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.

Changes unrelated to reviewer comments

This manuscript also comprises a chapter of the recently accepted PhD Thesis. Here we have implemented some changes resulting from the thesis revision. The modifications are minor, but enhance the readability substantially or clarify important questions. These modifications are listed in the following (T abbreviates Thesis):

T1: We have further specified that the simulation results used to estimate tidal current speed are from one month only (additions in bold):

“Annual average and decomposition results have been derived from the final year of the 4 km simulations, while mean tidal current speed is based on additional subsequent integration for 30 days ***(January)*** *of the tidal case.”*

T2: We have included the definition of friction velocity and thermal driving in the methodology:

“The friction velocity controls the exchange rates of heat and salt through the boundary layer and is calculated using the surface quadratic stress

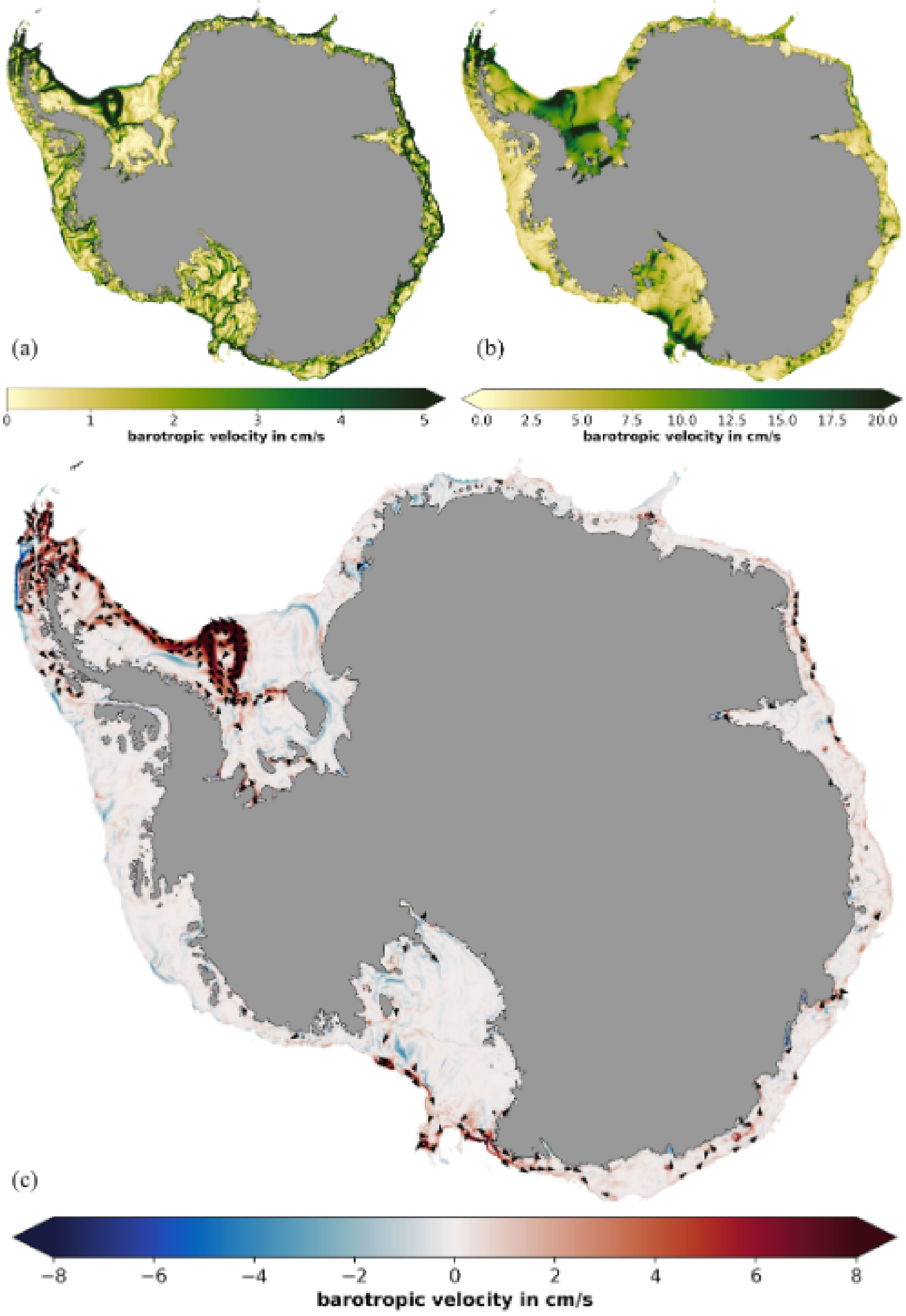
$$u^* = \sqrt{C_d(u_{top}^2 + v_{top}^2)} \qquad [\mathrm{m/s}] \ .$$

Here, Cd is a quadratic drag coefficient and u_top and v_top are the orthogonal velocity components of the uppermost sigma layer. Thermal driving is defined as the difference in temperature across the ice-ocean boundary layer:

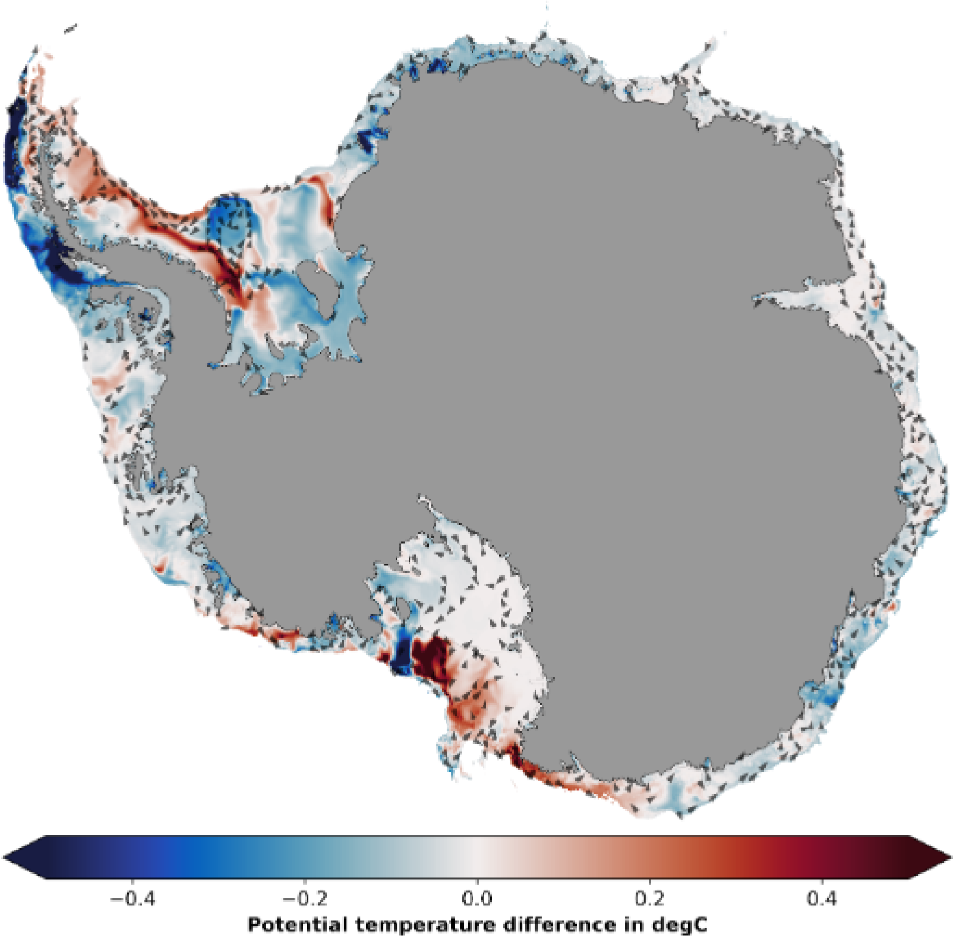
$$T^* = T_M - T_B \qquad [^\circ C] \ ,$$

where T_M is the temperature in the top model cell (approximately 0.3 m to 5.0 m below the ice base; assumed to be in the ‘mixed layer’), while T_B is assumed to be at the insitu freezing point.”

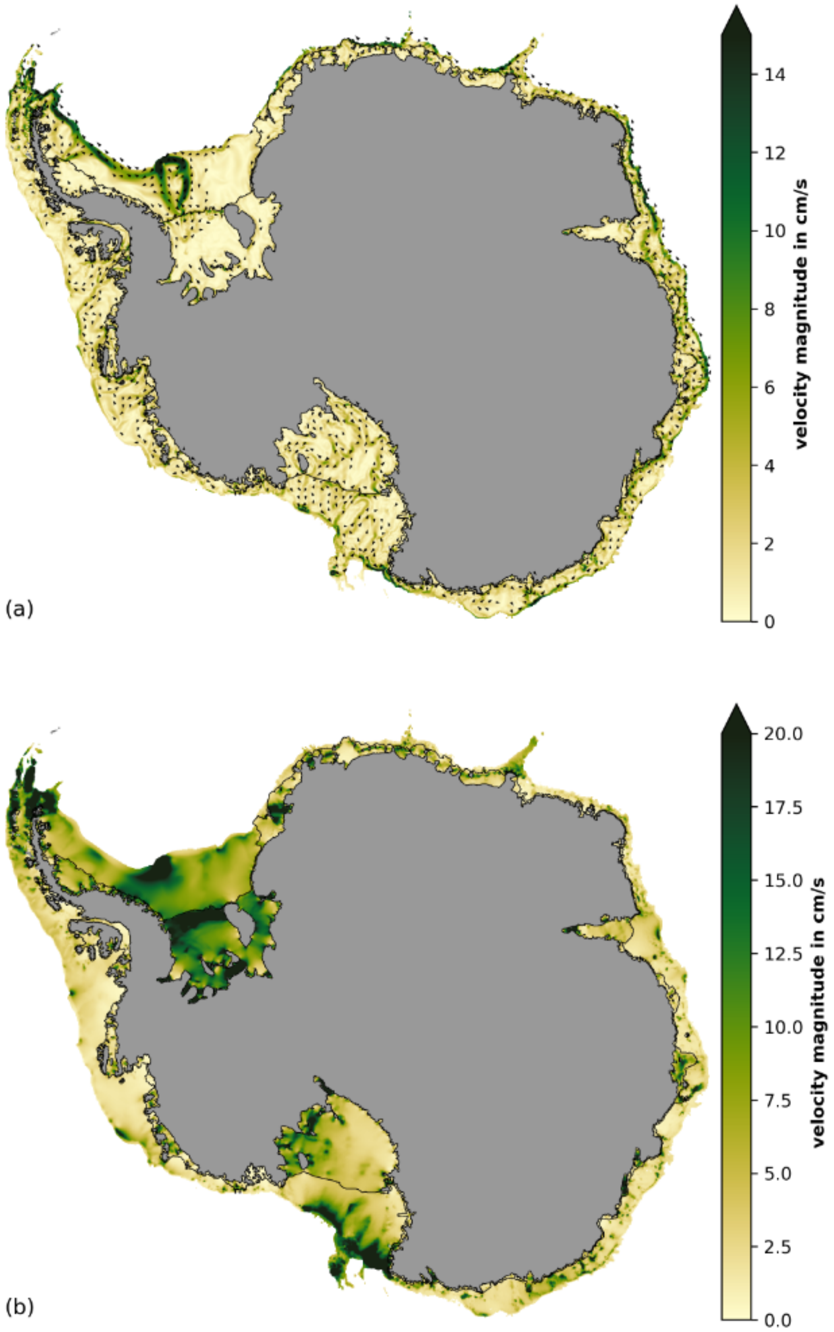
T3: We have restructured the figures presenting velocities and temperatures (old Fig. 3 and old Fig. 4) to now show either exclusively quantities of the reference run (new Fig. 3) or exclusively tide induced changes (new Fig. 4):



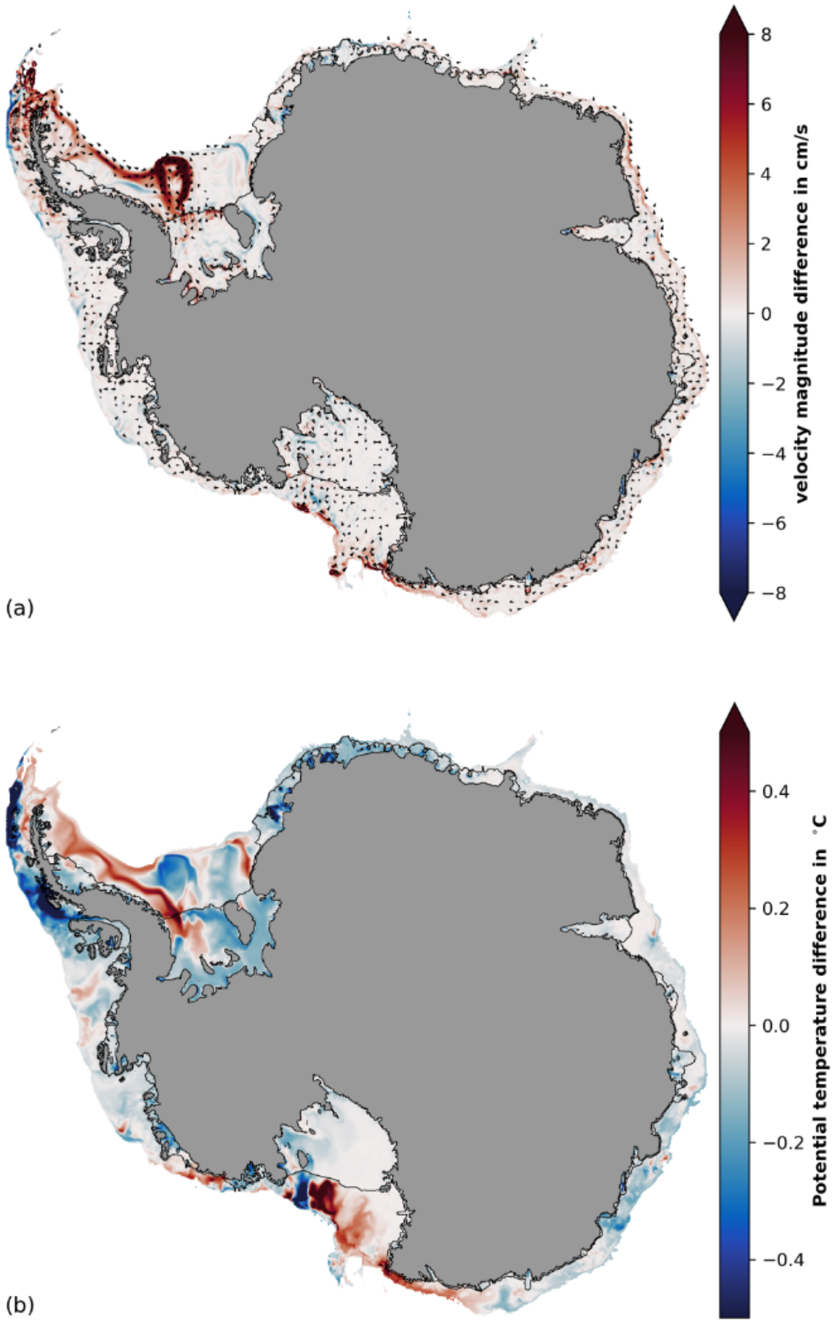
Old Figure 3: Tide induced currents around Antarctica. a) mean barotropic velocities from the simulation with tides, b) mean speed of oscillating tidal currents (|u|_tide, calculated following Eqn. 1; see Section 2.3) and c) the difference of a) to the simulation without tides (...). Arrows in (c) indicate the change in barotropic flow direction when activating tides and are only shown in places where velocity change is greater than 1 cm/s.



Old Figure 4: Changes in continental shelf ocean temperature induced by tides. Difference in depth-average potential temperature when activating versus not activating tides in the model (...). Arrows indicate barotropic flow direction of the run with tides and are shown only where velocities are stronger than 2 cm/s.



New Figure 3: Mean and tidal current speed. a) mean barotropic velocities from the simulation with tides, b) mean speed of oscillating tidal currents (|u|_tide, calculated following Eqn. 1; see Section 2.3). Arrows in (a) indicate flow direction and are shown only where velocities are stronger than 1 cm/s.



New Figure 4: Tide induced change in (a) continental shelf barotropic velocity and (b) potential temperature. Differences show impact when activating tides in the model (Tides - No-Tides). Arrows in (a) are shown only where velocity change is larger than 1 cm/s.

Dynamical-Thermodynamical Decomposition

The overall aim of the second half of the paper 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*. Both reviewers have challenged this approach (see R1C7, R2C3). We acknowledge that there is no direct mapping from spectral analysis to mean melting and, hence, the informative value for this study is limited. We now have found a more direct way to derive insights: Dynamical-thermodynamical decomposition. We have applied this technique to key regions around Antarctica and believe that the new findings strengthen the manuscript substantially. Previous findings regarding spectral techniques have been removed from the manuscript. Related modifications in all sections are outlined coherently in the following. Later, when responding to the individual reviewer comments, we refer back to these changes.

Analysis

Any methodology regarding the SSA analysis (in Sec. 2.3) has been replaced by the following:

“We perform a dynamical-thermodynamical decomposition to explore the mechanisms that govern tidal melting in our simulation (similar to Jourdain et al. 2019). The main characteristics of ice shelf basal melting as derived from the three equation melt parameterisation (w_b) can be approximated using the covariance of friction velocity (u) and thermal driving (T*, see Holland and Jenkins, 1999):*

$$w_b \,[\mathrm{m\ yr}^{-1}] \quad \propto \quad u^* T^* \,[\mathrm{m\ s}^{-1} {}^\circ C] \,. \qquad (2)$$

The friction velocity controls the exchange rates of heat and salt through the boundary layer and is calculated using the surface quadratic stress

$$u^* = \sqrt{C_d(u_{top}^2 + v_{top}^2)} \quad [\mathrm{m/s}] \,. \qquad (3)$$

Here, Cd is a quadratic drag coefficient and u_top and v_top are the orthogonal velocities components of the uppermost sigma layer. Thermal driving is defined as the difference in temperature across the ice-ocean boundary layer:

$$T^* = T_M - T_B \quad [{}^\circ C] \,, \qquad (4)$$

where T_M is the temperature in the top model cell (approximately 0.3 m to 5.0 m below the ice base; assumed to be in the ‘mixed layer’), while T_B is assumed to be at the insitu freezing point.

The approximation of melt rate variability using friction velocity and thermal driving (Eq. 2) allows a decomposition of the melt difference between the tidal and non-tidal experiment into dynamical, thermodynamical and covariational component (equivalent to Jourdain et al. 2019, their Eq. 5):

$$
\begin{aligned}
w_{b,T} - w_{b,NT} &\propto \overline{u^*_T T^*_T} - \overline{u^*_{NT} T^*_{NT}} \\
&= \overline{(u^*_{NT} + \Delta u^*)(T^*_{NT} + \Delta T^*)} - \overline{u^*_{NT} T^*_{NT}} \\
&= \overline{u^*_{NT} \Delta T^*} + \overline{\Delta u^* T^*_{NT}} + \overline{\Delta u^* \Delta T^*} \; .
\end{aligned}
\tag{5}
$$

Here, the overbar denotes temporal averaging and the Delta describes the difference between the tidal (T) and non-tidal run (NT):

$$
\begin{aligned}
\Delta u^* &= u^*_T - u^*_{NT} \\
\Delta T^* &= T^*_T - T^*_{NT} \; .
\end{aligned}
\tag{6}
$$

We have applied this decomposition to key regions around Antarctica using one year of hourly averages. The individual terms offer a priori a good physical interpretation. The dynamical component describes the mean effect of tidal current velocity as well as tidal residual flow (including tide induced buoyant plumes from melting upstream). Thermodynamical changes account for tidal vertical mixing of heat below the turbulent boundary layer (TBL) as well as upstream thermal effects associated with tides (including tidal modulation of meltwater input). The local and instantaneous interplay of thermal forcing and friction velocity via melting is captured by the covariational term.”

Results

The title of Section 3.2 has changed from “Time Series Analysis of Melt Drivers at the Ice Base” to “Dynamical-Thermodynamical Decomposition of Tidal Melting”. Any previous text and figures (Fig. 5 to 7) has been replaced by the following:

“*We have decomposed the mean impact of tides on ice shelf basal melting into dynamical, thermodynamical and covariational parts (see Sec. 2.3). Figure 5 and 6, respectively, show the results of this decomposition for the Filchner-Ronne Ice Shelf System and the Amundsen Sea ice shelves. The results for other key regions are presented in the supplemental material as Figures B1 to B7.*

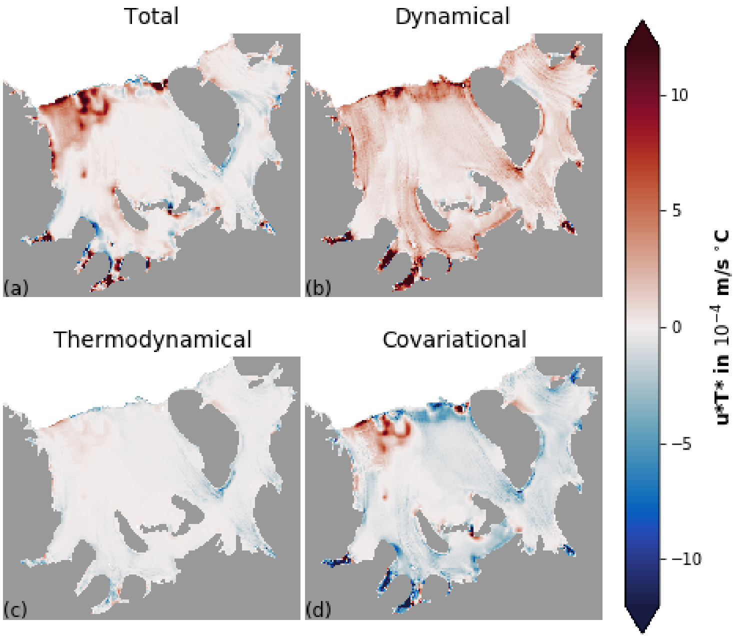


Figure 5. *Dynamical-thermodynamical decomposition of tidal melting under Filchner-Ronne Ice Shelf. Difference in melting, when accounting for (a) all components, (b) only the dynamical component, (c) only the thermodynamical component and (d) only the covariational component (following Eq. 5).*

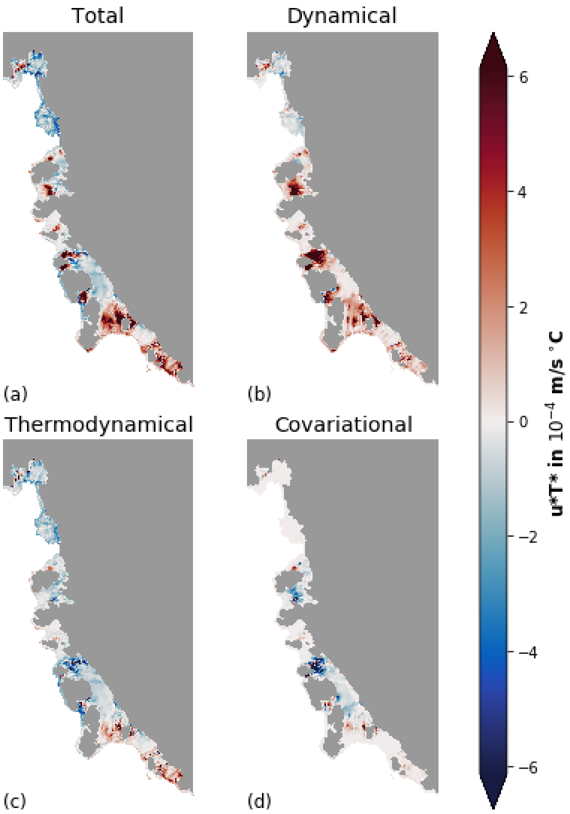


Figure 6. *Same as Fig. 5, but for the ice shelves of the Amundsen Sea.*

In most regions, dynamical effects have a major positive contribution to melting or refreezing. These regions also feature elevated tidal current speed (Fig. 3b), and, hence, we associated these changes with tidal currents rather than tidal residual flow. Tidal current induced friction, for example, acts at shallow grounding zones of cold water ice shelves where it amplifies the ice pump mechanism, that is melting at the deepest parts followed by refreezing along outflow regions (e.g. under Filchner-Ronne Ice Shelf , Fig. 5b, along the Siple Coast under Ross Ice Shelf, Fig. B1b, under Larsen C Ice Shelf, Fig. B2b), and under the Amery Ice Shelf, Fig. B3b). In warm regimes, tidal current induced friction can also have large contributions to melting. Here the effects are more pronounced at the trunk of ice shelves (e.g. under Bach and Abbot Ice Shelf, Fig. B6b, or under Getz Ice Shelf, Fig. 6b). Generally, where tidal currents are weak, dynamical tidal melting is also less strong (e.g. under the western half of Ross Ice Shelf, in trunk regions of Filchner-Ronne, Amery and Larsen C Ice Shelf, under George V Ice Shelf).

The covariational contribution often opposes dynamical effects (see, e.g. the reversed dipole pattern under Totten Ice Shelf, Fig. B7). This contrasting behaviour can be well explained by friction modulated glacial melt water input at short timescales (hourly in our case; also discussed by Jourdain et al., 2019). In melting regions, dynamically enhanced TBL transport causes heat loss in the uppermost ocean layer and, consequently, reduces thermal driving. In regions of marine ice accretion, the effect is reversed.

In some regions, however, melt rate contributions do not follow this pattern. Under large parts of North-West Ronne Ice Shelf the covariational and dynamical terms are both positive and the covariational contribution is, in part, stronger than the dynamical one. To generate this signature, thermodynamic changes must be taken into account. Indeed, thermodynamic changes are slightly positive despite increased meltwater input from dynamical effects. This thermodynamical contribution is amplified by tidal current friction, which is strong under the entire frontal zone of Ronne Ice Shelf (Fig. 3b), resulting in a strongly positive covariational term.

Thermodynamical changes by itself, however, play a secondary role in cold regimes (e.g. under Filchner-Ronne, Fig. 5c, or Ross Ice Shelf, Fig. B1c). In warm regions, thermal effects can be more relevant and mostly reduce melt (see, e.g., Pine Island, Thwaites andeastern Getz Ice Shelf in the Amundsen Sea, Fig. 6c and all ice shelves in the Bellingshausen Sea, Fig. B6c). This is also the case for the deep glacier keels within the Fimbul (Fig. B4c) and Shackleton Ice Shelf (Fig. B5), which are in contact with relatively warm water in our simulation (Fig. 2a). This cooling might be in part a consequence of increased meltwater input from tidal current enhanced TBL transport (similar to the covariational effect; see, e.g. eastern Getz Ice Shelf, Fig. 6, and Moscow University Ice Shelf, Fig. B7). However, in all of the aforementioned regions the adjacent continental shelf seas cools with tides (Fig. 4b), supporting that changes in shore-ward heat transport play an important role.

Discussion

The results of the dynamic-thermodynamic decomposition motivate an assessment of available tidal melt parameterisation for the pan-Antarctic domain. This conclusion deviates from the previous one and we have changed the respective part of the discussion accordingly from:

"~~*We have shown that tidal effects on Antarctic ice-shelf-ocean interaction are large and this means that robust modelling of ice shelves or the ice sheet requires inclusion of tides. That is, models including ice shelves, which are critical to the stability of the ice sheet and hence accurate ice sheet projections (Shepherd et al., 2004; Pritchard et al., 2012), need to explicitly resolve tidal currents. Tides are understood to be critically important for ocean-ice shelf interaction (e.g. Padman et al., 2018; Galton-240Fenzi et al., 2012), but resolving tides in larger scale models is expensive. Hence, several studies have focused on attempts to include the influence of tides on ice shelf melting without explicitly resolving tidal currents. Jourdain et al. (2019), for example, accounts for tide-driven changes in modelled melting of the Amundsen Sea ice shelves by adding a tidal component to the description of the friction velocity (following, e.g. Jenkins et al., 2010). The results of this study, however, suggest that such approaches would cause large biases in circum-Antarctic ice sheet projections.*~~

~~*We identified that under large parts of the cold water ice shelves tidal currents primarily interact with ice shelf melting by changing temperatures at the ice base, rather than velocities. Further, tide-induced warm water intrusions on the Weddell Seacontinental shelf cause significant increases in mass loss from nearby ice shelves. Ocean changes are most pronounced near the continental shelf break and, hence, we suspect tidal mixing or rectified currents to be the driving mechanism, rather than cavity overturning from buoyant meltwater plumes.*~~" to:

"*Tides are understood to be critically important for ocean-ice shelf interaction (e.g. Galton-Fenzi et al., 2012; Padman et al.,2018), but explicitly resolving tides in larger scale models is expensive. Hence, several studies have developed and applied parameterisations of tidal melting (see, e.g. Jenkins et al., 2010; Hattermann et al., 2014; Asay-Davis et al., 2016; Jourdain et al., 2019). Jourdain et al. (2019) accounts for tide-driven changes in modelled melting of the Amundsen Sea ice shelves by adding a tidal component to the description of the friction velocity (following Jenkins et al., 2010; similar to enhancing bottom drag in non-tide-resolving estuary models). Using this approach, they reproduce not only the dynamical, but also the thermodynamical and covariational effects of tides on melting, showing that the latter two are a consequence of changes in meltwater input from modulated friction in their simulation. In our study, the dynamical component also plays an important role in most regions and covariational effects can, to a large degree, be explained as a dynamical consequence (see Fig. 5 and 6).*

In some regions, however, thermodynamic drivers govern the melt change. In particular, we have attributed the coherent melt increase under North-West Ronne Ice Shelf to temperature modulations outside the TBL. These changes might originate from an ocean warming that spans across the ice shelf, associated with a tide-topography gyre on the adjacent continental shelf (Fig. 4). However, tidal vertical mixing (below the TBL) is also known to be strong here (see Makinson and Nicholls, 1999; in agreement with our tidal current strength, Fig. 3). The strength of the gyre is very uncertain (Makinson and Nicholls, 1999) and a warm bias in WAOM might overestimate its importance for melting (discussed below). We suggest further investigation into the role of the gyre for Filchner-Ronne Ice Shelf melting using a regional model, e.g. based on the ROMS configuration developed by Mueller et al. (2018)

with northern boundaries extended up to the continental shelf break. Any melt rate modulation that is indeed induced by the gyre or tidal mixing will not be captured by accounting for dynamical tidal effects on the TBL alone. Overall, the results from this study motivate a direct assessment of the tidal melt parameterisation described by Jourdain et al. (2019) in a pan-Antarctic context. WAOM would be well suited to perform these experiments."

Summary and Abstract

We also have modified respective parts of the summary and abstract to reflect the adapted conclusion. Simultaneously, we have condensed the summary to only reflect the main points (as requested under R1C11, R2C35).

The summary sections have changed from:
"~~*The modelling results presented here indicate that tides modulate Antarctic-wide ice shelf basal melting by various means:*~~

- ~~*Tide induced warm water intrusions drive strong melting under the northwestern part of Ronne Ice Shelf and are likely driven by processes near the continental shelf break rather than ice-ocean interaction.*~~
- ~~*Near the grounding zones of warm and cold water ice shelves, tidal currents control warm water advection and boundary layer exchange rates over sub-monthly timescales.*~~
- ~~*These interactions act to strengthen buoyant meltwater plumes, which modulate according to the spring-neap tidal cycle on fortnightly timescales.*~~
- ~~*Under some parts of the cold water ice shelves, tidal mixing governs ice-ocean interaction.*~~

[...]

~~*We conclude that tidal currents modulate Antarctic ice shelf melting not just by impacting boundary layer exchange rates, but also by means of vertical mixing, advection and continental shelf processes. Hence, parameterisations of tidal melting that account for boundary layer processes alone (as suggested by, e.g. Jourdain et al., 2019) will likely result in large biases in Antarctic-wide applications. Further, tidal strength changes with local and far field ice sheet retreat (e.g. Mueller et al., 2018; Rosier et al., 2014) and consequent changes in meltwater will impact ice-ocean interaction far downstream. Hence, models that aim to accurately predict future changes in ice shelf melting will need to explicitly resolve tides and have continent-wide coverage.*~~" to:

"*Dynamical-thermodynamical decomposition of tidal melting highlights the importance of tidal current enhanced exchange rates of heat and salt in the TBL. This motivates future studies to assess available tidal melt parameterisations (e.g. by Jourdain et al., 2019) to the pan-Antarctic domain. Thermodynamic driven changes due to mixing or residual flow play a role in some regions, but might be overestimated due to biases in the control run.*"

The Abstract has changed from:

“[...]. ~~*Further, by means of a singular spectrum analysis, we explore the processes that cause variations in melting and its drivers in the boundary layer over periods of up to one month. At most places friction velocity varies at tidal timescales (one day or faster), while thermal driving changes at slower rates (longer than one day). In some key regions under the large cold-water ice shelves, however, thermal driving varies faster than friction velocity and this can not be explained by tidal modulations in boundary layer exchange rates alone. Our results suggest that large scale ocean models aiming to predict accurate ice shelf melt rates will need to explicitly resolve tides.*~~” to:

“Further, to explore the processes that cause variations in melting we apply dynamical-thermodynamical decomposition to the melt drivers in the boundary layer. In most regions, tidal current modulation of the turbulent exchange of heat and salt across the ice-ocean boundary layer has a strong contribution. In some regions, however, mechanisms driven by thermodynamic effects are equally or more important, including under the frontal parts of Ronne Ice Shelf. Our results support the importance of capturing tides for robust modelling of glacier systems and coastal oceans, and motivate future studies to directly assess friction-based parameterisations for the pan-Antarctic domain.”

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 the revised manuscript 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.

During the revision of the model development paper, we now have identified biases in deep ocean temperature on the continental shelf. These biases potentially affect the results of this paper. We are explicit about this issue, by discussing it as the major limitation:

"*The major limitation of this study roots in the early development stage of the underlying ocean model. WAOM v1.0 qualitatively reproduces the large-scale characteristics of Antarctic ice shelf-ocean interaction, but biases have also been identified (Richter et al., 2020 open access review), limiting the quantitative conclusions that can be drawn regarding the tidal sensitivity. A warm bias on the western Weddell Sea continental shelf, for example, might lead to an overestimation of the here reported tide driven melting under North-West Ronne Ice Shelf (see Richter et al., 2020 their Figure 9). Likewise, a cold biason the Amundsen-Bellingshausen Seas continental shelf potentially leads to an overestimation of tidal melting driven by thermodynamic effects in this region. [...] In the light of these limitations, the findings of 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 other research groups to use different pan-Antarctic ocean-ice shelf applications to repeat the experiments of this study.*"

In the light of this issue previously mentioned limitations are secondary and have been removed from the manuscript:

"~~*A major limitation of this study originates from the three-equation melt parameterisation used in the model. Observations suggest that in regions with weak currents or strong stratification, the boundary layer exchange rates of heat and salt are controlled by diffusive-convection, rather than shear-driven turbulence, and the three equation melt parameterisation used in the model does not account for this regime (Kimura et al., 2014; Begeman et al., 2018). Results of this study, however, might help to inform where diffusive-convection takes place, as we expect favourable regions to exhibit weak tides and mean flow (see Fig. 3b and 3a, respectively) as well as aT*variability that is equally or more impacted by tidal currents than the u*variability (Fig. 7).*~~"

Based on the new comparison to CTD measurements in the development paper revision, outcomes of this study now also inform about further development of WAOM. We have added this important connection to the discussion:

"*Future studies that aim to apply WAOM, for example to past or future periods, should calibrate vertical tidal mixing first. The tide-induced changes in continental shelf temperature (Fig. 4b) show some similarity with the reported biases (Richter et al., 2020 their Fig. 9), hinting towards a connection. Richter et al. (2020) has also identified overly mixed conditions*

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

on the continental shelf and linked these to the temperature biases (via erosion of warm deep water in the Amundsen Sea and missing HSSW formation in the Weddell Sea). 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).”

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.

Closely following the reviewers suggestions (also see R2C2) we have changed our approach to investigate the governing mechanisms that drive tidal melting (see Sec. Dynamical-Thermodynamical Decomposition). We no longer use frequency analysis, but dynamical-thermodynamical decomposition. However, we do apply high pass filtering to derive an estimate of mean tidal current speed and some of the suggestions that follow have been considered in this respect.

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. We now use a traditional time domain filter to estimate mean tidal current speed (Fig. 3b). The exact methodology and related changes to the manuscript are outlined under R2C13.

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. Changes to the manuscript are outlined under R2C13.

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 now use a different proxy for the contribution of tidal velocity shear to tidal melting (see Sec. Dynamical-Thermodynamical Decomposition). We no longer decompose u* using frequency analysis and, hence, the representation of u* (scalar or vector) is not a concern. However, we now account for tidal currents with circular ellipses when calculating the mean tidal current speed (see R2C13).

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.

We now have applied a different method to derive insights into the governing mechanisms behind tidal melting in our simulation. The new approach offers a more direct interpretation regarding the importance of individual mechanisms for mean melting. The new insights fall in line with the reviewers conclusion that tidal changes in u* governs tidal melting in most places. Changes to the manuscript regarding methodology, results, discussion and conclusion are presented coherently under Sec. Dynamical-Thermodynamical Decomposition.

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 have removed any arguments based on this connection from the revised manuscript.

Results:
“~~When activating tides in our model, a strong gyre forms with its centre close to the midpoint of the rotating tidal wave, called the amphidromic point (for location of the M2 amphidromic point in the Weddell Sea see, e.g. Rosier et al., 2014, their Fig. 1a).~~“

Discussion:
“~~More drastic ice retreat in this region, however, might reduce the warm water intrusion under the Ronne Ice Shelf, as the M2 amphidromic point on the Weddell Sea migrates northward (Rosier et al., 2014) and our results suggest a connection between this point and the warm water intrusions.~~”

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 MN99 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. MN99 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 now discuss the uncertainty related to the gyre strength and propose future work::

“*[...]. These changes might originate from an ocean warming that spans across the ice shelf front, associated with a tide-topography gyre on the adjacent continental shelf (Fig. 4). However, tidal vertical mixing (below the TBL) is also known to be strong here (see Makinson and Nicholls, 1999; in agreement with our tidal current strength, Fig. 3). The strength of the gyre is very uncertain (Makinson and Nicholls, 1999) and a warm bias in WAOM might overestimate its importance for melting. We suggest further investigations are required into the role of the gyre for Filchner-Ronne Ice Shelf melting using a regional model, e.g. based on the ROMS configuration developed by Mueller et al. (2018) with northern boundaries extended up to the continental shelf break. [...]* “

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. First, we do not show that tidal melting under North-West Ronne Ice Shelf is governed by the gyre and, second, our model might overestimate the impact of the gyre on melting (due to the gyre strength and warm bias on the shelf). There is some evidence, however, that the gyre plays an important role. First, the ice shelf with the largest integrated mass loss due to tides coincides with the strongest residual current feature on the continental shelf. Second, Makinson and Nicholls (1999) also resolve the gyre and a southward flow along the eastern Weddell Sea coast. They also show that the gyre strength is sensitive to the local topography which is uncertain in this region (e.g. minimum depth estimates range from 10 m to 400 m). Third, our decomposition analysis supports that tidal melting under north-west Ronne is driven by thermodynamic effects. The most recent study of regional tidal melting does not include Belgrano Bank (Mueller et al. 2018). We believe that there is enough evidence to encourage further investigation of this phenomenon.

In the revised manuscript, we have added a discussion about these points, explicitly communicating the uncertainties. We also propose design details for a follow-up study:

“In particular, we have attributed the coherent melt increase under North-West Ronne Ice Shelf to temperature modulations outside the TBL. These changes might originate from an ocean warming that spans across the ice shelf front, associated with a tide-topography gyre on the adjacent continental shelf (Fig. 4). However, tidal vertical mixing (below the TBL) is also known to be strong here (see Makinson and Nicholls, 1999; in agreement with our tidal current strength, Fig. 3). The strength of the gyre is very uncertain (Makinson and Nicholls, 1999) and a warm bias in WAOM might overestimate its importance for melting. We suggest to further investigate the role of the gyre for Filchner-Ronne Ice Shelf melting using a regional model, e.g. based on the ROMS configuration developed by Mueller et al. (2018) with northern boundaries extended up to the continental shelf break.”

Most importantly for this study, the conclusions regarding the prospects of known tidal melt parameterisations are less affected by the uncertainty related with the gyre. We subsequently note this point:

“*Any melt rate modulation that is indeed induced by the gyre or tidal mixing will not be captured by accounting for dynamical tidal effects on the TBL alone.*”

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.

This comment is related to R2C5 and R2C35. Below we have compiled a list of all points raised in the discussion and summary. In the light of the new findings (decomposition analysis) and recently revealed biases in the control run, we have changed some statements and restructured the discussion. The new discussion can be structured into three main parts: *Implications for Ice Sheets and AABW, Resolving or Parameterising Tidal Effects in Larger Scale Models, and Limitations.* The list below also outlines how previous points fit into this new structure and how the statements have changed.

Old Discussion	**New Discussion**
“Large scale models need to explicitly resolve tides.”	Now in *Implications for Ice Sheets and AABW; relaxed in the light of the new findings*
“Tides modulate melting in cold regimes via thermodynamic mechanisms.”	Now under *Resolving or Parameterising Tidal Effects in Larger Scale Models; relaxed in the light of the new findings*
“Tides play a secondary role for frontal melting.”	Now removed from the discussion and only presented as secondary result
“Major limitation originates from three equation melt parameterisation.”	Removed, as only secondary in the light of the recently identified model biases
“Future studies should apply tidal harmonic analysis and perform additional experiments without ice shelf thermodynamics.”	Removed, as there are more urgent recommendations for future studies, originating from the recently revealed model biases
“Ice shelf teleconnections are important.”	Removed, as only speculative (in agreement with reviewers comment R2C21)
“Tidal melting might be important for instantaneous buttressing.”	Now under *Implications for Ice Sheets and AABW*
“Tidal melting might be important for long term ice shelf dynamics.”	Now under *Implications for Ice Sheets and AABW*
“Tidal melting might be important for water mass transformation.”	Now under *Implications for Ice Sheets and AABW*

Old Summary	**New Summary**
“Conversion efficiency”	Now only in discussion under *Implications for Ice Sheets and AABW*
“Weddell Sea tidal gyre and its connection	Listed in summary, as the largest regional

to melting.”	phenomenon of our simulation; Further discussed under *Limitations* and *Resolving or Parameterising Tidal Effects in Larger Scale Models*
“Frontal melting”	Now only presented as a secondary result (no discussion or conclusion)
“Ice shelf teleconnections”	Removed, as only speculative (in agreement with reviewers comment R2C21)
“Model bias”	Removed from summary and now only discussed under *Limitations*
“Tidal melt mechanisms and their prospects to be parameterized.”	Discussed under *Resolving or Parameterising Tidal Effects in Larger Scale Models*; Main conclusion reiterated in summary

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

We thank the reviewer for this thorough review. Most suggestions regarding the language have been gratefully accepted. Some questions are redundant due to removed or substantially rewritten parts due to earlier comments. The questions and comments that needed further clarification are answered in the following.

R1C13 L80: "*At 10 km horizontal resolution WAOM has a combined root-mean-square error in tidal height **complex** amplitude of 27 cm, [...]* " why complex? The amplitude is a scaler.

To account for the phase, tides can be expressed using complex numbers. King and Padman (2005) use the phrasing: " *[...] the complex expression of the interpolated modeled and observed tide amplitudes and Greenwich phases [...].* " We agree that only saying complex amplitude is wrong. We have changed our wording to: "*the complex expression of tides*"

R1C14 L89: "*At the surface, we prescribe daily heat and salt fluxes, which have been derived using satellite sea ice data and heat flux calculations (Tamura et al., 2011), and daily wind stress is calculated from ERA-Interim 10-m winds and bulk flux formula (Dee et al., 2011). To constrain model drift, sea surface temperature and salinity are relaxed, on an annual timescale, to estimates from the Southern Ocean State Estimate reanalysis (Mazloff et al., 2010).*"
I'm not a modeller, so I am a bit puzzled how this all works. How do the Tamura sea ice and heat flux data relate to the use of the ERA Interim data with the bulk flux formula? Do you get two different sets of heat flux, one from each? I assume that the Tamura results are used in the vicinity of shoreleads, which are below ERA Interim resolution? And how does this all work with the relaxation to SOSE, which provides another set of surface heat and salt fluxes? This all needs to be fully explained (for the likes of non-modeller like me).

We acknowledge that these practices are less clear for non-modellers. The wind stress is calculated by applying a bulk flux formula to ERA-Interim 10-m winds. The heat and salt fluxes are derived from Tamura results and SOSE. Tamura provides the high frequency variability, while SOSE is used to correct model drift over annual time scales. We have revised the original paragraph to make this more clear and elaborate on the relaxation scheme.

"*At the surface, daily wind stress is calculated by applying a bulk flux formula to ERA-Interim 10-m winds (Dee et al., 2011). We prescribe daily heat and salt fluxes, which have been derived using satellite sea ice data and heat flux calculations (Tamura et al., 2011). In addition, a small correction term is added to the heat and salt fluxes to constrain model drift* *over annual time scales. These corrections are based on the difference between the model's solution of surface temperature and salinity compared to monthly estimates from the Southern Ocean State Estimate reanalysis (Mazloff et al., 2010).*"

R1C15 L94: "Initial temperatures and salinities are also derived from ECCO2, [...]" Why not use SOSE for this?

This has historical reasons. WAOM has been developed based on the experience of regional modelling in our group (Galton-Fenzi et al., 2012; Cougnon et al., 2013; Gwyther et al., 2014, 2016). ECCO2 has much longer time series than SOSE, making it more applicable for interannual studies. Surface relaxation has only recently been included to stabilize the pan-Antarctic application of the code. For annual estimates, surface relaxation to SOSE is a strong choice due to its increased accuracy. Future versions of WAOM should include the option to use different products for boundary conditions. The historical background of WAOM is pointed out in the GMD companion paper, which should be consulted for in depth questions regarding development. We have not made any changes to this manuscript in this regard.

R1C16 Figure3: "Arrows in (c) indicate the change in barotropic flow direction when activating tides [...]" I don't understand. How can an arrow indicate a change in direction?

This has to be seen together with the underlying color. The color denotes the magnitude of change and the arrow the direction. A red region with a westward pointing arrow, for example, denotes that tides act to increase westward flow (or decrease eastward flow).

To make the connection between color and arrows more intuitive, we now only talk from tide induced change::

"Figure 3. *Tide induced change in (a) continental shelf barotropic velocity and (b) potential temperature. Differences show impact when activating tides in the model. Arrows in (a) are shown only where velocity change is larger than 1 cm/s.*"

R1C17 L245: *"We identified that under large parts of the cold water ice shelves tidal currents primarily interact with ice shelf melting by changing temperatures at the ice base, rather than velocities.*"
(a) This is a key result, but one that doesn't seem to be consistent with the model data that have been presented. The majority of the ice shelf areas in Figure 5c are red, implying that tidally-driven variability is dominating u*. (b) There is one part of Ronne Ice Shelf where u* is not dominated by tidal variability. Here we come to a bit of a problem. Variability in u* at sub-daily timescales is not a good indicator of the contribution to u* of tides. The reason is that, for it to be a good indicator, the tidal ellipse would need to be flat. For some of the central parts of Ronne Ice Shelf, for example, the tidal ellipses are nearly circular, both for semi-diurnal and diurnal species. If they were circular, there would be no variability in u*.

(c) Notably, some of the key features in the melt rate distribution shown in Figure 2a disagree quite significantly from data that are available from satellite observations and from other model results, and so it's not clear that results for that ice shelf can be trusted. In particular, the intense freezing that activating the tides appears to cause near the very deep grounding lines of some of the large ice streams (Rutford and Foundation, for example) is incompatible with every other study, either observational or modelling.

In regards to (a), we now agree with the reviewer about their conclusion. Our new decomposition results lead to the same conclusion (see R1C7 and Section *Dynamical-Thermodynamical Decomposition*).

Regarding (b), we now have considered circular tidal ellipses in the calculation of mean tidal velocity. Our new decomposition results are not based on frequency analysis and account for any tide induced current. Considering both fields (mean tidal velocity, Fig. 3b, and the dynamical component of tidal melting under Ronne Ice Shelf, Fig. 5b) we derive at the same conclusion as the reviewer: In the central Ronne Ice Shelf region, tidal melting is dominated by tidal current enhanced TBL exchange. In the revised manuscript, we do this comparison between tidal current speed and the dynamical term:

"In most regions, dynamical effects have a major positive contribution to melting or refreezing. These regions also feature elevated tidal current speed (Fig. 3b), and, hence, we associated these changes with tidal currents rather than tidal residual flow."

Central Ronne Ice Shelf is implied in "*most regions"*, as we do not list it as an exception later on.

In regards to (c): Evaluation of the control run is subject of the accompanying GMD paper. In this manuscript, we interpret tidal sensitivity in the light of the known biases (see R1C2). We do not expect melting and refreezing at small scales to be accurate everywhere in a pan-Antarctic model. The GMD paper shows that WAOM generally captures the three known modes of melting and, hence, we can look at the tidal sensitivities of these modes where our simulation predicts them.

R1C18 L355: "vertical mixing". Vertical mixing is all that boundary layer exchange consists of.

In the revised manuscript, we are more specific now by saying "*vertical mixing below the TBL*".

Response to Review #2

Major concerns

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[2]. We have chosen an interactive discussion journal for the model development paper so that the review process is fully accessible to the reviewers of this paper. Having said so, we have added some key information regarding the model description following the reviewers suggestions below (R2C6 to R2C12).

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.

We thank the reviewer for this valuable comment, which has helped to develop the new analysis outlined under R1C7. The dynamical-thermodynamical decomposition components directly map onto mean melting.

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. Also, in the revised manuscript, the line between robust results and hypotheses is drawn more clearly using phrases, such as "might be in part a consequence of" or "supporting that".

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 have followed this advice closely and repartitioned our discussion and summary (see R1C11). Any unnecessary overlap has been removed.

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

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

Comments/questions

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

We have included the respective citations. The revised sentence is (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. We have added this information (additions in bold):

"*In this way we achieve an accuracy in the tidal height signal around the coast of Antarctica that is comparable to available barotropic tide models (assessed in King and Padman, 2005;* ***see Richter et al., 2020, their 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.

Regarding Figure 1, we have included an inset showing the full model domain:

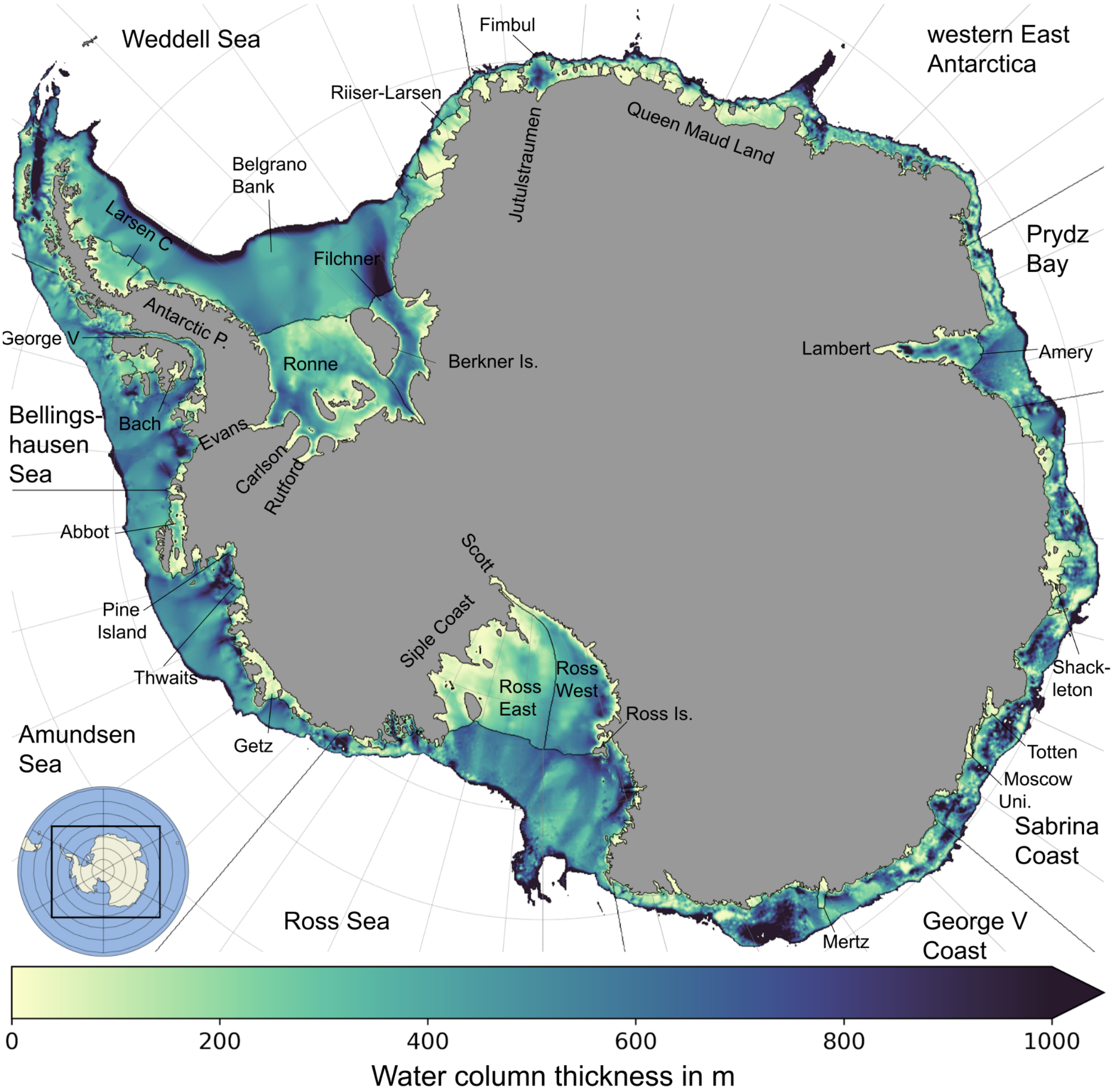

Figure 1. *Study area and water column thickness on the continental shelf. Colours show the seafloor depth on the open continental shelf and water column thickness where ice shelves are present. The labels indicate locations referred to in the text with ice sheet regions and tributary glaciers on land, and ice shelves and ocean sectors on water. Abbreviations are Island (Is.), Ice Rise (I.R.) and Peninsula (P.). Inlet shows model boundaries.*

R2C9: L85-86: At what frequency are the open boundary condition data prescribed?

The open boundary conditions are monthly averages. We have included this information in the revised manuscript:

*“The ocean outside the model domain is described using the ECCO2 reanalysis (Menemenlis et al., 2008) and includes **monthly averages** of sea surface height, barotropic and baroclinic velocities, temperature and salinity.”*

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[3]. The evaluation of the ocean conditions, including the surface ocean, is done in the accompanying model development paper[4]. 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[5] (see R1C12 and R3C20 of this revision). Prescribing surface buoyancy fluxes has the advantage of accurate surface salt flux location and strength from sea ice polynyas. We have added a clarifying sentence to the revised manuscript of this paper:

"*Prescribing surface buoyancy fluxes rather than including a sea ice model has the advantage of accurate surface salt flux location and strength from sea ice polynyas, but does not allow for sea ice-ocean interaction (see discussion by Richter et al., 2020).*"

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 of mean ice shelf melting in the companion paper (see Fig. 2). In the revised manuscript we have included this information and a reference to Figure 2 of the companion paper:

"By performing parts of the spin up at lower resolution, we save computational costs, while still ensuring a quasi-equilibrium of the continental shelf seas ***(measured using Antarctic average melting; see Richter et al.,2020, their Fig. 2)****."*

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

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[3] 10.31223/osf.io/stcqg

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

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

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 (also see R1C4) and then calculate the magnitude from these filtered components. The revised equation is as follows:

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

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. The revised figure is shown as *new Figure 3b* under *Changes unrelated to reviewer comments*.

Figure R1 compares the tidal current speed as calculated from Eq. 1 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.

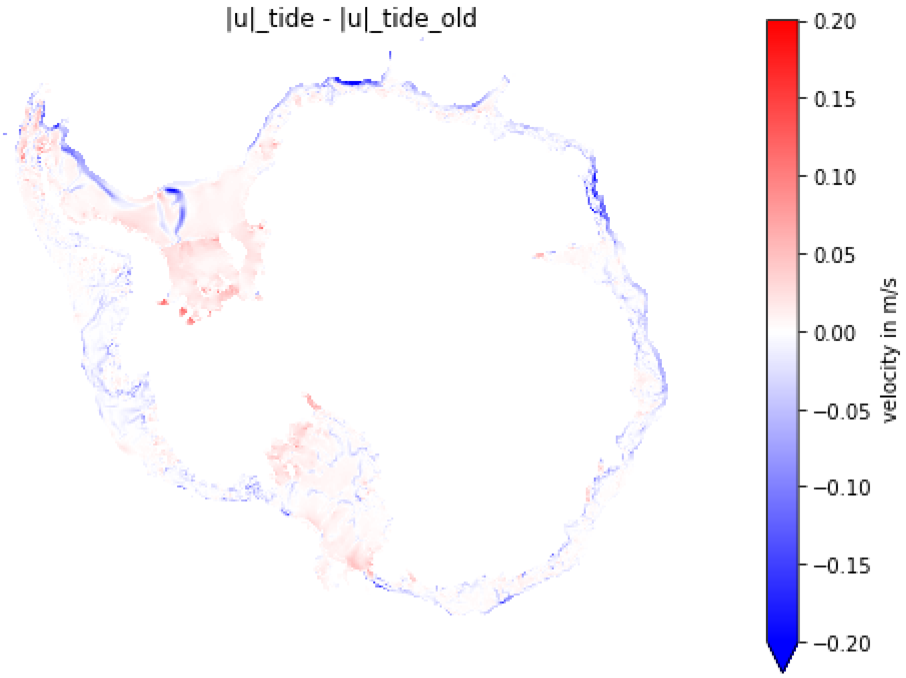

Figure R1: Difference in tidal current speed as calculated using the new and old methodology (see text).

The temporal filtering approach captures most of the high frequency variability associated with tides (see Stewart et al. 2018, their Fig. S2). Tidal harmonic analysis would be the most accurate way, but we find it impractical for our large amounts of data. Stewart et al. (2018) confirms this challenge:

> “*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, temporal filtering is accurate enough.

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

W_b denotes ice shelf melting. We have defined this explicitly in the revised manuscript:

“*The main characteristics of ice shelf basal melting as derived from the three equation melt parameterisation* ***(w_b)*** *can be approximated using the covariance of friction velocity (u*) and thermal driving (T*, see Holland and Jenkins, 1999):*”.

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. While the temporal averaging has already been describe, we now have included additional information regarding the spatial averaging:

“The area-integrated impact of tides on modelled annual-average melting and continental shelf seas temperature is small as shown in Table 1. The total basal mass loss increases by 4 % when including tides in the model, while ocean temperatures slightly drop ***(calculated as volume average of the entire ocean south of the 1000~m isobath)****.”*

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[6]. The revision of the development paper[7] now includes a comparison of the bottom layer temperature and salinity against estimates by Schmidtko et al. (2014, see

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

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

Fig. 6 and 7 in the revision). As the reviewer expected, this bias estimate has implications for the tidal sensitivities presented in this study. In the revised manuscript we explicitly communicate these biases and discuss the implications (see R1C2).

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 (Fig. 2b):

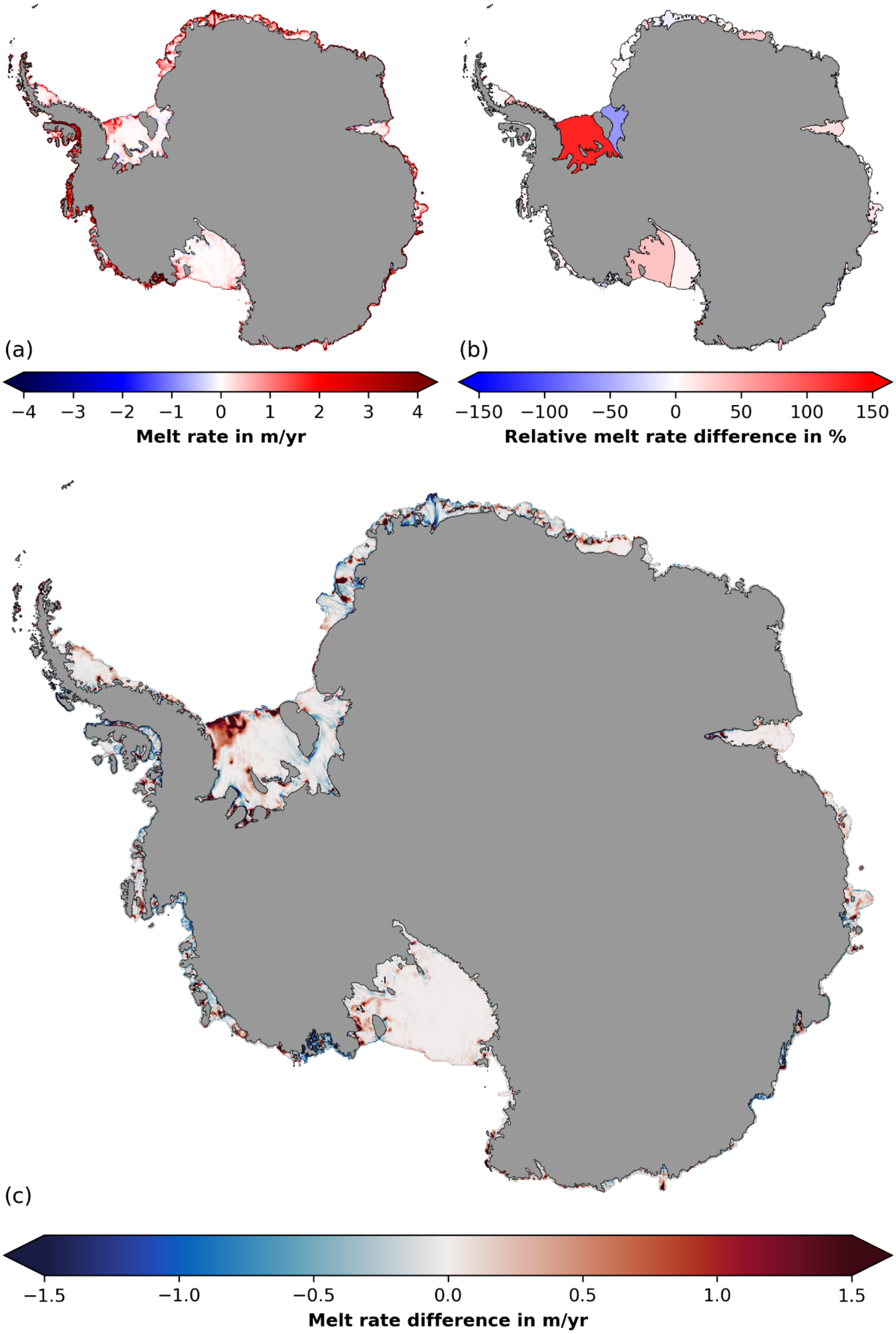

Figure 2: Tidal melting of Antarctic ice shelves. a) Annual average ice shelf melting for the case with tides, b) its relative difference to the case without tides averaged over individual ice shelves ([Tides−No-Tides]/No-Tides) and c) its absolute difference to the case without tides (Tides−No-Tides).

The revised figure does not change any conclusions drawn in the text, but highlights the importance of tides for melting under Ronne and Filchner Ice Shelves. The respective paragraph in the results section has been adapted from:

“*The effects of tides on individual ice shelves, however, can be large. Figure 2 presents the spatial distribution of ice shelf melting around Antarctica as well as the sensitivity of these melt rates to tides. Tides modulate melting all around the continent (Fig. 2c), ~~but mostly impact ice-ocean interaction underneath the large cold cavity ice shelves (Filchner-Ronne, Ross, Ameryand Larsen C; Fig. 2b), where ambient melt rates are small (Fig. 2a). Modulations in melt rate (at model horizontal resolutionof 4 km) have a standard deviation (std) of 352 % and can be larger than 100 times of the original melt rate (calculated as(wbtides−wbno tides)/wbno tides) and shown in Figure 2c).~~* ***but impact ice shelf integrated mass loss mostly in cold regions where ambient melt rates are small (e.g. Filchner, Ronne, Ross and Larsen C Ice Shelf; Fig. 2b and 2a). The Ronne Ice Shelf features by far the highest increase in mass loss (44 Gt/yr, 128 %; see Table A1), only in part compensated by reduced melting under the adjacent Filchner Ice Shelf (-8 Gt/yr, -60 %). Modulation at model resolution (4 km) has a standard deviation of 352 % (not shown).*** *Areas of increased melting are often in close vicinity of areas of reduced melting or increased marine ice accretion, leading to smaller effects when considering ice shelf area averages ~~(std =37 %; see Table A1)~~.”*

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[8] (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. Instead, we have included a reference to the discussion in the GMD paper:

“*While our results indicate strong melting at the ice shelf front all around the continent (see Fig. 2a* ***; discussed by Richter et al., 2020)****, in most regions this melting is independent from tides (as shown in Fig. 2b)*.”

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

The *strength* of the Antarctic Slope Current has increased. However, arguments around ice shelf teleconnections have been removed from the revised manuscript, as these are highly speculative (in agreement with comment R2C21) and do not contribute to the restructured discussion.

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

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

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. Instead of softening the language, we have removed any arguments around ice shelf teleconnections from the revised manuscript. After restructuring the discussion (R1C11), we find that this speculative result only distracts from deriving the main conclusions.

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.

We thank the reviewer for investing the time to understand SSA. We now use a dynamical-thermodynamical decomposition instead of frequency analysis (see R1C7 and Sec. *Dynamical-Thermodynamical Decomposition*). The decomposition approach is more straightforward than SSA and has been well explained in the revised manuscript.

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 differences from Fig. 6, but only in a few selected locations.

We thank the reviewer for this valuable suggestion. We believe that dynamical-thermodynamical decomposition offers an even more direct way to map tidal changes in u* and T* to mean melting.

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.

Our new decomposition approach implicitly accounts for phase differences between u* and T*. The time mean is always taken from the covariance of u* and T* at hourly resolution (see Eq. 5).

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.

The Section 3.2 has been rewritten entirely as a consequence of the changed analysis. Presenting the results of the decomposition has been more straightforward and we have paid particular attention to a logical structure. E.g. the term with the highest contribution (dynamical) has been described first.

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

Figure 5 is redundant due to the new analysis (R1C7). The decomposition results are best presented with a linear scale.

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

We have included a quantification:
“*While circum-Antarctic total melt is small, regional changes in ice shelf melting and continental shelf temperature can be large* ***(up to orders of magnitude and half a degree Celsius, respectively)*** *with potential implications for ice sheet dynamics and AABW formation*.”

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.

The results from the dynamical-thermodynamical decomposition offer stronger evidence regarding the prospects of traditional tidal melt parameterisations. We now agree with the reviewer that such parameterisations would likely do a fair job in most regions in our simulation. We are explicit about this point, while at the same time acknowledging remaining uncertainties by proposing a follow up study that directly assesses the parameterisation:

“*Overall, the results from this study motivate a direct assessment of the tidal melt parameterisation described by Jourdain et al.(2019) in a pan-Antarctic context*.”

Further, we are more specific about the regions, where such approaches will likely have large biases. North-West Ronne Ice Shelf is the only region with strong evidence against traditional tidal melt parameterisations:

“*In some regions, however, thermodynamic drivers govern the melt change. In particular, we have attributed the coherent melt increase under North-West Ronne Ice Shelf to temperature modulations outside the TBL. [...] Any melt rate modulation that is indeed induced by the gyre or tidal mixing will not be captured by accounting for dynamical tidal effects on the TBL alone.*”

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).

Our approach regarding the surface forcing has been discussed above (please see R2C11). We do acknowledge that interactions between tidal melting and sea ice might play a role and have included missing sea ice interaction as a limitation of our study:

“*The major limitation of this study has its roots in [...]. Further, sea ice interacts with ice shelf melting (e.g. Hellmer, 2004; Silvano et al., 2018) and tides (Padman et al., 2018), and our approach does not account for these interactions, potentially missing important feedbacks. [...]*”

R2C30: L272: Missing word at the end of this sentence.

Actually, the referencing went wrong.
“ *[...] of tide-driven shoreward heat transport of (Stewart et al., 2018).*” was meant to be “*of tide-driven shoreward heat transport by Stewart et al. (2018).*”.

We have restructured the discussion and tailored down the summary according to the reviewers comments (see R1C11, R2C5, R2C35). During this process this recommendation for future work has been rated as secondary and removed from the manuscript.

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 have tailored down the summary to only include the main conclusions (see R1C11). Bullet points are no longer used in the summary of the revised manuscript.

R2C32: L323: Citation should not be in parentheses.
We have revisited all citations for correct formatting.

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. The summary now includes quantitative statements about changes at all scales. In addition, we have relaxed our small scale estimate to “*can exceed 100 % in cold regimes*” to accommodate large numbers due to small values in the denominator.

“*[...] Activating tides in the model increases total mass loss by 4 % and mass loss modulations for most ice shelves are below 10 %. The Ronne-Filchner IceShelf system exhibits larger coherent changes (Ronne melt increases by 128 %), potentially related to a strong tide induced gyre over Belgrano Bank. Melt modulations at smaller scales can exceed 100 % in cold regimes and are in part located near grounding lines and lateral boundaries, regions important for ice shelf buttressing. The ocean temperature of the entire continental shelf decreases by only 0.04◦C. Regional modulations can exceed 0.5◦C including a strong warming of the Western Weddell Sea.*”

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. We are now explicit about this in the discussion section:
"*The relevance of tide driven currents and temperature changes for water mass formation and transformation on the Antarctic continental shelf, and indeed on the global oceans and climate, is yet to be explored.*"

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 have revisited the statements and structure of the discussion and summary (see R1C11). The results from the decomposition analysis also have a stronger link to the rest of the paper (direct mapping to mean melting), resulting in a better structure. We are convinced that the discussion has a clear structure now and that the summary reiterates the main points exclusively.

Supplemental Material

Appendix B: Dynamical-Thermodynamical Decomposition in Other Key Regions

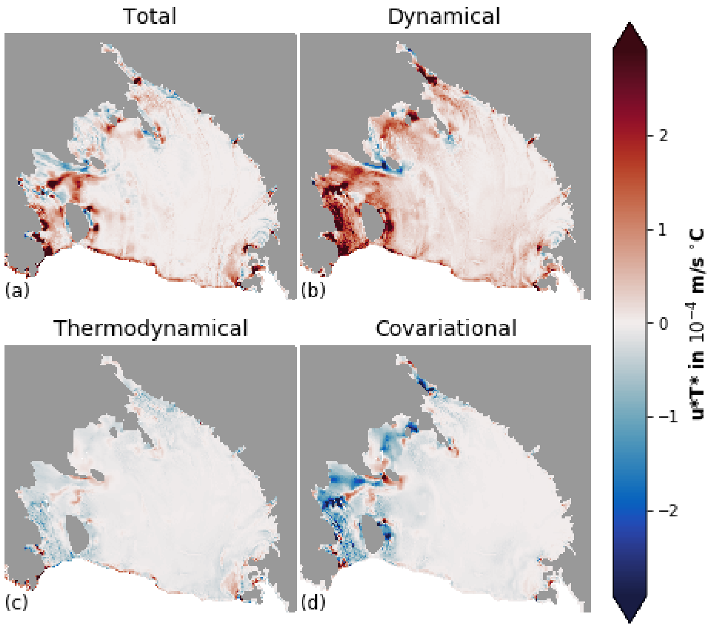

Figure B1. Dynamical-thermodynamical decomposition of tidal melting under Ross Ice Shelf (same as Fig. 5). Difference in melting,when accounting for (a) all components, (b) only the dynamical component, (c) only the thermodynamical components and (d) only the covariational component (following Eq. 5).

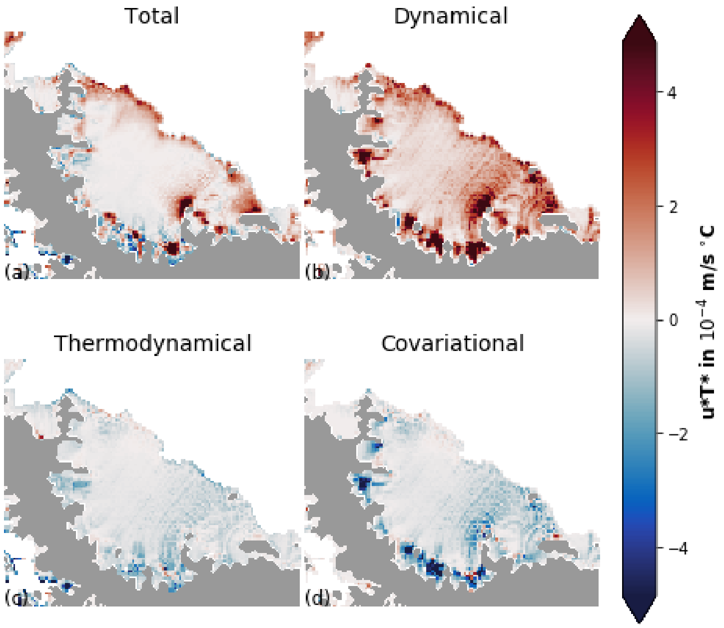

Figure B2. Same as Fig. B1, but for the Larsen C Ice Shelf.

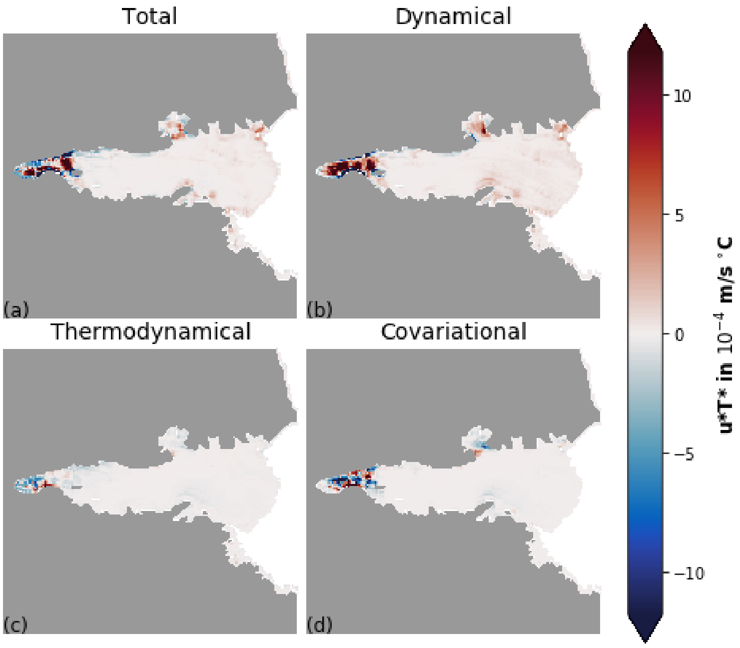

Figure B3. Same as Fig. B1, but for the Amery Ice Shelf.

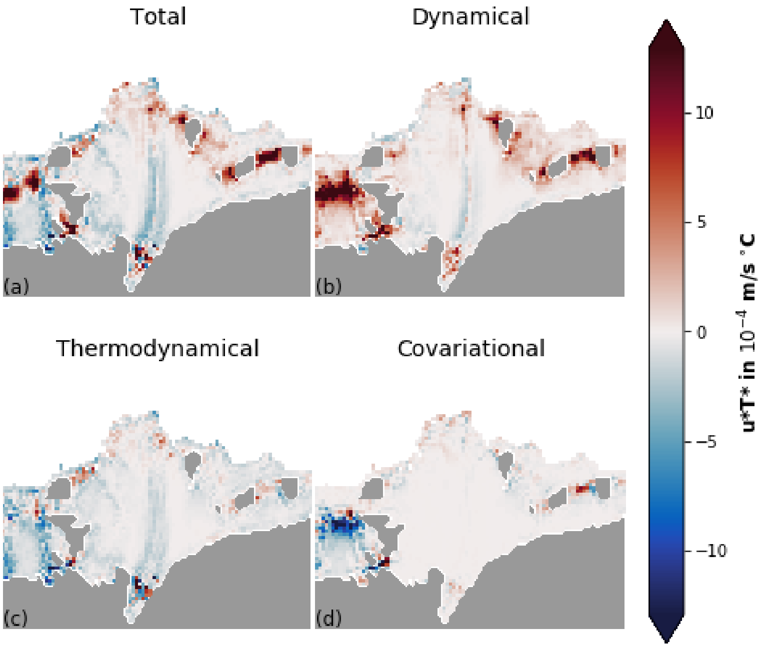

Figure B4. Same as Fig. B1, but for the Fimbul Ice Shelf.

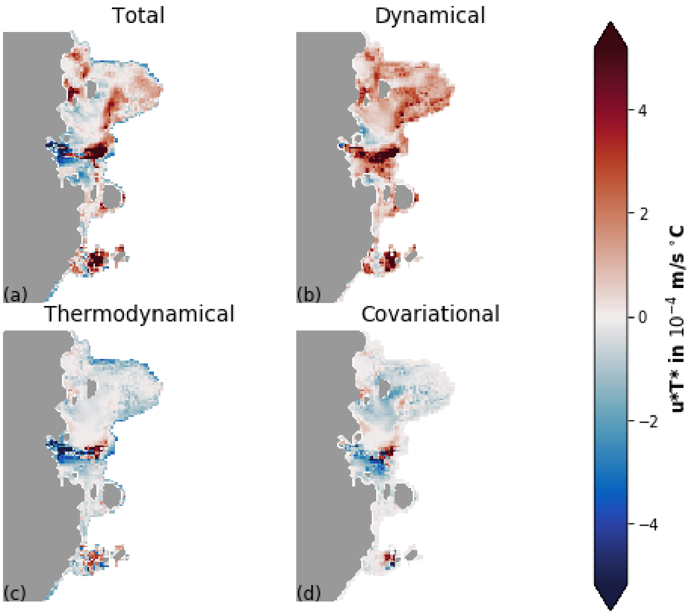

Figure B5. Same as Fig. B1, but for the Shackleton Ice Shelf.

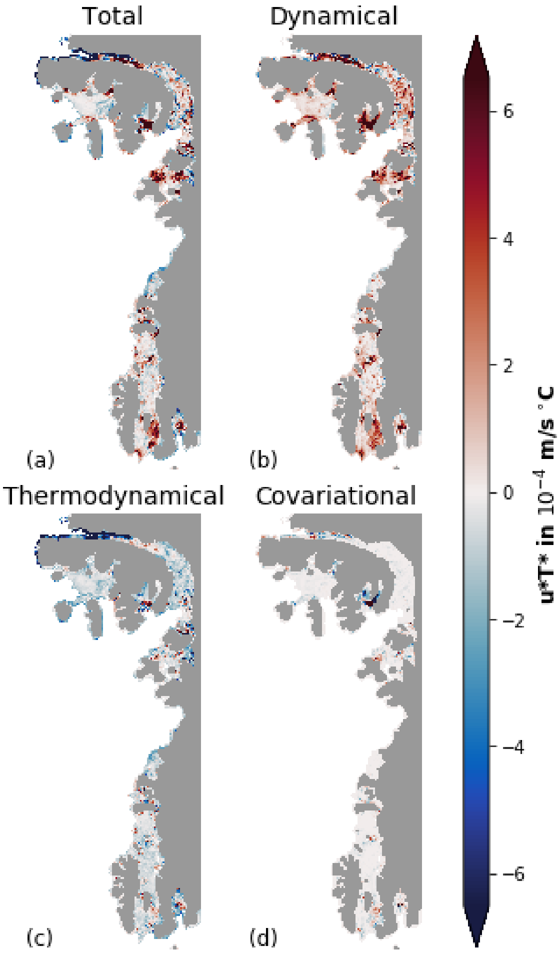

Figure B6. Same as Fig. B1, but for the ice shelves of the Bellingshausen Seas.

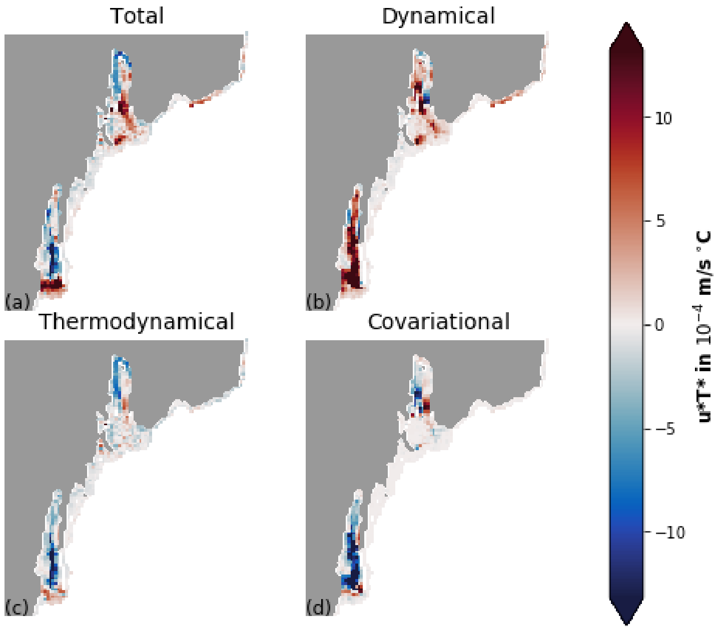

Figure B7. Same as Fig. B1, but for the Totten-Moscow University Ice Shelf System.

References

Asay-Davis, Xylar S., Stephen L. Cornford, Gaël Durand, Benjamin K. Galton-Fenzi, Rupert M. Gladstone, G. Hilmar Gudmundsson, Tore Hattermann, et al. 2016. “Experimental Design for Three Interrelated Marine Ice Sheet and Ocean Model Intercomparison Projects: MISMIP v. 3 (MISMIP +), ISOMIP v. 2 (ISOMIP +) and MISOMIP v. 1 (MISOMIP1).” *Geoscientific Model Development* 9 (7): 2471–97. https://doi.org/10.5194/gmd-9-2471-2016.

Cougnon, E. A., B. K. Galton-Fenzi, A. J. S. Meijers, and B. Legrésy. 2013. “Modeling Interannual Dense Shelf Water Export in the Region of the Mertz Glacier Tongue (1992–2007).” *Journal of Geophysical Research: Oceans* 118 (10): 5858–72. https://doi.org/10.1002/2013JC008790.

Dee, D. P., S. M. Uppala, A. J. Simmons, P. Berrisford, P. Poli, S. Kobayashi, U. Andrae, et al. 2011. “The ERA-Interim Reanalysis: Configuration and Performance of the Data Assimilation System.” *Quarterly Journal of the Royal Meteorological Society* 137 (656): 553–97. https://doi.org/10.1002/qj.828.

Galton-Fenzi, B. K., J. R. Hunter, R. Coleman, S. J. Marsland, and R. C. Warner. 2012. “Modeling the Basal Melting and Marine Ice Accretion of the Amery Ice Shelf.” *Journal of Geophysical Research: Oceans* 117 (C9). https://doi.org/10.1029/2012JC008214.

Gwyther, D. E., B. K. Galton-Fenzi, J. R. Hunter, and J. L. Roberts. 2014. “Simulated Melt Rates for the Totten and Dalton Ice Shelves.” *Ocean Science* 10 (3): 267–79. https://doi.org/10.5194/os-10-267-2014.

Gwyther, David E., Eva A. Cougnon, Benjamin K. Galton-Fenzi, Jason L. Roberts, John R. Hunter, and Michael S. Dinniman. 2016. “Modelling the Response of Ice Shelf Basal Melting to Different Ocean Cavity Environmental Regimes.” *Annals of Glaciology* 57 (73): 131–41. https://doi.org/10.1017/aog.2016.31.

Haney, Robert L. 1991. “On the Pressure Gradient Force over Steep Topography in Sigma Coordinate Ocean Models.” *Journal of Physical Oceanography* 21 (4): 610–19. https://doi.org/10.1175/1520-0485(1991)021<0610:OTPGFO>2.0.CO;2.

Hattermann, T., L. H. Smedsrud, O. A. Nøst, J. M. Lilly, and B. K. Galton-Fenzi. 2014. “Eddy-Resolving Simulations of the Fimbul Ice Shelf Cavity Circulation: Basal Melting and Exchange with Open Ocean.” *Ocean Modelling* 82 (October): 28–44. https://doi.org/10.1016/j.ocemod.2014.07.004.

Jenkins, Adrian, Keith W. Nicholls, and Hugh F. J. Corr. 2010. “Observation and Parameterization of Ablation at the Base of Ronne Ice Shelf, Antarctica.” *Journal of Physical Oceanography* 40 (10): 2298–2312. https://doi.org/10.1175/2010JPO4317.1.

Jourdain, Nicolas C., Jean-Marc Molines, Julien Le Sommer, Pierre Mathiot, Jérôme Chanut, Casimir de Lavergne, and Gurvan Madec. 2019. “Simulating or Prescribing the Influence of Tides on the Amundsen Sea Ice Shelves.” *Ocean Modelling* 133 (January): 44–55. https://doi.org/10.1016/j.ocemod.2018.11.001.

King, Matt A., and Laurie Padman. 2005. “Accuracy Assessment of Ocean Tide Models around Antarctica.” *Geophysical Research Letters* 32 (23). https://doi.org/10.1029/2005GL023901.

Makinson, Keith, and Keith W. Nicholls. 1999. “Modeling Tidal Currents beneath Filchner-Ronne Ice Shelf and on the Adjacent Continental Shelf: Their Effect on Mixing and Transport.” *Journal of Geophysical Research: Oceans* 104 (C6): 13449–65. https://doi.org/10.1029/1999JC900008.

Mazloff, Matthew R., Patrick Heimbach, and Carl Wunsch. 2010. “An Eddy-Permitting Southern Ocean State Estimate.” *Journal of Physical Oceanography* 40 (5): 880–99. https://doi.org/10.1175/2009JPO4236.1.

Mellor, G. L., T. Ezer, and L-Y. Oey. 1994. “The Pressure Gradient Conundrum of Sigma Coordinate Ocean Models.” *Journal of Atmospheric and Oceanic Technology* 11 (4):

1126–34. https://doi.org/10.1175/1520-0426(1994)011<1126:TPGCOS>2.0.CO;2.

Mueller, Rachael D., Tore Hattermann, Susan L. Howard, and Laurie Padman. 2018. “Tidal Influences on a Future Evolution of the Filchner–Ronne Ice Shelf Cavity in the Weddell Sea, Antarctica.” *The Cryosphere* 12 (2): 453–76. https://doi.org/10.5194/tc-12-453-2018.

Padman, Laurie, Matthew R. Siegfried, and Helen A. Fricker. 2018. “Ocean Tide Influences on the Antarctic and Greenland Ice Sheets.” *Reviews of Geophysics* 56 (1): 142–84. https://doi.org/10.1002/2016RG000546.

Richter, Ole, David E. Gwyther, Benjamin K. Galton-Fenzi, and Kaitlin A. Naughten. 2020. “The Whole Antarctic Ocean Model (WAOM v1.0): Development and Evaluation,” June. https://doi.org/10.5194/gmd-2020-164.

Robertson, Robin. 2006. “Modeling Internal Tides over Fieberling Guyot: Resolution, Parameterization, Performance.” *Ocean Dynamics* 56 (5): 430–44. https://doi.org/10.1007/s10236-006-0062-5.

Robertson, Robin, and Changming Dong. 2019. “An Evaluation of the Performance of Vertical Mixing Parameterizations for Tidal Mixing in the Regional Ocean Modeling System (ROMS).” *Geoscience Letters* 6 (1): 15. https://doi.org/10.1186/s40562-019-0146-y.

Rosier, S. H. R., J. a. M. Green, J. D. Scourse, and R. Winkelmann. 2014. “Modeling Antarctic Tides in Response to Ice Shelf Thinning and Retreat.” *Journal of Geophysical Research: Oceans* 119 (1): 87–97. https://doi.org/10.1002/2013JC009240.

Schmidtko, Sunke, Karen J. Heywood, Andrew F. Thompson, and Shigeru Aoki. 2014. “Multidecadal Warming of Antarctic Waters.” *Science* 346 (6214): 1227–31. https://doi.org/10.1126/science.1256117.

Silvano, Alessandro, Stephen Rich Rintoul, Beatriz Peña-Molino, William Richard Hobbs, Esmee van Wijk, Shigeru Aoki, Takeshi Tamura, and Guy Darvall Williams. 2018. “Freshening by Glacial Meltwater Enhances Melting of Ice Shelves and Reduces Formation of Antarctic Bottom Water.” *Science Advances* 4 (4): eaap9467. https://doi.org/10.1126/sciadv.aap9467.

Stewart, Andrew L., Andreas Klocker, and Dimitris Menemenlis. 2018. “Circum-Antarctic Shoreward Heat Transport Derived From an Eddy- and Tide-Resolving Simulation.” *Geophysical Research Letters* 45 (2): 834–45. https://doi.org/10.1002/2017GL075677.

Tamura, Takeshi, Kay I. Ohshima, Sohey Nihashi, and Hiroyasu Hasumi. 2011. “Estimation of Surface Heat/Salt Fluxes Associated with Sea Ice Growth/Melt in the Southern Ocean.” *SOLA* 7: 17–20. https://doi.org/10.2151/sola.2011-005.