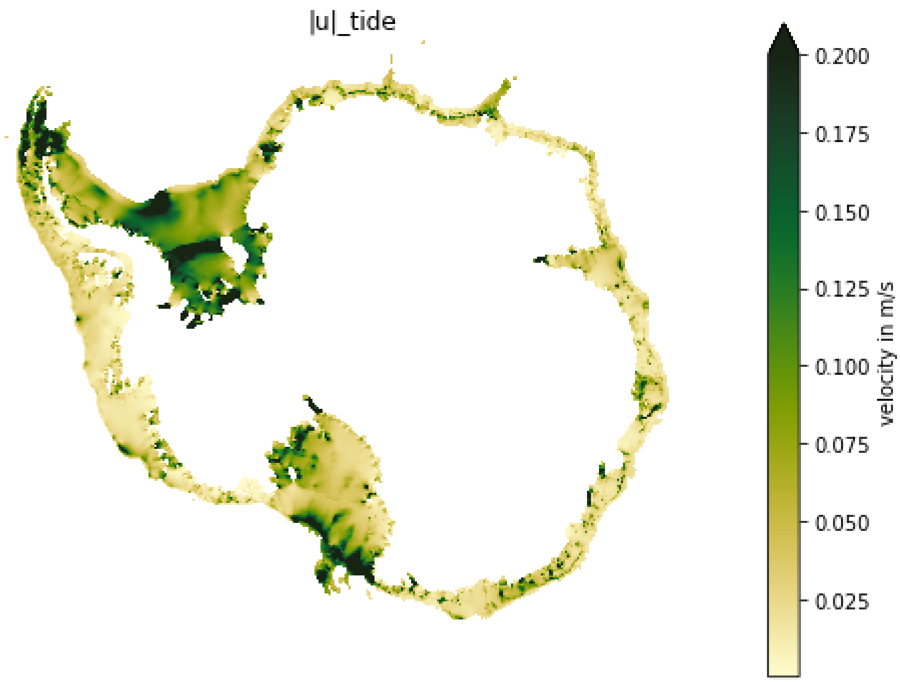

Figure R4: Tidal current speed, calculated following Eqn. (R6).

Figure R5 compares the tidal current speed as calculated from Eqn. R6 with the results from the old methodology. While the new methodology results in quantitative differences comparable to the absolute values it does not change any conclusions drawn from this figure.

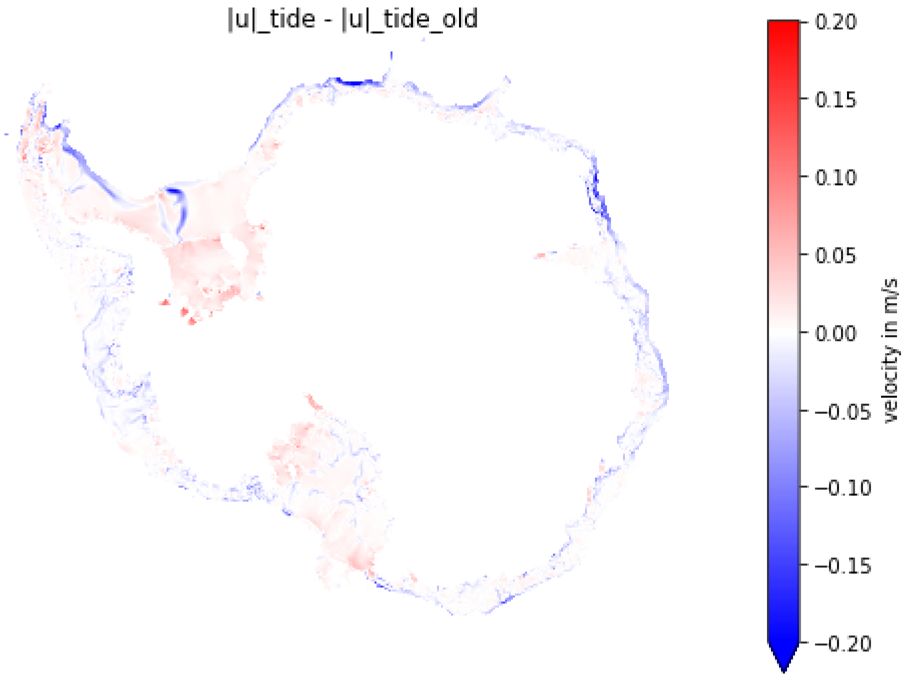

Figure R5: Difference in tidal current speed. |u|_tide has been calculated following Eqn. R6 and |u|_tide_old following Eqn. 1 (see original manuscript, line 102).

We have shown that temporal filtering captures most of the high frequency variability associated with tides (see R1C4). Tidal harmonic analysis would be the most accurate way, but we find it impractical for our large amounts of data. Stewart et al. (2018) also states that

> "*While nonlinear interaction between these phenomena precludes an exact decomposition (e.g., Ardhuin et al., 2017; Rocha et al., 2016), a closer approximation might be achieved using harmonic analysis to isolate the tidal variability (e.g., Foreman & Henry, 1989) and by separating the seasonal cycle from the eddies (e.g., Dufour et al., 2015).* ***These approaches were found to be impractical due to the very large volume of model output****, which is provided as hourly snapshots and occupies several petabytes of storage space; even simply time averaging the circum-Antarctic model output requires the use of hundreds of compute cores for several days, even after the process has been optimized for computational efficiency.*"

For the purpose of estimating tidal current strength (or surface stress at the ice base) temporal filtering is accurate enough.

R2C14: Eqn. (2): Please define w_b.

W_b denotes ice shelf melting. We will define this explicitly in the revised manuscript.

R2C15: 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.

We have calculated the mean potential temperature of the continental shelf from all ocean south of the 1000 m isobath. It is a grid cell-volume weighted average of the entire year. We will provide these details in the revised manuscript.

R2C16: 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.

The evaluation of the ocean conditions has been done in the accompanying model development paper[7]. The revision of the development paper[8] now includes a comparison of the bottom layer temperature and salinity against estimates by Schmidtko et al. (2014, see Fig. 6 and 7 in the revision).

R2C17: Table 1: Comparing these numbers against observational estimates, where possible, would provide a useful reference point.

The evaluation of the ocean conditions is the subject of the accompanying model development paper. There, Antarctic mean ice shelf melting and total basal mass loss has been compared against observational estimates. Mean bottom layer temperatures of individual regions are now also evaluated against observations (see Fig. 7 in the revision of the GMD paper). We are not aware of a meaningful observational estimate of Antarctic-wide potential temperature on the continental shelf, due to the sparsity of observations. Schmidtko et al. (2014), e.g., excludes East Antarctic regions.

R2C18: 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.

We fully agree with the reviewer. Here is the revised figure:

[7] https://doi.org/10.5194/gmd-2020-164
[8] https://gmd.copernicus.org/preprints/gmd-2020-164/gmd-2020-164-AC1-supplement.pdf

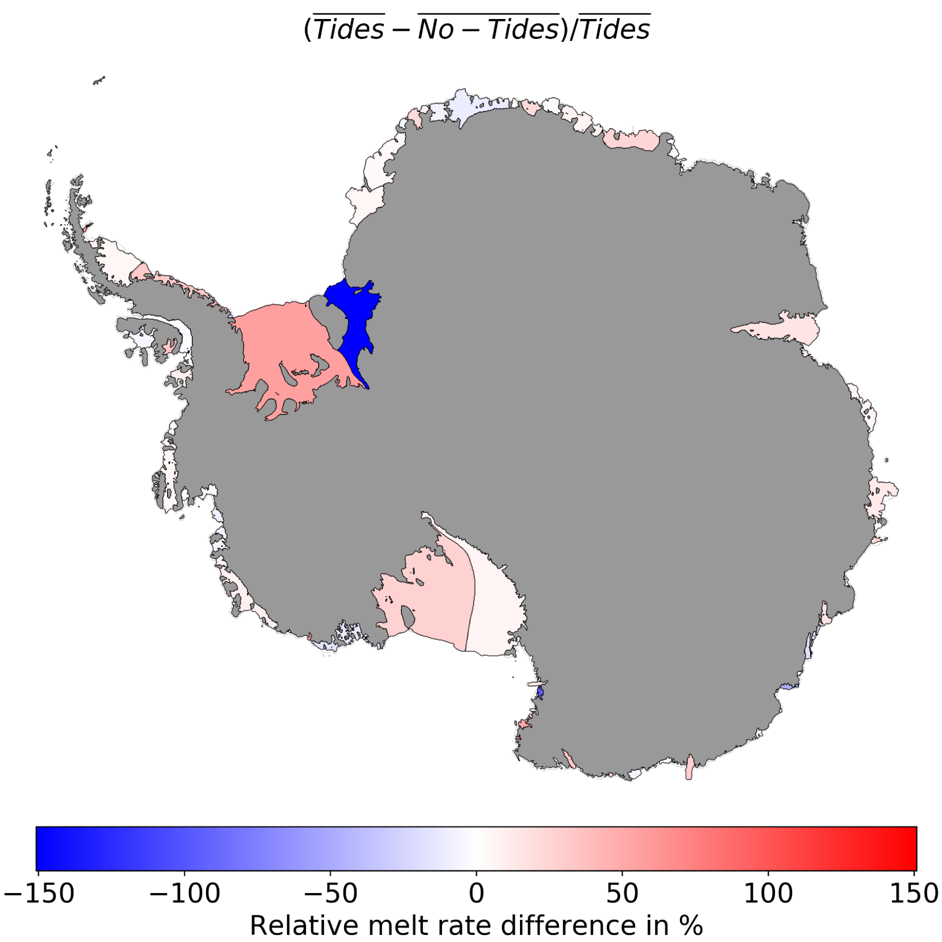

Figure R5: Tidal melting of Antarctic ice shelves presented in relative terms and averaged over each ice shelf.

In the revised manuscript, we will also include a description of how these numbers have been derived (first we computed average melt rate over each ice shelf with tides, then without tides, and then the relative difference of the area average).
The revised figure does not change any conclusions drawn in the text.

R2C19: 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).

This is a good point, which has been discussed in detail in the GMD revisions[9] (see R1C7 of the GMD revision). As frontal melting is mostly independent from tides in our simulation we will not include this discussion again here.

R2C20: L177: “increase” - please specify what is increased.

[9] https://gmd.copernicus.org/preprints/gmd-2020-164/gmd-2020-164-AC1-supplement.pdf

The **strength** of the Antarctic Slope Current is increased. We will include this in the revised manuscript.

R2C21: 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.

We fully agree with the reviewer and will change the language accordingly.

R2C22: 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.

With our new decomposition approach (see R1C7) SSA has become redundant.

R2C23: 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.

We thank the reviewer for this valuable suggestion. We believe we have found a better method to map tidal changes in u* T* directly to the mean melt rates. This is described under R1C7.

R2C24: Furthermore, the amplitudes of u* and T* variances are of less consequence if they are out of phase with one another, i.e. if = 0, were u*'=u*- and T*' = T*-. Thus the importance of these fluctuations for the melt rate depends on both their amplitude relative to the means 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.

We now account for differences in magnitude and phase by filtering orthogonal vector components (see R1C6). In the revised manuscript, we will include figures that convey this information.

R2C25: Additionally, I generally found that this section tended to “wander” somewhat between topics, and might be improved by some restructuring to improve the flow.

We expect some more structure from the new analysis (R1C7). In the new analysis we decompose melt rates, which has more connection to the first half of the paper. In the revised manuscript, we will draw these connections explicitly.

R2C26: Fig. 5(a,b): Plotting the logarithm of the variance would help to show more of the range in these plots.

Figure 5 will be redundant due to the new analysis (R1C7).

R2C27: L237: “large” is subjective: a quantification would be preferable here.

We will soften this to “large in some regions, that is Ronne Ice Shelf” and provide a quantitative estimate in brackets.

R2C28: 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.

We will be more specific in the revised manuscript and avoid subjective descriptions such as “large”. The proposed decomposition of melting into dynamical and thermodynamical components (described under R1C7) has been used before to inform the prospects of traditional tidal melt parameterisations in the Amundsen-Bellingshausen Seas (Jourdain et al. 2019). With the new analysis we apply the same argument to a pan-Antarctic domain. In addition, the Weddell Sea gyre will not be captured by traditional parameterisations, as it is induced by tide-topography interaction. If melting under North-West Ronne Ice Shelf is indeed driven by this gyre, traditional tidal melt parameterisations will perform poorly in this region. We will discuss these points in detail in the revised manuscript.

R2C29: 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).

This has been discussed above. Please see R1C11.

R2C30: L272: Missing word at the end of this sentence.
Actually, the referencing went wrong. We will correct
“ *[...] of tide-driven shoreward heat transport of (Stewart et al., 2018).*” to
“*of tide-driven shoreward heat transport by Stewart et al. (2018)*.”.

R2C31: 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.

We will revise this paragraph in the revised manuscript, following the reviewer suggestion.

R2C32: L323: Citation should not be in parentheses.
We will revisit all citations for formatting mistakes.

R2C33: L326-327: Again, these relative melt rate differences are a little misleading, and I recommend an area-averaged quantification instead.
Yes, we agree. Please see R2C18.

R2C34: L331-332: Is deep water formation sensitive to these changes in the authors' model?
We do not know. We only speculate here. Investigating dense water formation changes would be an interesting follow-up study.

R2C35: 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.
We will tailor the summary section down and add discussion points from here to the discussion section.

The following points will be removed or tailored:

- The total change in mass loss and continental shelf temperature are main results that will remain in the summary. However, the related increase in conversion efficiency of heat into melting, its comparison to idealized studies and our speculation about changes over glaciation timescales will be moved to the discussion section.
- The point about the tide induced gyre on the Weddell Sea continental shelf and its connection to elevated melt rates under North-West Ronne Ice Shelf will be tailored down to only contain the evidence and robust conclusions. In the discussion we will add more details around this phenomenon.
- Results concerning frontal melting are secondary and will only be presented in the discussion.
- Conclusions regarding tidal melt mechanisms might change due to the new decomposition analysis (see R1C7). We will discuss respective results in detail in the discussion and only reiterate the main conclusion in the summary.

The implications of this study for the prospects of parameterising tidal melting at large scales will remain in the summary.

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