Response to Reviewer Comments

Journal: The Cryosphere
Title: **Impact of the melt-albedo feedback on the future evolution of the Greenland Ice Sheet with PISM-dEBM-simple**
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First of all, we would like to thank the editor Kerim Nisancioglu and the two reviewers, Signe Hillerup Larsen and Mario Krapp for their helpful and excellent comments and their efforts to create the detailed reviews! In our revision of the manuscript we addressed the main issues:

1. We have now included a comparison with the positive degree day melt model, which is widely used in the ice-sheet modeling community.
2. We have rewritten the methods section in order to increase clarity.
3. We have clarified the framing of the results and included some of the discussion points closer to the results.

We provide detailed answers to all comments below. The reviewers' comments are given in black and the authors' in blue. The changes made to the main document can be found at the end of this document (created with latexdiff).

Signe Hillerup Larsen

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Review of Zeitz et al in the cryosphere discussion

Summary

The study presents a new module for the ice flow model PISM, making it possible to include effects of changes in global radiation and albedo, in the model. In order to make a computationally efficient experiments, MAR albedo and global radiation is not used directly but implemented as parametrisations. This model setup is then used to explore the effect of the melt-albedo feedback in PISM. The manuscript covers a description of the method and performed experiments testing the effect of the implemented module and parametrisation on model prescribed ice mass loss.

The originality is that the study presents a new module to calculate melt in PISM. This module is as simple to implement as the pdd melt model, given that albedo and transmissivity is parametrised, but then offers the benefit of being able to add both effects of changing surface reflection, and incoming shortwave radiation. In this way it is possible to model the effect of changing incoming radiation on melt in for example the Eemian interglacial as well as investigating the effect of the melt-albedo feedback in predictive ice-sheet models.

The significance of the study is the implementation of a new module in PISM, making it possible to experiment with the melt-albedo feedback as well as orbital parameters in PISM, and that the modelled ice loss is significantly altered when using the melt-albedo feedback. A revision of the presentation of the study is my opinion needed in order for the paper to be published, but I think it is a matter making sure that the conclusions are discussed in the right context. See my comments below:

General comments

My impression of the manuscript is that conclusions are drawn on the mass loss due to the melt albedo feedback in general. This is, in my opinion, a job for a focussed study using an advanced (regional) climate model. I suggest to alter the focus of the manuscript to investigate the effect of adding the melt-albedo feedback in PISM projections. As an example changing the following sentence from the conclusions:

”Using PISM-dEBM-simple we find that the melt–albedo feedback can lead to additional 12 cm sea-level equivalent of mass loss in RCP2.6 and additional 70 cm in RCP8.5 until the year 2300”

to something like this:

”Using PISM-dEBM-simple we find that the melt–albedo feedback can lead to additional 12 cm sea-level equivalent of mass loss in RCP2.6 and additional 70 cm in RCP8.5 in the projected mass loss from PISM until the year 2300”

This is a good suggestestion to be clear about the distinction between the simple approach used in this manuscript and a full climate model. We incorporated the suggested framing in the manuscript.

The model performance is investigated from many different angles, but I think a comparison between using dEBM-simple with using the simple pdd module, on the MAR historical time series, could add some insights into that dEMB is actually a more physically based model – that as is also shown also makes it possible to make more realistic experiments further back in time where pdd factors are certain not to be the same, as is shown in the Eem experiment.

We now show the melt rates computed with pdd from the historic validation experiment with spatially resolved temperature fields (similar to Figure 1 and 2) and discuss them in Appendix D. We find that the pdd module, with standard parameters, gives a similar root mean square error over the historic period, both over the cumulated melt per year (39.85 Gt with pdd vs. 32.92 Gt with dEBM-simple) and over the spatial June, July and August melt (0.47 mWE/year with pdd vs. 0.36 mWE/year with dEBM simple). However, the pdd melt introduces a North-South gradient to the melt anomaly (compared to MAR data), overestimating the melt in the North and underestimating the melt in the South which underlines that the additional physics modelled with dEBM-simple improves the melt calculation in comparison to the pdd.

Abstract:

Page 1

Line 1- 3: The melt-albedo feedback that is investigated here is only on the snow part of the ice sheet.

While it is true that the albedo is only varied between a snow-albedo and an ice-albedo value, the feedback itself does not depend on the presence of snow. The parametrization can be easily adjusted to go below the ice-albedo value, possibly with a different slope (as we tested in Appendix B).

We have slightly adjusted the wording in the abstract, to reflect that the albedo can change over the whole ice sheet (“... may lower the reflectivity over the ice-sheet surface...”)

Line 3: add a sentence like: In order to test the effect of melt-albedo feedback in a prognostic ice sheet model, we implement...

done

See also general comments about focus of the conclusions.

We have adapted the conclusions accordingly.

Introduction:

From reading the introduction, I don't think it is entirely clear what the "-simple" refer to? Does it refer to the simple version of dEBM presented in the 2018 paper or does it refer to simplifications made in this study?

Thank you for the good question. dEBM-simple refers to the simplifications made in this manuscript. It is based on the version presented in (Krebs-Kanzow et al., 2018) paper. The main changes are adapted empirical parameters and the parameterization of albedo and atmospheric transmissivity. It is implemented in the Parallel Ice Sheet Model (PISM) and can replace the Positive Degree Day method for the calculation of climatic mass balance.

We have clarified the abstract and the introduction in that regard.

Page 2

Line 5-6: These are in fact areas that are not considered or discussed in the present study, later on this should be discussed in more detail.

Those areas serve as a motivation to perform the experiment with lowered ice albedo and overall the darkening experiment. We now mention this at the end of the introduction.

Line 7: Replace "As darker snow ..." with something like: As darker surface absorb more radiation than lighter surfaces, the effect of darkening due to increased melt could trigger...

done

Line 14: Replace "covered by meltwater" with: at melting point

done

Line 24-25: perhaps move the references up into the text: The insolation-temperature-melt equations defined/used by van den Berg and Robinson ...

done

Line 32: This is the first time you mention PROMICE – you need to add info on what that is. Or rewrite to say that the model showed good correlation with observations.

done

Page 3

Line 3: Delete "in addition". It is not in addition, but in order to do what you do as you state later in the sentence.

"In addition" should have referred to the fact that the parametrizations of albedo and transmissivity are in addition to the work of Krebs-Kanzow et al. (2018). This is clarified now in the manuscript.

Line 7: This is the first time you mention MAR – spell it out and add references

done

Line 18 – 19: "PISM was shown ..." I guess this is actually mostly true when run on a spatial grid below 1km. Here you run on 4.5 km (and that is completely fine for this purpose), but perhaps state the resolution issues here somehow.

done. We now say "PISM was shown to be capable of reproducing the complex flow patterns evident in Greenland's outlet glaciers at high resolution of less than 1km" and mention explicitly the lower resolution used in this manuscript at the end of the subsection.

Methods

The parametrisation of the melt-albedo feedback is the weakest point of the study, and care should be taken that aspects of the consequences of the simple melt-albedo feedback parametrisation are discussed thoroughly perhaps already in the methods section. For example – has this been done in other studies before? Discussion of in particular the melt albedo feedback parametrization is needed in order to be able to draw any conclusions of the contribution of this to the ice sheet mass balance. After reading the methods section. I have questions such as: What is the consequences of only looking at the snow zone – and thereby neglecting albedo increase in the bare ice zone? Where has the ice sheet been observed to have the alpha_min value that the study is using? How large is the part of the ablation zone that is snow covered compared to the bare ice zone?

As far as we know, this is the first study to parameterize the albedo with the melt rate directly. Many other studies, which use simple albedo parameterizations, include the snow (and sometimes firn) thickness explicitly, linearly or exponentially scaled (see Robinson et al., 2010 or Krapp et al., 2017). Some consider wet snow explicitly (e.g. Robinson et al., 2010). These methods do not in fact consider an albedo decrease in the bare ice zone, in contrast to e.g. the regional climate model MAR, which explicitly considers excess surface melt water, but not impurities, algae or bacteria or changes in the ice structure.

In our setup, scaling albedo with snow thickness led consistently to an underestimation of albedo and an overestimation of melt in the dry and cold northern parts of Greenland, which motivated us to find an alternative parameterization.

Lowering α_{min} expands the range of the feedback and serves as a first approximation to changes in the bare ice albedo. In order to distinguish albedo changes in snow and ice, one could use a different slope for albedo changes in ice. However, for simplicity the experiments as in Figure 5, where we explore the sensitivity of the melt to the minimal ice albedo, can be used as a first approximation. We find that, with the parametrization presented here, only high melt scenarios are sensitive to changes in minimal albedo.

We also include an analysis of daily observed MODIS albedos over Greenland in the Appendix A in order to show the areas where the albedo is equal to or lower than the minimal albedo αmin. We also discuss the parameterization in the methods in a more detailed way.

Page 3

Line 32: "We neglect ..." Do you allow for shelfs when you have fixed calving front position?

Yes, we do allow for shelves. As mentioned in the manuscript the outline is given by the BedMachine Data, (Morlighem et al., 2017). We do not allow the shelves to grow and do not consider changes in sub-shelf melt due to increasing temperatures.

Page 4

Line 9-11: I think this becomes a bit confusing. I suggest that you consider the following two paragraphs as the place where you introduce all the different parts of equation (1). This means that you do not need to mention albedo and transmissivity in line 9, as you will go through them later and the following sentences describing c1 and c2, should go down after you introduce Teff.

Thank you for the suggestion. We cleaned up the subsection and hope that it is clearer now.

Page 5

Line 17: Does this basically mean that the transmissivity is an average over 2019? A sentence or two of what this actually means in relation to the real transmissivity would be informative in order to understand the prognostic potential of the parametrisation.

The average is over the years 1958 to 2019, considering only the summer months (June, July and August). The parametrization relies on the assumption that the mean transmissivity does not change in a changing climate. In particular the impact of extremes, e.g. Greenland blocking, which might become more frequent in future, is not captured with this approach.

We have added this explanation to the manuscript.

Line 24: Perhaps mention here what you then neglect by introducing the linear relation, like effect of clouds.

Added in the paragraph above

Line 28: delete the sentence starting with "Regional climate models..." And start next sentence with something like "Snow albedo in MAR is calculated using a snowpack model, explicitly ..."

done

Page 6

Line 1: The sentence starting with "Ice albedo ..." could be rephrased to: "In MAR, ice albedo is explicitly ..."

done

Line 6: add information about MAR version and reference to data

done

Line 7: "Allow us to capture melt processes". I am not sure exactly what is meant here

We clarified the sentence.

Line 9: Introducing alpha_min: This means that you do not consider the darkening of ice at all. This should be pointed out somehow, maybe here or somewhere else, but it is an important point, and also, have alpha min been observed at anytime across the ice sheet during melt events?

We have pointed out now that the ice is not darkened in this framework, but it could be easily expanded into this direction. We also include an analysis of observed MODIS albedos in Appendix A and refer the reader to it. This analysis shows that albedo values between 0.45 and 0.5 are reached regionally for up to 50 days a year (average over 2000-2019). However, the majority of albedo values is close to the snow albedo value (with the 0.82 being the most frequent value).

Line 18: Is the geometry kept fixed?

During the spin-up the geometry is not kept fixed. We clarify that paragraph because it is misleading.

Line 20: perhaps just write that precipitation is kept constant – in stead of writing that it is not scaled...

done

Line 25-31: I think this needs to be elaborated. You do this calibration experiment where you do not use PISM-dEBM-simple but force with the data that you have parametrized.

We do use PISM-dEBM-simple, but it still needs precipitation and near-surface air-temperatures. Those are taken from the MAR data. We have clarified the paragraph.

I think this is more or less what it already says, but I think it needs to be reformulated. You need to explain why you do the Eem test? I suppose it is to test the sensitivity to insolation values - but it needs to be clearer.

Exactly, the Eemian experiment is designed to show how the melt rates depend on a change in insolation values, in particular because the temperature field is not changed in this case. We clarified this in the text.

Page 7

Line 4-7: Rephrase: Does these monthly temperature fields come from MAR? And where does these scalar temperature anomalies come from?

done

From line 8: I would like to have the reasons for each of the experiment series before the method is describe. Basically moving the information from page 8 line 8-20 up. Would it somehow be possible to group the experiments into four groups, so that it is easier to follow which group of experiments that are being discussed in section 4?

We have reorganized the section and introduced a table with seven groups of experiments. Now the section starts with the motivation for the experiments.

Page 8

Line 21: Should this title refer better to sec 2.5? By using the word calibration perhaps?

Now "validation" is used for both sections, since it describes more accurately what is done in this section.

Line 22-23: I am missing a sentence like: As described in the Calibration experiments in the methods section?

done

Section 3.1:

This section is not completely clear to me. Perhaps spell out a bit more what is calibration and what is validation.

Line 27: Is this an experiment? I thought perhaps it could be called a calibration run?

Line 31: The root mean square error of what field? Melt?

Indeed, we show the root mean square error of the melt field. It is now clarified in the text.

Page 9

Line 4: Maybe add a sentence here to sort of conclude that using the RMSE method described, you find the parametrisation constants?

done

Line 5: "Yearly total melt computed with PISM" While using the dEBM-simple method?

This is now clarified in the text.

Line 12: Could this also relate to the fact that you do not consider the darkening of ice?

This is indeed possible. This thought is taken up in the manuscript.

Page 10

Section 3.2:

I think that it needs to clarified throughout the text that this experiment is done to test the sensitivity to the orbital parameters (or something like that).

done

Page 11

Line 4: clarify which historic variability?

done

Line 5-6: "This is in line with ..." So I guess this is the point of the experiment - basically to test if you get similar results to others.

Exactly. We have added a sentence in the beginning of the subsection, so that this information does not come as a surprise to the reader.

Section 4

It would be nice if it was clear here which of the experiments are being discussed. Perhaps if they are put into four groups as suggested above, this would be easier. Keep in mind that the resolution of 4.5km actually prevents the model from resolving the ice streams properly - this could have an influence on the surface-elevation feedback. Then on the other side, the fixed calving front must add some effect of inducing ice streaming.

Page 12

Line 1-2: Needs to be rephrased. Here the lower bounds of the experiments in this study. It is a lower bound for the model ice losses. I do no believe that this model set-up is able or discussed to high enough detail to give the lower bound for actual ice losses.

done

Page 13

Line 1-2: "But also ..." rephrase sentence

done

Line 7: By this point I have forgotten the timescales that the experiments are being conducted at. Perhaps remind the reader

Line 9: melt-albedo

done

Line 11-12: Explain why the melt-albedo feedback becomes less important with time? I suspect that this is due to the fact that the entire ice sheet gets the minimum albedo.

Exactly. We have added this interpretation to the manuscript.

Page 15

Line 23: Reducing the frequency how? I think actually, the frequency is really interesting. What effect do we get if we get more extreme years like 2012, and with this module this could actually be tested.

The darkening experiment as described in the main text is an extreme scenario, where the reflectivity drops over the whole ice sheet for the entire summer for each year.

Reducing the frequency of those events, i.e. the reflectivity drops over the whole ice sheet for the entire summer, but not each year but every two years, leads to less ice losses compared to the more extreme case. If darkening in every year increases ice losses by 70%, the increase would drop to approx. 35% if only every second year experiences a darkening event.

Knowing the projected frequency of extreme melt years could thus give an estimate of the additional mass losses, which we think also would be a nice future application of the model.

We have clarified this section.

And why do you think June is most sensitive to darkening?

Two effects might play a role: 1) The days are longest and the insolation highest. 2) In the beginning of the melt season the albedo is still high. The artificial reduction of albedo has the strongest effect then.

We briefly discuss this in the manuscript now.

Discussion

I like the discussion, and it shows that considerations

Page 16

Line 15: Sentence starting with "This is because..." Perhaps the sentence should be slightly rephrased, however, this is the kind of reasoning I think is missing in the two sections above.

done

Line 30 - page 17 line 4: Paragraph starting with "Therefore the only ..." This is a great paragraph and it really frames the whole study.

Thank you!

Page 17

Line 6-7: Perhaps mention why the model overestimates early melt and underestimates late melt.

done

Line 17-18: "It is a coarse representation of what is important of the albedo of snow and ice". Actually it is only a representation of the albedo of snow – as the minimum albedo is clean ice?

We reformulated the sentence in the manuscript.

Conclusion

Page 19

Line 11-12: I think this sentence could be expanded to something like: Using dEBM-simple we find that the melt-albedo feed back can lead to additional 12 cm SLE … in the projected mass loss in PISM.

done

Mario Krapp

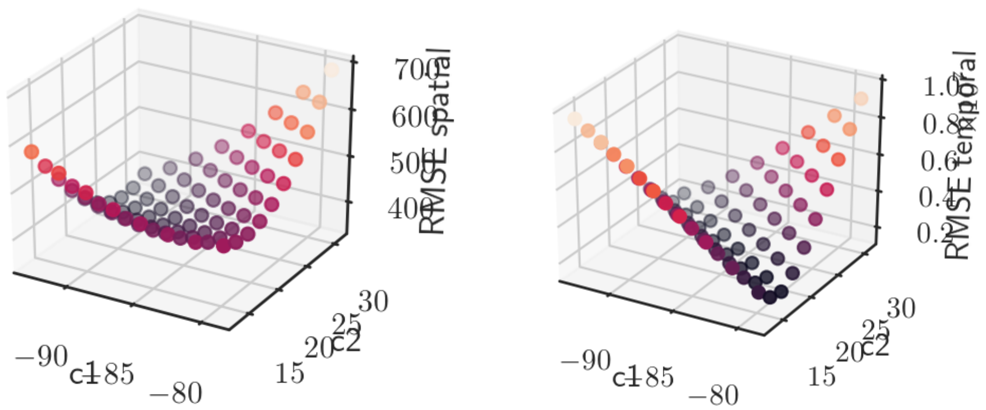
Received and published: 21 June 2021

The paper describes how a melt parametrisation affects surface melting on the Greenland Ice Sheet for different forcing scenarios. It is a simplified version of a previously publishes version of a melt scheme and uses temperature and insolation as inputs to calculate surface melt rates. This scheme is fast and simple (in terms of its input) and can therefore replace the positive-degree-day melt scheme which is the current melt scheme used in the numerical ice sheet model PISM. This paper is well written and it can be a valuable contribution for the ice sheet modelling community and should therefore, certainly find a home in TC. However, I cannot recommend this paper being published in its current form as it leaves some open questions that I feel need to be addressed first. The authors have put a lot of effort in the experimental setup but, I think, they almost tried to do too many things at once. I would recommend to them sorting out priorities of what experiments add to the story they want to tell and why, and how they then tell it. Therefore, it requires major revisions for which I have some, hopefully helpful, comments and suggestions. Find below a list of major and minor comments that should be addressed by the authors.

Major Comments

- **Abstract:** the relative changes in surface melt don't mean much without a reference value to relate them to. What do you compare your ice loss with the new melt scheme with? I assume the default melt scheme for PISM is PDD, so why don't you compare your results against that?
  The focus of the manuscript is not the comparison between PDD and dEBM-simple. Therefore we do not compare those two schemes in the abstract. However, as pointed out correctly, a relative change is meaningless without a reference value. We now make explicit that the reference value in this case is the simulation with fixed albedo values.

- It is not easy to follow the results **(Sect 3 and 4)**. The flow of the paper is interrupted by the need too flip pages back and forth too many time to find the respective experiment for the respective results (flip between Sect 2.5 and Sect 3/4). My suggestions: Present the experiments in order with their results in the appropriate (sub)sections. Provide a matrix of the experiments and what you are testing with those (in a table) and briefly describe that matrix in Sect 2.6.
  We have rewritten and clarified the methods- and results sections, so that they are easier to follow. We have also introduced a table summarizing the experiments.
- **Sect 2.5:** It's not clear to me what the calibration experiments are (2.5) What model parameters have been calibrated and what parametrisations have been tested?
  Indeed, the title of the section was misleading. We have changed the title to "validation experiments", since this title reflects more precisely what has been done. We do not present a thorough calibration in this manuscript. See Krebs-Kanzow et al. (2018) for a calibration.
  These experiments are performed with the coupled PISM-dEBM-simple, however, the topography is held fixed in order to ensure comparability to the MAR simulations, also performed with a fixed ice-sheet geometry.



  However, see the spatial and temporal RMSE for the parameters we have tested before running the simulations of the study.
- **Sect 2.6**: Reading on from the previous section, are these experiments now coupled and what is turned on and off?
  These experiments are also performed with PISM-dEBM-simple, however, compared to the previous section, the ice-sheet geometry is now allowed to evolve due to changes in climatic mass balance and ice flow. Changes in the topography induce the melt-elevation feedback via an atmospheric temperature lapse rate.
  We have clarified these points in the manuscript.
- What the major uncertainties/limitations of your melt scheme? For example, cloud cover is not explicitly considered here. Is that a problem and how much can the transmissivity parametrisation account for that? (Extreme) surface melt "events" as mentioned in the introduction are another example. Do they even exists in your forcing? For how much surface melt do they account for?
  Figure 1 shows that the melt rate in most years is well represented without considering changing atmospheric transmissivity. Indeed, changes in cloud cover are not considered. To the contrary, the

parameterization of the atmospheric transmissivity with surface altitude entails the assumption that average cloud cover does not change in the future.

Forcing with historic MAR temperature data shows that indeed, the extreme melt in the years 2012 and 2019 is underestimated in the PISM-dEBM-simple scheme (Figure 1). While the melt of the year 2019 is underestimated due to the parametrization of albedo and transmissivity and can be improved by taking the shortwave downward radiation and the albedo fields as input rather than parametrizing (see Figure A3), the melt in the year 2012 can not be reproduced. However, in the full dEBM model, the cloud cover strengthens the 2012 melt (Krebs-Kanzow, personal communication). Otherwise cloud cover changes do not seem to drive the variability (Krebs-Kanzow, personal communication)

Moreover, the forcing for the future scenarios, as RCP2.6 or RCP8.5, does not take extremes specifically into account but uses the temperature data from CMIP5, which is likely to underestimate temperature extremes, since the associated strong negative NAO index that led to persistent anticyclonic pressure heights over Greenland (Tedesco et al., 2020, Hofer et al., 2017, Bevas et al., 2019) is absent in any future CMIP5 projection(Hanna et al., 2018).

- I don’t understand how PISM is used here, offline or interactive simulations, and which experiments are what? In the abstract you refer to ”dynamic simulations” of the GIS, but I can’t seem to find which of your experiments are coupled and which are uncoupled

  PISM-dEBM-simple is always used in coupled mode, since dEBM-simple is implemented as a surface module in the Parallel Ice Sheet Model PISM. However, the validation simulations use a fixed topography, mainly in order to ensure the comparability to MAR simulations.

  The forward simulations with the RCP forcings use the full ice dynamics and allow for changes in topography, which also allows for the melt-elevation feedback. We have added a clarification in Section 2.3.

- One aim of this paper is to provide a fast scheme for centennial- to millennial scale time scales. I was just wondering, in case it has been coupled, it would be interesting for the readers to see what the long-term effect of using the new melt scheme over PDD might be.

  We now show the melt rates computed with pdd from the historic validation experiment with spatially resolved temperature fields (similar to Figure 1 and 2). We find that the pdd module, with standard parameters, gives a similar root mean square error over the historic period, both over the cumulated melt per year (39.85 Gt with pdd vs. 32.92 Gt with dEBM-simple) and over the spatial June, July and August melt (0.47 mWE/year with pdd vs. 0.36 mWE/year with dEBM simple). The pdd melt introduces a North-South gradient to the melt anomaly (compared to MAR data), overestimating the melt in the North and underestimating the melt in the South.

  We now also present the ice losses of the RCP2.6 and the RCP8.5 scenarios as computed with the standard pdd, finding that the mass loss computed with pdd is greater than with dEBM simple, in particular with the RCP8.5 scenario. Here we find a relative increase of 12% in the year 2100 and of 47% in the year 2300. This could be due to the fact that the melt factors in pdd are optimized for a present day melt rate and might be not valid in a future warming scenario. In pdd, the sensitivity of ice melt to T_eff is given by the degree day factor, usually assumed to be 8 mm liquid-water-equivalent / pos degree day. The temperature dependent melt of dEBM on the other hand scales with $\Delta t_{\Phi} / \Delta t * 1/(\varrho * L_m) * c_1 \cong 4.37$ mm liquid-water-equivalent / pos degree day (if expressed in the same units. Thus, once the snow cover is gone pdd will react more sensitively to temperature changes.

In addition, increased ice losses are amplified via increasing temperatures via the atmospheric temperature lapse rate, as the melt-elevation feedback sets in.

- It would be interesting to see what is the relative importance of the parameters ta, c1 and c2 in Eq 1, (and s In Eq 3, is that a parameter or calculated from the daily temperatures over a month?) in their contribution the melt rate M would be. Could you show how that partitioning between the temperature-driven and the insolation driven melt looks like (a spatial map of sorts, or a stacked time series line plot)?

  We now include a map with average relative importance of temperature driven melt over the historic time period.

- For the RCPs the surface mass loss is expressed as SLE but the comparison with MAR is done as melt rates. I suggest you show the same quantity in Fig 1.

  Both plots show different quantities. In Fig. 1 we explicitly compare the melt rates of PISM-dEBM-simple to the MAR melt rates. Therefore, we think it does make sense to show the melt rates in Gt/year. This quantity can not be expressed in SLE in a meaningful way, since it does not include the full mass balance. In contrast, we show the total ice losses in Figure 4, which include the full climatic mass balance, basal mass balance, and discharge into the ocean. Even though we do not aim to provide projections, we still think that SLE (which is the accumulated mass change rate) is a useful unit to express these results by making them more comparable to previous results. We have added a conversion from mSLE to Gt in the figure captions.

- I'm not fully convinced that the RCP scenarios are helpful for the conclusions. Wouldn't you want to show that the new melt scheme is better than the old scheme in a controllable way? I understand that you get a bigger signal with RCP8.5 but that doesn't seem to be the point here. For instance, let's assume that PDD is the better approach, than whatever you show with an alternative model doesn't really matter. Of course, I'm pretty convinced that your melt scheme is superior. That's why I want you to make sure that this is the point of the paper and that you have demonstrated it.

  In fact proving that dEBM-simple is superior to pdd is not the point of the paper. The melt scheme is very comparable to the Krebs-Kanzow et al. (2018), only simplified by the parametrizations for albedo and atmospheric transmissivity. The above mentioned paper rigorously compares the dEBM to pdd and benchmarks both against MAR data.

  In this manuscript, after showing that the parametrizations for albedo and transmissivity and the implementation in PISM work as expected, we aim to apply the melt scheme in RCP scenarios, with the influence of the melt-albedo feedback being one of the main scientific questions.

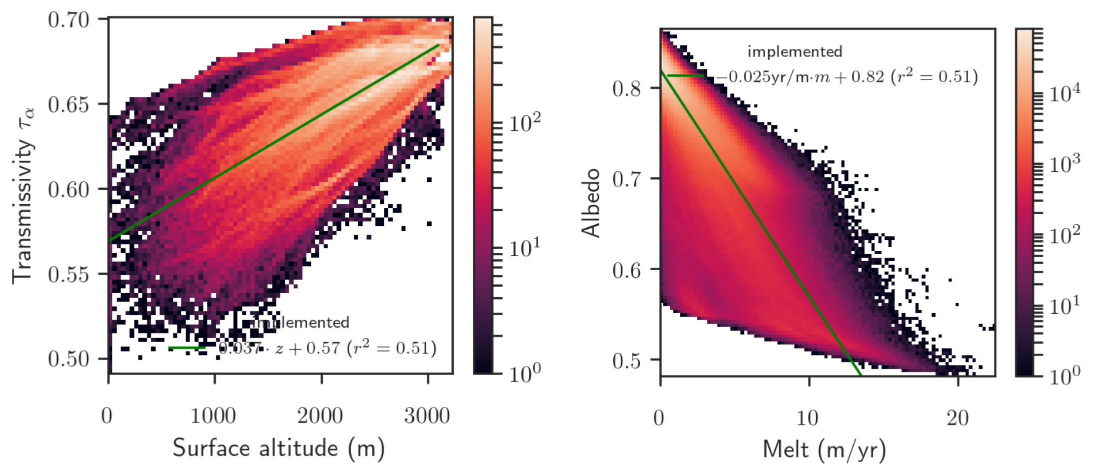
  We make sure to refer to Krebs-Kanzow et al. (2018) more prominently, so that the interested reader can convince themselves about the differences in dEBM and PDD melt scheme. Further statistics about the performance of the full dEBM model are found in Fettweis et al. (2020). We make sure to cite this more prominently. We have included a comparison with PDD in Appendix D.

- In your current experimental setup, you can't be certain that the optimal parameters for the different linear fits, i.e., the individual terms as diagnosed from MAR (i.e., one for melt, one for transmissivity, etc, Fig. A1 and 2), would minimise your error with respect to the melt rates. I would therefore suggest that you do one single optimisation sweep. The other parameterisations are just means to combine different terms into a single equation (Eq. 1). For example, there is no point in minimising ta but would be still a useful diagnostic to check after your optimisation.

In this manuscript, we made a distinction between the dEBM-simple melt equation (Equation 1) and the parametrizations for the albedo and transmissivity fields. optimizing both individually.
It is true that the total fit could be optimized by including the values chosen for the parametrization of albedo and transmissivity.
We selected the optimal values based on MAR data, but another set of parameters might require a different transmissivity or a different albedo parameterization, while keeping the other dEBM parameters constant.
We have included this point in the discussion of the results.

- The albedo-melt relationship (Fig. A1) and the transmissivity-altitude (Fig. A2) don't seem to be centred very much around the line that has been fitted. Furthermore, these relationships are not time-invariant, which means that the surface response differently to local climate, depending when and where it is. Maybe summer snowfall events play a role (as you said, they interrupt the whole melt process) and you can show that in the MAR data.

  Right, the temporal variation is not transformed into the parametrization. We here show all points from all grid cells on the ice sheet over 62 years for the months June, July and August individually. This visualization showcases the differences between those months. Moreover, it has been difficult to choose a transparency value, which both does not saturate where the density of markers is high and is still visible enough in a low density. However, if all data is taken together and visualized in a 2d-histograms with a logarithmic color scale, the visualization shows that the fit is more centered:



  Summer snowfall events might alter the albedo on a submonthly time scale, just as well as aging of snow. Other processes, which might increase the spread in albedo are shading, wind exposure or rain spells. We now discuss those in the manuscript.

- A thorough uncertainty assessment of your free model parameters is necessary, and, in my view, this would add credibility to your melt scheme and your paper. This can be done by randomly sampling from a multi-variate distribution whose mean is given by the optimised parameter set and whose scaling is given by the standard error thereof. I'm not asking for a full ensemble of coupled PISM simulations (that would be perfect, of course) but to analyse the variable space more systematically, e.g., in the (Te f f , Sf , M) hyper volume

Thank you very much for the suggestion. We have simulated an ensemble of 100 members by drawing the parameters for c_1, c_2, the slope for the albedo parameterization α and both, slope and intercept for the transmissivity parameterization τ. The volume changes under RCP 8.5 forcing are in between the upper and lower bound given by the experiments with albedo forcing (darkening and interannually constant yearly cycle respectively). These simulations are fully coupled to PISM.

The analysis of the ensemble allows us to identify the parameters which increase the temperature sensitivity of the ice sheet. While an ensemble of 100 members is too small to draw statistically sound conclusions, we can deduce that the dEBM parameter c_2 and the slope of the transmissivity parametrization do not seem to have a large effect on the variability of ice losses (with a Spearsman correlation coefficient of $r = -0.06$ and $r = 0.25$ respectively), in contrast the slope of the albedo and the intercept of the transmissivity τ seem to have a larger effect.

We have included this analysis in Appendix E and in the discussion.

In addition see Krebs-Kanzow et al., 2018 and Krebs-Kanzow et al. 2021 for further sensitivity analysis, e.g. for the sensitivity to the solar angle Φ.

- Summary statistics for fits (Fig. A1,A2) are missing (R2, standard errors or confidence intervals of slope and intercepts, etc.)

  Done, we added the coefficient of determination to the plots. The standard errors of the slope and the interval are not meaningful (i.e. zero) due to the large number of data points over which the fit is performed.
- Is it possible to compare the melt rate with observational (AWS) data and use AWS data as input, if they are available at all? e.g., PROMICE, GEUS

  The historic data in MAR is already tuned to AWS. As we want to compare the data to a full field, we think that MAR is more appropriate than extrapolating the individual points from AWS data.

Minor Comments

- P2/L13: ",where when large parts"

  done
- P2/L32: reference to PROMICE is missing

  We removed the direct reference to PROMICE, as suggested by the first reviewer Signe Hillup Larssen. The reference to the SMB-MIP remains, so the interested reader can read further.
- P3/L26: What is the present-day reference period?

  it is from 1971-1990, we added that to the manuscript
- P3/L29: "which is are modelled"

  done
- P3/L32: How is the snowfall determined from rainfalls and near-surface temperature?

  temperatures below 0°C lead to snow only, temperatures above 2°C lead to rain only, with linear interpolation in between
- P4/L1: Where does the "0.047" come from?

There was a typo, it's supposed to be 0.05193 m/yr and it's the default value in PISM. Compared to observed values, the default value used here is likely to underestimate the mass losses. However, at the resolution of 4.5km the ice losses through sub-shelf melting are not the main driver. The area of ice shelves in the simulation constitutes approximately 2000 km^2, compared to approximately 2*10^6 km^2 ice sheet area (0.1% of the ice area is floating ice in this simulation). Even in the RCP8.5 simulations, as the ice area decreases and the area of floating ice increases, it does not exceed 0.5% of the total ice area.

- Eq 1: the a in ta suggests link with albedo, maybe change the subscript to "a" or "A" for atmosphere
  done
- P5/L8: add "(TOA)" after "top of the atmosphere"
  done
- L15: It is unclear whether the cosine approximation is used here or the version by Berger (1978), which you refer to in A2. The Berger (1978) values are likely to differ from that present-day approximation.
  For the present day, the Liou (2002) expansion is used, for paleo simulations the Berger approximation is used.
- P5/LL1: I'm confused as to why the melt module is evaluated weekly if Te f f is monthly
  The melt is calculated as a function of insolation, atmospheric transmissivity, 2m air-temperature, and albedo. While it is true that the 2m air-temperature, an input field, has only monthly values, the insolation, the transmissivity and most importantly the albedo can change on a time scale shorter than one month. This is taken into account with the weekly evaluation of the melt scheme. In particular, as the albedo depends on the melt, frequent evaluation of the melt and the albedo reduces the error in albedo calculation while being more computationally fast compared to an iterative scheme in each, monthly, time-step.
  This line of thought is now reflected in the manuscript.
- how is refreezing calculated?
  It is a fixed value of 0.6, which means that 60% of the melt refreezes independently of space or time. Only snow melt can refreeze, it is assumed that ice melt runs off into the ocean before it can refreeze.
- P5/L6: Can you explain why ice/snow can melt below freezing point?
  If the monthly mean temperature is below freezing it does not mean that each day of this month would have a temperature below freezing. So there can be melt days in a month, even if the average temperature of this month is below freezing point.
  This is also the reasoning behind the positive degree day approach. Now Equation (1) uses for T_eff the same effective positive temperature as pdd would (see Eq. 3), which is always T_eff >0. But while the melt in pdd would simply approach 0 for small temperature, as the number of positive degree days goes to zero, the dEBM melt has an additional positive term coming from the insolation, so in contrast to a pdd scheme a lower threshold for melt is needed to avoid artificial insolation driven melt under very cold conditions. The cutoff at monthly mean temperatures below -6.5°C is in line with observations (see Krebs-Kanzow et al., 2018 and the references cited therein).
- P5/L29 and P6/LL1: There is something wrong with this sentence
  The sentence is changed based on comments of Signe.

- P6/L3-5: I understand that ”several iterations” mean that melt rate converges to some equilibrium value. Is that it?
  Yes, if the melt is evaluated under otherwise same conditions. See also the reasoning above, to why the melt rate should be evaluated weekly rather than monthly.
- ”Beckmann and Winckelmann” is a pre-print available for others to read? Otherwise, you need to explain the details.
  We have now included the statistics over the initial state in the supplementary information.
- P8/L22 ”as and example”
  done
- L25/26 there is a duplication of ”as described in Section 2.5”
  Removed the duplicated sentence.
- P11/L9 move the comma to after ”Here” in ”Here we analyse, how”
  done
- Fig. 4: The caption doesn’t say what the shading means (check the other figure captions, too)
  corrected in the manuscript
- P13/L1,2: Too many ”also”
  done
- P16/L27: Add comma after ”In dEBM”
  done
- P17/l1: remove ”classical”
  replaced with “widely-used”
- P18/L15: ”parameterized”
  done