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

We thank the editor and reviewer for their remarks. Reviewer words are in blue text, while our response is in black. We have labelled the major comments for cross referencing, e.g. R1C1 means Reviewer 1 Comment 1. We would like to note that the accompanying model development paper has now been accepted for publication in GMD[1].

Response to Review #1

R1C1

The authors present an updated version of a manuscript reporting on the effects of including tidal forcing within a circum-Antarctic ice shelf-ocean model. The principal aims of the study was to identify what is lost from models that do not include tides, and what is likely to remain lost when such non-tidally enabled models attempt to include the effect of tides by accounting for the shear induced vertical heat and salt transport.

It seems to this reviewer that the study advances the discussion of the impact of tides on ice shelf basal melt in a useful way. In particular, the authors make a serious effort to identify the weaknesses in the model, and how those weaknesses are likely to have affected the results.

I have attached a marked-up copy of the pdf, which points out a few typographical slips and makes occasional suggestions on how clarity might be improved. I have one main concern that needs to be addressed before the manuscript can be published.

The definition of thermal driving needs to be corrected. At the moment, the authors have used “thermal forcing”, the difference between the mixed layer temperature and the freezing point at the ice-ocean interface. “Thermal driving” is the difference between the mixed layer temperature, and its freezing point calculated at the pressure of the ice base. The ratio of the two numbers is unlikely to change much, and so I imagine it will have no effect on the results of the study. But it needs to be corrected.

We thank the reviewer for pointing this mistake out. We have used the correct definition of thermal driving in our analysis, but have stated the wrong one in the text. We now state the correct definition of thermal driving in the manuscript.

Before (L119 ff.):
“Thermal driving is defined as the difference in temperature across the ice-ocean boundary layer:

[1] https://gmd.copernicus.org/preprints/gmd-2020-164/

T* = T_M − T_B [◦ C] , (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."

After:
"Thermal driving is defined as the difference between the mixed layer temperature and its freezing point calculated at the pressure of the ice base (see Holland and Jenkins 1999 their Eqn. 32):

T* = T_M - a(S_M + b + c p_B) [◦ C]. (4)

Here T_M and S_M are the temperature and salinity in the top model cell (approximately 0.3 m to 5.0 m below the ice base; assumed to be in the 'mixed layer'), a is the slope of liquidus for seawater (-5.73 $10^{-2}\ C/psu$), b is the offset of liquidus for seawater (9.39 $10^{-2}\ C$), c is the change in freezing temperature with pressure (-7.61 $10^{-4}\ C/dbar$) and p_B is the pressure at the ice shelf base."

R1C2

The only other major comment I have is about the description of the interpretation of the results of the decomposition into dynamical, thermodynamic and covariational effects of tidal forcing on simulated melt rate. I like the approach, introduced by Jourdain et al 2019, but I'm unsure about some aspects of the interpretation. I accept this might be a point for discussion (and maybe I need to think harder about it). The relevant text is in lines 129 to 134. I've made a few comments below, which might help the authors revise that short piece of text.

The authors start with the second term in (5) (it would be worth going in order of the terms in the equation):

"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)."

This term is mean(delta_u* x T*_non-tidal). delta_u* is the difference between the friction velocity for tidal and non-tidal runs, and T*_non-tidal is the thermal driving for the non-tidal run. This term contributes to the difference in meltrates between tidal and non-tidal simulations. Does the "mean effect of the tidal current velocity" refer to an oscillatory component that drives shear in the boundary layer, and therefore turbulent exchange of heat to the ice base? I can understand that, if so. "Tidal residual flow" presumably again refers to its notional effect on vertical shear? "Tide induced buoyant plumes" presumably refers to the effect of tidally-induced melting on the buoyancy driven flow in the cavity. As I mention in the comments on the pdf, I think the use of the word "plumes" is confusing. This is a full, 3-D model and any plumes are part of the general buoyancy-driven flow. In general, I think all that needs to be stressed is that the dynamical term refers to the representation of shear-driven turbulent mixing in the three equation model, and any tidally-sourced processes that contribute to the speed of water flow in the cavity. And I would delete mention of plumes.

“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).”

This term is mean(u*_non-tidal x delta_T*), where delta_T* is the difference in temperature in the upper boundary layer between tidal and non-tidal simulations. As I said in a comment on the PDF, mention of meltwater input is potentially confusing, as it might be read as meaning sub-glacial discharge at the grounding line. More generally, though, it’s the description I have difficulty with. Tidally-induced vertical mixing into the boundary layer is important, but also the way tidal rectification contributes to the horizontal circulation of heat and salt in the cavity is also relevant (for example). Any tidally-induced change in the distribution of temperature within the cavity will contribute to this term, and that of course includes the effect of chilled meltwater from tidally-induced melting.

We agree with the reviewer that the original phrasing can be improved to be comprehensive and avoid misunderstanding (e.g. about plumes). We have revised this paragraph using the suggested phrases. In addition, we illustrate the descriptions of the individual terms with examples of mechanisms that are included. The order in which the terms are described does now reflect the equation. The following changes in the text have been made.

Before (L130 ff.):
“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.”

After:
“The thermodynamical component accounts for any tidally-induced change in the distribution of temperature within the cavity. This includes changes in heat flux upstream, tidal vertical mixing below the turbulent boundary layer (TBL) and effects of chilled meltwater from tidally-induced melting. The dynamical term represents changes in shear-driven turbulent mixing in the three equation model, and, thus, any tidally-sourced process that contributes to the speed of water flow in the cavity. This covers shear from tidal currents as well as tidal residual flow, including changes in buoyancy from tidally-induced melting.”

R1C3

One final note (apart from the comments in the pdf). There is a lot of use of the word “modulation” in the text. It's usual to reserve it for something whose strength is being varied in a pseudo-periodic fashion. In a manuscript that is primarily about the effect of tides, I would strongly recommend that the authors reserve the word for tidal modulation. So not the effect of tides being switched on or off, but the effect of the variation within the tidal signal. ie melt rates (for example) being modulated at tidal frequencies, or it could also refer to the spatial modulation of the effects of tides. But not the effect of having tides switched on or off. Perhaps words like "impact", "effect", "consequence" etc might be useful.

We agree with the reviewer and have changed all occurrences of modulation, where used to describe the impact of activating tides:

Title: “~~Tidal Modulation of~~ The Impact of Tides on Antarctic Ice Shelf Melting”

L6 ff.: “[...]. Regional variations can be an order of magnitude larger, with strong ~~melt modulations~~ melt rate changes in buttressing important regions of cold regimes. [...] In most regions, the impact of tidal currents ~~modulation of~~ on the turbulent exchange of heat and salt across the ice-ocean boundary layer has a strong contribution. [...] ”

L15: “Changes in the ocean have been identified to ~~modulate~~ impact melting at the base of Antarctic ice shelves with consequences for sea-level rise and global climate (e.g. Pritchard et al., 2012; Liu et al., 2015; Bronselaer et al., 2018).”

L22: “At the ice base, tidal currents enhance the turbulent exchange of heat and salt through the ice-ocean boundary layer and therefore ~~modulate~~ impact local melt rates as well as melt water driven buoyant plumes that affect ice-ocean interaction downstream (MacAyeal, 1984; Makinson and Nicholls, 1999).”

L133: No longer apparent due to changes under R1C2.

L141: “Tides ~~modulate~~ affect melting all around the continent (Fig. 2c), but impact ice shelf integrated mass loss mostly in cold regions where ambient melt rates are small [...] .”

L145: “Modulation at model resolution (4 km) has a standard deviation of 352 % (not shown).” has been changed to (including changes due to comment R2C7): “Melt rate changes at model resolution (4km) are larger, featuring a standard deviation of 352 % (not shown).”

L162: “The largest ~~modulation of~~ impact on ice shelf integrated mass loss (Ronne Ice Shelf) coincides with the most pronounced feature of tidal residual flow in our simulation. When activating tides in the model, a strong gyre forms on the Weddell Sea continental shelf featuring mean velocities of up to tens of centimeter per second (Fig. 3a) and temperature ~~modulations~~ differences of up to half a degree Celsius (Fig. 4b).”

L164: “When activating tides in the model, a strong gyre forms on the Weddell Sea continental shelf featuring mean velocities of up to tens of centimeter per second (Fig. 3a) and temperature ~~modulations~~ differences of up to half a degree Celsius (Fig. 4b).”

L190: “This contrasting behaviour can be well explained by friction ~~modulated~~ controlled glacial melt water input[...]”

L246: “showing that the latter two are a consequence of changes in meltwater input from ~~modulated~~ friction effects in their simulation.”

L250: “In particular, we have attributed the coherent melt increase under North-West Ronne Ice Shelf to temperature ~~modulations~~ differences outside the TBL.”

L256: “Any melt rate ~~modulation~~ difference that is indeed induced by the gyre or tidal mixing will not be captured by accounting for dynamical tidal effects on the TBL alone.”

L281: “Activating tides in the model increases total mass loss by 4 % and mass loss ~~modulations~~ differences for most ice shelves are below 10 %.”

L283: “The impact on melt rates ~~Melt rate modulation~~ 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.”

L285: “The ocean temperature of the entire continental shelf decreases by only 0.04 ◦C. Regional ~~modulations~~ differences can exceed 0.5 ◦ C including a strong warming of the Western Weddell Sea.”

Pdf comments

All remarks in the pdf were addressed other than:

L6: “Regional variations can be an order of magnitude larger, with strong melt modulations in buttressing important regions of cold regimes. ~~Further, to~~ explore the processes that cause variations in melting we apply dynamical-thermodynamical decomposition to the melt drivers in the boundary layer.”

Concept of "buttressing" and "cold regimes" needs some sort of introduction. For the abstract, should be able to get away with a comment than in some large ice shelves you see that tides strongly affect melting in regions where the ice thickness is of dynamic importance to the ice shelf.

We agree and have changed the sentence from:
“Regional variations can be an order of magnitude larger, with strong melt modulations in buttressing important regions of cold regimes.”

to:
“In some large ice shelves tides strongly affect melting in regions where the ice thickness is of dynamic importance to grounded ice flow.”

L15: “Changes in the ocean have been identified to modulate melting at the base of Antarctic ice shelves with consequences for sea-level rise and global climate [...] .” is annotated with:

use of "identified" here is a bit strange. Do you mean "shown" or do you think the evidence is not that strong? If you believe that the cited authors have proved this to be the case, you can use "shown". Or even just say "Changes in the ocean modulate melting....". Otherwise, I think you need to be more precise and say something like "There is strong evidence that changes in the ocean modulate...", or "Changes in the ocean modulate melting at the base of Antarctic ice shelves, and it is thought that this has consequences for....". That is probably most accurate - changes in the ocean definitely affect melting, but some might still dispute that this has consequences for sea level."

We agree with the reviewer that the last suggestion is most accurate and have changed the text accordingly.

L20: "One relevant mechanism is ocean tides, which interact with ice shelves in many ways including ice shelf basal melting (Padman et al., 2018)."
The reviewer suggested changing the word "including" to "influencing" here. We disagree, as we would like to imply that there are other mechanisms (besides basal melting) by which tides interact with ice shelves (such as, e.g. tidal flexure).

L47: "We force the tidal run with 13 major constituents (M2, S2, N2, K2, K1, O1, P1, Q1, MF, MM, M4, MS4, MN4) derived from the global tidal solution TPXO7.2 [...]."

This is fine, and a lot more than is necessary for this study, but it's interesting in that it omits Msf, which would presumably be the key fortnightly tide for the Weddell Sea. Put another way, was there any particular rationale adopted for this selection of constituents? Just a question - no action needed.

No, there has not been a particular rationale for this. This has historical reasons in the implementation.

L95: "Initial temperatures and salinities are also derived from ECCO2, whereby we extrapolate values under the ice shelves from ice front conditions."

This seems important. Can you state how this was done? If the extrapolation is done badly, a 5-year spin-up might not be adequate. Do you state anywhere how you determine if the model is fully spun-up?

The spin-up contains a 6-year period: 5 years at 10 km resolution and 1 year at 4 km resolution. The extrapolation has been done using nearest neighbours along sigma coordinates. We do state our measure for model equilibrium in the same paragraph: "[...] 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)". The respective text in the model development paper includes additional information about the remaining mode drift: "Interannual monthly mean melting at each resolution drifts by less than 3% at the end of the integration period, [...]." Considering that this study discusses differences, which are an order of magnitude larger (see, e.g. Fig. 2b), we rate the remaining model drift as acceptable for this study.

We have included the following additional information about the extrapolation (right behind the sentence quoted above):

“The extrapolation has been done along sigma levels and, for the horizontal dimensions, using nearest neighbours in cartesian space.”

As the spin-up procedure is the same as for the model development and evaluation, we feel that the provided reference to this paper is sufficient here. We have made no changes regarding this aspect to the text.

L130: “The dynamical component describes the mean effect of tidal current velocity as well as tidal residual flow [...]”

I think it would be useful to have a map of tidal residual flow. I would expect residual currents to be very weak, and mainly contribute via T*, ie, modifying the temperature field by contributing to the background circulation.

We do show a very similar estimate to tidal residual flow in Figure 4a, the tide induced change of the annual mean depth averaged velocity vector. From the information provided we believe that the reviewer is referring to the mean circulation caused by tide topography interaction. In previous studies this component has often been estimated using a pseudo-barotropic experiments that are forced by tides alone (see, e.g. Jourdain et al. 2019; Mueller et al. 2012; 2018). Our estimate shows changes in mean circulation due to tide-topography interaction and tide-buoyancy interaction (caused by ice shelf melting and, potentially, surface forcing or open boundary condition). It has been suggested before that tide-topography interaction is the main contributor to residual flow (Robinson 1981; Makinson and Nicholls 1999). We confirm this for our domain using a pseudo-barotropic experiment (similar to Mueller et al. 2012; see Fig. D1).

As expected, underneath the ice shelves residual currents are weak compared to the oscillating component (Fig. 3b). The only relevant feature is the tide induced gyre on the continental shelf, which is discussed later in the text.

We have included this additional study as Appendix D

“Appendix D: Tidal residual circulation

To estimate the residual circulation due to tide-topography interaction alone, we have performed a pseudo-barotropic simulation (similar to Mueller et al. 2012; 2018; Maraldi et al. 2013; Jourdain et al. 2019). For this experiment, heat and salt fluxes at the ice shelf base are set to zero, ocean surface fluxes are turned off, and we impose no velocities at the lateral boundaries other than from the tidal forcing. We use a constant density of 1027.83 kg/m3. Tidal forcing is applied as described in the main manuscript using sea surface height and barotropic currents (see Sec. 2.2). The ocean starts from a state of rest and the spin-up period is two years. Figure D1 shows the depth-averaged annual mean circulation of the pseudo-barotropic experiment.

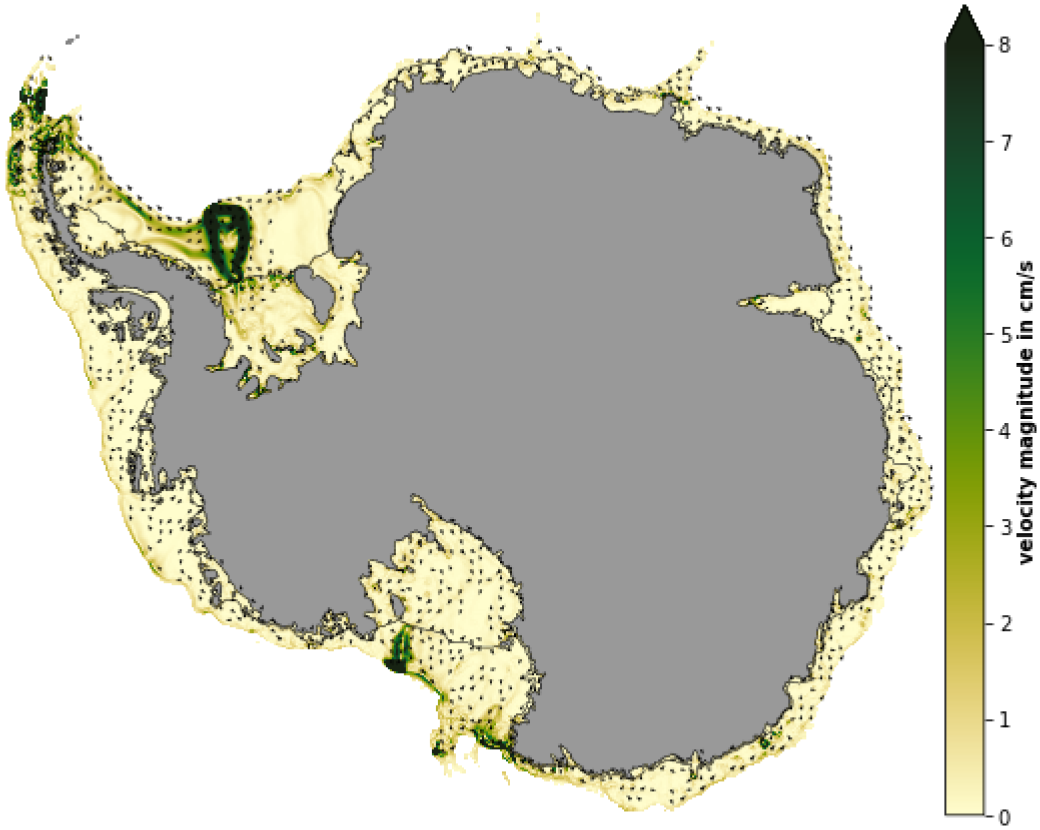


Figure D1: Tidal residual circulation from tide-topography interaction. Annual mean circulation of a tidal simulation without surface forcing, thermodynamic ice shelf interaction and stratification."

To be more clear about the relation of our estimate to residual currents, we have added a note to the caption of Figure 4:

"[...]. (a) is very similar to residual flow due to tide-topography interaction alone (see Appendix D)."

Further, we have brought the first mentioning of Figure 4 closer to its interpretation and added a note about its relation to pseudo-barotropic experiments:

L150 removed: "Figure 4a shows the sensitivity of the mean circulation to tides."

L162 Before:
"The largest modulation of ice shelf integrated mass loss (Ronne Ice Shelf) coincides with the most pronounced feature of tidal residual flow in our simulation."

After:
"Figure 4a shows the sensitivity of the mean circulation to tides. This estimate is very similar to the mean circulation of an additional experiment without thermodynamic forcing (see Fig. D1), confirming that tide-topography interaction is the main contributor to tidal residual flow (suggested by Robinson 1981; also see Makinson and Nicholls 1999). The largest

modulation of ice shelf integrated mass loss (Ronne Ice Shelf) coincides with the most pronounced feature of tidal residual flow in our simulation.[...]"

The reviewer has added this note to L130, but we have made no further changes at this point, as the paragraph is only an a priori discussion of the physical interpretation of the decomposition terms (clearly stated in the sentence before).

L131: "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)."

I have some difficulties with this. "Tidally-induced buoyant plumes" was a concept introduced by MacAyeal in the mid 80s, before full 3-D models were developed for sub-ice shelf cavities. I think we've moved beyond that concept now. Tidal currents are just one more contributor to vertical mixing of heat and salt to the ice base, and whether a plume-concept retains any value in general terms is debatable. I think you've picked out the two key contributions already - tidal residuals, which contribute to the horizontal transport of heat and salt into and around the cavities, and vertical transport of heat and salt from (in the 3-eqn formulation) turbulence induced by vertical shear against frictional boundaries. I would delete any mention of tidally-induced buoyant plumes.

We accept that a more modern understanding suggests too many coupled processes to further maintain the concept of tidally-induced buoyant plumes. Tidally induced buoyancy via ice shelf melting is part of the residual circulation. As such it is known to be able to contribute to ice shelf melting. Jourdain et al (1999), for example states that "Finally, the extra melting caused by tides induces an additional buoyancy-driven residual circulation, which in turn increases ice-shelf melting (MacAyeal, 1984b; Makinson and Nicholls, 1999)."
The literature suggests that this impact is mostly done via the transport of heat and salt without mentions of increased turbulence at the ice base (Makinson and Nicholls 1999; Padman et al. 2009; Wang et al. 2013). For many sub-ice shelf cavities, however, this study provides the first assessment of tidal effects and, hence, we should consider finding all (theoretically) possible mechanisms. Later, we do not find evidence that this process is important in our study and we do not further discuss it.

In the updated manuscript, we have changed the term "tidally-induced buoyant plumes" to "changes in buoyancy from tidally-induced melting" (see R1C2 for other changes regarding this paragraph).

In the introduction (L21-23), we now avoid the concept of tidally-induced buoyant plumes and rather attribute this process to the residual circulation.

Before:
"At the ice base, tidal currents enhance the turbulent exchange of heat and salt through the ice-ocean boundary layer and therefore modulate local melt rates as well as melt water driven buoyant plumes that affect ice-ocean interaction downstream (MacAyeal, 1984; Makinson and Nicholls, 1999)."

After:

“At the ice base, tidal currents enhance the turbulent exchange of heat and salt through the ice-ocean boundary layer and therefore modulate local melt rates as well as melt water driven residual flow that affect ice-ocean interaction downstream (MacAyeal, 1984; Makinson and Nicholls, 1999).”

L142: “Tides modulate melting all around the continent (Fig. 2c), but impact ice shelf integrated mass loss mostly in cold regions where ambient melt rates are small [...]”

why ambient? What does that refer to? Does it mean observed melt rates?

We intended to refer to the general difference in melt rates between warm and cold regimes, which is apparent in both, the tidal and non-tidal run. We acknowledge that the word “ambient” can cause confusion here and we have changed it to “typical”:

“[...] where melt rates are typically small [...]”.

L145: “Areas of increased melting are often close to areas of reduced melting or increased marine ice accretion, leading to smaller effects when considering ice shelf area averages.”

This needs to be rephrased. Perhaps say that the increased net melting for some ice shelves is partly offset by a concurrent increase in freezing in areas of marine ice accumulation? I assume this is what's meant here.

Just read the first points in the discussion, and I see that this is perhaps referring to the difficulty in isolating the effect of having tides on areas that are buttressing the ice shelf? So local effects rather than ice shelf-wide?

The discussion points the reviewer is referring to are the following: ”Our model predicts that the strongest changes in basal mass loss often occur in exactly these parts of the ice shelves [is referring to regions important for buttressing]. Within these regions, however, increased melting is often in close vicinity to equally strong reduction in melting or enhanced refreezing, making it difficult to assess the overall impact on buttressing.”

Yes, for these discussion points it is important to point out in the results that the net balancing of melt rate change occurs over spatial scales potentially small enough to impact the dynamics of the same ice stream (and not just over entire ice shelves). We have rephrased the paragraph to be more precise about this.

Before:
“Areas of increased melting are often close to areas of reduced melting or increased marine ice accretion leading to smaller effects when considering ice shelf area averages.”

After:
“Areas of increased melting are often close enough to areas of reduced melting or increased marine ice accretion to potentially impact the dynamics of the same ice stream. This net balancing also leads to smaller effects when considering ice shelf area averages.”

L166: "The potential contribution of the gyre to the coherent melt increase under the north-western part of Ronne Ice Shelf [...]"

increase as a result of having tides turned on?

Yes. We have modified the text to be more precise (additions in bold):

"The potential contribution of the **tidal** gyre to the coherent melt increase **when activating tides** under the north-western part of Ronne Ice Shelf [...] "

L170: "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 of tides (as shown in Fig. 2b)."

That's not shown in Figure 2b. Authors might mean 2c, but it's not clear there. Would be better to see a non-tidal melt map to be able to see it properly.

Yes, this was a mistake, we meant to refer to Figure 2c (absolute difference in melting at model resolution). We do not think that the inclusion of another map with melt rates from the non-tidal run (next to the tidal result and the difference map) is necessary for this point. The result about the frontal melting is only secondary in this paper (the focus of another study[2]) and can be inferred from the figures shown: If strong melting in the tidal solution is not met by an equally strong melt increase due to tides, then the melting is non-tidal.

We have added some information to clarify this point (additions in bold):

"[...] in most regions this melting is independent of tides (**not met by an equally strong increase in melting due to tides;** shown in Fig. 2b)."

L173 ff.:"Only at few places do tides contribute substantially to melting near ice fronts, for example, west of Berkner Island, east of Ross Island and under the Mertz Glacier tongue. Figure 4 shows the sensitivity of depth averaged continental shelf seas temperature to tides and, in the regions mentioned, adjacent shelf temperatures do not show significant warming. Hence, we attribute near-ice front melting at these locations to tidal advection of solar heated surface waters (proposed by Jacobs et al., 1992; see, e.g. Stewart et al., 2019, for observational evidence)."

That attribution being for the observed increase in melting in those specific locations, rather than any modelled increase? Worth making that clear.

We agree and have clarified (additions in bold):

[2] 10.31223/osf.io/stcqg

“Hence, we attribute **observed** near-ice front melting at these locations to tidal advection of solar heated surface waters [...].”

L181 f.: “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 (Fig. 4a). “

But I don't quite see how could it ever have been due to tidal residuals. Residuals are generally very small, so can only really contribute to the background circulation - the horizontal transport of heat and salt. So they can only contribute significantly to the thermodynamic component via delta T*

We agree about this point. The unimportance of turbulence at the ice base due to residual flow can be inferred before the decomposition analysis (by comparing residual flow strength, Fig, 4a, and tidal current strength, Fig. 3b).

And have removed this point from the decomposition section.

Before:
“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 (Fig. 4a).“

After:
“In most regions, dynamical effects have a major positive contribution to melting or refreezing. We associated these changes with tidal currents, as these regions also feature elevated tidal current speed (Fig. 3b).“

Also, where the residual flow strength is described, we now explicitly mention that tidal residual flow is weak compared to the oscillatory component (L 150 ff.):

“Figure 4a shows the sensitivity of the mean circulation to tides. [...] **Within the sub-ice shelf cavities, residual flow strength is typically an order of magnitude weaker than tidal currents and, hence, can only potentially play a role for tidal melting via transport of heat and salt.** [...]”

L205: “This cooling [in the thermodynamic term] might be in part a consequence of increased meltwater input from tidal current enhanced TBL transport”

How does this work? Is it because of tidally induced melting increasing buoyancy-driven flows, or tidal rectification, or both?

Neither, the mechanism we describe here is cooling due to tidal current enhanced TBL exchange (via increased melting).

While this paragraph has been removed due to other changes, the same mechanism is mentioned elsewhere (L191): “In melting regions, dynamically enhanced TBL transport causes heat loss in the uppermost ocean layer and, consequently, reduces thermal driving.”

We believe the description in L191 is more precise and the reviewer did not comment on this point, hence, no changes have been made to the manuscript.

L207: “However, in all of the aforementioned regions the adjacent water on the continental shelf cools with tides (Fig. 4b), supporting that changes in shore-ward heat transport play an important role.”

Shore-ward heat transport due to tides sounds like an active process conveying water from the open continental shelf to the ice base. That might indeed be happening, but I would have though the important process is the cooling over the continental shelf as a result of changes in the cross shelf break heat transport. Perhaps that's what the authors mean. It just reads as though the tides are transporting cold water to the cavity, rather than stopping (presumably) some warmer waters crossing the shelf break. Possibly I'm mis-reading it. Clarification with a couple of sentences would help.

The reviewer describes correctly what we had intended to communicate, that is, the respective melt rates are impacted by tide-induced changes in heat flux across the shelf break. However, we now have realised that changes in surface heat flux could also play a role. While we prescribe surface fluxes, we also use surface temperature restoring to SOSE and this is dependent on the model solution.

The original paragraph has been strongly modified due to comment R2C10, but the process in question is also described in the new text. We now avoid to imply that this is an active process by using the phrase “impacts on upstream heat flux” and we are precise about the boundaries these changes could occur:

“Coherent changes in continental shelf temperature, for example warming in front of Ronne Northwest and Western Getz Ice Shelf or cooling of the Eastern Bellingshausen Seas, indicate that tidal impacts on upstream heat flux plays an important role. These heat flux differences could take place across the continental shelf break or the ocean surface (due to our surface temperature restoring scheme).”

L217: “Diagnostic experiments with ice sheet flow models could be used to quantify the instantaneous response of tide-driven ice shelf thinning on the ice flux across the grounding lines (similar to experiments by Reese et al., 2018). Such approaches~~, however,~~ do not include longer term consequences.”

Suggested rephrasing to avoid sounding as if you are now going to go on to solve that problem in this study.

We agree that the phrasing suggests a scientific gap that we are going to address. This sentence was included to introduce the next paragraph, which discusses possible feedbacks over longer timescales. To avoid confusion, we now have pushed the sentence in question to the beginning of the next paragraph and have rephrased it:

“Diagnostic experiments with ice sheet flow models could be used to quantify the instantaneous response of tide-driven ice shelf thinning on the ice flux across the grounding lines (similar to experiments by Reese et al., 2018).
Longer term consequences will be more difficult to assess. Antarctic tides are sensitive to changes in ice shelf geometry and sea levels, offering potential feedback on ice sheet relevant timescales.[...]”

Response to Review #2

R2C1 The authors have substantially revised this manuscript in response to the original round of reviews, and overall it has substantially improved. Below I have listed several further comments and questions, largely aimed at clarifying aspects of the manuscript and the caveats to the authors' modeling approach. My most major comment concerns the authors' decomposition of the ice shelf melt rate into dynamical and thermodynamical drivers. Specifically, I am concerned that the authors' decomposition may be strongly sensitive to the order in which the difference is taken between simulations (quantifying the effect of adding tides vs. the effect of removing tides), and thus may be producing a misleading partitioning of the dynamical and thermodynamical contributions. Below I explain this concern in detail and suggest an approach that circumvents this issue. This issue has the potential to require wide-ranging changes to the manuscript's figures, text and conclusions. While I consider this an important issue, I am confident that the authors can resolve it after another round of revisions.

We thank the reviewer for this positive feedback. All further comments and questions have been addressed below. We now have included a sensitivity study, showing that the above mentioned major concern is justified and that the reviewers suggested approach is suitable to address this issue. The new results do not change the main conclusions of the paper, but require some adjustments.

Comments/questions:

R2C2 L6-7: I had to read this sentence several times to parse it properly. I suggest rephrasing.

The sentence referred to is:

"Regional variations can be an order of magnitude larger, with strong melt modulations in buttressing important regions of cold regimes.",

Which has already been rephrased using suggestions from reviewer 1 (pdf annotations L6) to:

"In some large ice shelves tides strongly affect melting in regions where the ice thickness is of dynamic importance to grounded ice flow."

R2C3 L86-88: The prescribed surface fluxes are "accurate" in the sense that they are consistent with observations, but I am not convinced that this is an "advantage", as the fluxes may be inconsistent with the simulated state of the ocean. For example, negative heat tendencies may be applied to waters that are already at the surface freezing temperature, producing super-cooled waters (presumably the authors have implemented a fix for this issue). Or surface currents could be accelerated by the imposed momentum fluxes in regions where the sea ice is thick and largely immobile, and should be retarding the near-surface flow. I do see the advantage in ensuring that the area-integrated buoyancy gain and loss is correct in different parts of the continental shelf, but I find it misleading to describe this as "accurate". The authors should rephrase the text to be more candid about the advantages and disadvantages of this approach.

The advantages and disadvantages of prescribing surface fluxes in comparison to including a sea-ice model has been discussed during the revision of the model development paper[3] and the first revision of this study[4]. This discussion has led to the careful formulation of "accurate surface salt flux locations and strength from sea ice polynyas" rather than saying "accurate polynyas" or "accurate fluxes from polynya activity". However, we acknowledge that the word "advantage" might still convey that our approach is superior to including a sea ice model when simulating ice shelf melting. We now have rephrased this paragraph to avoid the word "advantage". Further, we have added an explanation about why missing sea-ice ocean interaction can be a problem. This way we now present a better balance between the advantages and disadvantages of our approach in this manuscript (in addition to the model development paper). Further details, including code fixes to compensate for missing sea ice coupling, are described in the model development paper to which we refer to.

We have changed the text from:
"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)."

to:
"Prescribing surface buoyancy fluxes rather than including a sea ice model ensures accurate surface salt flux location and strength from sea ice polynyas. However, discrepancies between the fluxes that correspond to sea ice formation or reduction and the underlying ocean state can lead to the creation of artificial water masses, which can only be compensated in part without full sea ice interaction (for further details and discussion see Richter et al., 2020)."

Missing wind stress modulation by sea ice has already been acknowledged a few lines below (L93): "We do not account for the effect of sea ice on wind stress or include an explicit model of frazil ice (as in, e.g. Galton-Fenzi et al., 2012)." No further changes have been made to the text.

[3] see R1C12 in AC1: 'Comment on gmd-2020-164', Ole Richter, 18 Mar 2021; https://gmd.copernicus.org/preprints/gmd-2020-164/gmd-2020-164-AC1-supplement.pdf
[4] See R2C11 in Author's Response from 23 July 2021

R2C4 L87-94: Have the authors compared the buoyancy fluxes that result from these modifications with the buoyancy fluxes derived from Tamura et al. 2011? Do they substantially modify the surface fluxes in any particular regions?

We have not done a comprehensive analysis of the effects of our modifications, but rather focused on model stability and evaluating the resulting ocean state. We do have, however, checked that our surface salt fluxes in winter compare well with the observations by Tamura et al. (2011; compare Fig. R1 below with their Fig. 2f). This fact is also stated in the model development paper[5] and Fig. R1 is also shown in Ole Richter's publicly available PhD Thesis. Salt fluxes in winter are arguably the most important ones for ice shelf ocean interaction. The precise effects of our modifications (compensating for the missing sea ice coupling and surface flux nudging to SOSE) would be worthwhile to determine in future studies that aim to improve WAOM. As this is a matter of model development, we have not included suggestions in this paper, which is concerned with the effect of tides.

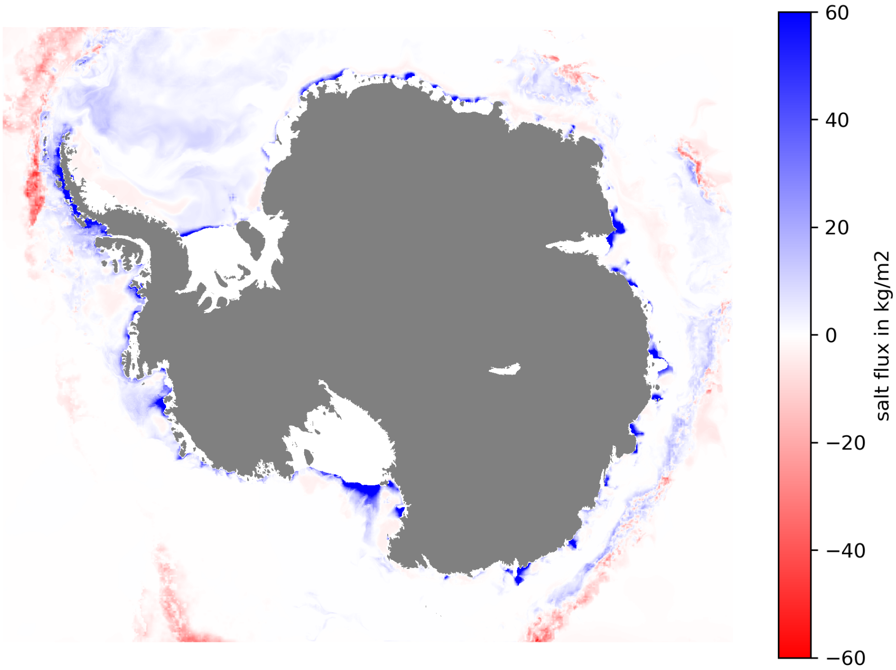


Figure R1: WAOM's surface salt flux integrated over June, July and August 2007. Positive is into the ocean. The values are derived from model output, that is after relaxation to surface salinity from SOSE and tuning has been applied. The colorbar has been scaled to ease a comparison against observational estimates from Tamura et al. (2011; see their Fig. 2f).

No changes have been made to the manuscript.

[5] See *Changes unrelated to reviewer comments*: *Discussion: Biases* in AC1: 'Comment on gmd-2020-164', Ole Richter, 18 Mar 2021; https://gmd.copernicus.org/preprints/gmd-2020-164/gmd-2020-164-AC1-supplement.pdf

R2C5 L106: Please specify which month(s) the 30-day analysis period occurs in. The authors should also discuss potential sensitivities to performing this analysis in summer vs. winter months. If the analysis is performed over multiple 30-day periods to span the full simulation duration then this should be stated.

The 30-day period is January and this is specified 6 lines earlier (where we talk about spin-up and integration times, L100): “Annual average and decomposition results were derived from the final year of the 4 km simulations, while mean tidal current speed was based on an additional subsequent 30-day integration (January) of the tidal case.”
To investigate the impact of seasonality on tidal strength, we now have calculated tidal current speed in July and compared this estimate with the previous one from January (see Figure R2 below). In most regions, the difference is an order of magnitude smaller than the absolute values (see Figure 3 in the manuscript). We use our estimate of tidal current strength for qualitative comparisons of spatial characteristics, e.g. with the dynamical term of the decomposition analysis. Hence, for the purpose of this study, the seasonal variation in tidal strength is negligible.

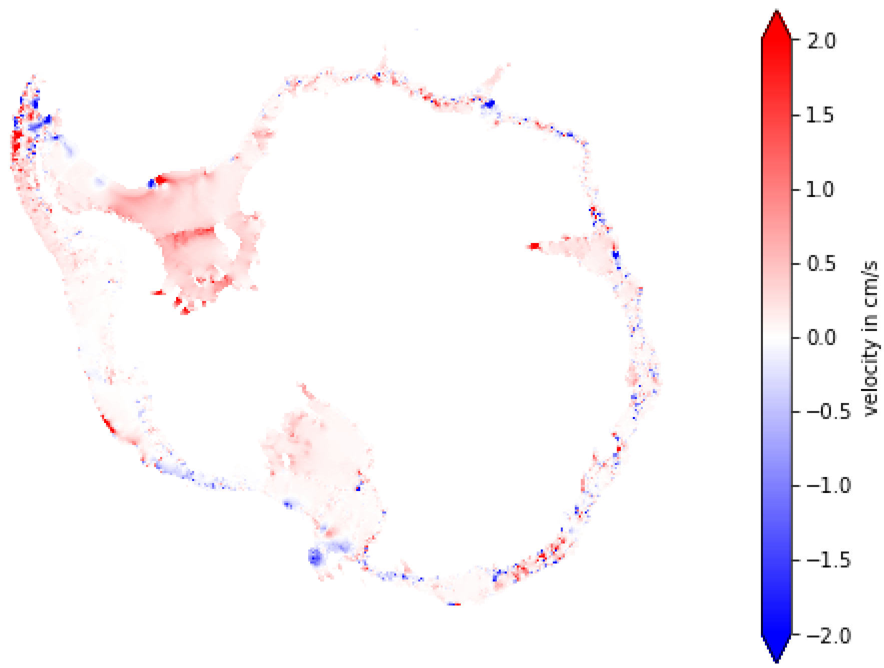


Figure R2: Seasonal impact on tidal current speed. Difference in mean speed of the oscillating tidal current (calculated following Eqn. 1 in the manuscript) between winter and summer (July - January). The seasonal impact is generally an order of magnitude smaller than the absolute values (Figure 3b).

We have added a clarifying sentence to the manuscript (additions in bold):

“With 30 days we cover 2 full spring-neap cycles of the major semidiurnal and diurnal tidal constituents M2, S2, K1 and O1. Tidal currents typically reach a maximum speed of 2 |u|tide. **We find that the seasonal variation in tidal current speed is typically an order of magnitude smaller than the absolute values (not shown) and, hence, negligible for the purpose of this study.**”

R2C6 L122: “inset”

We believe there is a typo and the reviewer meant “insitu”. This line is no longer apparent due to earlier changes (R1C1) and this mistake does not occur elsewhere in the manuscript.

R2C7 L145: I do not understand this sentence: please clarify.

The sentence in question is the following (highlighted): “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).”

We are describing the impact of tides on melt rates at different spatial scales here - first ice shelf averages and then at model resolution (4 km). We use the standard deviation to convey that differences at model resolution are much larger than the impact on ice shelf averages.

We have modified the sentence in question to be more clear:

“Melt rate changes at model resolution (4km) are larger, featuring a standard deviation of 352 % (not shown).”

R2C8 Fig. 2: I suggest using fewer color graduations in the colorbar to make it easier to read values in these plots, particularly panel (b).

We agree for the ice shelf averages (panel b) and are now using a discontinued colorbar. For the other panels we prefer the continuous style.

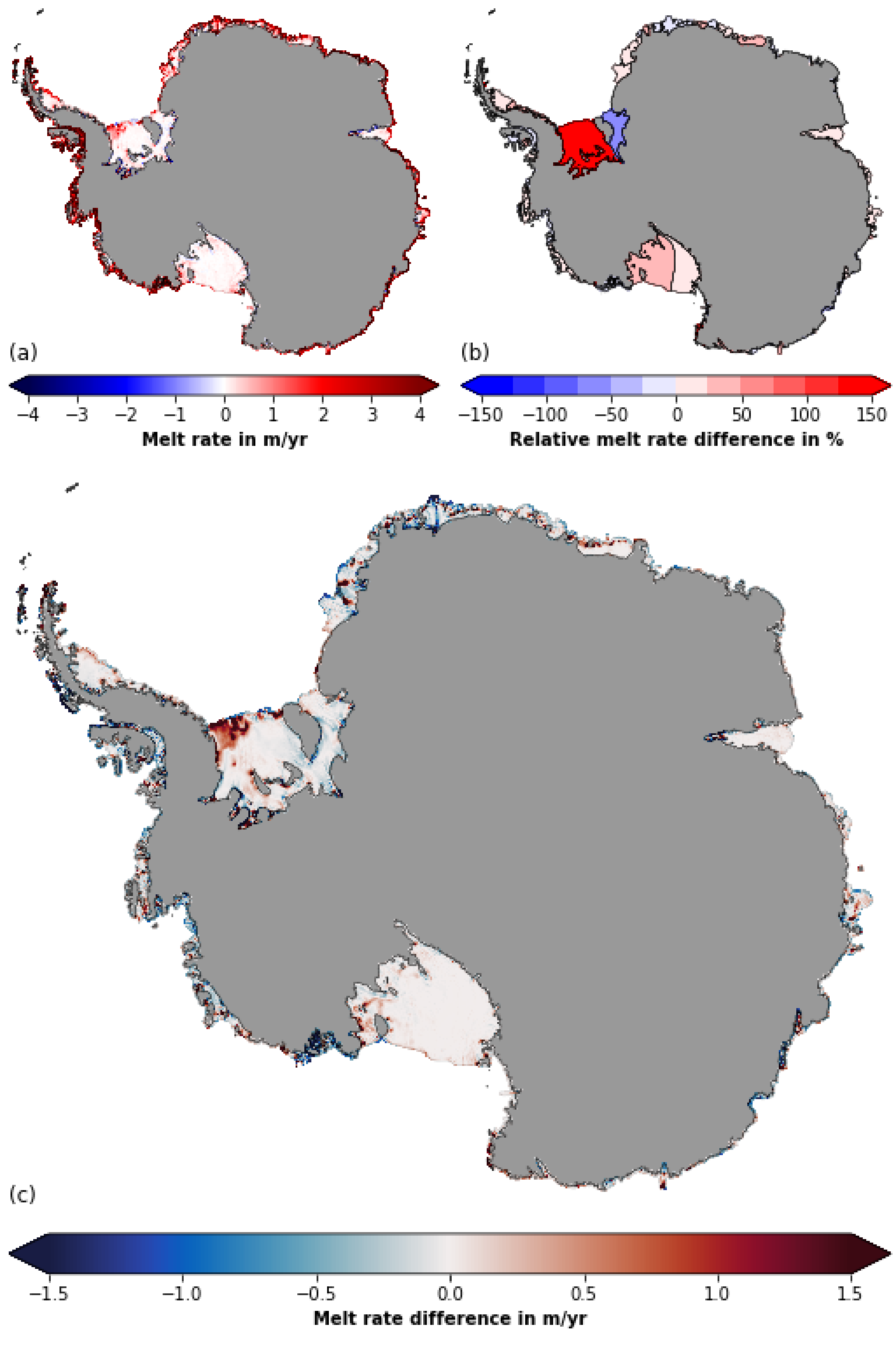


Figure 2. Tidal melting of Antarctic ice shelves. a) Annual average ice shelf melting for the case with tides, b) its relative difference to the case without tides averaged over individual ice shelves ([Tides−No-Tides]/No-Tides) and c) its absolute difference to the case without tides (Tides − No-Tides).

R2C9 Fig. 4: I assume that the barotropic velocity here is defined as the magnitude of the depth-averaged velocity vector - is this correct? In panel (b), how is the continental shelf potential temperature defined - surface, bottom, or depth-average (or something else)?

Yes, it is the magnitude of the depth averaged velocity vector. The potential temperature is also depth averaged. We have clarified this in the caption.

Before:
"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.[...]"

After:
"Tide induced change in (a) magnitude and direction of the depth-averaged velocity vector and (b) depth-averaged 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. [...]"

R2C10 L180-188: I am concerned that the authors' formulation of the dynamical/thermodynamical decomposition may be producing misleading results here. The core issue is that their decomposition (eqn. (5)) may produce qualitatively different results depending on whether they subtract the no-tides (nt) case from the tides (t) case or vice versa. Consider these two approaches:

Approach 1, quantifying the change in heat flux due to adding tides:
w_b,t - w_b,nt ~
u_nt (Delta t) <thermodynamical>
+ (Delta u) T_nt <dynamical>
+ (Delta u) (Delta T) <covariational>

Approach 2, quantifying the change in heat flux due to removing tides:
w_b,nt - w_b,t ~
- u_t (Delta T) <thermodynamical>
- (Delta u) T_t <dynamical>
+ (Delta u) (Delta T) <covariational>

In each case Delta u = u_t - u_nt and Delta T = T_t - T_nt.

Clearly the covariational term is insensitive to the order of the subtraction. However, the thermodynamical and dynamical terms may be. For example, in order for the thermodynamical terms to be comparable, we require

u_nt (Delta_t) ~ u_t (Delta_t)
=> u_nt/u_t ~ 1
=> Delta t << u_nt, u_t

However, the addition of tides is likely to create situations in which u_nt is approximately zero, but u_t is on the order of 10 cm/s. In such situations, Approach 1 will yield a very weak thermodynamical contribution (perhaps explaining Fig. 5c), whereas Approach 2 will yield a large thermodynamical contribution.

For a concrete example, take
u_t = 0.11 m/s
u_nt = 0.01 m/s
T_t = 0.5 deg C above freezing
T_nt = 0.3 deg C above freezing
Delta u = 0.1 m/s
Delta T = 0.2 deg C

Approach 1 yields
<thermodynamical>: 0.002
<dynamical>: 0.03
<covariational> 0.02
Total: 0.052

Approach 2 yields
<thermodynamical>: - 0.022
<dynamical>: - 0.05
<covariational> 0.02
Total: - 0.052

With approach 1, we conclude that <thermodynamical> is an order of magnitude weaker than the other contributions. With approach 2, we conclude that <thermodynamical> is comparable to the other contributions.

It is not clear whether the authors have considered this, so I would ask that they revisit their decomposition in this in mind as they revise the manuscript. My suggestion would be that they decompose the heat flux by defining

u_m = (u_t + u_nt)/2 and T_m = (T_t + T_nt)/2,

and

w_b,t - w_b,nt ~ (u_m + Delta U/2)(T_m + Delta T/2) - (u_m - Delta U/2)(T_m - Delta T/2)
= u_m (Delta T) <thermodynamical>
+ (Delta u) T_m <dynamical>

This eliminates the covariational component and ensures that the thermodynamical and dynamical components are insensitive to the order of subtraction.

We thank the reviewer for this valuable and well explained comment. We have performed an additional analysis to test the sensitivity of the results to this issue. The contributions of the individual terms are sensitive to the chosen reference state (tidal, non-tidal or central) as suggested by the reviewer. This finding implies that the reference state must be chosen carefully and hints towards limitations of model perturbation studies. As suggested by the reviewer we now adopt the central approach for the purpose of this study and also communicate the sensitivity to adopting other approaches (Supp Fig C1). The main conclusions of the paper remain valid, as the dynamical contribution is still dominant in most regions.

Changes in the appendix:

We have included the sensitivity study in the appendix, including a recommendation for future work on the limitations of model perturbation experiments:

“Appendix C. The importance of the reference state for the dynamical-thermodynamical decomposition

The results of the dynamical-thermodynamical decomposition are sensitive to the choice of the reference state. With our experiments, three different approaches can be considered.

Approach 1 uses the non-tidal case as reference:

$$\begin{aligned} & w_{b,T} - w_{b,NT} \propto \\ & \overline{u^*_{NT} \Delta T^*} && \text{(thermodynamical)} \\ & + \overline{T^*_{NT} \Delta u^*} && \text{(dynamical)} \\ & + \overline{\Delta u^* \Delta T^*} && \text{(covariational).} \end{aligned}$$

Approach 2 uses the tidal case as reference:

$$\begin{aligned} & w_{b,T} - w_{b,NT} \propto \\ & \overline{u^*_{T} \Delta T^*} && \text{(thermodynamical)} \\ & + \overline{T^*_{T} \Delta u^*} && \text{(dynamical)} \\ & + \overline{\Delta u^* \Delta T^*} && \text{(covariational).} \end{aligned}$$

In Approach 3 we define a mean state between the tidal and the non-tidal case:

$$u^*_m = (u^*_T + u^*_{NT})/2$$
$$T^*_m = (T^*_T + T^*_{NT})/2$$

and develop the difference around this mean state:

$$\begin{aligned} & w_{b,T} - w_{b,NT} \propto \\ & (u^*_m + \Delta u^*/2)(T^*_m + \Delta T^*/2) - (u^*_m - \Delta u^*/2)(T^*_m - \Delta T^*/2) = \\ & \overline{u^*_m \Delta T^*} && \text{(thermodynamical)} \\ & + \overline{T^*_m \Delta u^*} && \text{(dynamical).} \end{aligned}$$

In each case

$$\Delta u^* = u^*_T - u^*_{NT}$$
$$\Delta T^* = T^*_T - T^*_{NT}\ .$$

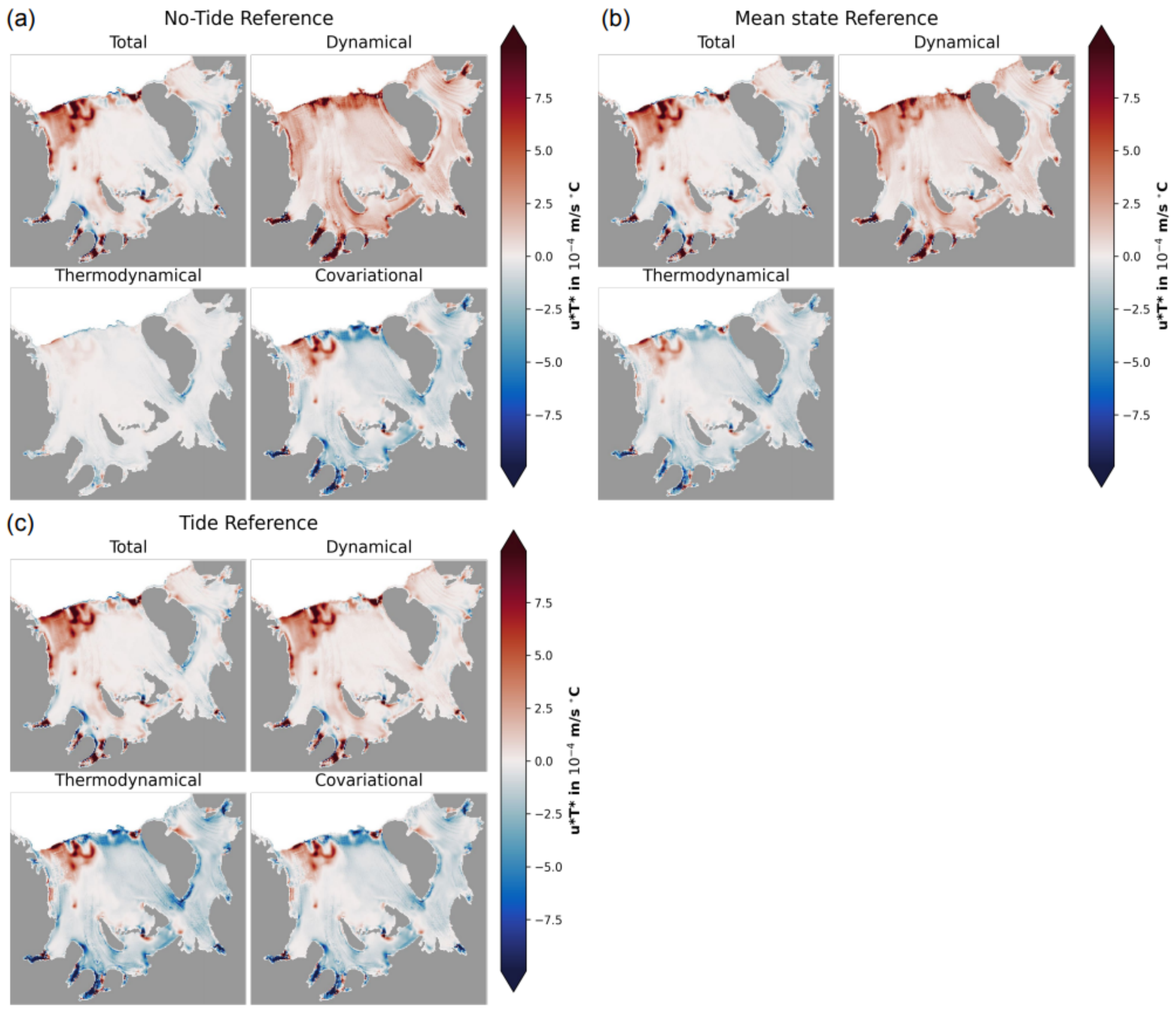
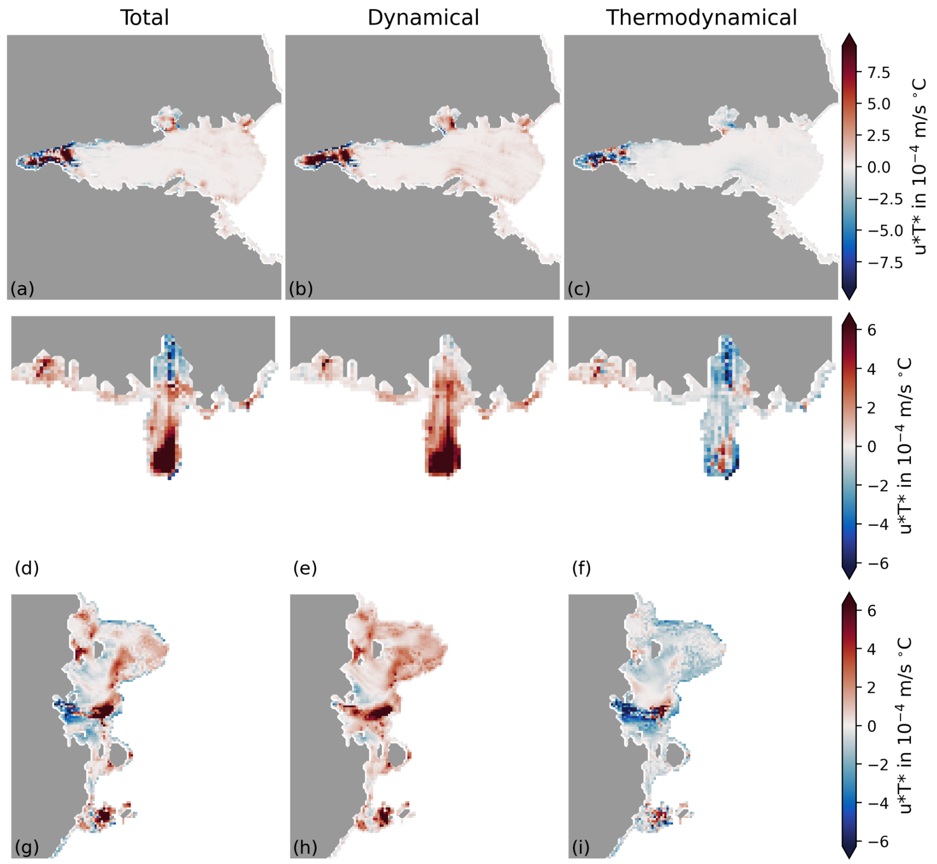


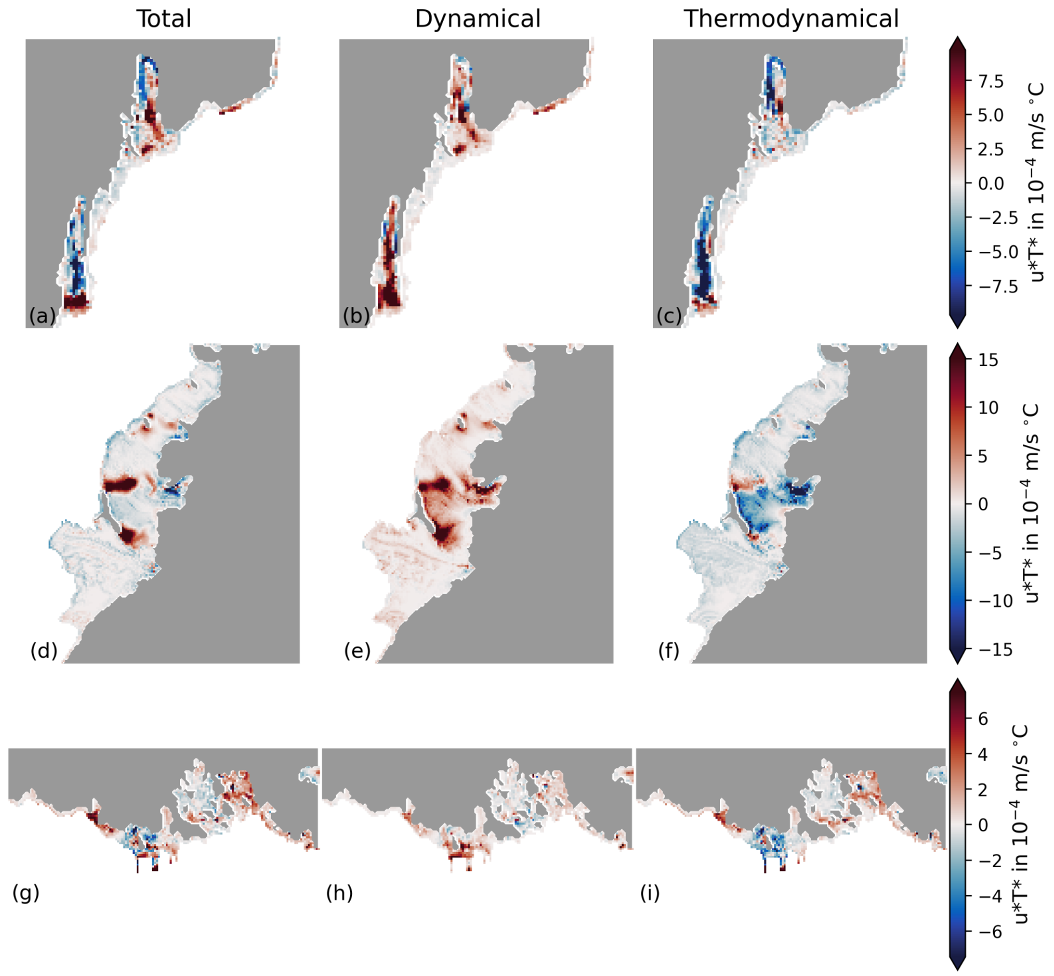
Figure C1. Dynamical-Thermodynamical Decomposition: The impact of the reference state. Contributions of thermodynamical, dynamical and covariational effects to tidal melting when choosing (a) the no-tide experiment, (b) the mean between no-tide and tide case and (c) the tide experiment as reference. The mathematical descriptions for each case are shown in equation 1 to 3, also explaining why the central case has no covariational contribution. For the Tide Reference case the negative of the total, dynamical and thermodynamical contributions have been plotted for better comparison. The contributions are sensitive to the choice of the reference.

Figure C1 shows the results of the decomposition analysis for all three approaches for the Filchner-Ronne Ice Shelf. The contributions of the individual components are qualitatively different, exhibiting progression in the thermodynamic and dynamic terms when going from the non-tidal to the central to the tidal reference state (or vice versa). We attribute this behaviour to approximation errors that occur when using linear methods to model a large perturbation (tides on/off) in a highly non-linear system (ocean-ice shelf interaction). This study aims to understand the processes that are responsible for the difference between the two states and, thus, we approximate using the mean. This choice has the advantages of being direction invariant (the removal of tides leads to the exact negative) and a disappearing covariational term, which simplifies the interpretation. However, the other cases might be more useful in other studies. For example, when developing a tidal-melt parameterisation that is applied to non-tidal models (as done by Jourdain et al. 2019), the non-tidal case as the reference state might be the most straightforward approach. Similarly, to understand what models without tides miss out on, choosing the tidal state as reference seems logical. We encourage future studies to pick up on these findings and lead a comprehensive discussion about the limitations of perturbation experiments using models of highly non-linear systems. For example, the realism of the non-linear influence of tidal parameterisations may be assessed with a similar perturbation approach."

In appendix B we have updated the figures that show decomposition results now using the central reference case (Fig. B1 to B2; in addition to the regions shown in the main text). Note that some statements in the results now point to different regions and we have included them here.



"Figure B1. Dynamical-Thermodynamical decomposition of tidal melting under Amery (a to c), Mertz (d to f) and Shackleton Ice Shelf (g to i; see Sec. 2.3)."



"Figure B2. Dynamical-Thermodynamical decomposition of tidal melting under the Totten-Moscow University Ice Shelf system (a to c), Riiser-Larsen Ice Shelf (d to f) and the ice shelves of the eastern Ross Sea (g to i; see Sec. 2.3)."

Changes in the methods:

In the methods section, we now describe approach 3 (the central reference case), state our choice regarding the reference case and refer to the appendix for further discussion.

Before (L122 ff.):
"The approximation of melt rate variability using friction velocity and thermal driving (Eq. 2) allows to decompose melt rate difference between the tidal and non-tidal experiment into a dynamical, thermodynamical and covariational component (equivalent to Jourdain et al., 2019, their Eq. 5):

$$\begin{aligned} w_{b,T} - w_{b,NT} &\propto \overline{u_T^* T_T^*} - \overline{u_{NT}^* T_{NT}^*} \\ &= \overline{(u_{NT}^* + \Delta u^*)(T_{NT}^* + \Delta T^*)} - \overline{u_{NT}^* T_{NT}^*} \\ &= \overline{u_{NT}^* \Delta T^*} + \overline{\Delta u^* T_{NT}^*} + \overline{\Delta u^* \Delta T^*}\,. \end{aligned}$$

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

$$\Delta u^* = u_T^* - u_{NT}^*$$
$$\Delta T^* = T_T^* - T_{NT}^* \,.$$

After:
“The approximation of melt rate variability using friction velocity and thermal driving (Eq. 2) allows us to decompose the melt rate difference between the tidal and non-tidal experiment into dynamical and thermodynamical components. First, we define a mean state between the tidal and non-tidal case:

$$u_m^* = (u_T^* + u_{NT}^*)/2$$
$$T_m^* = (T_T^* + T_{NT}^*)/2 \,,$$

to then develop differences around the mean state:

$$\begin{aligned} & w_{b,T} - w_{b,NT} \propto \\ & (u_m^* + \Delta u^*/2)(T_m^* + \Delta T^*/2) - (u_m^* - \Delta u^*/2)(T_m^* - \Delta T^*/2) = \\ & \overline{u_m^* \Delta T^*} && \text{(thermodynamical)} \\ & + \overline{T_m^* \Delta u^*} && \text{(dynamical).} \end{aligned}$$

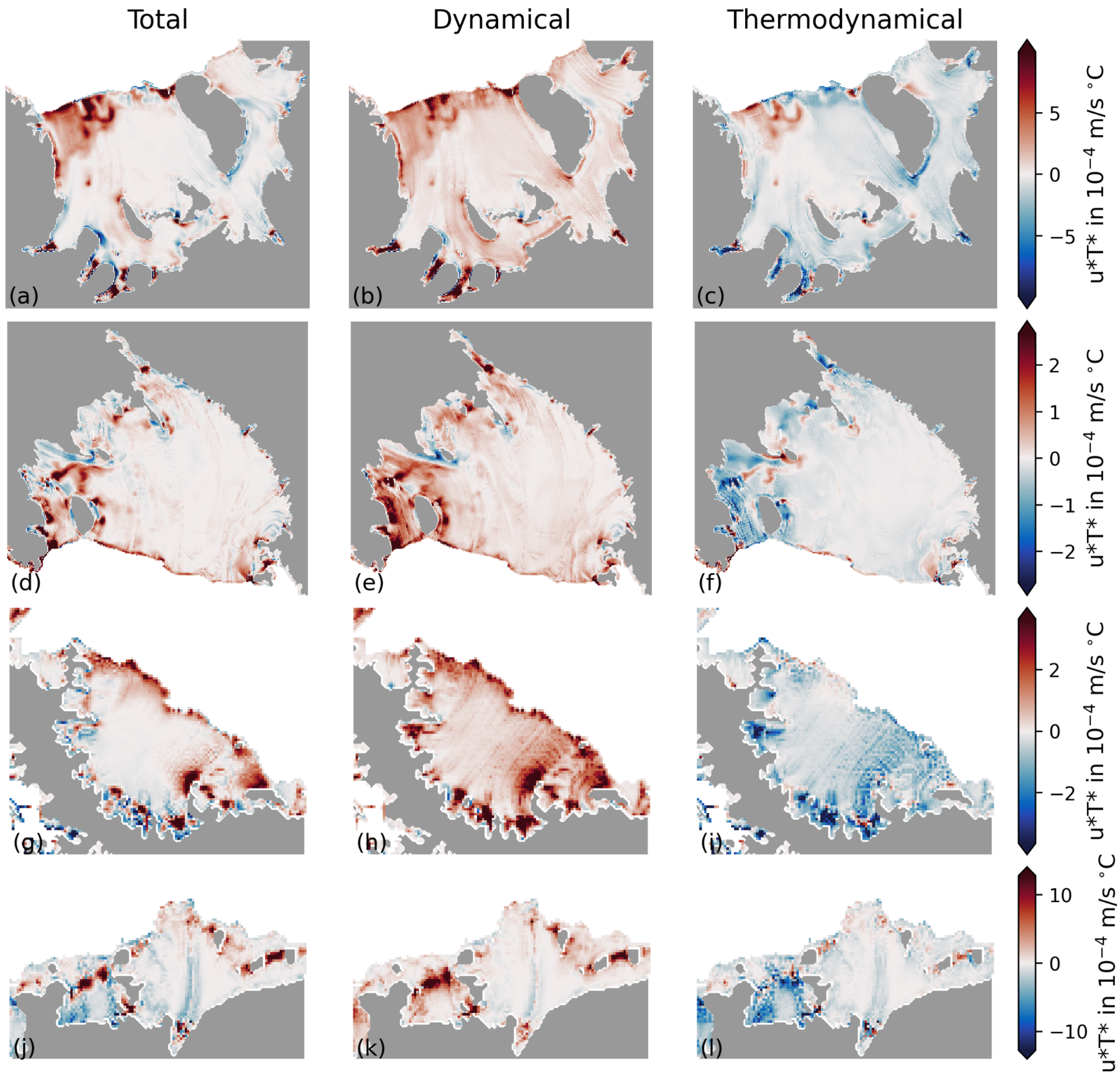
Here, the overbar denotes temporal averaging, the Δ describes the difference between the tidal (T) and non-tidal run (NT):

$$\Delta u^* = u_T^* - u_{NT}^*$$
$$\Delta T^* = T_T^* - T_{NT}^* \,.$$

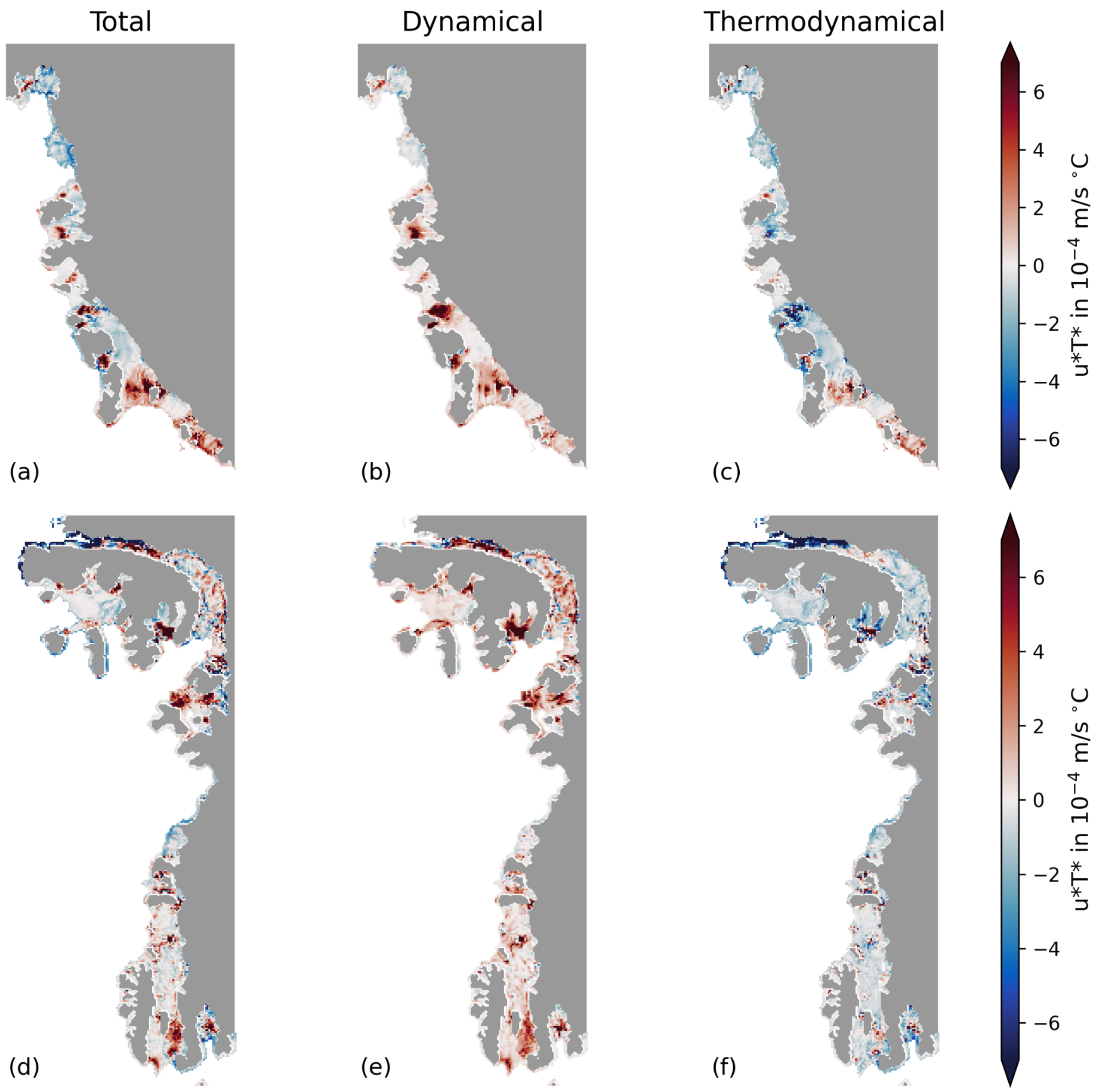
We approximate using the mean, as this study aims to understand the processes that are responsible for the difference between the tidal and non-tidal state of the model (see SUP for further discussion). [...] ”

Changes in the results section

In the results, we have updated the figures (Fig. 5 and 6) to now show the outcome of the central reference case. We now show more regions in the main text for ease of readability. For some statements we now point to different regions and we have labelled these in Fig. 1. We have adapted the interpretation of the individual terms. In addition, we now find that the thermodynamical contribution correlates better with the change in depth averaged temperature (Fig. 4b), which allowed us to speculate about the exact drivers.



“Figure 5. Dynamical-Thermodynamical decomposition of tidal melting in cold regimes. Difference in melting, when accounting for all components, only the dynamical component and only the thermodynamical component for Filchner-Ronne (a to c), Ross (d to f), Larsen C (g to i), and Jelbart and Fimbul Ice Shelf (j to l; following Eq. 5).”



"Figure 6. Dynamical-Thermodynamical decomposition of tidal melting in warm regimes. Difference in melting, when accounting for all components, only the dynamical component and only the thermodynamical component for the ice shelves of the Amundsen (a to c) and Bellingshausen Sea (d to f; following Eq. 5)."

L177 ff. Before:
"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 Figure B1 to B7."

After:
"Figure 5 and 6, respectively, show the results of this decomposition for some key regions, organised into cold and warm regimes. The results for other regions of interest are presented in the supplemental material as Figure B1 and B2.

L185 ff. (additions in bold): ”In warm regimes dynamic effects are more pronounced at the trunk of ice shelves (e.g. under **Dotson and Eastern** Getz Ice Shelf, or under Bach and Abbot Ice Shelf**, Fig. 6b and e)**. 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~~ **Pine Island and Thwaites** Ice Shelf).”

L189 ff. has changed from:
“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).”

To:
“The thermodynamical contribution often opposes dynamical effects (see, e.g. Sipple Coast under Ross Ice Shelf, Larsen C Ice Shelf, Fig. 5f and i, Dottson and Eastern Getz Ice Shelf, Fig. 6c). This contrasting behaviour can be well explained by friction controlled glacial melt water input.”

L194-207 has changed from:
“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. 3), 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 and eastern 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 water on the continental shelf cools with tides (Fig. 4b), supporting that changes in shore-ward heat transport play an important role.”

To:
“In some regions, however, melt rate contributions do not follow this pattern. For example, under large parts of North-West Ronne Ice Shelf or under the Western half of Getz Ice Shelf the thermodynamical and dynamical terms are both positive. Further, a thermodynamically driven reduction in melt, which exceeds dynamic effects, is apparent under large parts of Pine Island and Thwaites Ice Shelf, under Eastern George V Ice Shelf (Fig. 6f) and within deeper parts of the cavities under Fimbul (Fig. 5l), Mertz and Shackleton (Fig. B1f and i) and the Totten-Moscow University Ice Shelf System (Fig. B2c). In such regions, thermodynamical

contributions can not be explained as a dynamical consequence alone. Here, some insights into the thermodynamic drivers can be derived considering tide induced temperature change (Fig. 4b). Coherent changes in continental shelf temperature, for example warming in front of Ronne Northwest and Western Getz Ice Shelf or cooling of the Eastern Bellingshausen Seas, indicate that tidal impacts on upstream heat flux plays an important role. These heat flux differences could take place across the continental shelf break or the ocean surface (due to our surface temperature restoring scheme). In contrast, some parts of, e.g. Jelbart (Fig. 5l) Totten, Riiser-Larsen and Nickerson (Fig. B2c, f and i), and Mertz and Shackleton Ice Shelf (Fig. B1f and i) exhibit a strong thermodynamic reduction in melt and a cooling that is confined to these parts within the cavity. This signature is likely related to tidal vertical mixing, that lifts heat into contact with the ice and consequently cools the water column though melt water production."

Changes in the discussion and conclusion

The dynamical component is still the dominant term in many regions, and, hence, the discussion and conclusion only needed few modifications (e.g. accommodating the strengthened evidence for the importance of tidal vertical mixing; additions are in bold).

L242-248: "[...] 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~~ **is** a consequence of changes in meltwater input from friction effects in their simulation. In our study, the dynamical component also plays an important role in most regions and ~~covariational~~ **thermodynamical** effects **in these regions** can, to a large degree, be explained as a dynamical consequence (see Fig. 5 and 6)."

L249-259: "In some regions, however, thermodynamic drivers govern the melt change. [...] **Further, we have identified several regions, where tidal vertical mixing below the TBL offers the best explanation for the resolved changes.** Any melt rate difference that is indeed induced by the gyre or tidal mixing will not be captured by accounting for dynamical tidal effects on the TBL alone. [...]"

L287-290: "Thermodynamic driven changes due to mixing or residual flow play a role in some regions, but **the importance of residual flow** might be overestimated due to biases in the control run."

R2C11 L189: "dipol"

This line has been removed due to changes under R2C10 and this issue does not apply elsewhere.

R2C12 L209: “While circum-Antarctic total melt is small” - is it? Did the authors mean to refer to the change in melt due to tides instead?

The reviewer is correct. As suggested by reviewer 1 (pdf annotations), we have already changed this to:
“While the impact of tides on circum-Antarctic total melt is small, [...].“

R2C13 L213: “strongest changes” - meaning changes due to the introduction of tides? This needs to be clarified because as currently written this is likely to be misinterpreted as changes with time.

Okay. We have added “driven by tides” to the sentence to be more precise:

“Our model predicts that the strongest changes in basal mass loss driven by tides often occur in exactly these parts of the ice shelves.”

References

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