Answers Reviewer 1

In this paper, the authors provide insights into the importance of considering spatial variations in temperature and precipitation anomalies for accurate modeling of the Greenland ice sheet's response to climate change, highlighting the potential impact of modeling choices on long-term ice sheet stability and sea level rise projections. To do so, Bochow et al. analyzed CMIP6 temperature and precipitation changes in Greenland, deriving spatially resolved and scalar scaling factors for near-surface temperatures and precipitation against global mean temperature and time. The authors then used the Parallel Ice Sheet Model (PISM) to compare the impact of spatially uniform versus spatially resolved anomalies on the Greenland ice sheet's short and long-term behavior. I think this is an interesting paper, worthy of publication for The Cryosphere. I do have though a couple of major comments which I think the authors should address before publication: one regarding how the methodology is presented, and another one regarding an aspect of the methodology (run-time lapse rate and precipitation correction factors).

Thank you for this positive assessment of our manuscript! In the following we will answer all comments and concerns (in blue font).

Major comments:

1) I think that the methodology subsection 2.2 PISM lacks details and is not clear enough. For instance, there are too few details about the initialization procedure and some aspects of the model forcing (calving rates, see specific comments). Also, while I understand in principle how the simulations were forced, I found the text explaining the forcing not entirely clear. For instance, it seems to me that applying spatially variable anomalies of temperature and precipitation is not different from any other study using GCM output (including ISMIP6 studies). In this regard, the link with the scaling factors is not entirely clear (see second comment). I suggest expanding the text explaining clearly the different experiments (perhaps including a table for different experiments, or a diagram showing how the forcing is produced).

We agree that this section was not detailed enough, and we will expand it in a revised version, and we will now also include a table to visualize the different experiments.

2) I was a bit disappointed to read that you applied constant lapse rate and elevation-change-induced precipitation scaling throughout the run. Somehow it is contradicting the premises of the manuscript - that is, going beyond values calculated with previous version of CMIP models. I think that using the values you inferred for precipitation correction would be the missing link between the first part of your results (deriving parameters) and the

second part (ice sheet model simulations). This would shed light not only on scalar vs spatially varying temperature and precipitation, but also on scalar vs spatially varying precipitation correction. If what I suggest is too complicated, it would be enough to test at least different scalar values for the precipitation correction (within the range values found in this study). A similar thing could (should) be done for the lapse rate (although the lapse rate is not calculated in this study: perhaps you could use values taken from Feenstra et al., 2024 "Effect of elevation feedbacks and climate mitigation on future Greenland ice sheet melt", The Cryosphere (preprint).

Thank you for the comment! We think there might be some misunderstanding here. Primarily, we wanted to show the effect of using a scalar/uniform or spatially varying temperature-change-induced, i.e., due to changes of the "background climate" temperature, precipitation correction independent of the elevation-induced effects. Unfortunately, PISM only supports spatially and temporally uniform height-change-induced precipitation correction factors and temporally and spatially uniform lapse rates. That means without restarting the ice sheet model regularly and calculating the precipitation and temperature fields manually, it is not possible to include a spatially varying lapse rate or height-change induced precipitation corrections. We decided to implement the spatially height-change induced precipitation corrections manually and include it now in the revised version alongside the original experiments, at least for the short-term experiments.

What additionally might lead to confusion is that instead of letting PISM internally apply the corrections via the uniform or spatial scaling factors, we directly used the precipitation and temperature anomalies derived from CMIP6 to force the model. In the revised version we force the model directly with the uniform and spatially variable precipitation sensitivities instead of using anomalies to make it more consistent with our analysis.

We also extended our experiments in the revised version and now run all simulations on a 10km resolution instead of 20km. We will now test different scalar height-change induced precipitation correction factors and lapse rates, as much as computational constrains will allow, and additionally run the 85-year simulations with the suggested spatially varying height-change induced precipitation correction factors. Unfortunately, the data from the Feenstra et al. (2024) paper are not published together with the preprint, so we are not able to apply spatially varying lapse rates (easily).

Specific comments:

Abstract

L2: I think uniform anomalies should go first in the sentence than parametrisation schemes.

We changed it accordingly in the revised version.

L3: Is this paper looking at different model parameters, or precipitation correction only? Suggest rephrasing like 'it is often assumed ... based on old generation models'.

We rephrased the sentence accordingly to make it clearer. In this manuscript we do not investigate the effect of different model parameters but only the effect of temperature and precipitation anomalies/sensitivities.

L12: I would try to also add here why there is such an overestimation.

We extended the sentence in the revised version.

1. Introduction

L20: surface mass balance is accumulation - ablation; I am not clear if here you also consider ice discharge into the ocean, which seems the case from L23: then I suggest explaining better how mass balance is calculated. Also, missing the reference (probably Otosaka et al. 2023: perhaps you can put together this sentence with the next about global sea level rise).

Thank you for this comment. We agree that we were imprecise with the mass balance and explain it better now.

L25: would explain why (ice margin retreating and losing contact with the ocean).

Thank you, we extended the sentence now.

L27: I would suggest rephrasing the second part, something like: "to ice sheet models coupled to Earth system models of intermediate or full complexity". Or at least you should specify that full complexity ESMs are coupled to a dynamic ice sheet model component - which is not obvious, as most full complexity ESMs are not. Also, I would suggest citing at least ISMIP6 work under the stand-alone ice sheet modelling.

We rephrased the sentence and added the appropriate references.

L31: I find 'latest generation' a bit confusing, is it CMIP5? CESM and UKESM are CMIP6 models with ice sheet coupling. Also, I think you could specifically mention the coupling work with a dynamic Greenland ice sheet component.

We extended the sentence to make clear that we meant the released runs of the ESMs in the CMIP6 intercomparison.

L38: maybe uniform instead of scalar?

Thank you, we changed it accordingly.

L43: I find the notation L(T) a bit confusing, as it looks like a function of the temperature T - but it is instead a constant (although it depends on the temperature). Maybe L_T?

We changed it throughout the manuscript.

L51: is 23C the annual mean temperature between 1996 and 2019? I am not entirely convinced how this simple calculation is relevant for the paper. Perhaps the authors can expand on that?

We wanted to show that already a simple calculation using the Clausius-Clapeyron relationship and Greenland specific values gives a deviation from the commonly used 7-8% for the precipitation sensitivity. The temperature of -23C is the approximate inland annual temperature as defined in the Jiang et al. (2020) paper. We expanded a little bit on it to make clear what we wanted to show with this calculation.

L65: Please add citation for CMIP6.

Thank you, we added a reference.

L66: Please add citation for SSPs. Also, SSP abbreviation was already used before introducing it (L75).

We fixed it in the revised version.

2. Data and Methods

L80: Gaussian with capital G?

Thank you.

L83: What did you do when the land-ice fraction variable was not available?

We only checked the models that have the land-ice fraction variable available. We only do this to verify our approach of using geopandas instead of some model-dependent variable. We agree that ideally one would check the land-ice fractions of all models using some other variable. However, (i) there are simply no land-ice variables available for some models, (ii) we do not see this need here as we are confident that our approach also holds for the other models. We elaborate the sentences now to explain why we look at the land-ice fractions.

L86: precipitations

Thank you, we fixed it.

L99: Perhaps spatially variable scaling factors?

Thank you, “spatially” must have been lost somewhere in the process. We changed it throughout the manuscript where it was missing.

L111: I think this choice should be explained.

This choice was to avoid any noise or outliers on an annual scale in the forcing. We will explain it in the text now.

L117: I think you should give an overview of what has been done here. Also, “mostly” is too vague - please state clearly what has been done differently.

We expanded the section substantially (also see answer major comment #1).

L123: I am a bit confused here. If you are calculating precipitation scaling with temperature, why then use a constant value for the surface-height-induced near-surface temperature change? I am not necessarily saying you should not do that, but I think you should explain this choice.

Thank you for this comment, it is indeed not obvious from the text. We would have liked to use spatially varying values for the surface-height-induced near-surface temperature changes and precipitation changes, but PISM does not offer this feature (yet). However, we decided to manually implement spatially varying factors in PISM. We will at least run the short simulations with this feature in the revised manuscript.

L125: I suggest adding a few more details about front-retreat calving. It’s not clear to me how and why this is implemented, especially for the long-term runs.

Thanks, we extended the model description section and now give more details overall, including the calving implementation.

L127: I assume what comes between L127 and L132 is about the spin-up. I suggest then moving the last sentence (resolution) upwards, as I imagine it also regards the future projections.

Thank you!

3. Greenlands climate in CMIP6

L135-L140: the beginning of this section feels a bit strange, as the first lines seem to belong either to the introduction or the discussion - which would come after showing your results. I suggest removing and/or displacing this text. I would also suggest a more traditional organization of the text in Results/Discussion/Conclusions (where two of these three can be grouped together depending on the author's preference). Maybe I am being a bit too rigid here, but when I started reading this section, I was not sure what I was about to read (the introduction-type text at the beginning did not help in this regard).

We will try to reorganize the manuscript in the revised version and try to follow a more classical order of the sections.

L151: Please, also list values for the remaining scenarios (if not in the text, in a table including also scaling values for precipitation, Greenland warming, and other relevant values).

Thank you for the suggestion, we now mention all important values in the text, not only in the figures.

L152: Not entirely clear to me what you mean by state-dependence. If it is a state-dependence, shouldn't the value keep increasing for SSP5-8.5? It could be interesting to speculate why this is not the case (e.g., thermodynamic limit at ice surface).

Here, state dependence does not necessarily mean a monotonous increase with increasing temperature. But there is an obvious dependence of the precipitation sensitivity on the emission scenario which is increasing from SSP1-2.6 to SSP5-8.5 (Fig. 3a). When looking at Fig. 3a in the SSP5-8.5 scenario, there also seems to be a slightly stronger increase in the precipitation rates at higher temperatures than for lower temperatures. We will try to make this clearer in the revised version.

L153: I am not a native speaker, but "analogously" sounds bad.

Thanks for noticing, we rephrased the sentence.

L150-175: In general, there is a bit of back and forth between historical and projections, which makes the text a bit hard to follow. I would suggest splitting more clearly between historical and projections, or at least avoid this back and forth as much as possible.

We tried to follow a different organization of the results that doesn't necessarily correspond to the timeline of the scenarios. However, we agree that there is too much back and forth between historical and future scenarios. In the revised version we try to avoid this as much as possible.

L171-L174: "The ensemble mean of the spatially averaged scaling factors agrees with the scaling factors derived from the ensemble mean of the near-surface temperatures" and "...the relationship between the GMT and the spatially averaged seasonal temperatures for some models does not necessarily follow a clear linear relationship.". I find these two sentences somehow contradictory: the first sentence implies that there is linearity (avg(f(T)) ~ f(avg(T))), but the second is not. Am I interpreting this wrong?

Thank you for the comment. Here we indeed mean (avg(f(T)) ~ f(avg(T))) with avg being the mean over the ensemble but that does not necessarily imply that f(T) for each member has to be linear but approximately linear since we do not state (avg(f(T)) = f(avg(T))). We extend these sentences to resolve this lack of clarity.

L185: Please discuss the historical spatial pattern before projections.

Thanks for this suggestion, we will change it in the revised version.

L194-208: I like this analysis of different models; I think it's going to be useful to the community. Perhaps you could try to discuss in some cases why there are differences? E.g., AMOC decline, persistent atmospheric blocking in some models…

In the revised version, we will discuss in more detail potential reasons for the different model responses.

L216: See first comment of this section.

Thanks.

L247-261: I find also this part very interesting, and I think the authors could try, again, to link some of the spatial differences to some specific processes (again, I am thinking about AMOC slowdown which would influence moisture transport to southern Greenland). Also, it would be good to frame these results in the context of the baseline precipitation (e.g., present-day northern Greenland quite dry vs south-eastern Greenland more wet).

Thanks for this suggestion, we now discuss possible reasons for the observed difference in the model responses in the revised manuscript. We thank the reviewer for marking this link between the AMOC response and the climate in Greenland. Indeed, we find a relationship between the decline in the AMOC strength and the precipitation rates/temperature anomalies and discuss them in the revised manuscript.

4. Modelling the response of the ice sheet

L287: There is a clear difference between…

Thanks, we change it in the revised version.

L304: Similar comment about AMOC before.

Thanks, see other comments.

L306: "In fact, the spatial patterns of the precipitation sensitivity agree very well with the ice thickness difference in eastern Greenland observed by the year 2100.". Isn't this obvious as the precipitation sensitivity is derived from the same data you are forcing the ice sheet model with?

We agree that this is more or less obvious. The order of the sentence was probably not optimal, we changed it in the revised version.

L375-L381: I think this part of discussion should be introduced earlier in the text (see comments above about climate processes).

Thanks for the suggestion, we will move this part of the discussion in the revised manuscript.