Authors Response to Review 2 of "Brief Communication: Representation of heat conduction into the ice in marine ice shelf melt modeling"

RC = Reviewer Comment (in italics) ; AR = Authors Response

***RC**: "This paper presents a brief report on the representation of heat conduction into the overlying ice shelf in models of sub-ice circulation. While that is potentially valuable, in that it makes a comparison of different approaches readily accessible, the paper is largely a reiteration of material that is already in the literature, and I struggled to see much added value. It is a pity that what would be the main contribution of the study (a series of model runs that show the differences in computed melt rate resulting from the use of the different approaches) is mentioned only briefly with no in-depth analyses of the results. The reader must refer to an earlier paper even to see the model setup. That suggests that this brief summary would have been more appropriate as an Appendix or Supplement to that earlier paper. In its present form, I don't think it makes a sufficiently significant contribution to warrant publication as a separate paper in The Cryosphere."*

AR: We thank the reviewer for taking the time to review our manuscript. We are sorry to read that they struggled to see the novel insights brought by our manuscript. We do believe that there are novel aspects in our study, but the reviewer's comments probably mean that we did not sufficiently emphasise these aspects, and this will be corrected in the revised manuscript. In short, the main novel aspects are:

- Assessment of all methods currently used in ocean models to account for heat conduction into the ice with respect to observed ice temperature profiles at three locations (Fig. 1). Previous theories (e.g. Wexler 1960) were developed using borehole temperature measurements, but usually at one location, with one type of profile, and as far as we know, this is the first time that three very distinct types of temperature profiles are used together for such an assessment.
- As far as we know, Fig. 3 is the first map providing estimates of the impact of heat conduction on melt rates at the scale of Antarctica. Previous studies only mentioned an effect of the order of 10%, but our estimates show that it varies from one place to another. There are of course caveats in our estimation, and this will be better discussed in our revised manuscript.
- A series of ocean model simulations to compare the 3 methods in realistic conditions, with possible feedbacks between the ocean properties and heat conduction. Although this is one of the novel aspects, we do not agree that this is the only one or even the most important one as the model results largely confirm the theoretical calculations (i.e., feedbacks are not very important).

One of our motivations for writing such a paper was that we realised in several workshops that many ocean modelling groups used a heat-conduction scheme for random or historical reasons, without knowing whether this was a good choice. Arguably, a lot was already done in Holland and Jenkins (1999), but there is so much material in that paper that it is not obvious to extract the main information on heat conduction, and there is no evaluation vs observational profiles in that paper. So our objective with this submission is to issue a clear statement on the best way to parameterise heat conduction in ocean models resolving ice-shelf cavities, and of course on the remaining caveats. We consider that putting all this information in a large Appendix section (as suggested by the reviewer) would not have been a good way to achieve this objective.

We nonetheless agree that some aspects need to be improved, as detailed in our answers below.

RC: *"In addition to the absence of significant findings, there are a number of issues that should be addressed in any rewriting:"*

AR: We thank the reviewer for these interesting comments. We plan to address them as follows:

1. **RC:** *"In line 105 (and elsewhere) there is mention of the "error" made by two of the approximations, but I assume that "error" estimate comes from a comparison with the third approximation. That suggests an implicit, but unfounded, assumption about the correctness of the third approximation. Likewise Figure 3 shows differences between two approximations, but neither is correct, and the evidence needed to favour one or the other isn't shown. Herein lies the main weakness of the study in that there is no correct answer with which any of the approximations can be compared. If the authors really want to make a definitive statement about which approximation gives the best results, those approximations should be compared with the results of a full model of heat advection and diffusion in the ice shelf. I realise that makes for quite a different study, but without that, nothing authoritative can be said."*

   AR: We agree that using a full ice-sheet model with heat advection and diffusion would be an interesting approach (we already had this statement in the discussion of our initial manuscript), even though it would still be dependent on the ice sheet model parameters. Here, we nonetheless chose an alternative approach, which consists of looking at which parameterisation is consistent with the ice temperature profiles measured through three very different ice shelves. We find that Approximation (C) is the only reasonable one, which is why it is used as a reference in Fig. 3. In Fig. 2, we also evaluate Approximation (C) with respect to the more complex formulation of Holland and Jenkins (1999), but this is still assuming that ice advection can be approximated as a downward motion that exactly compensates basal melting.

2. **RC:** *"Perhaps a partial solution would be to show and discuss in more detail the results that are briefly mentioned in lines 97-107, and figures 2b,c. While still not a demonstration of how good or bad the various approximations are, that does at least give a demonstration of how influential the possible errors are on the results of an ocean model. Ideally other simulations would be added to show the impact in a range of ice shelf environments. The simulations are described as "idealised", but if*

real ice shelf geometry were used, it might be possible to compare results with melt rates inferred from observation. While there could be many other causes for a model/observation mismatch, that would give an idea of how large the uncertainties are compared with other sources of error. That might lend support to the statements in lines 113-120 that suggest the use of approximation (C) might be preferable to making other adjustments to the model, a statement that at present is not backed up by evidence."

AR: We do not believe that circum-Antarctic ocean simulations are good enough to attribute any model bias to the misrepresentation of heat conduction. Biases in bathymetry, ice topography, atmospheric forcing, sea-ice model, tides, ice roughness, etc, would a priori lead to similarly large errors in simulated ice-shelf melt rates. In our study, the main added value of the ocean simulations are to (1) test a practical implementation, (2) check that there are no additional ocean processes leading to any nonlinear feedback that would change the effect of the parameterised heat conduction.

3. **RC:** *"The authors seem to base their preference for approximation (C) on its ability to simulate the effect of temperature profiles observed in ice shelves. However, the question of whether those observed profiles are in steady state with the present melt rate is not addressed. If the profiles are not consistent with steady state vertical advection, then approximation (C) will be in error. While the errors are likely to be small for an ice column that has experienced a long period of high or low melting, they could be significant where an ice column has recently been subjected to high melt, such as close to a grounding line. In that key region approximation (A) or (B) might be preferable."*

   AR: We thank the reviewer for this important comment. We agree on the need for further discussing the validity of the steady state assumption for vertical advection, and for clearly mentioning the remaining caveats.

   In a non-steady state, the ice velocity that matters is the velocity with respect to the moving ice–ocean interface, which can be expressed accounting for the ice-shelf floatation as:

$$w_{\mathrm{i}}' = \frac{\rho_{\mathrm{i}} - \rho_{\mathrm{w}}}{\rho_{\mathrm{w}}}\, m_{\mathrm{steady}} - \frac{\rho_{\mathrm{i}}}{\rho_{\mathrm{w}}} m$$

   where ρ_{i} and ρ_{w} are the ice and seawater densities, m_{steady} is the steady-state melt rate, i.e., the melt rate that would exactly balance the vertical ice advection, and m the actual (non-steady) melt rate (expressed in meters of ice per time unit). In steady state, this gives $w_{\mathrm{i}}' = -m$, as assumed in approximation (C). The ice shelves of the Amundsen Sea, like Pine Island, Dotson and Getz, are not in steady state and the observational estimates of Davison et al. (2023) indicate $m \approx 3\, m_{\mathrm{steady}}$ over 1997-2021.

This gives $w_i' \approx -0.93\ m$, i.e., an error of ~7% in approximation (C). Obviously, the mismatch is more important in future projections with increasing melt rates, but even with $m \approx 10\ m_{\text{steady}}$, which is unlikely for the Amundsen Sea, the error does not exceed 10%. The steady state assumption in approximation (C) therefore seems preferable to approximations (A) and (B) that give near-zero heat flux into the ice (Fig. 2), which is not consistent with the observational temperature profile in the Pine Island ice shelf (Fig. 1).

Things are obviously more complex near the grounding line of warm ice shelf cavities because the ice advected from upstream is not in thermal equilibrium. There are actually two time scales relevant for this: the time scale of vertical ice advection throughout the ice shelf thickness, and the time scale of vertical advection through the basal ice layer with a sharp thermal gradient (Fig. 1c). For typical values of the Amundsen Sea ice shelves*, the first time scale is several decades, which may be longer than the ice life time from the grounding line to the front, while the second is closer to 1 year. The first time scale is relevant for the slow temperature change of the ice interior, which is nearly uniform far from the ice base. This means that instead of temperature T_S in approximation (C), the ice temperature at depth would be more accurate, although it is difficult to estimate without an ice-sheet model that resolves heat advection. The second time scale of ~1 year means that approximation (C) is not very good within a few km from the grounding line, even if it depends on the thermal state at the ice base upstream of the grounding line. We also don't see any reason to believe that approximations (A) and (B) would be better than (C) near the grounding line.

* e.g. Pine Island: horizontal velocity of 3 km/yr, vertical velocity of 30 m/yr, ice-shelf thickness of 1000 m, and 30 m thickness for the basal layer of high thermal gradient.

4. **RC:** "*The discussion in lines 89-96 is a little misleading. When approximation (C) is used, melting will always occur when the ocean is above the pressure freezing point. There is no possibility of freezing due to heat conduction into the ice when the water is slightly warmer than the freezing point. The conduction term scales exactly with the melting and can never change the sign of the phase change. Thus, using the thermal driving to determine if there will be melting or freezing is the correct procedure, and one that has been followed in all implementations of approximation (C), at least to my knowledge.*"

   AR: We agree and will reformulate along these lines.

5. **RC:** "*If this paper is really to be an authoritative summary of approaches to estimating heat conduction into ice shelf, that of Sergienko et al. (2013, J. Geophys. Res. Earth Surf., 118, 970–981, doi:10.1002/jgrf.20054), which considers lateral heat advection, should also be included, or at least discussed.*"

   AR: We agree that the suggested publication is relevant to the current study. In fact, we also come to the conclusion that coupled ice-ocean models would give more accurate melt rates when basal melt is a leading order mass balance and urge future work to use coupled models to improve current parametrization in lines 121-130.

6. **RC:** “*On line 33, kapp-sub-i is a thermal diffusivity (not a conductivity).”*

   AR: We will correct the formulation accordingly.

7. **RC: “***On line 73, I think you mean "when neglecting heat advection".”*

   AR: We will reformulate the sentence accordingly.

8. **RC:** “*In figure 2b,c the horizontal axis label is "AW Temperature". The meaning of "AW" is never clarified, and the reader must refer to the earlier study to make any sense of it. A more appropriate axis label should be used.”*

   AR: We will revise the axis labels to make them clear.