Author response to Referee #2

Review of "On the non-linear response of Antarctic ice shelf surface melt to Warming"

This study investigates non-linearity in the temperature-melt relationship over Antarctic ice shelves using a modeling approach. The goals of the study are twofold, first to investigate spatial variability in the temperature-melt relationship and, second, to identify the dominant components driving non-linearity in the temperature-melt relationship. The authors pursue both goals by analyzing outputs from a regional climate model forced over the historical period (1979-2023) using ERA5 and two future climate scenarios (SSP3-7.0) using CESM2 and MPI-ESM (2015-2099). I think the study addresses an important research topic that is certainly within the scope of The Cryosphere and it is generally presented in clear, concise way. However, I do have some concerns about the scientific rigor and significance which I think would require some fairly substantiative changes to address. My recommendation is that the manuscript is considered after major revisions.

General comments

1. It's not clear how Section 3.1 and 3.2 contribute to the goals of the study. I understand that the authors need some temperature variation to investigate its relationship with melt. But it looks like there is plenty of variation in the historical forcings (e.g. Fig. 5a and 6a). The authors should consider removing these sections or strengthening the links between ESM forcing/trends and the goals of the study.

Section 3.1 is included to get confidence in the performance of RACMO forced with ESMs before using these in the analysis. The trends and variability in temperature and melt in the future simulations in Section 3.2 form the basis of the analysis in the temperature/melt relationship, and therefore we critically assess these underlying data here. To make this clearer for the reader, we add a brief overview at the start of the Results section outlining what to expect:
'First, the performance of RACMO forced by the ESMs is evaluated to gain confidence in in the simulations before using them in the analysis. We then assess the temperature and melt trends in the future projection runs, which provide the basis for studying the temperature–melt relationship. We analyse the spatial variability in the relationship between temperature and melt, the role of albedo feedbacks in the non-linearity and systematically assess how all SEB terms depend on temperature and contribute to the melt response.'

2. On a related note, it's not clear why two different ESMs are used to force RACMO. The current use of two ESMs makes the figures cluttered (e.g. Figures 5 and 6) and distracts from the main message. The authors acknowledge that the relationship between temperature and melt is consistent across the ERA5 and ESM model forcings (L201-202). I recommend that the authors provide better motivation for using two models or consider streamlining the analysis by not using any (see previous comment) or just one.

Including the ESM-forced simulations helps to answer our research question because regions that experience little melt in the current climate experience more melt and variability towards the end of the 21st century (eg Filchner-Ronne Ice Shelf, Fig 3). Therefore the temperature-melt relationship can be analysed for more ice shelves, and reaching higher summer temperatures. For example, the RACMO(ERA5) simulation lacked data in the temperature bin around 0 °C, whereas the CESM2- and MPI-ESM-forced simulations do provide data in this range (Fig. 7). Including multiple ESM forcings and showing the consistency among them strenghtens our findings.

We added this motivation at the start of Section 2.3:
"Using ERA5 provides present-day conditions, while the ESM-forced simulations extend the analysis into future climates, allowing the temperature-melt relationship to be examined across a wider range of melt intensities and summer temperatures than occur today."

3. The authors categorize ice shelves in several ways. In Figure 3 the ice shelves are categorized regionally. In Figures 7 and 8, ice shelves are categorized by annual precipitation. In the text, the authors state that the ice shelves are categorized by summer air temperatures as well. The central message of the manuscript would be strengthened if the analysis categorized ice shelves in just one way.

We decided to keep the temperature and melt trends for different regions in Figure 3 because it serves two purposes: 1) it allows for evaluation of regional melt trends and variability and 2) highlights differences between ERA5- and ESM-forced simulations which would not be visible if ice shelves were already classified only as dry or wet. The dry/wet classification is introduced only after Section 3.4, once we demonstrate that the temperature-melt relationship differs between ice shelves in dry and warmer climates and provide an initial physical explanation based on albedo feedbacks. From that point onward, the analysis focuses on the wet/dry categorization to examine the underlying processes in more detail.

We have clarified this methodology by adding a section in the Methods (see following comment). From your comment we realise that the wording 'cold ice shelves' and 'warm ice shelves' in Section 3.5 could be interpreted as an additional classification. These terms referred to the lower and higher temperature bins used in the SEB-temperature slope analysis, and we have reworded them to lower and higher temperature bins to avoid confusion.

4. The motivation for categorizing ice shelves by annual precipitation is not clear. As written, it feels like the authors tuned an annual precipitation threshold until the data were split according to their narrative. I recommend that the authors better explain their decision to use a 500 mm threshold in both introduction and the methods sections. Better would be to provide a sensitivity analysis on this threshold.

We have now clarified this categorization in the Methods section as follows:
"Albedo and cloud feedback's are expected to play a dominant role, and therefore the analysis is split between iceshelves in relatively dry and wet climates, based on a

threshold in annual snowfall. Following van Wessem et al. (2023), we use the median annual snowfall of 500 mm yr−1 as the threshold separating 'dry' and 'wet' ice shelves, which results in two equally sized groups."

5. The authors claim that there is large spatial variability in the temperature-melt relationship on ice shelves but the current analysis is not very convincing. I acknowledge that there is some qualitative support for this finding in Figure 4. But the analysis would be improved by demonstrating this variability quantitatively or statistically.

Thank you for your comment. We agree that Fig 4 illustrates our statement, but does not provide quantitative support, therefore following your later suggestion at L209, we have moved Fig C1 to the main text. We have adapted the second paragraph of Section 3.3 to the following:
"Ice shelves in drier climates tend to experience more melt at the same temperatures compared those in wetter climates. For example, the Amery ice shelf, which receives less than 100 mm snowfall during summer (boxplot in Fig. 5), has a temperature-melt curve where melt start increases at much lower temperatures compared to other ice shelves (grey scatter Fig. 5). In contrast, the Nickerson Ice Shelf, located in a much wetter climate on the Marie Byrd Land coast, shows melt rates increasing only at substantially higher summer air temperatures. These examples show that the temperature required to reach a certain melt rate varies between ice shelves.

To further quantify this, we use the fitted exponential temperature-melt relationships and determine for each ice shelf the summer air temperature at which 200 mm of melt would occur. This temperature is plotted against summer snowfall rates in Fig 6. Although the slope and R2 of the linear regression vary between the simulations, all show a clear pattern: ice shelves in drier climates reach 200 mm of melt at summer air temperatures several degrees lower than those in wetter climates."

6. Too much of the main text and figures are focused on SEB components at very low temperatures when there is little or no melt. There is therefore a mismatch between the goals of the study and the findings. The manuscript would be improved by expanding Section 3.3. and focusing Sections 3.4. and 3.5 (and Figures 5-8) on air temperatures that are associated with melt. Alternatively, given that some of the most interesting findings occur at low air temperatures, perhaps the goal of the study could be modified (e.g. impact of air temperature on SEB).

We have revised these sections to better emphasize temperature ranges associated with melt. In Sections 3.4 and 3.5, we have strengthened the focus on higher temperature bins where melt occurs, for example by adding more discussion on SWnet curve at higher temperature bins for dry ice shelves: "The slope between SWnet and temperature continues to increase beyond this point in all simulations, initially driven by snowmelt refreezing and dry snow metamorphism. At the warmest temperature bins, albedo can decrease further due to the refreezing of rainfall and the increasing exposure of bare ice."

Similar for wet ice shelves: "The slope of SWnet is relatively constant across the temperature bins, except for the bin around 0C, where SWnet increases more rapidly with warming. In this temperature range, a fraction of the precipitation falls as rain, which removes the damping effect of fresh snowfall and reduces albedo through refreezing of rainwater."

The discussion of turbulent heat fluxes already focused on higher temperature bins, as it explicitly describes how sensible and latent heat fluxes change sign or level off in the highest bins, thereby contributing additional energy for melt alongside the increase in net radiation. Lastly, in the Discussion and conclusions section we have now emphasized more on conclusions about contribution of SEB sensitivity to melt sensitivity.

Specific comments

L24-25: It would be useful to add a brief statement here to explain why the relationship between temperature and melt is highly non-linear.

More explanation for this is given later in the introduction and by the extra discussion given in the following comment.

L47-49: The problem statement should be expanded by reviewing the findings of Jakobs et al. (2019) in a bit more detail and clarifying how this study differs from van Wessem et al. (2023). One way the authors could do this is by briefly reviewing how SEB components (e.g. albedo, clouds) are thought to respond to temperature in Antarctica.

We have split the last paragraph of the introduction in two and have expanded on the problem statement as follows: "One of the possible explanations for this non-linearity is the snowmelt-albedo feedback (Jakobs et al., 2019, 2021) in which melt and subsequent refreezing lowers the albedo of snow, increasing absorption of solar radiation and melt. The potential of this feedback to enhance surface melt is modulated by the frequency and timing of snowfall events in summer. But as noted by Jakobs et al. (2019), under warmer conditions such as those currently observed on the Antarctic Peninsula, the snow-melt albedo feedback becomes less important in enhancing melt, and other processes such as exposure of bare ice or turbulent fluxes play a role. In addition, other surface energy balance terms such as longwave radiation and the latent heat flux may also respond non-linearly to air temperature through changes in atmospheric moisture and cloud conditions that are associated with the atmospheric warming."

L65-66: Recommend placing the "van Dalum" reference before "are" and after "RACMO2.3p2"

We placed the reference instead in the previous sentence where the new version is mentioned for the first time: "In this study, we used the latest RACMO version 2.4p1 (van Dalum et al., 2024), in this paper referred to as RACMO. The main differences between RACMO2.4p1 and the previous operational version, RACMO2.3p2, are:"

L73: “part of” is redundant here if “grid cells can be partially glaciated…”

We have removed ‘part of’.

L74: “…and therefore better representing areas…” is a little awkward, consider revising.

This is now rephrased to: “and therefore provide a better representation of areas such as the McMurdo Dry Valleys”

L82: Should it not be “SEB of skin layer is defined as:”?

We have included this now.

L92: energy “available” for melt since some will be used for warming

We have rephrased to “energy available for melt”.

Section 2.2: It’s not clear to me why SMB is defined here since it is not required to complete the goals of the study. Given the importance of melt in the study, it would be more useful to describe the relationship between QM and melt, instead of SMB.

We have included the SMB definition here because the precipitation component is strongly linked with the SEB and melt in this study, through its effect on albedo. We have added the following in the Introduction to make more clear the role that precipitation is expected to play for surface melt:
“.. One of the possible explanations for this non-linearity is the snowmelt-albedo feedback (Jakobs et. al, 2019) in which melt and subsequent refreezing lowers the albedo of snow, increasing absorption of solar radiation and melt. The potential of this feedback to enhance surface melt is modulated by the frequency and timing of snowfall events in summer.”

In the methods we have made the link more clear between melt and SMB, including how melt mass rate is calculated from QM:

‘The resulting melt rate (kg m−2) is then obtained by converting the available melt energy into a meltwater mass flux using the latent heat of fusion, such that the energy remaining after warming the snow to the melting point produces melt. Melt contributes to mass loss when the resulting meltwater is not refrozen or retained in the snow and instead leads to runoff (RU). The surface mass balance (SMB) describes the balance between accumulation and ablation in the near-surface firn or ice:
SMB = Ptot − SUs − SUds − RU − ER (3)
with Ptot total precipitation, SUs is surface sublimation and SUds sublimation of blowing snow, RU runoff and ER drifting snow erosion which can be both ablative (erosion, ER > 0) and accumulative (deposition, ER < 0).’

L107-108: “have been done” is a little awkward, consider revising.

We have revised this to "The simulations for this domain were performed as part of the PolarRES project"

L129-134: Recommend moving this text to the Methods

We have now moved this section to the Methods, and more discussion is given there on the biases as suggested by reviewer #1.

L143: How was the anomaly computed? Recommend that this is described in the Methods

We have replaced 'anomaly' by 'difference' to clarify that we refer to the difference between simulation forced by ERA5 and by the ESMs. This is further clarified in the methods as follows:
"As a second evaluation step, historical simulations (1985-2014) forced with ESMs are compared to RACMO(ERA5) in Section 3.1. We evaluate whether the mean difference between the ESM-forced simulations and RACMO(ERA5) exceed the inter-annual standard deviation of RACMO(ERA5)."

L151-152: Recommend moving this text to the Methods

We have adapted the difference maps here so they show the difference in DJF mean instead of annual mean. This is done to more consistently show results focussed on the summer season throughout the manuscript. Therefore this statement is removed here.

L158-159: Not sure that Table 1 supports this statement.

We have now moved Table 1 to the Appendix and expanded the discussion of differences between the ESM-forced simulations and the ERA5-forced simulation by adding difference maps for melt and SEB terms. We have therefore replaced the original statement with a more nuanced statement of the overall performance:
"Overall, the ESM-forced simulations reproduce Antarctic near-surface climate, SEB, and melt patterns well over the ice shelves, with differences relative to RACMO(ERA5) generally within the inter-annual standard deviation, except in a few localized regions such as the Wilkins and George VI Ice Shelves."

L180: "trends of" near-surface air temperature?

We see that L180-183 are confusing here and give repeated information about the difference in temperature in the historical period that are already discussed with the difference maps. Therefore these sentences are removed here.

L181: Recommend that "inter-annual variability" is quantified to support this statement.

Not relevant anymore, see previous comment.

Fig. 3: "COLD" ice shelf category has not been defined yet, recommend doing so in the Methods

We have written out the abbreviations in full in this figure, making clear that "COLD" and the other abbreviations refer to a regional grouping and is not intended as an additional ice-shelf category.

L207: Not clear how drier and wetter climates are defined. Recommend describing in the Methods. Also see general comment.

This is now defined in the Methods section, see reponse to general comment 4.

L209-214: Reasoning is unclear – does Nickerson Ice Shelf also have a mean summer air temperature of -4C? It would be better is the study could quantitatively demonstrate this point instead of cherry-picking examples. I think Figure C1 could serve this purpose, consider adding it to the main text.

We have adapted this section in line with general comment 5. We have reworded this sentence to first give Amery Ice Shelf and Nickerson as examples of contrasting temperature-melt relationships in a wet versus dry climate. After this, we quantitatively demonstrate the link with snowfall using Fig C1. See general comment 5 for the proposed revised text.

L211-212: Awkward phrasing, consider revising.

Revised to 'These examples show that the temperature required to reach a certain melt rate varies between ice shelves.'

L212-213: Not clear what this sentence is getting at.

The revised text is given in our response to general comment 5.

L223: Recommend moving to Methods

Done

L226: How were these hypothetical clear-sky conditions derived? Was this a separate model run or something that is produced implicitly by RACMO. The methods could be improved by outlining the key metrics that are used for the analysis.

In RACMO, the shortwave radiation profiles are calculated separately for the case that the sky would be clear sky, and for a full overcast case. The clear-sky radiation fluxes are stored in this process. Then depending on the cloud cover, the actual shortwave radiation fluxes are calculated as weighted average based on cloud cover. We have explained this in the new methods section (2.4) as follows: "The influence of clouds on albedo is examined by comparing clear-sky and all-sky albedo, making use of the fact that RACMO separately calculates radiative fluxes under clear-sky and cloudy-sky conditions and subsequently combines them into total-sky fluxes based on cloud fraction."

L248-255: Recommend moving to Methods

We will move this to the methods under a new section "2.4 Assessing temperature-dependency of SEB"

L258: Should "SWnet" actually b "the correlation between SWnet and air temperature"?

We have clarified this text as follows:

The relationship between net shortwave radiation (SWnet) and temperature is not constant across temperature bins (Fig. 8a,b,c). In the lower temperature bins, the slope of SWnet is negative, indicating that SWnet decreases with warming. This decrease occurs because higher atmospheric moisture and increased cloud cover reduce incoming shortwave radiation (SWin) by reflecting more sunlight. This decrease in SWin is larger than the reduction in outgoing shortwave radiation (SWout), because surface albedo increases with temperature in this range and ∂SWnet/ ∂T remains negative (Fig. 6).

L267: "peaking" seems to imply that the strength of the snowmelt-albedo feedback reduces at temperatures higher than -8 but Fig. 7 does not show that. Please consider clarifying.

Thank you for this comment. We agree that the previous wording was misleading. In Jakobs et al., the "strength" of the snowmelt–albedo feedback refers to the contribution of this feedback to total melt. Thus, a "peak" in their study indicates that, beyond this point, other processes become increasingly important in driving melt, even though the albedo itself can continue to decrease. To avoid confusion, we have removed this reference from the main text here and instead discuss it in the Discussion section:

"This is consistent with Jakobs et al. (2021), who found that the snowmelt-albedo feedback begins to strengthen around -12 ◦C and peaks between -9 and -7 ◦C. At higher temperatures, additional albedo-lowering processes become important, including the transition from snowfall to rainfall and increasing exposure of bare ice."