Dear editor, please find below our point-by-point reply to both reviewer's comments in green.

Reviewer 1

Dear Editor and Authors,

Thank you for sending the revised manuscript and the point-by-point responses to my comments. I enjoyed reading the manuscript again, and it is still a very nice paper that the community will value tremendously.

I have reviewed the revisions and am satisfied with the changes made. The authors have addressed all of my concerns appropriately, and I believe the manuscript has improved. I am happy to recommend the manuscript for publication pending a few minor revisions (see below).

We thank the reviewer for yet another review and their useful comments. Please find our replies below.

Line 437 (in the tracked-changes document): I am happy with the added discussion here. But: "An inter-model comparison within the SOFIA initiative (Swart et al., 2023) showed a qualitative agreement on subsurface warming along the Antarctic coast (Chen et al., 2023)." This is not correct and needs to be changed/removed. Chen et al. show a qualitative agreement on subsurface warming in the zonal mean. This does not mean that the models show a warming along the coast. Current work in prep in the SOFIA community shows that this "zonal mean" warming is dominated by certain regions and that there is cooling along the coast in some regions in some models. This work is, sadly, not yet citable. I would either remove this sentence completely or state that a recent intermodel comparison shows that, in the zonal average, models agree on a subsurface warming but that regional differences are the focus of current investigations, such as those within the SOFIA initiative.

We agree that this statement should clarify the zonal mean aspect of the results of Chen et al. As we cannot cite these unpublished results, and removal of this sentence would require removing a large substance of this discussion paragraph, we have retained the sentence and stress that this regards zonal mean changes.

Regarding the constant freezing temperature, -1.7. I understand your wish for simplicity and transparency, and I see that rerunning the experiments with different values would be a lot of work for likely a very small change. The added argumentation in the methods section is an improvement, but I am not yet fully satisfied with it. Firstly, you state that you have chosen the surface freezing temperature, correct, but I think you need to add that this an unrealistic value and that the freezing temperature at depth is closer to –2.0 - -2.3 (for your depth ranges). Secondly, I think you need to be more quantitative and precise in the following statement: “We consider the uncertainty stemming from this idealization to be minimal in comparison to uncertainties from other methodological assumptions.” What is minimal? What difference approximately can we expect if we use -1.7 instead of -2.3? Can you provide an order of magnitude? And can you be more precise on which other assumptions you refer to her?

We agree that the choice of freezing temperature is in hindsight not ideal, but also agree with the reviewer that at this stage, new simulations are undesirable. Without a sensitivity experiment, we cannot provide a quantitative assessment of this uncertainty however. Instead, we have more explicitly stated that lower freezing temperatures would be more appropriate, and have explicitly formulated the methodological choices we consider to contribute (much) more to the overall uncertainty and idealisation of this study.

Gt/yr vs. Sv: I see you’ve noted that 1 Gt/yr = 0.0317 mSv, which is helpful. However, to improve clarity for the reader, I recommend consistently including the equivalent value in Sverdrups whenever you present a value in Gt/yr. This will save readers from having to do the conversion themselves each time. For instance, "an increase in ice mass loss of 400 Gt/yr (≈0.012 Sv)".

We have moved the conversion factor to the first point where the unit Gt/yr is introduced. Additionally, for all unique values cited (200, 400, 1000, 2000, and 3300) we have added the conversion to Sv wherever these values is first introduced. We have not included this every time any value in Gt/yr is mentioned however, as this would limit the readability of our manuscript.

Reviewer 2

Second review of "Quantifying the feedback between Antarctic meltwater release and subsurface Southern Ocean warming" by Lambert et al.

In their revision, the authors have suitable addressed my comments from the first review. The revised manuscript makes it clear where results are based on a single coupled model and includes an expanded discussion on biases in EC-Earth. Where practicable for this study, a comparison of results across coupled models are presented. The findings of this manuscript will be useful to the scientific community working towards coupling dynamic ice sheets and shelves into Earth system models, and I consider this manuscript in its revised form suitable for publication in ESD.

We thank the reviewer for another review. Please find below our replies.

I have two very minor comments for consideration:

Line 159-160 (of tracked changes version): I appreciate the clarification regarding dedrifting. It would be useful to briefly include mention of the period over which the piControl linear trend was calculated, e.g.100 years? The full available piControl length?

The dedrifiting is based on the full piControl. We have included this in the manuscript.

Line 257-260 (of tracked changes version): The revised text makes your methodology and reasoning clearer, but I wonder, for all regions, is regional net cooling over the historical period actually unrealistic? Over shorter periods (1975-2012) there is cooling in the Weddell and Ross (Schmidtko et al. 2014), which may be due to natural variability, a forced change causing the cooling, or sparse observations not fully sampling the real change. I don't expect you to change your methodology here, but I think your phrasing of "unrealistic" needs refining, i.e. "which we consider to be unrealistic over a 150-year period with increasing global temperatures" or something like that – then it is explicit why you assume it to be unrealistic.

Indeed, a historical cooling is certainly plausible in specific regions. What we referred to as unrealistic is the negative basal melt sensitivity that results from this. We have clarified this in the text.