Author’s response to referee comments for *“The evolution of future Antarctic surface melt using PISM-dEBM-simple”*

by J. Garbe et al., The Cryosphere Discuss., 2023
(manuscript no. TC-2022-249)

Dear Michiel van den Broeke,

Thank you once again for handling the editing process of our manuscript. We have addressed all comments made by the referees and have made a conscious and thorough effort to implement all their suggestions in the revised version of our manuscript. The reviews contributed considerably to the improvement of our manuscript for which we are very grateful.

In our revision of the manuscript, we particularly addressed the following major issues raised by the referees:

1. **Mask mismatch:** Based on the 3rd major comment by Referee #1, we have decided to re-do the model calibration using the observed (instead of PISM’s) ice-sheet topography to overcome the inconsistencies between the surface temperature / melt rates and the topography in areas where insolation-driven melt is relevant. This also required re-running all simulations (incl. the SSP5 projection + sensitivity ensemble and the long-term commitment runs). The new calibration results in slightly different best-fit parameters and model results, showing a considerable improvement in projected 2100 melt volume compared to RACMO (Fig. 5), as well as a better match with contemporary melt patterns from RACMO (Figs. 1 and 2). All main conclusions of the paper remain unaffected.
2. **Model evaluation:** We now have added a thorough comparison with observation-derived melt estimates (QuikSCAT) for dEBM-simple as well as for RACMO (incl. 2 additional supplementary figures), have strengthened the justification of RACMO being used for the calibration and performance evaluation, and added a discussion of its limitations.
3. **Comparison to PDD:** We have reemphasized that the main goal of the study is not to denigrate the PDD approach, but instead to conceptually show that in principle the more physically based dEBM approach is capable of replacing the traditionally used PDD in long-term ice sheet simulations by including a further feedback that is missing in the PDD approach. We added an extensive comparison between dEBM and PDD and more discussion of the PDD limitations.

4. **Refreezing / runoff:** In the revised manuscript, we added sensitivity runs to estimate the uncertainty of our model assumptions regarding meltwater refreezing / runoff. While the associated parameter value has no influence on the model calibration, it turns out that the exact parameter value even only has a minor influence on the overall modeled ice dynamics in the long-term commitment experiments. Alongside extensive discussion, we now include three more supplementary figures related to the refreezing parameter (Figs. S1, S19 and S20).

Please find our detailed point-by-point responses to all referee comments below. In this response letter, the reviewer comments are in black, while our responses are in blue. Line numbers mentioned in our responses refer to the “tracked changes” manuscript version, which is attached at the end of this document.

Yours sincerely,

Julius Garbe, on behalf of all co-authors

Referee comment RC1 by Anonymous Referee #1

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Summary

This paper presents a new surface melt scheme in the Parallel Ice Sheet Model (PISM) coined dEBM-simple. The scheme is forced by air temperature and includes parameterizations for solar radiation, atmospheric transmissivity, and albedo evolution. The authors first tune their model using historical melt simulated by the regional climate model RACMO2.3p2 forced by CESM2. Ensuing comparison between RACMO2-CESM2 and dEBM-simple reveals a reasonably strong agreement in the simulation of mean annual and monthly surface melt rates. Based on this, the authors then force their model to 2100 using output from a SSP5-8.5 forcing of RACMO2-CESM2, and then to 2300 by repeating end-of-century forcing. Results show increasing melt, runoff, and snowfall, ice acceleration, and the importance of ice-albedo and ice-elevation feedbacks.

This is a very well written manuscript supported by clear, nicely illustrated figures. The introduction to the paper provides a great overview of the science and motivation for the work. Development of the dEBM-simple model is an important effort to bridge the gap between simple PDD melt schemes and more complex and computationally expensive energy balance approaches implemented in regional climate models. The model and methods are well described and the uncertainty quantification by varying parameter values is welcome. I do have several concerns that I would like the authors to address, particularly as it relates to the evaluation of the dEBM-simple results and determination of meltwater runoff.

I thank the authors for their time in considering my evaluation.

Response: We want to sincerely thank the referee for their valuable time and effort that went into this detailed review as well as for their nice words! Their excellent and insightful comments helped us considerably to improve this manuscript.
We have taken a conscious effort to address all comments in the revised manuscript and provide our point-by-point responses to them below.

Major comments

The authors claim the validity of their new dEBM-simple approach by comparing it with historical (1950-2015) melt rates simulated by RACMO2. The major issue I see with this is that model parameters were specifically tuned to match RACMO2 over this same period, and thus this comparison does not represent an independent check on dEBM-simple's validity.
Response: We fully agree with the referee that a comparison with RACMO melt rates to assess the model performance of dEBM-simple constitutes no independent validation of the model given that it is calibrated based on forcing data from RACMO. In fact, we acknowledge

that “validation” is a misleading choice of words that was made rather accidently. We have removed it from the manuscript and instead replaced it with “evaluation”. Just to briefly reiterate: the aim of this manuscript is not to prove the validity of the approach, but to apply the novel melt scheme to the Antarctic Ice Sheet and conceptually show that in principle the more physically based melt parameterization that includes another feedback is capable of replacing the traditionally-used PDD (given proper tuning), without compromising on computational efficiency or number of required inputs. The validity of the dEBM method is already thoroughly proven in Krebs-Kanzow et al. (2018, 2021), both of which are prominently referenced in the manuscript. The application of the “simple” version to the Greenland Ice Sheet (incl. showing that the parameterizations for albedo and transmissivity as well as the implementation in PISM work as expected) are thoroughly discussed in Zeitz et al. (2021).

That said, we have tried to make an effort of improvement in this respect by including a comparison of both the dEBM-simple as well as the RACMO-predicted melt rate estimates with satellite-derived estimates based on QuikSCAT data (Trusel et al., 2013) in the form of supplementary figures (Figs. S4, S9) and additional text in the main body of the manuscript. We like to emphasize that this comparison resembles only a brief snapshot in time from the perspective of an ice sheet, for which the long-term melt “climate” is more important than the melt “weather”.

A comprehensive evaluation of Antarctic surface melt conditions based on observations is still hampered by spatially and temporally scarce in situ meteorological observations that are unevenly distributed with no continental coverage. Observations derived from satellites and remote sensing can indeed cover the entire continent but span only relatively short time periods and bring along problems in their ‘correct’ interpretation, as the sensors do not measure the actual physical process of surface melt, but rather observe the presence of liquid water, leading to large spatiotemporal inconsistencies in the derived melt estimates from different sensors (Husman et al., 2023).

Consequently, melt estimates from regional climate models like RACMO that include the temporal intra-annual variability, the continent-wide as well as the long-term coverage, are thus currently “the best we have”. We chose to tune our scheme to RACMO because we think that this is currently the best model available for Antarctica (Mottram et al., 2021), but in principle dEBM-simple can be tuned to any other RCM as well. The tuning parameters are not hard-coded but can easily be changed in other PISM simulations.

Accordingly, we have strengthened the justification of our use of RACMO for the model calibration in the Introduction (lines 134-145) and expanded on its limitations (Sect. 3.2.1).

I am also not fully convinced that dEBM-simple is doing a better job than the PDD scheme. For example, all analyses for the historical period are quite similar, and the PDD scheme better matches RACMO2’s melt magnitude at the end of the century (Table 2). In addition, the maps of “present-day” (via the CESM2 historical scenario) melt (Figure 2a, Figure S7a) and their difference with RACMO2 (Figure 2b, Figure S7b) seem to suggest that the PDD scheme may be doing a better job at capturing some of the spatial characteristics of melt (e.g., across Ross, Ronne-Filchner, the Ross/Amundsen/Bellingshausen coasts) compared to RACMO2-CESM2 that is shown in the Figure S4 inset. The RACMO2 data in the Figure S4 inset also seem to better agree with the spatial distribution of melt determined via satellite observations (cf. the cited Trusel et al 2013 paper), which suggests that dEBM-simple is underestimating melt in these low-melt regions. Note that this is counter to what the authors describe in the text of a general overestimation of melt in low-melt areas. The maps of dEBM-simple and PDD vs RACMO2 over the AP also seem to show lower biases in melt over the high-melt AP using the PDD scheme. The statistics listed (slopes and R values) are all quite similar as well, so I am somewhat concerned about whether dEBM-simple is doing a better job.

Response: First, we would like to clarify a potential misconception: in fact, proving that the dEBM-simple model is superior to PDD is not the point of the paper. Indeed, PDD has over the past decades repeatedly demonstrated to be an effective method to model surface melt in long-term ice sheet modeling studies with a surprisingly good overall performance given its simplicity and computational efficiency. That said, the main caveat of PDD is its negligence of the melt–albedo feedback, a limitation which is improved upon in the more physically-based dEBM-simple approach, without compromising on the number of required forcing inputs or computationally efficiency. The melt scheme is very comparable to the originally proposed by Krebs-Kanzow et al. (2018), only simplified by the parametrizations for albedo and atmospheric transmissivity (Zeitz et al., 2021). Krebs-Kanzow et al. (2018) rigorously compare the dEBM to PDD for Greenland and benchmark both against RCM data. We made sure to refer to Krebs-Kanzow et al. (2018) and Zeitz et al. (2021) prominently, so that interested readers can convince themselves about the differences between the dEBM and PDD melt schemes. Statistics about the performance of the "full" dEBM model can further be found in Fettweis et al. (2020) (also cited in the manuscript) and below.
In the revised manuscript we have reemphasized the main goal of the study, have added a more extensive comparison between dEBM-simple and PDD (incl. 2 additional supplementary figures Fig. S11 and S17) and added more discussion of the PDD limitations (lines 102-110; Appendix A).

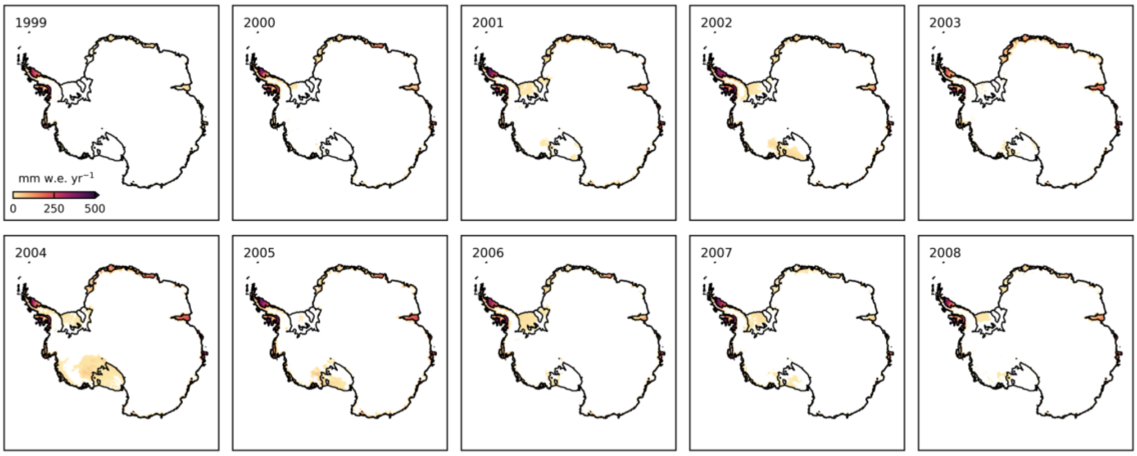
That said, we like to address some of the specific points raised by the referee below in more detail:

- *"(...) the PDD scheme better matches RACMO2's melt magnitude at the end of the century (...)"*
  While this seems true on first glance, we like to point out that this better match with RACMO may partly happen by coincidence: PDD's substantial overestimation of the mean 2090-2100 melt area (Figs. S16, S17) in combination with slightly less underestimated melt rates on Larsen Ice Shelf (compare Figs. 15b and 17b) likely compensated for the overall slightly worse correlation with respect to RACMO compared with dEBM-simple (which is also much improved with the new calibration). Please further bear in mind that the fixed-geometry assumption of RACMO might overestimate total melt volume in 2100, since calving may reduce the ice shelf area particularly along the shelf edges that make up most of the cumulative melt. In that sense, ice-sheet models that account for calving are expected to predict lower totals compared to RCMs.

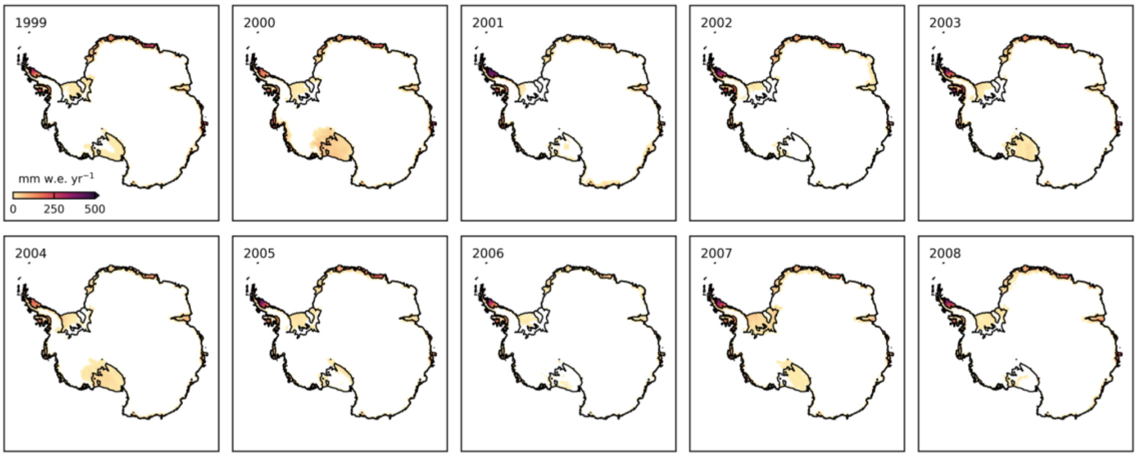
- *“(...) the PDD scheme may be doing a better job at capturing some of the spatial characteristics of melt (...)”*
  That is not entirely true. Their performance is indeed comparable, but the overall agreement with respect to RACMO is better for dEBM than for PDD (for both the whole AIS and the AP). This can be seen by the smaller deviations of the regression line slopes of dEBM compared to PDD in Figs. S9 and S11. While PDD shows less spread (R-value) with respect to RACMO, its overall bias towards lower melt rates is stronger.

- *“The RACMO2 data (...) also seem to better agree with the spatial distribution of melt determined via satellite observations (...), which suggests that dEBM-simple is underestimating melt in these low-melt regions. Note that this is counter to what the authors describe in the text of a general overestimation of melt in low-melt areas.”*
  There seems to be a misconception here and we kindly disagree. Mean melt rates on Filchner-Ronne or Ross ice shelves at present are almost negligible in the cumulative melt volumes, so the extent of the melt area is not an appropriate measure and may perhaps be misleading, especially when averaging over multiple years. In fact, the apparent large melt area extents in FRIS / RIS in the Fig. S6 inset are rather an artifact stemming from only a few individual “outlier” years. The figures below compare the 10 individual years from the QuikSCAT record with RACMO, highlighting in particular that QuikSCAT for example shows only one shelf-wide melt event over RIS over that 10-year period (2004), whereas RACMO simulates at least four over the same period (2000, 2003, 2004, 2007). The absence of significant melt in these regions has been confirmed in other studies as well (e.g., Orr et al., 2023).

In conclusion, since the overall performance of dEBM-simple and PDD is comparable (with a slight tendency towards the calibrated dEBM-simple doing the better job), it is in general more preferable to use the more physically-based approach, which can account for the melt–albedo feedback, whereas the validity of the linear temperature–melt relationship of the PDD is unlikely to remain valid under future warming conditions.



QuikSCAT



RACMO2.3p2-CESM2

The coastline/ice shelf extent displayed in Figure 2 and other maps is not an accurate reflection of the current extent of grounded and floating ice in Antarctica. For example, most ice shelves in West Antarctica appear to be missing, as is George VI on the AP. I presume this is because the PISM model does not simulate ice shelves here? Could the authors please comment on this in the text? Also, while ice shelves of Queen Maud Land appear to be present, they do not all seem to be marked as ice shelves. For example, the melt pattern of what looks to be Roi Baudouin ice shelf exists, but it's not shown as an ice shelf. Why is this?
Response: This is indeed true: the discrepancy arises due to the difference in ice-sheet topographies that were used in PISM and in RACMO as a result of the atmospheric temperature lapse-rate effect. In PISM, the present-day ice sheet state is derived via a model spin-up procedure that spans tens of thousands of years in which the grounding line is allowed to evolve freely according to the (fixed) boundary conditions and the thickness evolution on the basis of the flotation criterion. Consequently, slight deviations in modeled ice geometry with respect to observations are to be expected.
Nonetheless, based on the referee's comment and to overcome this inconsistency between air temperatures / melt rates and topography we have decided to re-do the calibration on the basis of the observed (Bedmap2) ice sheet topography.

Indeed, this resolves the grounding line mismatch in the historical simulations and leads to a slight overall improvement in performance with respect to modeled melt patterns. However, in contrast to the calibration experiments that were run assuming a fixed topography, the prognostic future simulations can only be initialized from the spun-up PISM state and thus have a slightly different initial geometry. In these simulations it would be 'unrealistic' to assume a fixed present-day geometry (as for example done in RACMO).

My last, and perhaps most important, major concern is with how runoff is calculated. The authors estimate 50% of surface melt becomes runoff. In comparison, the RACMO2.3p2 melt/runoff ratio over the contemporary period is ~6%, and in 2090-2100 is ~23%. Over the contemporary period, the dEBM-simple results therefore unrealistically suggest 9x the amount of runoff compared to RACMO2. At the end of the century, dEBM-simple produces ~160% of the runoff of RACMO2, yet only 70% of the amount of RACMO2's melt. Estimating runoff to be fixed at 50% of melt is not physical, and this has very important consequences for other conclusions of the presented manuscript including constraining the future SMB, total mass budget, surface elevation changes, and ice dynamics.

Response: We completely agree with the reviewer's concern that the assumption made in PISM of a uniform (temporally & spatially constant) refreezing factor poses indeed a model caveat that might introduce considerable uncertainty, especially in long-term projection runs and that we aim to improve in the future.

First of all, however, please also note that our runoff calculation does not affect the dEBM-simple calibration, as this is run using a fixed geometry (see line 424) and thus modeled meltwater runoff rates have no effect on the ice-dynamical evolution. This is different in the prognostic future runs, where runoff impacts SMB and hence elevation changes.

For the time being, we account for the uncertainty arising from the PISM assumption by performing additional sensitivity runs with different values for the refreezing parameter. Since the parameter is just a plain scaling of the produced meltwater, we can cover a large range of uncertainty by using a "higher-end" refreezing value and a "medium-/lower-end" value. We use both values in all simulations and contrast the respective results but use the lower value as default for the future scenarios. Thereby, the higher value is intended to be more representative of present-day climatic conditions and the lower value is intended to be more representative of end-of-century climatic conditions assuming an SSP5 warming scenario.

In the supplement, we now show an additional figure that contrasts the refreeze-per-melt fraction based on RACMO data for present day and at the end of the century (Fig. S1). Under present conditions, the AIS-wide mean refreeze fraction of RACMO is about 100%, while the full range of values is between ~20-140% (which may result from other processes not considered). Since melt occurs predominantly on ice shelves (with highest melt on the AP, which already today shows runoff fractions up to ~40-50%; see Fig. 3 from Gilbert & Kittel, 2021) while melt on grounded ice is generally so low that the influence of the exact refreezing amount on ice dynamics is negligible compared to the ice shelves, a value of 90% ("high" refreezing) seems plausible as a continental mean for present-day conditions.

For the future warming scenario, the RACMO freezing fraction averages about 70% across the ice sheet, while the full range of values shows an enormous spread of nearly zero up to 120%. In many parts of most ice shelves where intense melting occurs, the value is at or below

50%. Gilbert & Kittel (2021) calculate a mean runoff fraction of ~70% in 2100 and a mean runoff-per-melt fraction of ~45% across all ice shelves at a warming of 4°C, which could be exceeded in the second half of the century under SSP5. For the "medium-/lower-end" refreezing fraction, we thus assume a value of 50% as a continental mean representative for future warmer conditions.

Comparing the results obtained with both refreezing values in our sensitivity runs shows that the overall patterns of committed ice-dynamical changes under ~2100 climate remain similar and have only little effect on the presented results, so all main conclusions from the study remain unaffected. We thoroughly discuss these findings in Sect. 5.5 of the revised manuscript (starting from line 742) and in the Discussion (lines 853-858).

Further changes of the manuscript include, among others, an expanded paragraph on the refreezing parameter in Sect. 2.2.2 (lines 269-277) as well as two more supplementary figures assessing the impact of the uncertainty in the refreezing parameter in the commitment simulations (Figs. S19 and S20).

Minor comments

L17: Please clarify here if the speed up in ice flow from elevation reductions are related to SMB decreases

Response: Done. Based on the reviewer's comment, we have also expanded on the respective text in Sect. 5.5 (lines 748-754). As explained in more detail there, the ice flow speed-up and elevation reductions are not *purely driven* by SMB decreases (mean SMB is still positive in the respective regions) but are *caused* by them. Furthermore, surface melting acts as a trigger for the melt–elevation feedback, which in turn causes more thinning and consequently enhanced dynamically-driven ice discharge into the ocean.

Please also note that all variables in Fig. 6 are plotted with respect to a present-day control simulation, i.e., no changes in ocean conditions, sub-shelf melting, or calving are applied.

L58: Regarding supraglacial lakes "play a major role in the ice sheet mass balance in East Antarctica" – My understanding is that the cited Stokes et al 2019 and Arthur et al 2022 papers assess the presence and variability of supraglacial lakes, but not their role in the ice sheet mass balance. Given that the cited papers do not discuss runoff of water from the lakes to the ocean (to my knowledge), the lakes are important in an energy balance/surface hydrology/ice shelf stability perspective, but not currently important in terms of the ice sheet mass budget.

Response: In fact, that's what we wanted to say – SGLs play an *indirect* yet important role in the ice sheet's mass balance via at least three major mechanisms: 1) they decrease the ice surface albedo and thus increase the absorption of incoming solar energy, enhanced by the positive melt–albedo feedback; 2) the rapid drainage of SGLs has been linked to the collapse of ice shelves, which can cause increased ice discharge from grounded ice via the reduced buttressing effect; 3) the drainage of SGLs to the bed of grounded ice in Greenland has been linked to transient speed-ups in ice velocity.

We agree that our wording might have been misleading and have rephrased the text accordingly.

L97: Description of PDD schemes here not entirely correct. They do not assume melt is “proportional to the number of days” above zero, but rather the cumulative sum of air temperatures above zero. I believe you more correctly describe PDD schemes later in the manuscript.

Response: Thank you for pointing us to this mistake. We have reworded the PDD description accordingly.

L245: Does the atmospheric transmissivity calculation include the effects of cloud climatologies in any way? This seems like it would be a relatively easy way to crudely account for cloudiness and perhaps reduce discrepancies with RACMO2 in cloudy areas like on the western AP, where dEBM-simple produces more melt than RACMO2.

Response: As detailed in Zeitz et al. (2021) and in Sect. 2.2.3, the parameterization of insolation-dependent melt in dEBM-simple is based on a linear fit of the atmospheric transmissivity as a function of the ice-sheet surface altitude. Since the fit parameters are derived from 1950–2015 average RACMO data, they are based on long-term average Antarctic summer cloud conditions. However, following the philosophy of the dEBM-simple approach, all parameters in this parameterization are constant in time and space.

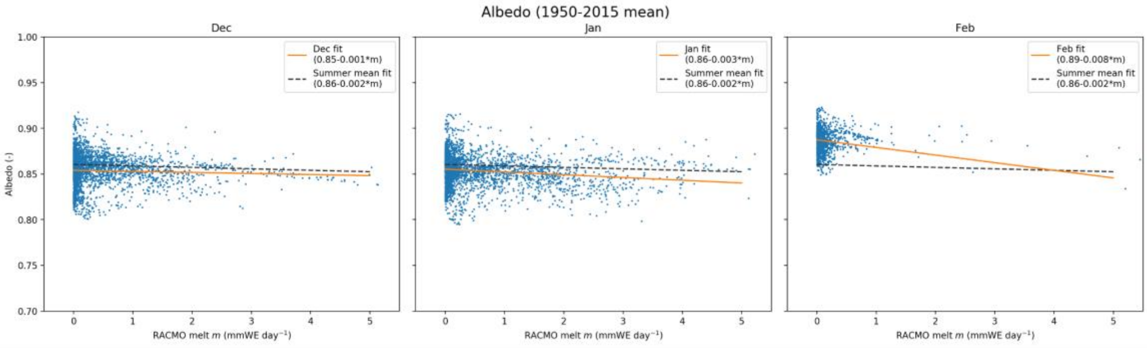
As one of the main differences to the “simple” approach, seasonal and spatial cloud cover changes are indeed accounted for in the “full” dEBM model, published by Krebs-Kanzow et al. (2021) (see lines 234-237). In fact, it appears that the “full” version of the dEBM model is able to reduce some of the spatial discrepancies with respect to RACMO especially on the western Antarctic Peninsula that are present in the “simple” version.

For more details, please also see our response to the 2nd major comment by Referee #2 below. Note that those results are preliminary and derived with a standard model configuration calibrated for the Greenland Ice Sheet which is not yet calibrated for Antarctica – but will hopefully be done in the future!

L263: It’s stated here that “RACMO data show no clear dependence between melt and albedo values under historic and present-day climate conditions”, but is this true? For example, the cited Jakobs et al 2021 paper uses RACMO2 to show the melt-albedo feedback is important. Other work, like that of Lenaerts et al 2016 (Nature Climate Change; doi:10.1038/NCLIMATE3180) also show the importance of the melt-albedo feedback.

Response: We are sorry for the confusion caused, as we certainly do not debate that there is a dependency / feedback between melt and albedo in reality (as shown, e.g., in Lenaerts et al., 2016, Jakobs et al., 2019, or Jakobs et al., 2021).

However, as a matter of fact, there is no clear dependence between the two variables visible in the monthly averaged RACMO data over the historical period, at least when looking at Antarctic-wide multi-year monthly mean values (see figure below). To avoid misunderstandings, we included “AIS-wide monthly mean” in the respective sentence (line 307). Since in the dEBM-simple implementation albedo is a function of melt (Sect. 2.2.4; Zeitz et al., 2021), comparing albedo and melt from the period 2085–2100 resolves this dependence more clearly (Fig. S3). Please note that our sensitivity ensemble explores the impact of the related parameter uncertainties in more detail (Sect. 5.4).



L370: Change to "historical"
<u>Response:</u> Done.

Figure 1c/d: The units here (and associated manuscript text) regarding monthly melt rates are confusing to me. First, should these be per month, not year (as these are monthly melt rates)? Second, are the values actually mm w.e. averaged over some area, not Gt? I fail to see how monthly melt rates could be several hundred Gt, yet yearly rates shown in panel a are <160 Gt/yr.

<u>Response:</u> In an effort to avoid too much confusion and for reasons of better comparability, we chose to present Antarctic-wide aggregated melt rates (as, for example, in Fig. 1) as a melt volume flux that is always given in Gt/yr for easier comparison. Monthly values (like, for example, in panel b) hence need to be divided by a factor of ~12 accordingly, to get the respective total melt volume of the given month.

The units of total (monthly or yearly) melt volumes as shown in Fig. 1 are indeed Gt/yr, as they are derived through aggregation of melt given in mm w.e./yr (or kg/sqm/yr) across the total ice sheet area.

We have added a clarifying note to the caption of Fig. 1 as well as a footnote in the main text.

L450: When discussing how dEBM-simple tends to underestimate melt in high-intensity regions and overestimate melt in low-intensity regions (notwithstanding my above 'major' comments to this regard), it would be helpful to include as supplementary figures maps of the difference between dEBM- and PDD-derived melt and RACMO2 across all of Antarctica, both for the present-day and future. This would allow for a better understanding of where (and potentially why) discrepancies exist between the methods.

<u>Response:</u> Thank you for this suggestion, which we appreciate. We have added the respective maps in the supplement (Fig. S9, S11, S15, and S17). The initial idea of just showing a cutout of the Antarctic Peninsula region was done in an effort to zoom in to the region with the highest biases, as smaller biases would otherwise be almost unrecognizable.

Figure 2: There's an apparent circular/wavy pattern appearing between 1000 and 1500 mm w.e./yr. Could you comment on what is producing this? This also appears in Figure S7.

Response: The wavy pattern appears to be an artifact stemming from RACMO, rather than from the dEBM-simple model (it is already present in Fig. S4, which compares RACMO with satellite-derived melt estimates from QuikSCAT). It could potentially be related to grid resolution, but we would rather like to avoid too much speculation here.

Figure 2c, S7c: Please define what “n” is.

Response: *m* and *n* are the slope and intercept of the regression lines, respectively, and *R* is the Pearson correlation coefficient. We have added this explanation in all relevant figure captions.

Figure 3: The colors for melt and runoff are hard to distinguish. Please use a different color for runoff. Also, in the caption, it states that positive values of surface melt and runoff denote mass losses. Presumably, this should only say that runoff is mass loss, correct? Lastly, the albedo map in panel c is difficult to assess as it is not compared with present-day albedo. I would suggest rather than plotting the absolute value of albedo, the difference with present-day albedo could be plotted.

Response: We have adjusted the colors in this figure and amended the figure caption. Following the referee’s suggestion, panel c now shows the albedo change from 2015 to 2100.

L474+L492: This is actually the “western” margin considering south is up.

Response: Thank you, that’s a good spot! We have corrected the mistake in the revised manuscript.

L657: Again, I suggest the authors create maps of difference between their results and RACMO2 because the regional/spatial perspective (i.e., “high-intensity melt regions” and “low-intensity melt regions”) cannot be assessed in the scatter plots (i.e., Figures 1d, 2c, etc.).

Response: Done. Please also refer to our response to your comment on L450.

L693: Following on from my final “major” comment – I do not think the uncertainty produced by assuming runoff to be 50% of melt is properly explored. What would happen if runoff was fixed at 10%? 25%? Alternatively, what would the future ice dynamical evolution be if PISM was forced using (presumably more reliable) runoff rates prescribed directly from RACMO2?

Response: We fully agree with the referee’s concern regarding the uncertainty resulting from modeled runoff. As further explained above, our analysis now includes a high and a low refreezing parameter in all simulations in order to cover the full uncertainty range related to this model choice.

Our amendments of the manuscript include among others: an expanded paragraph on the refreezing parameter in Sect. 2.2.2 (lines 269-277); a new supplementary figure showing the refreeze-per-melt fraction from RACMO (Fig. S1); adding both values in Table 2; two more supplementary figures assessing the impact of the uncertainty in the refreezing parameter in the commitment simulations (Figs. S19, S20) alongside some more related discussion in Sect. 5.5 and in the Discussion section (Sect. 6).

As to the suggested alternative: we hope that the referee agrees that using modeled runoff rates from RACMO would go beyond the scope of the dEBM-simple approach, which is heavily based on the idea of computational efficiency and a least amount of forcing inputs.

Code and data availability: I would encourage the authors to consider uploading their code (particularly that to make the figures) to GitHub or Zenodo, rather than making it available “upon reasonable request”. The figures are all very nicely designed, and the community would benefit by being able to easily look at the underlying code!

Response: We thank the reviewer for their nice words and appreciate their suggestion. We will consider it until publication.

Referee comment RC2 by Ella Gilbert

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Review of "The evolution of future Antarctic surface melt using PISM-dEBM-simple", Garbe et al. submitted to *The Cryosphere* Jan 2023

Summary

The manuscript explores present and future Antarctic surface melting using a new surface melt module, dEBM-simple, coupled to the ice sheet model, PISM. The authors evaluate their configuration's robustness with respect to surface melting as calculated by a positive degree day (PDD) model and the RACMO regional climate model. They show good agreement between their surface melt results and those produced by the more sophisticated model, RACMO. They emphasise the relative computational efficiency of dEBM-simple-PISM in comparison to running a complex model like RACMO and its superiority over PDD-based melt estimates.

The manuscript is very well presented, with a compelling argument and clear figures. It is a welcome contribution to the field that showcases an important tool. I have some general comments and suggestions that I feel would improve the manuscript, which are detailed below. My main concerns relate to the method of tuning present day / historical melt parameters to the same model that is used for validation (especially without a thorough discussion of RACMO's own errors and limitations) and the need for more justification of what we can learn from the simulations out to 3000. However, in general I think it would be highly suited to publication in the journal, subject to the authors making adjustments in light of my and other reviewers' comments.

Thanks to the authors for an interesting paper. EG

Response: We want to thank the referee for their positive evaluation of our manuscript and that they consider the manuscript "highly suited for publication in the journal"! Their insightful comments and suggestions have greatly helped us to improve this manuscript. We have implemented them in the revised manuscript version and respond to them point by point below.

General comments / suggestions

Further discussion of the limitations of the PDD method could be included (e.g. in the lit review) to set up the significance of the work and usefulness of dEBM-simple

Response: We appreciate the reviewer's suggestion; however, we would also like to stress that it is not the aim of this study to denigrate the PDD approach. In fact, PDD has over the past decades repeatedly demonstrated to be an effective method to model surface melt in long-term ice sheet modeling studies with a surprisingly good overall performance given its simplicity and computational efficiency. That said, the main caveat of PDD is its negligence of the melt–albedo feedback, a limitation which is improved upon in the more physically-based dEBM-simple approach, without compromising on the number of required forcing inputs or computationally efficiency.

We have added a brief paragraph on the limitations of the PDD approach in the Introduction (lines 102-110; Appendix A) and try to make this point in the revised manuscript clearer now.

Some (brief) quantitative comparison between the simple/full versions of dEBM results would be informative

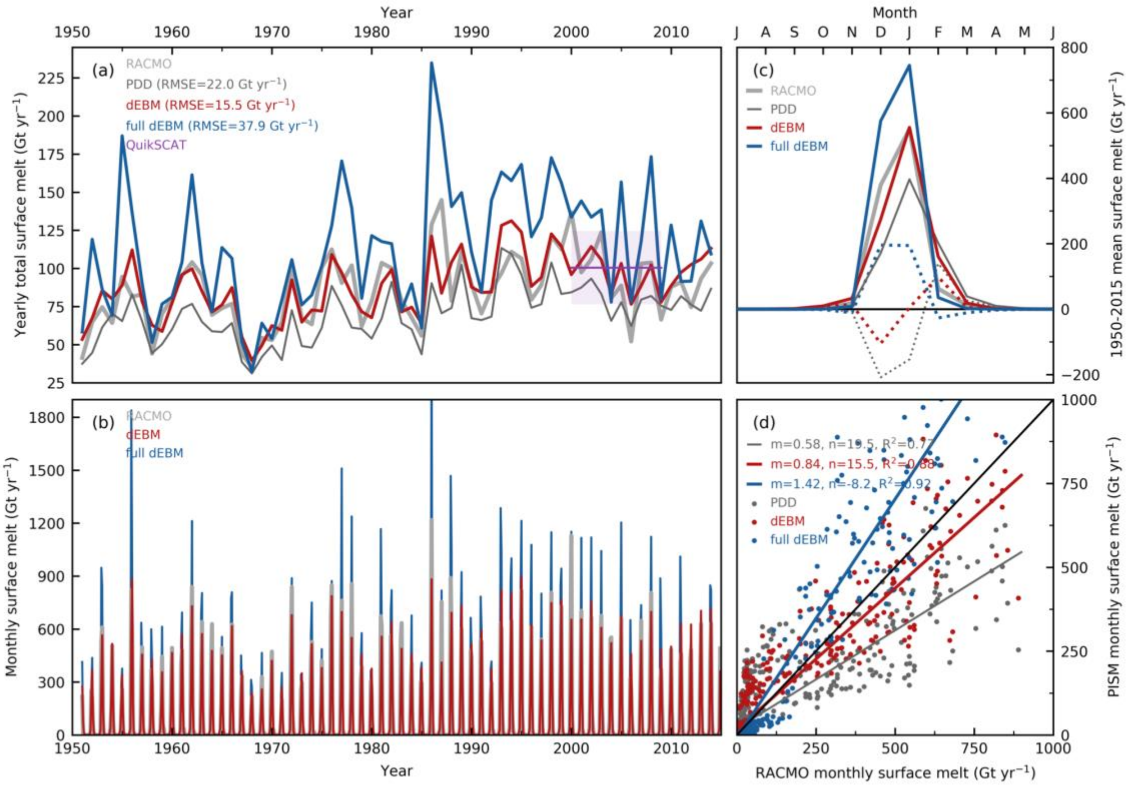
Response: While we absolutely agree that such a comparison would certainly be very informative, we would like to point out that the manuscript is already very comprehensive and applying the full dEBM scheme to the Antarctic Ice Sheet would be more worthy of a separate dedicated follow-on future project. Also, such a comparison is given in Krebs-Kanzow et al. (2021), who apply the full dEBM model to the Greenland Ice Sheet (we made sure to cite this paper prominently in our manuscript).

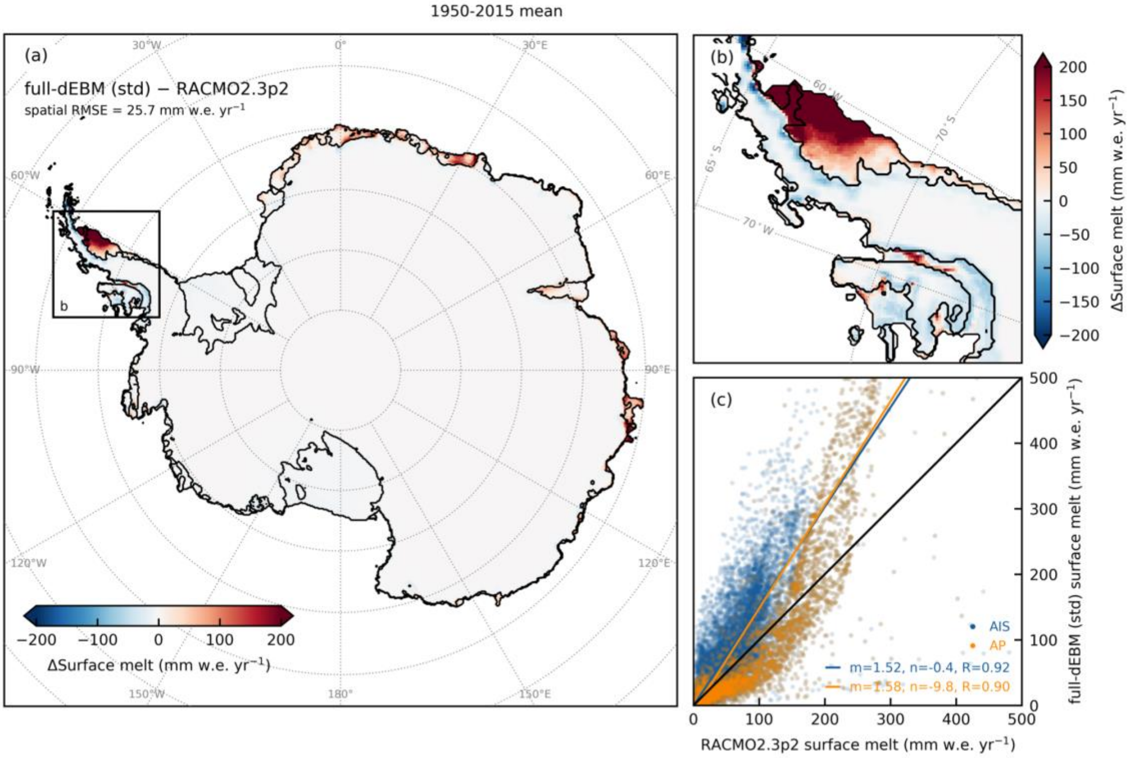
In the two figures below (similar to Fig. 1 and Fig. S9 from our manuscript), we show here a first rough comparison with the standard (Greenland-calibrated) version of the full dEBM scheme but would like to refrain from including it in our study.

As might be expected, the regression fits with respect to the 1950–2015 melt predictions from RACMO indicate an overall good agreement (AIS-wide temporal R-squared value of 0.92 in comparison to 0.88 from dEBM-simple and a AIS-wide spatial R value of 0.92 in comparison to 0.38 from dEBM-simple). Perhaps unsurprisingly, this indicates that the more complex the model (and the more forcings are used) the better is the overall correlation and regression fit (PDD < dEBM-simple < full dEBM).

On the other hand, however, high-intensity melt rates are strongly overestimated by the full dEBM in its un-tuned standard version (regression slopes of 1.42 in Fig. 1d and 1.52 in Fig. S9c, compared to 0.84 and 1.07 for dEBM-simple, respectively), which is most likely due to un-tuned albedo values; the slopes of the regression fits from the standard run could already be improved by preliminary calibration through adjusting the wet and dry snow albedo (not shown).

We like to stress again that the full dEBM is not “fit for purpose” in our scope, as more forcings from RCMs are needed which are not available on the long timescales that are the focus of the PISM-dEBM-simple approach and that are required for the study of some of the major ice-sheet–climate feedbacks.





The temperature-melt index of Orr et al. (2022) could also be an interesting comparison for your work, to put your results into further context. https://doi.org/10.1175/JCLI-D-22-0386.1

Response: Thank you for this suggestion. We have added a short paragraph on the Orr et al. (2022) temperature-melt index approach in the Introduction (lines 113-118).

How well do RACMO / dEBM-simple capture melt associated with orographic features around the edges of the ice sheet e.g. foehn winds / adiabatically warmed katabatic outflow? I'm thinking especially of the Antarctic Peninsula – it seems from Fig 2 that there is limited melt adjacent to the mountains over Larsen C for example. At 27 km it is doubtful that RACMO will capture these sort of dynamics – even the 8 km PISM grid might be too coarse. What does this mean for melt estimates?

Response: As far as we know, melt dynamics resulting from orographic features are in principle accounted for in RACMO, although, as the reviewer correctly points out, it is doubtful how well they are captured here given the coarser resolution of the continental ice sheet setup. To our knowledge, improvements in this regard are in progress by B. Noël et al., (EGU 2023 poster) who statistically downscale RACMO melt rates to higher resolutions and found a substantial increase in melt particularly near the grounding line (e.g., of Larsen Ice Shelf).

In the dEBM scheme, such orographic effects are not included. However, as the primary focus of the dEBM-simple model is on very long timescales (i.e., glacial-cycle paleo or deep-future applications) which usually require coarser model resolutions, we argue that smaller-scale melt characteristics or single extreme melt events are likely to be of less importance, and the melt 'climate' is more important than the melt 'weather' (c.f. Broeke et al., 2023).

We have added a corresponding paragraph in the Discussion (lines 859-864).

The tuning of melt parameters to RACMO foreshadows the results. Although RACMO is undeniably a good model for estimating melt, it still contains errors and there is limited discussion of this in the paper. It would be better if dEBM-simple could be independently validated, for example against observational/satellite datasets, and then compared with RACMO. Otherwise the comparison in sect 5.1 against RACMO is somewhat meaningless because if you tune your melt parameters to match RACMO output, it's unsurprising that the results are similar.

Response: Please be referred to our response to the first major comment by Referee #1.

Clearly it is difficult to project beyond 2100 without any kind of post-2100 emissions scenarios. I recognise that you have attempted to address the lack of such input data here, but how useful is the fixed 2100 simulation for telling us about the deep future? It tells us more about the feedbacks and impacts of the 21st century high-emissions scenario than anything beyond 2100. I would like to see a little more justification and discussion of what we can learn from this particular experiment in Sect 5.4.

Response: That is completely true and perhaps a misconception that is presumably based on our unfortunate choice of words, for which we apologize. It is indeed not the aim of these simulations to tell us something about the deep future, but instead help us to understand the 'real' ice-dynamical effects of enhanced surface melting that might be reached at the end of century under the high-emission SSP5 scenario but that might not have played out in full in the year 2100 due to the long response times of the ice sheet and feedbacks that operate on longer timescales (i.e., "committed impacts").

Accordingly, we have removed the word "long-term" from the manuscript and now use "commitment / committed" instead.

I have also noted this below, but I would benefit from better explanation of how the isolation-dependent and temperature-dependent components of melt are separated.
Response: The temperature- and insolation-driven melt components are defined in the beginning of Sect. 5.3. We have amended this part and added more explanation, including references to the corresponding equations from Krebs-Kanzow et al. (2018), in the hope that it is clearer now (starting from line 630).

Specific comments

170-172 Does the thinning/ loss of ice shelves feed back on the speed of glaciers and therefore ice discharge? Apologies if I've missed this elsewhere.
Response: Yes it does; due to the non-local solution of the shallow-shelf approximation that PISM is based on (Winkelmann et al., 2011) ice shelves influence upstream grounded glacier velocities as well. However, the 50 m thickness calving threshold has a negligible influence in this regard and is merely imposed for numerical stability, as noted in the next sentence of that paragraph (note that ice shelves with thicknesses < 50 m are also not observed in Antarctica). To clarify this, we added "dynamical" to the next sentence, which now reads: "The latter two calving conditions are mainly imposed due to numerical reasons and have only negligible influence on the overall ice-sheet dynamical evolution." (lines 207-208)

230 Is RACMO melt corrected/validated before optimising to it? Optimising or tuning to an RCM (even a good one) still introduces error. It is also not an entirely independent comparison for the results.
Response: We agree and acknowledge this as one of the caveats of our study. To account for the uncertainties introduced by the RACMO forcing, we have added a more thorough justification for our use of RACMO in the Introduction of the revised version of the manuscript (lines 134-145), expanded on RACMO's model limitations (lines 357-367), and added a comparison between RACMO-predicted melt rates and satellite-derived melt estimates from QuikSCAT in the supplement (Fig. S4).

232-234 So, this means half of the meltwater generated runs off the surface? This is surely a significant overestimate for the present climate, when very little runoff occurs except over ice shelves? As far as I understand, 50% runoff may be a valid assumption for ice shelves by the end of the century (c.f. Gilbert & Kittel, 2021 – Fig 3) but it still strikes me as high for the grounded ice sheet.
Response: Please be referred to our response to the fourth major comment by Referee #1.

292-295 Choice of averaging period for climate data (RACMO) – why this one? Does it affect the boundary conditions? And why does it differ from the ocean forcing? (presumably because of data availability?)
Response: Yes, the reason for this choice is indeed data availability. Note that the procedure described in this section (Sect. 3.1) only refers to the model spin-up that is run over 400 kyr (thermal spin-up) plus 22 kyr (full-physics spin-up) and which requires averaging the boundary

forcing climate over a sufficiently long period in order to represent the “mean” present-day climate state as well as possible. Also, on these timescales, other processes (e.g., basal sliding of ice, subglacial hydrology, ocean-induced melt, calving etc.) likely have a larger impact on the overall ice-sheet evolution compared to the influence of the surface mass balance. For more details regarding the model spin-up procedure, please refer to Reese et al. (2020), which is also cited in this section.

296 Are the climatic BCs just repeatedly applied? E.g. the same climatology is applied every year for 22000 years?
Response: As mentioned above, here, during the spin-up, only a mean climate state is used without a yearly climatology. This mean climate is held fixed over the entire spin-up period. The procedure is just briefly summarized in this section, but more details of the ISMIP6 experimental setup are given in Reese et al. (2020) (which is cited twice in this section).

310 - 314 Here you justify your use of RACMO2.3. I think this would be strengthened by acknowledging the biases in RACMO too (if I recall correctly from the Mottram et al paper, RACMO still under-estimated the SMB slightly, although less than some of the other ensemble members)? You could also note that RACMO has one of the more sophisticated surface schemes, which has feedbacks on the quality of its atmospheric outputs.
Response: We have added a more thorough justification for our use of RACMO in the Introduction of the revised version of the manuscript (lines 134-145). We have also expanded on RACMO’s model limitations in Sect. 3.2.1 (lines 357-367) and have added a comparison between RACMO-predicted melt rates and satellite-derived melt estimates from QuikSCAT in the supplement (Fig. S4).

323 “The precipitation field is independent of the evolving ice-sheet geometry” – meaning that there are no changes in orography-precipitation interactions as the ice sheet evolves? Could be worth spelling out the implications of this statement.
Response: In PISM, usually the SMB is adjusted according to changes in surface elevation (in addition to changes in near-surface air temperatures) to account for the atmospheric lapse rate effect. In terms of precipitation this means: a lower ice surface altitude generally implies warmer air temperature and hence in general more precipitation according to the Clausius-Clapeyron law (see e.g., Frieler et al., 2015). (In terms of temperature, this effect is indeed taken into account for surface melting both in dEBM-simple and PDD, accounting for the surface melt–elevation feedback.) In practice, to account for this orographic effect on precipitation rates, precipitation is usually scaled with surface elevation changes in PISM (see e.g., Garbe et al., 2020). However, since the precipitation increase due to Clausius Clapeyron is already accounted for in the forcing input from RACMO, we set the corresponding scaling coefficient to zero to avoid double accounting, thus, on the downside, neglecting the mitigating effect of precipitation increase in the warmer atmospheric layers of lower surface altitudes in case of substantial elevation changes of the PISM ice topography.
We have added some corresponding text in the revised manuscript in Sect. 3.2.1 (lines 375-382) and in the Discussion (lines 359-364).

Fig 1 caption. Add that RMSE values are shown for each model compared to RACMO in panel a)
Response: Done.

419-422 but the melt peak is captured well in Fig 1c. , with virtually zero difference between RACMO/the 2 models in Jan. This is encouraging given that this is when melt is most intense
Response: Thank you for pointing this out. We have added a corresponding note to the text.

423 Missing processes such as?
Response: Added in the revised manuscript (lines 489-490). Also see Sect. 2 of Krebs-Kanzow et al. (2018) for a derivation of the dEBM-simple melt equation.

425 did you do any sensitivity tests to explore the impact of piecewise vs default interpolation?
Response: Yes, we have done now and also have added a figure similar to Fig. 1 in the supplement, showing the differences between the piecewise-constant and the interpolated temperature inputs (Fig. S21). See Appendix A for more details.

Sect 5.2 / Tab 2 This is perhaps a point for the discussion, but it would be interesting to see how your results compare to previous estimates of future SMB change, e.g. Kittel et al 2021 (https://tc.copernicus.org/articles/15/1215/2021/), Lenaerts et al. 2016 (https://link.springer.com/article/10.1007/s00382-015-2907-4), Donat-Magnin et al 2021 (https://tc.copernicus.org/articles/15/571/2021/)
Response: We thank the reviewer for these suggestions and are happy to include them for comparative reasons in the revised version of the manuscript.

500-501 As per my previous point re: resolution/the hydrostatic nature of RACMO, could this under-estimate actually be even greater if RACMO itself is under-estimating high-intensity melt hotspots? Although I acknowledge that you state that this has limited impact on overall totals…
Response: In fact, the comparison between RACMO and the melt estimates from QuikSCAT (Fig. S4) reveals a slightly differential picture: while Alexander Island (Wilkins and George VI ice shelves) are underestimated in RACMO, the northern tip of the peninsula (including the northern Larsen Ice Shelf) are overestimated with respect to QuikSCAT, suggesting only less-important overlap with the regions of significant underestimations in dEBM-simple (i.e., northern Larsen ice Shelf; compare Fig. S12 and S15). We have added some more explanation on this in the text (starting from line 595).
Note that Dalum et al. (2022) also points out that the melt fluxes observed by QuikSCAT in the Alexander Island region may also be overestimated due to extensive melt ponding and/or saturated firn conditions there, a feature which negatively affects the QuikSCAT retrievals.
Lastly, note that the new calibration (in combination with the corrected common surface mask of PISM and RACMO that is used in the analysis) substantially decreased the mismatch between dEBM-simple an RACMO in 2100 from ~30% to ~13% (Tab. 2, Fig. 5).

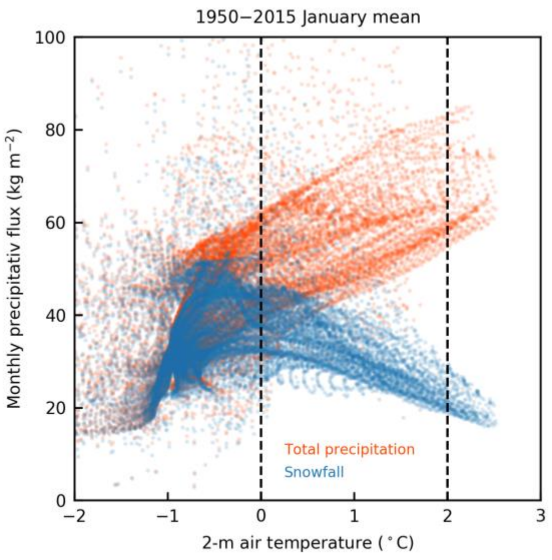
509+ Worth re-stating here that the melt equation describes average melt **when temperatures are above the melt threshold ** I was originally confused by this as melting can of course only occur when there is a surplus of energy available to do melting and (sub-)surface temperatures are at the melting point. But re-reading section 2.2.2 I see the equation considers only melting when the temperature condition is met. A little more explanation of what the separate components of ‘temperature-driven’ and ‘SW-driven’ melt really mean would be welcome.
Response: Done. We added the T>T_min condition to the dEBM melt equation (Eq. 6) and repeated it in the sentence at the beginning of Sect. 5.3 again. We expanded on the explanation of the different dEBM-simple melt components in that section, including adding again references to the corresponding equations from Krebs-Kanzow et al. (2018), in the hope that it is clearer now (starting from line 630).

What causes SW-driven melt to increase? Presumably this is related to the albedo feedback darkening the surface, reducing SWup and resulting in a measurable difference in SWnet?
Response: Correct, the increase in shortwave insolation-driven melt is due to the albedo effect. This follows directly from the definition of M_insol under reduced albedos, given that SW_Phi remains more or less unchanged and changes in tau are negligible. For more details on the positive melt–albedo feedback, please refer to Jakobs et al. (2019).

Fig 5 I can’t really see the purple shading in the main panel - can you make it darker? Also the grey text in the smaller panel is too light to read unless I zoom in really far and squint!
Response: We adjusted the colors in this figure.

Para beginning 620 – not sure this summary is needed. You can probably just launch straight into your discussion points.
Response: We have removed the first two paragraphs of this section.

643 How does the temperature-dependent split of rain/snow compare against rain/snow inputs from RACMO? Did you look at that?
Response: From looking at the RACMO inputs, it seems that the assumption made in PISM is justified (see figure below). Based on the average precipitation over the entire historical period (1950–2015), RACMO suggests values of about +2.5°C for the temperature threshold above which all precipitation falls as rain, and about –0.5°C for the temperature threshold below which all precipitation falls as snow, which is fairly close to the default values used in PISM (2.0°C and 0°C, respectively; black dashed lines).
Please kindly note that the dEBM-simple model only computes surface melt, while precipitation is not different from previous model configurations of PISM and not in the scope of the present study.



700 Evaporation may also become more important in future (especially under strong warming scenarios like SSP5-8.5)
Response: We have added a corresponding sentence in this section (lines 867-868).

Technical corrections

411 remove comma after “both”
Response: Done.

531 “as high as few degrees” → “as high as a few degrees”

Response: Done.

622 “to serve as full-fledged” → “to serve as a fully-fledged”

Response: Done.

706-710 Very long sentence! Suggest splitting into two.
Response: Done.

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