Authors Response to Review 3 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: "*General comments*

In this brief communication, the authors examine different methods of representing the heat conduction into the ice currently in use in modeling ocean/ice shelf interactions in ocean general circulation models. After presenting three different approaches, the authors show ice temperature profiles that are representative of the different approaches, compute the relative error over all ice shelves in Antarctica between two of the approaches (using an estimate that does not require running a circulation model), and compare the results from all three methods using an MITgcm circulation model with an idealized domain representing a fjord under a floating glacier tongue in Greenland.

I think the differences in how heat conduction into the ice is handled in current ocean GCMs with ice shelves is an important point that is worth the attention of The Cryosphere. The manuscript was clear and easy to understand. I would find this study more compelling if the authors had compared the different methodologies in either some realistic domain models or an idealized domain more representative of an Antarctic ice shelf (since the Antarctic ones are much more likely to be resolved in "Earth system models" (line 27)). Perhaps though that is beyond the scope of a brief communication whose purpose is mostly to identify the problem, but not explore the entire range of differences between the methods."

AR: We thank the reviewer for taking the time to review our manuscript. We are pleased that the reviewer found our work to be of interest to The Cryosphere and appreciate them advocating for publication of our results. While realistic domains or domains representative of Antarctica would certainly have been good additional examples to show the differences in melt rates between different heat flux approximations, we think that, due to the linearity of the difference (Eq. 3) the given examples capture the difference appropriately. The addition of more examples would indeed expand the article beyond the format of a brief communication while adding only little more insights.

RC: "*I have other comments which I think would require some minor revision of the manuscript. I am happy that someone is bringing this to the attention of the community and hope the authors are able to get this published.*"

AR: We thank the reviewer again advocating for publication of the article. We plan to address the specific comments as follows:

RC: "*Specific comments"*

1. **RC**: "*Abstract, lines 7-8: I think it would be helpful if the authors add the mean (or median) difference found between methods 1 and 3 in Figure 3.*"
   AR: We will add a mean difference value in the abstract.

2. **RC**: "*Lines 39-53 and Figure 1: I like the different temperature profiles showing dT/dz = 0 at the base (1a), vertically uniform throughout the ice shelf (1b), or much greater near the ice shelf base (1c) corresponding to the three common approximations in Eqn. 2. However, did the authors look at estimated values of the basal melt and surface temperature at the locations of the Pine Island boreholes to see how the estimated dT/dz from Eqn. 2C actually compares to the observed gradients near the base?*"
   AR: We did not compare the borehole data to temperature gradient data estimated from Eqn. 2C. We can indeed add a line showing the theoretical estimate of the temperature gradient applying Eqn. 2C to typical surface temperature and melt rate values to figure 1c.

3. **RC**: "*Lines 76-77: Suggest changing "overestimation is very similar" to "overestimation is often very similar" as I think there are significant parts of the Antarctic where using the linear temperature profile may not lead to a lower vertical temperature gradient than what you get including vertical heat advection (i.e. where the melt rate is low and/or the ice is not super thick). For example, if I did the math correctly, mean values of the right hand side of Eqns. 2B and 2C for the Ross ice shelf are pretty much the same: using mean depth 350m, mean melt rate 0.1 m/yr (Rignot et al., 2013), rho_i = 920 kg/m^3 (Holland and Jenkins, 1999), kappa_i = 1.14e-6 m^2/s (Holland and Jenkins, 1999):*
   *Eqn. 2B RHS = (T_s – T_zd) * 3.0e-6 kg/(m^2-s)*
   *Eqn. 2C RHS = (T_s – T_zd) * 2.9e-6 kg/(m^2-s)*"
   AR: We agree with the reviewer's suggestion. We point to this also in line 69 in relation to figure 2a and in line 84 in reference to figure 3. We will reformulate the sentence to fit the reviewer's suggestion.

4. **RC**: "*Lines 82-83: I'm not sure I agree that "most areas show a difference of around 12%" other than AmIS and RIS. From Figure 3, most of the FRIS does look to be around 12%, but much of the smaller ice shelves are close to zero (yellow) except (significantly) the shelves in the Amundsen Embayment. I suggest a slight modification of this text.*"
   AR: We agree with the reviewer's comment and will rewrite this part to more accurately describe the figure.

5. **RC**: "*Line 103: I don't understand the phrase "In line with Fig. 2a" with respect to melt rates being very similar with Eqns. 2A and 2B since Fig. 2a only shows differences between approximations B and C.*"
   AR: We agree that the formulation is a bit unclear here. Eqn 2A gives zero conductive heat flux independent of melt which would correspond to a horizontal line at y=0 in figure 2a and is hence very close to approximation 2B. We will add a line for Eqn 2A in figure 2a and reformulate in line 103 to clarify.

6. **RC:** “*Lines 113-114: This implies (to me anyway) that modelers are tuning the drag coefficient just to avoid using an accurate approximation for the heat conduction into the ice and I don’t think that’s the case. I believe it is just to get a better melt rate, regardless of how the heat conduction is being done, since the total errors are often much more than the ~ 12-15% shown here due to heat conduction. I suggest a slight re-write of this sentence. Also, I think it would be helpful to include a reference where tuning is mentioned.”*
   AR: We will reformulate the sentence and discuss in a more nuanced way how the drag coefficient is used to tune models. We will further add a reference where tuning is used to obtain melt rates that fit observations.

7. **RC:** “*Line 129: If someone is assuming the ice sheet is a perfect insulator (approximation A), than I would not say that switching to approximation C comes at “no additional computation cost” because the code may have to add and keep track of a new variable (the ice shelf surface temperature). Suggest changing “no additional” to “little additional” or “very little additional”.”*
   AR: We will adopt the suggested formulation in the manuscript

Technical corrections

8. **RC: “***Equation 1 and lines 31-35: Gamma_t (the temperature turbulent transfer parameter) in Eqn. 1 is not defined in the paragraph after the equation.”*
   AR: We will add a definition for gamma_t in the text.

9. **RC**: “*Line 38: Why is rho_0 (density of pure water) given?”*
   AR: We will delete the definition of rho_0 as it is not necessary.

10. **RC**: “*Line 66: Missing right parenthesis to match the left one just to the left of “m < 0”.”*
    AR: We will add the missing right parenthesis

11. **RC**: “*Line 104: Should “absolute melt rates” be “absolute melt rate differences”?”*
    AR: Yes, we will correct the manuscript accordingly.

12. **RC**: “*Line 106: “<=” here should be “>=”.”*
    AR: We will correct this in the manuscript.

13. **RC**: “*Line 115: Typo, “changes is ice shelf” should be “changes in ice shelf”.*”
    AR: We will correct the typo.

14. **RC**: “*Line 121: Subject/verb agreement: “parameterizations ... has been identified”.*”
    AR: We will correct the typo: “parameterization .. has been identified”