Answer to Referee #1 – Manuscript tc-2023-24

Mostue and co-authors explore summer and autumn differences in surface mass and energy budget (SMB and SEB) components as well as cloud cover based on output of a regional climate model forced by several global climate models from two generations under the high-end baseline scenario. They start by relating anomalies in near-surface temperature with individual SMB and SEB components, showing similar behavior and magnitude to SMB, melt, runoff, longwave down and longwave up. However, the set of models used from the most recent generation of global climate models projects more warming than previously. Finally, it is shown a contrasting relationship between the anomaly of near-surface temperature and cloud cover anomaly between the two generations of global climate models. The authors hypothesize that the cloud cover decrease allows more absorption of solar radiation by the surface, generating enhanced surface melt and runoff in the ablation zone in summer extending to autumn.

We would like to thank Referee #1 for taking the time to read our manuscript so thoroughly and provide in-depth feedback on our study. Below we have responded to their comments and how we would like to address them. We think that our manuscript will greatly benefit from the adjustments.

This piece of work explores relevant scientific aspects, but the methods and set of variables used are not sufficient to prove the robustness of the results. The level of detail provided in the Section 3.1 and 3.2 is commendable, but presents an unnecessary detailed picture between near-surface temperature with SMB and SEB components. The most relevant part starts with the lower panels in Figure 2, serving as motivation for the rest of the manuscript. Even though the authors show no relevant changes in cloud optical thickness, it would be worthwhile to explore changes in cloud microphysics and its relationship with cloud cover. In addition, summer and autumn precipitation should be included in the analysis, given its role to surface albedo.

My comments are provided by line number (LN) or specific figure below.

First of all thank you for highlighting that our manuscript 'explores relevant scientific aspects'. We have highlighted below how we will adjust our manuscript relating to more specific comments.

However, we do not agree with Reviewer #1 that Sections 3.1 and 3.2 contain too much detail when connecting the surface mass balance and surface energy budget projections over Greenland to the near-surface temperature anomaly. These results present some of the most novel findings. One of the main issues highlighted by the latest IPCC AR6 report is that the CMIP6 model mean or related impact studies (as presented here) can not be used at face value anymore, because some models have unrealistic temperature sensitivities compared to proxy data. The only way around that issue is to compare the surface mass balance and surface energy budget projections at a given temperature anomaly - something that has not been done for any of the two large ice sheets, Greenland and Antarctica. In addition, the literature about Greenland SMB and SEB projections is very sparse to begin with, and nobody has studied it for CMIP6 in great detail, let alone compared CMIP5 to CMIP6 high-resolution climate projections. While we agree that the level of detail might be too great for some parts of the scientific community more interested in the general magnitude of sea level rise, we still believe that there will be interest in our results.

Regarding the comment "The most relevant part starts with the lower panels in Figure 2..":

While Figure 2 (lower panel), showing cloud cover changes vs Greenland temperature anomaly for summer and autumn, might be one of the most interesting findings of the paper and could deserve its own publication, we would like to highlight that we have chosen to focus on the connection between SMB and all of the most important physical drivers of the differences between CMIP5 and CMIP6 SMB projections in this manuscript. Solely focusing on clouds and their microphysics is not the scope of this study, but would certainly present an interesting additional publication (the data of the presented simulations is open-source and therefore open to anyone who would like to perform this analysis).

The Introduction is short and does not summarize/highlight what the scientific community has recently done concerning the impact of clouds and surface albedo on SEB and SMB components over the Greenland ice sheet.
We agree that the 'Introduction' could be expanded with respect to the latest literature on the impacts of clouds, albedo, the SEB and SMB components. We have expanded our introduction accordingly in the revised manuscript.

LN22: the accelerating mass loss pace since the mid-1990s is not only a consequence of increased temperatures from anthropogenic greenhouse gases, but rather a consequence of a superimposed effect with extraordinary atmospheric conditions in recent summers (Bennartz et al. 2013, Fausto et al. 2012, Tedesco et al. 2011, Tedesco et al. 2016).
We agree and have rephrased and expanded this part of the 'Introduction' to highlight the recent anomalous phase of the North Atlantic Oscillation and the connected increase in high-pressure phases of the Greenland Ice Sheet during the summer months (JJA) (Bennartz et al. 2013, Fausto et al. 2012, Tedesco et al. 2011, Tedesco et al. 2016).

LN28: the SMB definition should not be part of the Introduction, but in the Methods section, naming individual components and explaining how do you define accumulation and ablation zones.
We agree that the SMB definition could be moved to the 'Methods'. We have changed this accordingly in the revised manuscript.

LN31: in addition to solar radiation, consider the role of sensible heat flux to darken the surface (Wang et al. 2021)
Sensible heat flux is mostly relevant very close to the tundra (i.e. where the ice sheet ends). We have studied the Sensible heat flux (and Latent heat flux) and they are insignificant in extreme-high-emission scenarios over Greenland when comparing CMIP5 and CMIP6 MAR projections. We have included this information in the revised manuscript and added the plots for these terms in the Supplementary Material S.4.

LN41: the authors should address the fact that as a consequence of more open waters, CMIP6 projects more precipitation and more rainfall in Greenland than CMIP5 (McCrystall et al. 2021). This point can also be later discussed as a factor contributing to decreasing albedo, as also shown by Box et al. (2022).
We agree that more open water and changes in precipitation certainly contribute to the difference between our CMIP5 and CMIP6. Both of these factors have been discussed in more detail in Hofer et al. (2020) using the same simulations as here. Rainfall projections only differ from 2070 onwards between CMIP5 and CMIP6 MAR simulations, however, the SMB and melt projections start to diverge already from 2020 onwards. We therefore do not think that extra rainfall is the main driver behind the diverging SMB sensitivities, albeit it might still be a contributing factor later on in our projections. We have now added references to McCrystall et al (2021) and Box et al. (2022) and a few more sentences highlighting this discussion in the revised manuscript.

LN50: state that a surplus in SEB is energy available for melt and not necessarily surface melt
This is a semantic discussion about what constitutes the “surface”. Most of the radiative and non-radiative energy fluxes are absorbed within a few cm of the first ice/snow crystals of the surface, and surface-energy-budget-triggered melt usually melts ice/snow crystals within the first few centimeters. Therefore, we do not see what the reviewer means when they distinguish between “melt” and “surface melt” in connection to the surface energy budget.

LN64: the last paragraph of Section 2.1 could be moved to the Introduction, where a few of these references could better distilled
This was done accordingly in the revised manuscript.

LN72: it would be relevant to explain here why only RCP8.5 and SS5-8.5 is chosen for the study, as Hofer et al. (2020) made use of all the projected scenarios
Hofer et al. (2020) also only used MAR simulations for the extreme-high-emission scenarios RCP8.5 and SSP5-8.5. The authors then used statistical extrapolation combined with the outputs of the actual CMIP5 and CMIP6 GCMs to infer their surface mass balance projections for other emission scenarios.

LN75: it is also unclear why the period 1961-1990 is chosen. I would assume the last 3 decades (1991-2020), responsible for the accelerated mass loss, a better period for comparison with future projections
We chose to use the period 1961-1990 as our thirty-year average reference period because the GrIS was assumed to be in a stable state (van den Broeke et.al. 2016).

Calculating the mean only really reflects the general distribution when the values for that period roughly follow a normal distribution. Therefore, choosing the mean over the indicated period of 1991-2020, which exhibits a clear linear trend, does not make too much sense in our opinion.

We have added more detail to that part of the manuscript explaining the choice of our reference period.

LN82: it should be indicated how the ice cover mask (more than 10\% ice cover) can influence the following results
With a 10% ice cover mask that does not change over time we expect our SMB reduction to be slightly overestimated compared to a dynamic ice mask, but recent research indicated this error to be somewhere between 1% and 6 % (Kjeldsen et.al. 2020, Hansen et al.2022).

We have expanded on that part of the manuscript considering this and added the corresponding references.

LN83: it is unclear why a twenty-averaged period for ~4ºC is chosen for the dissemination of certain the results
LN85 how can you gain insight of changes caused by rapid Greenland warming using a twenty-year averaged period?
Throughout the manuscript we use the full time-range available from our simulations, which

is until 2100 (e.g. Fig.1 and 2). We have chosen a +4C threshold (+-10 years) as a specific focus to be able to compare all models for the same temperature increase. The individual CMIP models warm at different rates, thus do not reach the same temperature by the end of the century. +4C is the highest temperature rise for a twenty-year-averaged period where we have data for all CMIP5 and CMIP6 models, therefore the reason for this choice.

We have added more detail to that part of the manuscript explaining the choice of this warming period.

Figure 4, 5, 6 and 7: use statistical inference to indicate the level of confidence in changes between CMIP5 and CMIP6.
LN115: could you present the same charts (Figure 1 and 2) but for the differences between CMIP5 and CMIP6, making use of statistical inference to state the robustness of the mentioned differences?
For the variables in Figure 1 and 2 (i.e. SMB, ME, RU, SEB components, and Cloud Cover) we have calculated the R2 score (Mostue 2022, Table B.2) and the one standard deviation around the regression lines (Supplementary Material Table T1). Where we state that there is a difference between CMIP5 and CMIP6 in the manuscript, we find the difference between the regression lines to be larger than the one standard deviation and therefore statistically significant.

Figure 1: legends and axis labels missing. Also, consider making the season as a subtitle of the subplot as in Figure 2
Thank you for pointing this out. This happened during the processing of our file in the TC submission system. We will ensure the labels are there in the revised manuscript.

LN139: start the sentence with "In SON" instead of "Here". Otherwise, it is not clear to which season this sentence belongs
As we state in LN136-139 we are commenting on the general trend of the SEB components for both seasons. Therefore in our opinion, ‘Here’ is appropriate in LN139, and should not be season specific.

LN147: in LN139 you explain that more SW$_{net}$ is due to darkening and here is due to SWD. Please, rephrase it.
In LN139 we explain the reason for the general increasing trend of SWnet for a given temperature increase for both seasons and both CMIP5 and CMIP6 individually. Then, in LN147 we consider the differences in SWnet between CMIP5 and CMIP6 (only seen in summer), and explain it by the difference in SWD.

We have slightly adapted the wording of these two sentences to avoid any confusion for the readers.

Figure 2: legends missing and temperature unit incomplete
Thank you for pointing this out. This happened during the processing of our file in the TC submission system. We will ensure the labels are there in the revised manuscript.

LN165: why do you assume that no differences in cloud optical depth means no differences in cloud microphysics? Isn't this statement contradicting Hofer et al. (2019)? Could you elaborate your thought?
In LN165 we talk about the reason for higher incoming solar radiation reaching the surface in CMIP6 simulations. Because we cannot find any difference in cloud optical depth it is only reasonable to assume that most of the extra solar radiation is coming from the decrease in cloud cover. Given that cloud optical depth for a given total cloud water content is defined by cloud microphysics (mostly the phase of the cloud particles) and we see no difference in cloud optical depth between CMIP5 and CMIP6 then the only explanation is that there is no

difference in cloud microphysics/cloud-phase between the two ensembles. We do not see why that statement would contradict Hofer et al. (2019).

LN181: The twenty-year averaged cloud cover anomaly is compiled by a wide variety of circulation patterns. Only high frequency of a certain circulation pattern would depict the topography influence on the cloud cover composite. Thus, there is no information enough to infer the likelihood of circulation-driven cloud cover change.
We are not sure what the reviewer here means by saying that “only high-frequency …. of circulation pattern would depict the topography influence on cloud cover.”.
We looked in detail at the cloud cover response for each of the 11 models chosen for downscaling. The overall message is that except for MIROC5, the individual models generally capture the ensemble mean really well. We have done this analysis for low-, mid- and high-level clouds, as well as for the total cloud cover. We have added the corresponding figures to the supplementary material of the manuscript (S13 and S14), as well as two sentences in the main text explaining why we think that the models capture the overall cloud cover response for CMIP5 and CMIP6.
Given that the individual models capture the overall ensemble mean cloud cover change well, we do not see why cannot make an inference about the topographic influence as described in Hahn et al. (2020) using similar MAR simulations. But maybe the reviewer did comment on a different aspect of our work here that we did not fully grasp.

Figure 5 and 6: consider two different color maps to stress the fact that colors shading in summer is not comparable with autumn. Perhaps, relative changes (e.g., ratio) instead of absolute changes could be here considered
The absolute magnitude can be seen from Fig.1 and Fig.2. In Figure 5 and 6 we are more interested in showing the pattern of changes and their physical explanation to what is causing them.

However, to avoid confusion for the reader we have added a sentence in Figure text 5 and 6 emphasizing the difference in colourbars of these two plots. Additionally, plots of the relative change for Figure 5 and 6 were added to the Supplementary Material (S.15 and S.16).

LN245: precipitation has so far been discarded of the analysis, but here it would be interesting to assess if precipitation, more specifically liquid precipitation, could play a role in the snow darkening and surface runoff.
Investigation of the rainfall has already been done before by Hofer et al. (2020), Fig. 6 (A-B). This figure shows that rainfall only really diverges towards the last two or three decades of the simulations between CMIP5 and CMIP6, but SMB starts diverging in the early 21st century (around 2020).

Technical corrections

LN9: spell the name of the regional climate model correctly
This was corrected in the revised manuscript.

LN11: indicate the corresponding level of uncertainty
We do not understand to what the referee thinks we should ‘indicate the corresponding level of uncertainty’ in LN11, as there is no result presented in LN11. The next closest value is presented in LN13 (“...during autumn with a reduction of 14.1 ± 4.8 mmW…”) which already indicates an uncertainty.

LN27: spell the surname of the main author correctly
This was corrected in the revised manuscript.

LN32: spell the surname of the main author correctly
This was corrected in the revised manuscript.

LN51: downwards instead of "down towards"
This was changed accordingly in the revised manuscript.

LN51: LWU is defined as LWD
This was corrected in the revised manuscript.

LN55: introduce SWD at the beginning of the sentence
We have slightly adapted the wording of this sentence to avoid any confusion for the readers.

LN56: suggested place to define SMB instead of doing it in the Introduction
Equation 1 and the definition of SMB have been moved to the 'Methods' section in the revised manuscript.

LN59: spell the surname of the main author correctly
This was corrected in the revised manuscript.

LN71: spell the name of the regional climate model correctly
This was corrected in the revised manuscript.

LN154: Figure 2 c and d, instead of "a and b"
This was corrected in the revised manuscript.

LN156 Figure 2 c instead of "a"
This was corrected in the revised manuscript.

LN179: total instead of "toal"
This was corrected in the revised manuscript.

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