The Cryosphere Discussions

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Reply on RC2

Fredrik Boberg et al.

Author comment on "Uncertainties in projected surface mass balance over the polar ice sheets from dynamically downscaled EC-Earth models" by Fredrik Boberg et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-140-AC2, 2021

Anonymous Referee #2

General comments

This paper presents projections of the surface mass balance (SMB) of the Greenland and Antarctic ice sheets produced with HIRHAM5 driven by two versions of EC-Earth that took part of the CMIP5 and CMIP6 exercises. Diversifying projections of ice sheet SMB is crucial for a better assessment of the uncertainties related to different members, emission scenarios, or global and regional model biases. While the paper fits well within this scope, in too many places the methodology and analysis still look insufficiently documented to consider publication without major revisions. Considerable improvements compared to the previous version of the manuscript have to be acknowledged for some parts of the paper. However, important comments have also not been taken into account and serious issues persist. Particularly, of major concern are the absence of justification of the chosen GCM and reference periods (in line with RC1), and the incomplete evaluation of the downscaled climate and SMB using HIRHAM5, all already noticed in previous reviewers' reports. Diversity in models is always beneficial, providing that the models perform adequately and are appropriately evaluated. In that sense I encourage the use of regional models other than the widely employed RACMO2 and MAR models, but efforts are still needed in this paper to reach a sufficient level of evaluation for HIRHAM5 products. Although the regional model results over Greenland for the reference present period seems more reasonable (which is a prerequisite before analyzing projections), those for Antarctica are in significant disagreement with the literature and results for both ice sheets need to be more properly discussed. Antarctic ice shelves, over which runoff for the reference present period was exaggerated by several orders of magnitude compared to the existing literature in a previous version of the manuscript, have simply disappeared from the revised version, which is not a satisfying way to deal with this issue. SMB changes in these areas do not contribute directly to SLR but local changes can affect the ice sheet dynamics, especially if these runs are used to drive an ice sheet model. Other minor, though important comments are provided below. I encourage the authors to go through previous reviewers' reports and identify the remaining comments to be considered (they are all meant to help you improving the paper) and adapt the manuscript accordingly or provide a justification otherwise.

Reply: Following a reviewer request from the first round (with which we agreed totally) we changed our analysis to only look at grounded ice. As a result, the SMB component values were lowered significantly to more realistic levels. If needed, we can add another table showing values for the Antarctic ice shelves and compare these values with the literature. Furthermore, we felt that we gave satisfactory answers to all comments from the first round. We are unsure which comments the reviewer are referring to. Can the reviewer provide more details?

Specific comments

Section 2: I get that you used EC-Earth because the CMIP runs have been performed at the DMI which is also the affiliation of most of the authors. But this is not enough to justify a choice for a model, and we would want to know more about EC-Earth performance over your regions of interest at least at the RCM boundaries. In the present version we only have a picture of how EC-Earth compares to other CMIP models in terms of relative change in precipitation as a function of change in temperature (which shows that EC-Earth is actually in the upper half of the model ensemble). Here temperature and humidity from the forcing fields are indirectly evaluated through the comparison of average, spatially integrated temperature and precipitation dynamically downscaled by HIRHAM5 against a reference run of HIRHAM5 driven by ERA-I and already evaluated in a previous publication, but what about regional differences ? what about other climatological variables (sea surface conditions, circulation in the forcing itself)?

Reply: The relative performance of EC-Earth on a regional scale in the polar regions has been investigated in several other papers, notably by Barthel et al., 2020 for CMIP5 models and also in a new work in preparation by Cecile Agosta (pers. comm) for EC-Earth v3 in the context of the full CMIP6. EC-Earth v2 has been used in a number of studies with a focus on Greenland and the Arctic, showing that it has an arctic cold bias (see figure 2). In EC-Earth v3, this arctic cold bias has more or less disappeared (see figure 2) and the current study aimed at investigating how this would affect the SMB for Greenland. We are not saying that EC-Earth is the best model to represent the two CMIP generations in any way. We will add more text on this in the introduction section and the references noted above. When it comes to regional differences between the HIRHAM5 downscalings of the two EC-Earth versions, we will add more text linked to figures 5 and 6.

It is not clear how these reference periods have been chosen neither why they differ between Greenland and Antarctica (20 vs 30 years).

Reply: The CMIP5 downscalings for Greenland and Antarctica were part of two separate projects with focus on two different time periods. When we planned for the CMIP6 downscalings we decided to use the same time periods as were used with CMIP5 to save time and computing power. We understand the confusion that can arise from this decision. We will add information on this.

Minor comments

P3L63: Instead of distinguishing between evaporation and sublimation, the paper would more gain in exhaustivity in distinguishing between surface sublimation and drifting snow processes, especially for Antarctica as the latters are currently considered to currently drive ablation at the surface of the ice sheet.

Reply: The evaporation and sublimation contributions are given in one single variable. We are not able to separate surface sublimation from other kinds of sublimation. As stated in the manuscript, the current version of HIRHAM5 does not have drifting snow implemented. Also, current studies (e.g. Mottram et al., 2021) show little difference on continental scales between models that include drifting snow processes and models that do not, although on a local scale these processes may be important. We will add this point in the discussion.

P4L122: Why is the subsurface model only applied to Antarctica? Please justify. Is it an offline method, i.e., Do the changes in surface properties feed back on the atmosphere ?

Reply: This is a good point. We have now applied the offline model to Greenland as well and the SMB component values will be updated in table 2, figure 7, figure 8 and in the text. Yes, the subsurface model is offline and therefore feedback to the atmosphere is excluded.

P4L122-123: I don't really understand the meaning of this sentence as it seems obvious to me that you will use your model results to comment on the SMB change. Do you mean SMB is a diagnostic variable (sum of SMB components vs prognostic = dz/dt * rho) of the model ? Is it computed offline?

Reply: Yes, the sentence is a bit confusing. We get the SMB values from the offline model which is using the HIRHAM output as input. We will make the sentence clearer.

P4L123: "better than" what? Please clarify/justify.

Reply: The sentence will be changed.

P5L143: Why this period and not the historical period?

Reply: The period 1991-2010 is in the historical period for the Greenland runs. This period was chosen for comparison against ERA-Interim in an earlier study when the ERA-I data were only available from 1989 onwards (see Lucas-Picher et al.)

P5L151-152: What are these results telling us regarding the main objective of this paper? what are their implications?

Reply: A sentence on the implications with regards to precipitation and surface runoff will be added.

P5L155, "over the ice sheets": Do you mean each model on their own ice sheet mask ? which region did you use specifically?

Reply: We regrid all model data to a common grid. Due to the poor horizontal resolution of the GCMs, we define all land grid points as ice sheet points for Antarctica as well as Greenland. We will add this information to the text.

P5L156: You could comment on the expected consequences for the downscaling.

Reply: We discuss this in the first paragraph on page 10.

P6L164-165: This does not justify why did you choose this specific model though. Note that HIRHAM5 downscalings are on the upper part (or half) for both model ensembles and for both ice sheets.

Reply: You are correct that this does not justify the model choice. See reply on your first specific comment above.

P6L181: This is confusing since the SMB of Antarctica is not negative. Please rephrase.

Reply: The end of the sentence will be rephrased.

P6, Section 3: I would advise being more explicit at the beginning of this section about the approach adopted to evaluate HIRHAM5 runs (HIRHAM5 driven by ERA-I taken as a reference instead of observations etc, performance of HIRHAM5 driven by ERA-I can be found in …). Add and discuss regional differences, instead of only comparing spatially and temporally average values of temperature and SMB components in Table. Compare with other RCMs, discuss the balance between individual components when needed (especially for HIRHAM5 driven by EC-Earth3 in Antarctica: 261 Gt of runoff over the grounded part of the ice sheet vs 3137 Gt of precipitation….)

Reply: We agree. We will add more information on our method and give a more detailed description of what is seen in the figures and also compare with other RCMs.

P7L198-200: So, does the period over which you compare the temperature between the GCM and the RCM driven by ERA-I differ?

Reply: Yes, for Antarctica we compare 1971-2000 means for HIRHAM5 downscaled using EC-Earth with 1979-2000 means for HIRHAM5 downscaled using ERA-I. However, we get very similar results if we use the 1979-2000 period for both runs. The 1979-2000 sums are between 1 to 1.7% higher than the 1971-2000 values for all three SMB components.

P7L219: Specify that this stands for precipitation on grounded ice only.

Reply: We will.

P8L223-224: Elaborate on the consequences for ice sheet SMB (integrated and regional). What about other surface (ablation) processes relative to the reference run?

Reply: This section deals with precipitation. Please see other sections for other processes (eg. lines 260-265). Also, higher precipitation leads to a higher SMB, which can then lead to a mitigated SLR effect/result from the AIS. Especially, when looking at regional precipitation distributions, because some of the ice shelves are thinning which means that they lose their buttressing effect on the grounded ice. So if the precipitation is falling over the ice shelves it might slow the thinning down.

P8L232: Why is the approach more realistic in a former study? Why it has not been conserved in the present work?

Reply: Thank you for noticing this. All SMB values in the current study are obtained using this so called "more realtistic approach" described in Langen et al. We will modify the text.

P8L238, "420 Gt/yr": Specify that this stands for grounded ice only.

Reply: We will.

P8L242-249: Please discuss the related implications for the present period as well as for projections, show regional differences and add ice shelves to the analysis. Are these differences significative? How do they compare, for instance, to natural variability?

Reply: We will add discussion on the implications and a comparison with natural variability.

P9L256-258: How do these numbers compare to previously published projections of GrIS SMB?

Reply: You are right. This information is of great interest. We will add this.

P9L260-265: Same than previous comment.

Reply: We will add this.

P10L299-300: see also Kittel et al. (2021) for Antarctica and Fettweis et al. (2013), Hofer et al. (2020) and Noël al. (2021) for Greenland.

Reply: We will add this to the paragraph.

P10L311-313: What about absolute values of the anomalies ? Are they also comparable?

Reply: The information will be added to the paragraph.

P11L323-325: You give here an argument that undermine your own methodology since you have not discussed the representation of surface oceanic properties in both versions of EC-Earth for the reference period.

Reply: The surface properties are shown in figures 2 and 3 and it is correct that these have a great influence on the SMB estimates. The warm bias around Antarctica present in EC-Earth v3 is clearly affecting the ice sheet conditions. Many CMIP6 models have common issues with clear biases in the Arctic and/or the Antarctic. We will add a reference to this in the paragraph.

P22, Figure 4: Report explanations of letters and colors directly on the plots instead of grouping everything in the caption to improve the readability of the figure.

Reply: We will do this.

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