

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.

### **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.

### **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.

L117: sea ice, without hyphenation

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

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?

L135-136: From the use of "a unique combination", I understand that  $\epsilon$  and  $e$  are the same for all 100 ensemble members, and that the combination between the 20 different ice sheet geometries and different CO<sub>2</sub> 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.

L160: representative of

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”.

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.

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

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.

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.

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

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

L217: remove “now”, and replace “increasing” for “increased”

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 CO<sub>2</sub> concentrations are kept constant”.

L238: equilibrate faster -with- the forcing.

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”.

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

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.

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.

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 CO<sub>2</sub> concentrations of 1000 and 1150 ppmv is due to...”

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?

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.

L461 and L465: “daily mean” as opposed to “mean daily”.

L467: today -> at present

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.

L530: “at high eccentricity values” reads better.

L562: remove “ice” from “ice continent”

L570: snapshots

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.

L581: typo on “dataset”

L583: “When climate model uncertainty is also included”. Using ‘additionally’ here reads odd.

L659: should the ice geometry and CO<sub>2</sub> levels also be included as boundary conditions here?

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.