Review of Bochow et al. 2024

General comment:

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).

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).

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).

Specific comments:

Abstract

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

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’.

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

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).

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

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.
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.

L38: maybe uniform instead of scalar?

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?
L51: is 23 °C 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?

L65: Please add citation for CMIP6.

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

2. *Data and Methods*

L80: Gaussian with capital G?

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

L86: precipitation~~s~~

L99: Perhaps spatial*ly variable* scaling factors?

L111: I think this choice should be explained.

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.

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.

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.

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.

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).

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).

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).

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

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.

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?

L185: Please discuss the historical spatial pattern before projections.

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…

L216: See first comment of this section.

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).

4. *Modelling the response of the ice sheet*

L287: There is a clear difference between…

L304: Similar comment about AMOC before.

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?

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