Answer to Referee #2 – Manuscript tc-2023-24

Review Cloud- and ice-albedo feedbacks drive greater Greenland ice sheet sensitivity to warming in CMIP6 than in CMIP5.

I've read the manuscript with interest, and the study is publishable after some questions are properly addressed. It analyses in detail the differences between projections for the Greenland Ice Sheet from CMIP5 and CMIP6 models. This has not been done before, and the study provides interesting new findings. However, addressing the issues below can strengthen the study and can take away potential concerns of readers.
We would like to thank Referee #2 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.

Main issues:

1: The focus on the radiative terms only.

With some references, the authors justify why only the radiative terms of the surface energy balance (SEB) are discussed in this manuscript. I'm not a prior convinced that this is justified. The sensible heat flux (SHF) is a significant contributor to melt in the ablation zone (you can find many papers about that), and the ablation zone is where the differences are made. I, therefore, ask the following:

1. a) The authors repeat the analysis as shown in figure 2a and 2b and figure 5 for SHF, LHF (latent heat flux) and GHF (ground heat flux - the residual most likely). If these terms are indeed insignificant, as the authors argue now, these figures may be added to the SOM and single references like "we've studied these terms too and they are insignificant" will do in the main text. However, if SHF/LHF/GHF changes are not negligible, their discussion needs to be included in the text.
   We have studied the SHF and LHF and they are insignificant compared to the other energy fluxes in MAR, especially when considering strong warming scenarios like SSP5-8.5 and RCP8.5. We have commented on this in the revised manuscript and added the plots for these terms in the Supplementary Material S.4.

   During melt, MAR simulates GHF close to zero.Therefore, we did not investigate this term further. We have included this information in the revised manuscript for clarity.

It might also add more clarity to the cloud arguments, as SHF, in contrast to LWD, is not influenced by changes in cloud cover. Conversely SHF is also influenced by surface warming too, I'm not sure a prior if changes in SHF are clearer than the LWD change. Well, the authors have to find out.
Given that the sensible heat flux is proportional to the gradient between surface temperature and near-surface air temperature one would assume that the SHF is also influenced by

changes in cloud cover, as shortwave radiation tends to warm darker surfaces more than brighter surface, in contrast to longwave radiation. Clouds do indeed shift the surface energy budget terms from more shortwave towards more longwave radiation, changing the surface temperature distribution. Furthermore, please see our previous comment why we did not include a more in-depth description of the sensible heat flux in our manuscript.

1. b) Consequently Equation 2, the SEB, should be adjusted to

ME = LWD - LWU + SWD(1 - \alpha) + SHF + LHF + GHF [W /m2]

Please remove the \epsilon \sigma T^4 term from the equation, as this equation is never used in the manuscript (and please remove the crosses as that denotes the outer product of matrices), this relation can be mentioned in the running text; and please correct the units. Finally, the equation as a whole is the SEB, not the right-hand side.
We agree with the referee and have changed Equation 2 accordingly. The units have also been corrected in the revised manuscript.

2: The role of melt water buffering by firn:

In brief, the manuscript now states this: The CIMP6-CMIP5 change in SMB for a given warming (fig. 1a) is occurring in fall (fig. 1c), while the fall SEB is virtually unchanged (fig 2b, 5-right row). However, the CMIP6 fall albedo of the ablation zone is lower, leading to more melt which, in absence of firn, runs off directly (fig 6 - right column). Correct, however, by zooming in on the fall, it ignores the relevance of the melt-refreezing-runoff pattern in summer. Furthermore, I'm highly puzzled by the lack of refreezing increase during any phase of the warming. This runoff increase is the overarching feature of all RACMO simulations and very visible (as far as I know) in MAR simulations driven by reanalyzes. Therefore, I ask the following.

1. a) A description of the exact run protocol of MAR needs to be added in the methods section. Furthermore, (as understanding the firn response is important in this study), expand the description of the firn model with details relevant for this study. For example, how thick can the firn column become in MAR? Hence, the authors need to be able to address to which extent coding choices have impacted the modelled melt water buffering capacity in a transient climate?
   Only the 30 first meter of snow is resolved in these simulations. A layer is automatically added/removed at the bottom if the total snow is < 29m or > 31m The maximum liquid water content in MAR is 7% vs 5% in HIRHAM vs 2% in RACMO. The choice of 7 % in MAR is discussed in Lefebre et al. (2003). Therefore, the densification and the warming of the snowpack is faster in MAR than in the other models. It also means the meltwater capacity retention of the snowpack decrease faster in MAR. Finally, the same MAR executable has been used in both CMIP5 and CMIP6 simulations.

We have extended the ‘Methods’ to include the run protocol for MAR in the revised manuscript.

The authors now state in lines 248-254 that the faster warming in CMIP6 induces that the percolation has more remaining melt water buffering capacity when the simulation crosses the 4 K warming point. I'm not sure if that is correct, as many MAR projections available nowadays are not run in single linear mode, thus one single MAR realization that started in 1950 and ended in 2100. Those many MAR projections that are in the community are run with a "each year initialized separate"-protocol; thus, that the weather of that year is repeated until the MAR firn column, and hence the SMB and its terms, have become in equilibrium.

1. b) If the latter protocol has been used, the 'faster warming' argument is invalid. Still, a detailed explanation of figure 6, left column, needs to be given. If the first protocol has been used, it is worth to show the difference in firn air content (or another firn state metric) to highlight that this different firn state strongly contributes to the pattern visible in the left column of figure 6. Furthermore, and that is really important IMHO, the conclusions should then be: CMIP6 melt more due to stronger warming and, our new point, decreasing (high) cloud cover, both only partly mitigated by more remaining melt water buffering capacity due to the faster pace of warming.
MAR has been run in "community" mode meaning that a member is started every 5 years over 1950-2090 and initialised by the snowpack simulated for this year by former MARv3.9 based simulations using the same GCM as forcing. Each member simulates at least 15 years (including 10 years of spin-up). As the period simulated by each member covers at least two members initialised at different years (5 and 10 years ago), the retained years of each member has been chosen to be independent of the initial conditions i.e. to have difference of SMB, runoff and refreezing lower than 1 GT/yr between the different members for the same year. Due to the high liquid water content allowed in MAR, a snowpack can lose quickly (~10years) its capacity to retain meltwater as it becomes too dense.

2. d) The authors should discuss in more detail why not part of the melt increase is buffered by runoff (figure 1), contrary to findings in preceding studies (like Noël 2021, doi: 10.1029/2020GL090471). From Figure S7, bottom row, I would expect a clearer visible increase in refreezing. Or is the increase in refreezing matched by the increase in rain? If so, please consider adding rain and runoff in Figure 1. Furthermore, is runoff indeed zero in the interior of Greenland? From Figure S7 it is not 100% clear.
We are not sure what the reviewer means by “melt buffered by runoff” in this case?

   Regarding adding rain and runoff, this had already been done in 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. Additionally, the reason why refreezing in Fig. S7 bottom row is not visible compared to the other surface mass balance components is because compared to the other terms refreezing in summer (JJA) is almost an order magnitude smaller (at least in MAR) and melt really dominates all other terms (as it has also done for recent extreme melt years already).

3. d) The discussion of the very different impact of clouds changes over the ablation zone, compared to the percolation/accumulation zone, could be much stronger and clearer if in the SEB analysis (figure 2) the ablation zone and accumulation zone are separated. Most likely a static separation mask is much easier than a transient mask. A good mask is to take the ablation zone outline for 4 K warming, as that is the "warming" point in time that is analyzed most in the paper.
We agree with the reviewer that this would be an interesting addition to the study. In the revised manuscript, we will perform an additional SEB analysis for a division between the ablation zone and accumulation zone for a 4 degrees Celsius warming.

3: Don't leave uncertain things you could verify in the model data:

On numerous points (about 10-20, I lost count) the authors are unsure as they use "we argue" (line 181), "we expect" (line 124), "can possibly be explained" (line 248), "we suspect" (line 267) or "we suggest" (line 270), "likely due" (line 277) while the answer can be found in the model data the authors should have. Go and check your ideas in the model data and write with certainty when it is true and remove the statement if it is untrue. I don't see a valid reason to be unsure. When some of those "unsure statements" cannot be verified and are retained in the manuscript, please address the reasons in the reply to this review.
We thank the referee for this comment. We have made changes to the revised manuscript to avoid leaving the readers with any uncertainty regarding our statements. However, we would also like to refute the claim that everything that can be checked in model data has to be communicated with absolute certainty. First, authors have different styles of writing and we strongly believe that these phrasings fall within that category. Second, over the last decades many findings in climate science have naturally been refined, sometimes to a point where old knowledge has been fully replaced with new findings. Therefore, we think using slight nuancing in scientific communication can also future-proof the communicated findings. However, it all boils down to personal preference.

4: Demonstrate that these results are not coincidental by the CMIP5 & CMIP6 model selection but a genuine difference between CMIP5 and CMIP6:

1. a) Hofer et al 2020 gives the arguments for the model selection. This information needs to be summarized in this paper as it should not be necessary to read Hofer et al 2020 to understand this model selection.
We have included a summary of the reasoning behind the model selection by Hofer et al 2020 in the revised manuscript.

2. b) In the discussion, it needs to be showed (as good as possible) that these 5 & 6 models are representative for change in the modelled cloud climatology over Greenland. It (representativeness for cloud cover changes) is not mentioned in Hofer et al, 2020. I know this can be a lot of work (as modelled cloud cover over Greenland

from ~30 models needs to be compared), so I can understand if the authors use existing studies to demonstrate this - if these are available. Nonetheless, the authors make implicitly this generalization, however, it should be justified.

We agree with the reviewer that we implicitly assume that our subset of models is representative of the overall climate of the relevant CMIP ensemble. While the models have been selected with great rigor for the CMIP5 models (see Barthel et al. (2020)), the same cannot be said about the subsample of CMIP6 models. However, in Hofer et al. (2020) it has been shown that the CMIP6 models represent the general climate and warming trends for the Arctic quite accurately. In addition, 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.

Minor general point on units:

1. a) I would prefer that K (Kelvin) is used instead of degrees Celsius.
   As either of these units are equally accessible to the audience and using either does not change the interpretation of our results we chose to not change our results to K (Kelvin).

2. b) Albedo is unitless, and percentual changes of albedo are meaningless. Please correct this in Figures 7, S9 and S10, and section 3.4. Especially the numbers in lines 274-275 are wrong wrt units.
   Albedo is a fractional quantity that by definition can take up values between 0 and 1 (fraction) - but in mathematics percentage is not seen as a unit, but rather as a different way of expressing a fraction (½, 0.5 or 50% all mean exactly the same thing). However, to avoid any confusion we have changed the indicated text and figures.

3. c) Use hPa and not Pa in lines 184-185.
   This was changed accordingly in the revised manuscript.

4. d) Equation 1 has units Gt yr, but no result is shown with that unit. mmWE yr-1 or mmWE season-1 is used everywhere. So why not for Equation 1 too? At the other hand, Gt yr-1 is a unit easier to interpret for a larger audience, so it is worth to consider to use this unit more often in the manuscript - like in the running text.
   We agree with the referee that the units should be consistent throughout the manuscript. We have changed the unit from ‘mmWE’ to ‘Gt yr-1’ for Figure 1, 2a, 2b, and for the corresponding results in the running text of the revised manuscript.

Minor comments:

13: Please remove these two numbers (4.8 & 12.5 mmWE) as they make no sense for anyone without more context. Alternatively, use Gt yr-1 here. But in case of the latter, Gt yr-1 should be used more often in the manuscript - as already stated above.
We have changed the units from mmWE to Gt yr-1 accordingly in the revised manuscript.

19: "Undocumented" Please rephrase by e.g. "unnotified". Those changes have been documented before - as they were in the output files of many simulations - but nobody has written a paper about it before.
This was changed accordingly in the revised manuscript.

24: Add Noël and van Kampenhout, 2021 (cited above) here.
Noël and van Kampenhout 2021 was added accordingly in the revised manuscript.

32: "Broek" is "Broeke". Check also other the references on typos.
Thank you for pointing this out, this was corrected in the revised manuscript.

51: The second LWD must be LWU.
Thank you, you are correct. This was corrected in the revised manuscript.

77: "In turn" is IMHO not the right connection word here.
We have removed 'In turn' from the revised manuscript.

83: Add a bit more detail - is this a 4 K a (near) surface (T0m, T2m) of, e.g., 500 hPa, warming? Is the year chosen for the GrIS as a whole or for each grid point separately? And what does it imply for the "consistency of the results" as the JJA and SON 4 K warming frames does not refer to the same years and thus firn state?
We use a 4 degree Celsius near surface warming (T2m) for the GrIS as a whole. We have added more detail to that part of the revised manuscript.

Since the IPCC AR6, some of the critiques that have come out state that you cannot directly compare CMIP5 and CMIP6 models because they have different warming rates and some might be too sensitive to greenhouse gas forcing. Therefore, we have focused on looking at differences for a given warming rate. However, we agree with the comment and would welcome any suggestion on how to treat the different firn states for the different seasons.

103: The exact locations of the model edges, expressed in lat & lon, give a reader little clue. Add a figure of the domain extend (in the SOM), or write it down in words, e.g. "extends till Svalbard", "extends xxx km" to all sides of Greenland", …
We have added a figure of the research domain to the revised Supplementary Material (S.1).

106: Please add the typical elevations of the lowermost model layers.
The 5 lower atmospheric vertical levels are ~20, 12.0, 8.0, 4.0, 2.0 m above surface. This was added to the description of MAR in the 'Methods' of the revised manuscript.

113: "near surface temperature anomaly [C (or K)]"
This was corrected accordingly in the revised manuscript

118: Consider not to start a new paragraph here.
This was changed accordingly in the revised manuscript.

Figures 1, 2, 3: Labels have turned into white in the official pdf. Ensure this is ok in the revised manuscript. Furthermore, if you consider a SMB anomaly for a season, the unit is mmWE season-1, not mmWE. I'm not aware of a nice abbreviation for season (like yr and s), so possibly the authors may conceive one.'
Thank you for pointing this out. We will ensure that all labels are visible in the revised manuscript. We have changed the unit for the SMB anomaly accordingly from 'mmWE ' to 'mmWE season-1'.

171: Please state more clearly at the end of the paragraph that despite the changes in Figs 2d, 3b, 4-"right", there is no discernable change in Fig 2b that explains Figure 1c.
We have changed the wording slightly to make this more clear for the readers.

241: "we do not see the same buffering effect". Please mention that this is due to the absence of significant melt in fall in the percolation zone. There is no water to buffer.
We agree and have changed the wording from "In autumn (SON) we do not see the same buffering effect of more refreezing in MAR CMIP6 in the percolation zone as we saw for summer (JJA)." to "In autumn (SON) we do not see the same buffering effect of more refreezing in MAR CMIP6 in the percolation zone as we saw for summer (JJA), partly due to a decrease in meltwater production in this region."

Figure 8: I like the idea of an explaining figure. This one, however, is very unclear, thus fails to meet its aim. It should show that that in summer, the LW effect dominates over the SW effect in the percolation and accumulation zone due to the high albedo of snow, while SW dominates in the ablation zone due to the lower albedo. Improve the figure or remove it.
We have made an improved version of Figure 8 in the revised manuscript, highlighting that SW radiation dominates over the darker surfaces and LW radiation over brighter surfaces in summer.

Citation: https://doi.org/10.5194/tc-2023-24-RC2

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