Response to reviewer #1

REVIEW OF 'HYSTERESIS AND ORBITAL PACING OF THE EARLY CENOZOIC ANTARCTIC ICE SHEET' BY VAN BREEDAM ET AL.

The authors have sufficiently addressed my earlier concerns, and I am now ready to recommend publication of the manuscript (largely) as is. Especially the inclusion of more details on the ice-sheet model is appreciated.

Author's response: We thank the reviewer for the appreciation of our revised manuscript.

I just have some recommendations for small (technical) corrections:

Throughout the manuscript the reference to Huybrechts (1994b) should be Huybrechts (1994).

Author's response: Corrected.

Methods section:
- It is still not mentioned how calving is modeled.
- I believe the settings for geothermal heat flux and glacial isostatic adjustment are mostly representative for the East Antarctic ice sheet, which is by far the most important part in this study. For the West Antarctic ice sheet other values may be more appropriate though.

Author's response: There is no special treatment of calving fronts and iceberg calving occurs when the ice shelf front is thinner than 150 m. We added this information to the manuscript.

L167-168: There is no special treatment of calving fronts and iceberg calving occurs when the ice shelf front is thinner than 150 m.

L274: '~6 deg C and ~8 deg C' should be '-6 deg C and -8 deg C' I think.

Author's response: We state that the temperature needs to be lower by ~6°C and ~8°C, that indeed also corresponds to a temperature difference of -6°C and -8°C.

L443-445:
In the previous review I noted:
One could also think of an ice-volume-CO2 feedback loop: initial ice volume increase leads to lower CO2, which stimulates further glaciation.

Author's response: This feedback has been proposed to explain CO2 variations during the Quaternary ice ages, possibly caused by changes in the ocean deepwater circulation or the Asian monsoon (Ruddiman, 2006). We do acknowledge that such a feedback mechanism could exist during the early stages of Antarctic glaciation, but the mechanisms behind it might be very different. Here we investigate the response of the ice sheet for prescribed CO2 values.

Ruddiman, W. F.: Ice-driven CO2 feedback on ice volume, Clim. Past, 2, 43-55, 2006

New comment:
I understand your modeling strategy, but I still think the potential for the existence of such a feedback in reality could be mentioned in the discussion as it could influence the time scale for glaciation and deglaciation as well.

Author's response: We briefly mention the existence of the feedback now in the discussion.

L522-524: One of the possible causes of the CO_2 variations during the early Cenozoic could be the build-up of the ice sheet itself, which causes a positive feedback loop with increased ice volume leading to lower CO_2 levels and amplified glaciations (Ruddiman, 2006).

Response to reviewer #2

After reading through the author's responses and modified manuscript, I am happy to see that its quality and structure have improved immensely. The study is now much easier to follow, and the text flows much better. Presenting all experiments upfront, as well as a good description of all model setup components really help understanding what and how experiments were performed. In light of the new explanations and more detailed description of their methodology, I have further suggestions to improve the readability, and to openly discuss potential shortcomings of their model setup. This is a very interesting and thorough study, and it would be a shame if parts of it are not well appreciated by the reader just because some parts of the text are confusing, hence my effort in providing suggestions for rephrasing and more clarity in the explanations. Below I present these suggestions, using the line numbering in the **"track changes version"** of the manuscript.

Author's response: We thank the reviewer for the positive appraisal of our revised manuscript and for the suggestions to improve the readability.

General
One item that I would like to see in the discussion is a more open discussion (even if brief) about the fact that the uncertainty to model parameters is not explored (L178), and changes in sea level are not included (L214). It would be worth (and honest) to outline the setup shortcomings (within of course, the limitations of a poorly geologically constrained period and long simulated period), and how this could have affected the results.

Author's response: We have added the following paragraph related to the ice sheet model uncertainty to the discussion:

L585-593: Despite the performance of a substantial amount of sensitivity experiments related to the orbital and CO_2 forcing, we did not perform a sensitivity analysis of the ice sheet parameter uncertainty. Ice sheet model parameters such as the enhancement factor, the basal sliding coefficient or the flow rate factor each have their uncertainty and they are tuned to reproduce the present-day Antarctic ice sheet. Also the parameters related to the isostatic model are not explored. For instance, the relaxation time of the asthenosphere and the flexural rigidity of the lithosphere add another degree of freedom. The most important parameters influencing ice sheet-climate model feedbacks are however parameters related to the albedo (Gandy et al., 2023). Because our modelling approach on very long timescales significantly improved the representation of albedo changes resulting from changes in the ice sheet extent (Van Breedam et al., 2021a), we believe that the ice sheet parameter uncertainty is of secondary importance here.

We also added the following justification to the experimental design:

L191-193: Sea-level changes (sea-level fall when the ice sheet is growing) are not included as an additional forcing because the number of grid points that become land-based when sea-level would fall by 70 m are very limited.

Line by line

L116: The "anomalous heat convergence" is still not clear. The explanation provided in the authors in the response letter should be added to the manuscript. Again, keep in mind that CP has a broad audience, and the authors should not assume that all readers are familiar with how slab-ocean models work.

Author's response: We added the following information:

L93-95: The slab ocean model equilibrates with the atmosphere in a 50 m thick layer where heat is exchanged with the atmosphere. In slab ocean-atmosphere models, an anomalous heat convergence is added to the ocean to mimic the real influence of oceanic circulation below the slab layer.

L117: sea ice, without hyphenation

Author's response: Corrected.

L121: cite Wilson et al. (2012) here, at the first reference of the bedrock topography.

Author's response: Done.

L122-125: This part is a bit confusing. The way it is written, it sounds like the model takes two different bedrock topographies. Is that correct, or are different experiments run using either the minimum or the maximum bedrock topography?

Author's response: Different ice-sheet experiments are run using either the minimum or the maximum bedrock topography, but the minimum and maximum bedrock topography are regridded to the lower-resolution ice sheet model grid as explained. For each of the two Wilson topographies the maximum and minimum bedrock height within the lower-resolution ice sheet grid cell is taken to quantify the bedrock uncertainty.

L135-136: From the use of "a unique combination", I understand that ε and e are the same for all 100 ensemble members, and that the combination between the 20 different ice sheet geometries and different CO2 levels yield the 100 members. Based on the remainder of the paragraph, that does not seem to be the case. The paragraph reads odd when comparing L135-136 with L143-146 and Eq. 1, as it seems like your emulator takes all parameters into account (which is a good thing!). I understand that the details are provided in Van Breedam et al. (2021), but providing the number of values tested (as done for the ice sheet geometry) is some simple clarification that can be done in the sentences already written.

Author's response: No, a unique combination means that each parameter combination is unique, so different.

L160: representative of

Author's response: Done.

L165-166: I suggest rephrasing for consistency and a better flow when reading: "while in summer snowfall is limited to the highest elevated regions, such as the Gamburtsev Mountains and Dronning Maud Land".

Author's response: Adapted.

L176-178: From the description it seems like basal sliding is dependent on bed elevation, and/or ice thickness, but then there is also a spatially uniform coefficient? I strongly recommend clarifying this, perhaps as an equation similar to Eq. 1, highlighting what is dependent on (x,y) and what is not.

Author's response: That is true, basal sliding is spatially dependent on bed elevation and ice thickness, but calculated for a spatially uniform basal sliding coefficient. We do not see any added value of adding the equation here.

L179-180: I assume based on simulating present-day Antarctica with AISMPALEO? Good to clarify.

Author's response: That information is added.

L182-183: Do you mean that you combine SIA and SSA, or that you apply the full Stokes equations over these grid cells? This needs to be clarified.

Author's response: No, we do not combine SIA and SSA, neither the full Stokes equations, at the grounding line all stress components contribute to the effective stress as we already mentioned.

L188-189: I suggest the following rephrasing, for better readability: "Nevertheless, even for present-day simulations large uncertainties exist in how changes in ocean temperature and salinity affect melt rates below the ice shelves.". This statement, however, would benefit from a reference, e.g.: https://doi.org/10.5194/tc-16-4931-2022. Finally, the thickness threshold for calving mentioned as a response to Reviewer 2 should also be included in the model description.

Author's response: We thank the reviewer for the suggestion, we rephrased the sentence accordingly and added the reference Burgard et al. (2022).

L198-199: I believe this sentence belongs to the next paragraph.

Author's response: The sentence has been moved.

L211: is it ice sheet initialisation or inception? Using "initiation" does not make this distinction clear

Author's response: We mean inception and replaced the word 'initiation' for 'inception'.

L217: remove "now", and replace "increasing" for "increased"

Author's response: This has been changed.

L227-229: I suggest rephrasing this sentence as follows, to make it clear that you are no longer referring to the runs shown in Table 1: “An additional set of runs (Table 2) explores the variation in orbital forcings to investigate the influence of insolation thresholds for ice sheet growth and decline in detail. In these runs, different values for the individual orbital parameters or the insolation are the control parameters explored (see Figure 1 and Table 2) and the CO2 concentrations are kept constant”.

Author’s response: Thank you for the suggestion. We rephrased the sentence.

L238: equilibrate faster -with- the forcing.

Author’s response: Corrected.

L320: are these temperature values absolute or relative? I assume the latter, in which case “by ~6˚C and ~8˚C” would be more appropriate than “with ~6˚C and ~8˚C”.

Author’s response: Corrected.

L339: is it really 1080 ppmv? If I understood Fig. 8 correctly, it looks like it is ~1040 ppmv.

Author’s response: Corrected.

L349: I would refrain from using “significantly” when not talking about statistical significance. This is something that is usually picked on by typesetters/copy editors, so best to change now.

Author’s response: We removed the word ‘significantly’.

L372-375: The authors provided in their response letter a valid justification for having both MAT_sur and MAT_clim. However, I still think the name MAT_clim is not intuitive for what it represents and is somehow misleading. I would suggest referring to it as MAT_corr or something similar, if the only difference between that and MAT_sur is the lapse-rate correction to bring it to a common reference level.

Author’s response: We still believe the MAT_clim is representative for what it actually is representing: the climatological mean annual temperature that is not taking into account the temperature changes resulting from changes in the height of the ice sheet.

L378: If I understood this sentence correctly, I think it would be best phrased as “As it occurs, the initial temperature difference of about 0.5˚C between both experiments with CO2 concentrations of 1000 and 1150 ppmv is due to…”

Author’s response: No, it is not only for CO_2 concentrations of 1000 and 1150 ppmv, but for all CO_2 concentrations in between as well.

L380: I am not sure what is meant by “the area of the continental scale ice sheet becomes nearly the same as the ice sheet extent”. I struggle to see how the area of the ice sheet would not be the same as its extent? Is it just the fact that the ice sheet will ultimately occupy the entire extent of the continent?

Author’s response: The sentence is reformulated as follows:

L328-330: After the transition to a continental scale ice sheet for a CO_2 concentration below 850 ppmv, this difference between both simulations is negligible because the area of the continental scale ice sheet is ultimately bounded by the size of the Antarctic continent.

L442 and elsewhere: using "melt" is not the most appropriate when referring to a complete meltdown of the ice sheet, as surface melt always happens regardless of whether your ice sheet vanishes or not. I would suggest using "demise" or "decline" instead, as used in other parts of the manuscript. Unless you indeed mean that there is no melt at all (i.e., SMB>0 always) below the thresholds discussed.

Author's response: The word 'melt' is replaced by 'decline', 'demise, 'disintegrate' and 'make the Antarctic ice sheet disappear'.

L461 and L465: "daily mean" as opposed to "mean daily".

Author's response: Corrected.

L467: today -> at present

Author's response: Corrected.

L459-461: Are these new experiments, or part of the ones listed in Table 2? If the latter, it is good to refer to the table here again.

Author's response: These specific experiments are not listed in Table 2. We added new labels in Table 2 (a-f) and refer to the table when the specific experiment is performed.

L530: "at high eccentricity values" reads better.

Author's response: Corrected.

L562: remove "ice" from "ice continent"

Author's response: Done.

L570: snapshots

Author's response: Done.

L573: If the authors agree that the message remains the same, this sentence could be rephrased as "Ice sheet hysteresis, as shown here and elsewhere (Oerlemans, 2002), is linked to the ice sheet geometry.". This would also clarify my confusion regarding sentence in the first review round.

Author's response: We thank the reviewer for the suggestion and adapted the sentence accordingly.

L581: typo on "dataset"

Author's response: Corrected.

L583: "When climate model uncertainty is also included". Using 'additionally' here reads odd.

Author's response: Corrected.

L659: should the ice geometry and CO2 levels also be included as boundary conditions here?

Author's response: We do not define them here as boundary conditions but as forcing/feedback, while the bedrock topography is regarded as a boundary condition.

Figure 11: use a shaded area as opposed to boxes (like in Fig. 4, which looks great) if the vertical axis is not supposed to be bounded. It should also make it easier to visualise the red boxes over the red curves and red dashed lines. A small tip so that it does not get too loaded to the eyes is to apply a lighter shade of grey than done for Fig. 4, and also without the black lines around the box.

Author's response: We thank the reviewer for this very good suggestion and adapted the figure accordingly.