RESPONSES TO REVIEWERS FOR
Brief Communication: Sensitivity of Antarctic ice-shelf melting to ocean warming across basal melt models
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Author response to reviewer #2

Summary
This paper applies five approaches to ice shelf basal melt modeling to the 40 largest Antarctic ice shelves in order to compare their sensitivity to an idealized ocean warming scenario. These include more established approaches such as a simple parameterization of pointwise melt rates based on the regional hydrography and local ice base slope and more complex parameterizations accounting for meltwater advection and refreezing, and a newer machine learning approach using a neural network. The neural network is trained on output from a 1/4º NEMO simulation, which employs the same three-equation melt parameterization used in the intermediate complexity models. The aim of the neural network approach is to capture more of the complex spatial structure of basal melt rates without the prohibitively high computational cost of the full NEMO simulation.

The ocean conditions of the 40 ice shelves are classified into 6 categories based on their deep and near-surface temperatures and thermocline depths, each with an idealized "reference" temperature and salinity profile. Simulations are run for each ice shelf with its reference hydrography and then with a (salinity-compensated) 1ºC warming applied to the deep waters. The resulting melt rate distributions for the reference simulations are compared to observed melt rates to evaluate the fidelity of each modeling approach, and the warm simulations are compared with the reference to evaluate the sensitivity to warming.

The results show large differences in the spatial distribution of melt rates, even in the reference simulations which were calibrated to have the same average magnitude. The increases in the warming scenario vary among the models in both magnitude and distribution. However the models generally agree that the fastest-melting ice shelves under present day conditions are also most sensitive to warming.

Characterizing these differences is useful to the Antarctic Ice Sheet/Ocean modeling community. I recommend this paper to be published with some revisions.

Thank you very much for underlining the significance of our work and for your insightful comments. We agree with most suggestions and will incorporate these in the revision of our manuscript. In the following, we provide a point-by-point response.

General comments
The paper is well-structured and concise, with the figures in particular accommodating a huge amount of information in an impressively small package.

Thank you for this positive feedback. We appreciate it.

My major question is related to the sensitivity calculations and comparison with other studies. Because there was only one warming experiment, the quadratic sensitivities were calculated using the additional point of zero melt at the freezing point. Is this a common approach to calculating climate sensitivity? Would it be possible/valuable to conduct an additional simulation with a larger forcing in order to better constrain both the linear and quadratic sensitivities? If feasible, this could also allow one to evaluate whether the sensitivity of each model (and/or ice shelf system) is better characterized as linear or quadratic.

We agree that the zero point appears somewhat arbitrary and will include, as suggested, an additional simulation with a +2 degree warming. The omission of this zero point naturally changes the meaning of the quadratic sensitivity, as it no longer sensibly applies to temperature forcings outside our explored range. Hence, we replace this quadratic sensitivity with a 'nonlinearity', which is defined as the second-order derivative over the explored range. The benefit is that this metric can still be compared to previous estimates of the quadratic sensitivity. In addition, the nonlinearity functions as an assessment of how valid it is to apply a linear sensitivity to larger temperature perturbations. We hope this solution satisfies the reviewer.

Relatedly, where the sensitivities calculated in this study are compared to published estimates (e.g. lines 187-192), it would be helpful to include at least a brief description of the approaches of those studies and how they differ from the present work. Certainly the reader can visit those references for more detail but I think that a bit of context within the text would be helpful and appropriate. There are a few other points where I think more discussion would be appropriate which I have highlighted in the line-by-line comments.
Thank you for pointing it out. We will provide additional context to these studies.

Line-by-line comments
Abstract: "diversity in basal melt forcing is presently unavoidable to prevent underestimating uncertainties in future projections." This statement is confusing to me because it's separated from the initial mention of sea level rise and also I'm not sure what you mean by "unavoidable." I would say something like "a range of basal melt forcings should be applied to incorporate this uncertainty in future projections of sea level rise."
We will reformulate to clarify.

Line 138: It might be helpful to refer here to the ice shelf label numbers, i.e. "(10-14 and 27 in Figure 1a)".
Good point, we will add this information.

Line 141: Does this imply that the contrast is reproduced better in the other models? Please clarify.
Yes, the large contrast is better in line with observations, so here we can conclude that there is an actual underestimation by these two models. We will clarify this.

Line 168-170: This has me a little confused about how the Neural Network approach works. I guess it is trained on simulations that include seasonality, but when given an ocean temperature profile modeled on winter conditions, the resulting melt pattern effectively represents an annual mean — is this true? (It doesn't seem like this has much impact on the melt sensitivity calculation since it looks like a lot of that signal cancels out, at least looking at Filchner-Ronne and Ross.)
Yes, the Neural Network has been trained on yearly-averaged melt rates, therefore implicitly reproducing seasonality in these particular cases. We will add one or two sentences to further clarify where this comes from and underline that it should not impact our main conclusions.

Line 177: At times I found it slightly confusing that "deep amplification" can refer to either the actual melt rate or the melt rate response/anomaly — this is a place where I think it is a bit unclear and you could clarify by writing "Combining the average melt rate response and its deep amplification…"
We agree and will use this term 'deep amplification' solely as representing the amplification in the linear melt sensitivities, as appearing in Fig. 3. Hence, we will express the melt rates and melt changes at depth (Fig. 1 and 2) in absolute terms to avoid confusion. Throughout the text, we will ensure that 'deep amplification' is used unambiguously.

Line 187-192: Why do you think the sensitivities calculated in this study are so much lower than previous estimates? Are there key contrasts with the approaches taken in those papers that can help the reader interpret your findings?
Yes, we have a preprint submitted (doi: 10.5194/egusphere-2024-2257) where we conclude that the discrepancy in sensitivities with van der Linden et al can be explained based on large-scale meltwater-ocean temperature feedbacks. We will refer to this preprint and briefly summarise the explanation.

Line 195: To me this is a somewhat uncommon use of the word "consensual," I would omit it as you've already said earlier in the sentence that the models agree so I don't think it's necessary (or it could be replaced with "consistent").
Thank you for spotting this. We will remove it.

Line 196-199: Some patterns begin to emerge here but they weren't immediately obvious to me with the large number of names, not all of which were completely familiar. One simple thing that would make it easier to parse is to reverse the order that you list the ice shelves in the sentence so they are in the same order as they are shown in Figure 3. You could also consider noting in the text what ocean conditions apply to each ice shelf, or including the number of each ice shelf corresponding to the legend in Figure 1a to make it easier to refer back.
As suggested, we will change the order in which we mention the ice shelves and include the numbers in the text. Where it does not affect readability, we will refer to the applied forcing as well.

Line 203: except for Getz.

You are right, we will add this information.

Line 208-210: What was the method/approach used by the study you're comparing to, and is there a clear reason to think that result is more realistic?
This study is based on a timeseries of observations in front of Dotson ice shelf and so we consider these to be a realistic guidance for the Dotson ice shelf and the neighboring ice shelves. We will elaborate on this briefly in the manuscript

Line 220-229: From what you've shown, I don't think it's possible to "reduce the intermodel spread" in reference to this suite of models because they are fundamentally so different from one another.
Rather, if the goal is to improve sea level projections, it seems to me that it's important to prioritize the regions of the ice shelf that exert the greatest influence on ice sheet dynamics and consider which models seem most trustworthy in those settings. Thinking of results from Reese et al. (2018) showing the disproportionate sensitivity of upstream ice dynamics to thinning in narrow channels near the grounding line, it's concerning to me that the Neural Network is trained on a model that likely performs worst in those areas. (But maybe you disagree!) On the other hand, if the goal is to capture the change in spatial distribution of basal melt more broadly under warmer ocean conditions, the Neural Network may be a good choice. I know you are limited in how much you can say about which model is "better" but I think it could add to the value of this paper if you went a bit further into the discussion of the implications of your findings.
We think that you are rightly pointing one of the main difficulties surrounding basal melt calibration and evaluation. Different applications require different aspects of basal melt to be realistic. Our aim is to provide an objective evaluation of the melt sensitivity based on several (widely applied) dedicated melt models, so that (ice-sheet) modelers can make decisions based on their research question. We will expand the discussion to outline potential lessons different readers may draw from our conclusions.

Figures 1 & 2: I think it would be helpful if you could add a coastline, or some shading to either the ocean or land, to help orient and delineate the ice shelves in the "puzzle" subplots.
Thank you for pointing this out. We will explore ways to visualise the grounding line and/or calving front to aid the orientation.

additional reference:

Reese, R., Gudmundsson, G.H., Levermann, A. et al. The far reach of ice-shelf thinning in Antarctica. Nature Clim Change 8, 53–57 (2018). https://doi.org/10.1038/s41558-017-0020-x