Answers Reviewer 2

General comments:

The manuscript by Bochow et al. analyses temperature and precipitation data from a large number of CMIP6 models for the Greenland ice sheet. In their analysis, they find that temperatures over the Greenland ice sheet increase faster than the global mean temperature in the CMIP6 ensemble. Changes in precipitation show a spatially inhomogeneous pattern, raising concerns about the assumption of spatially homogeneous changes made in previous Greenland ice sheet simulations. The authors quantify the effect of this assumption by performing 100,000 year-long simulations under different forcing scenarios using the ice-sheet model PISM. Overall, I believe the topic is of great interest to the cryospheric community. The manuscript will be a worthwhile contribution to TC. It is in large parts well-written, figures are appropriate and the drawn conclusions are sound. That said, I have a number of general comments as well as detailed comments listed below that should ideally be addressed. I hope the authors find my comments helpful.

Thank you for your positive assessment of our manuscript. Below, we address all of your comments and concerns (blue font).

Specific comments:

1. The description of the model setup in the methods section is in its current form insufficient. The authors spend >1 page on describing the CMIP6 data, and then dedicate only 1/2 page on the ice-sheet model description. While I certainly appreciate a succinct model description and I am not advocating for repeating every single model equation, I am in favour of self-containing papers. At the moment there is no mention of how the CMIP6 data is fed into the ice-sheet model. What is the temporal resolution of the data and what data is used as input to the dEBM (I presume temperature!). I am also unclear about what ”bootstrapping the model to present-day state” entails. Does that mean parameter values are chosen such that the geometry of the Greenland ice sheet remains close to present day? How do you initialise ice-sheet temperature? How long is the spin up? What is your tuning target (ice velocity, ice geometry or something else)? Moreover, it is mentioned that MAR fields are used in the spin-up procedure. Are anomalies then calculated with respect to the MAR output fields? I presume that the uniform lapse rate and height-induced near-surface temperature change are only used for the spinup, but not for the scenario simulations?

A potential structure to improve this section would be to have a subsection on ”PISM model description” followed by ”Model spinup”, followed by ”Scenario experiments” (or similar).

To circumvent cluttering the text with parameter values, the authors could also add a parameter and/or experiment table.

Thank you for your comment. We agree that the model description section was insufficient. Based on your feedback and that of the other reviewer, we have expanded the section to address all your concerns and questions. As suggested, we have divided the section into "Model Description," "Spinup," and "Experiments." Additionally, we have included a table summarizing the different experiment setups in the revised version.

2. Related to my previous point, I am surprised that the authors select a model resolution of 20 km. This is a relatively coarse resolution. Given that the authors present a total of 8 simulations over 100,000 years, I do not think that computational demands should be a major constraint. Ideally, the authors redo their simulations with a higher model resolution (10 km) and show that their presented results are still valid. At the very least, the authors should pick one of the higher emission scenarios and show, based on this, that the results are not model resolution dependent.

Thank you for the comment! In the initial manuscript we did not want to put too much weight on the model simulation part. We rather wanted to show the effect of the uniform vs spatially variable sensitivities by running idealized experiments. However, we now considerably extended all our simulations by varying some important parameters and run everything on a 10km resolution.

3. I think the paper could give a more circumspect perspective on the findings as well as potential shortcomings by a more in-depth discussion. I will point them out again in the technical corrections listed below, but a couple of things that I was wondering about as I read the manuscript: ”Do you see any differences in the precipitation changes between higher and lower resolution GCM output?”. Then, I would like to see some explicit mentioning that the forcing from the CMIP6 models that you use does NOT take into account any changes in ice-sheet geometry.

We extend the discussion in the revised manuscript and discuss, among others, the potential differences between higher and lower resolution models and the influence of different AMOC responses across the models on the climate in Greenland.

Technical corrections:

Abstract:

L1: I think at some point it would be good to define what you mean by long-term. Depending on the community this could mean anything between centennial to glacial cycle time scales.

We define it now in the abstract as “millennial time scale and beyond”.

L12: Delete ”state-of-the-art”

Thanks.

L20: I do not think the projection paper by Edwards et al, 2021 is the best citation for this

Indeed, we now cite the primary source for this number.

L29: I would recommend adding a couple of citations for the stand alone ice-sheet models in addition to the Bochow et al. 2023 study

Thank you, we added two more references to this part of the sentence.

L30: ”to fully Earth System Models (ESMs) with dynamically coupled ice sheets”

We rephrased the sentence.

L36: Swap order of millennial and deca-millennial

Thanks for the comment; however, we don't see a reason to change the word order, as we progress from a shorter (millennial) to a longer time scale (deca-millennial).

L36-39: What about the PDD method? Isn’t that one of the most commonly used parameterisations? Also the citations are extremely PISM ”heavy”. Maybe worth listing a couple of examples from other ice-sheet models.

Thank you. Yes, PDD is probably one of the most used parameterization methods for long-term ice sheet modelling. However, in this paper we concentrate on the assumptions made for the climate fields that are fed into these SMB parameterization methods rather than the validity of the SMB parameterization methods themselves. We rephrased the sentences to make it clearer and added a few more non-PISM references.

L58: Maybe better inaccurate instead of inappropriate

Thank you, we changed the word.

L66-70: Could be worth adding section references to guide the reader better through the manuscript.

Thanks for this suggestion.

L74: Introduce all abbreviations at first mention (SSP was used previously)

We fixed that in the revised manuscript.

L125: I do not understand what ”a front-retreat calving based on the observed present-day extent” is? Are you saying that your ice sheet front is not allowed to advance beyond the present-day geometry? Please clarify.

Yes, that is what it means. We clarify it now in the method section.

L126: As mentioned above, you need to describe your model setup a lot better including what data you feed from your CMIP6 data into PISM

We hope our revised method section is clearer now.

L126: Again, as suggested above, it would be good to have a experiment overview section or at least a table.

Thanks for this suggestion, we now have a table describing the different experiments.

L135: In this section or somewhere in the discussion, please add an explicit mention that your CMIP6 forcing assumes constant present-day ice sheets. Sections 3.1 and 3.2: Have you had a look if CMIP6 model resolution affects the spatial patterns for the temperature and/or precipitation changes? Especially for precip, I could imagine that higher resolution versions manage to better resolve the topography of Greenland and therefore show a slightly different pattern.

We mention now explicitly that the CMIP6 data assumes a fixed ice sheet topography. We will also discuss the model resolution, among other things, as a reason for differences in the spatial patterns. However, we also want to note that we think a thorough investigation of possible reasons for different model responses might be beyond this paper's scope.

L178: Please add the location of Ilulissatfjord to one of the Figures e.g. Fig 2

Thank you for this suggestion, we depict Ilulissatfjord now in Fig. 2.

L204-205 and L250ff: To me these results are not that surprising. In the high emission scenario you hit your model basically with a sledgehammer that overprints almost all of the internal variability that you might see.

Agreed. We removed the word “interestingly” in L250 as it is not very surprising.

L215-216: Repetition of ”similar”. Please rephrase.

Thanks for pointing this out.

L246: A bit of a strong claim in my view. Maybe better ”fails to capture spatially inhomogeneous patterns”. Otherwise, one could argue that using forcing from a climate model that does not account for ice-sheet changes is also not the right choice.

We agree and change the sentence accordingly.

L253, L257: Here and throughout: Delete vague language like ”relatively”

We reduced the word “relatively” throughout the manuscript.

L287: replace ”to branch” with ”to diverge”

We changed the wording accordingly.

L290ff: Here, I started to wonder how much ice dynamics play a role? This could be easily checked by turning off the velocity solve in PISM and keep the initial velocity field. This run would be super fast as you do not solve for the ice dynamics, but would indicate whether ice-sheet dynamics actually matter at all for your simulations.

Thanks, we now include a short paragraph about the dynamic contribution to the ice thickness change and also include a new figure.

L337: Is the regrowth due to the GIA feedback that you mentioned earlier?

Yes, we mention it now.

L369: Can you be more specific about which dynamic processes are involved here? Wind patterns for example?

Thanks, we are more specific now.

L397-404: This paragraph comes a little out of the blue. Either delete or improve transition into it.

Agreed, we moved it further up.

L418-420: This is a very strong statement. It is also ambiguous. EMICs can easily cover long time scales and are fully coupled. If you are referring to ESMs, you should explicitly mention it. Still, I would rather say ”they will remain challenging” or something like ”As long as fully coupled ESM simulations on long term timescales are not feasible, we recommend ...”

Indeed, we meant fully coupled ESM simulations, we mention it now and rephrased the sentence according to your suggestion.

Figures:

The Figures are appropriate and of good quality. For the appendix I suggest that you change the Figure numbering so that for example all precip Figures are contained in a single subsection of the Appendix.

Fig. 1: Dashed lines are somewhat difficult to see. I suggest reducing the transparency. Also consider changing units of subplot b to mm/yr as it is used in the text.

Fig. 2: Here and throughout the manuscript, there is not a coherent use of near-surface temperature, surface temperature, and temperature. I would suggest to chose one and use it consistently.

Fig. 4: Might be worth adding a second y-axis for panel a showing sea-level equivalent. Changes in ice volume in m are more difficult to put into context.

Fig. 5: Same as for Fig. 4. Caption: ”… are visible.”

Fig. 6: Caption: Please rephrase ”huge mistake” as it suggests that one of the simulations is the truth. I would rather say ”large discrepancies”.

Fig. B1: Consider adjusting colourmap or colourscale. Three of the six panels are basically black.

Fig. D1 and similar: R values and sub-panel titles are not visible. Either increase (I guess model names could also be displayed along the y-axis) or provide a code and a corresponding table where the reader can look up the model name.

Sincerely, Clemens Schannwell

Thank you for the comments on the figures! We will incorporate all your suggestions.