Sources of uncertainty in Greenland surface mass balance in the 21st century
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Summary

The authors utilize an offline energy and mass balance model forced with CMIP6 global climate model simulations to estimate uncertainty in projected Greenland ice sheet surface mass balance for the 21st century. The authors assess the impact of discrepancies between climate models, differences between future scenarios, and internal SMB model uncertainty in the projections. They find that the largest uncertainty results from differences between climate models, followed by scenario uncertainty, followed by snow model parameter uncertainty.

General Comments

The manuscript is well written and well laid-out. I find the authors' approach to be overall logical. I believe the manuscript is well suited for the cryosphere and should be accepted. However, I have some general and specific points that I think the authors should address before the manuscript can be published:

(1) I think the authors should provide some additional detail about the parameterizations used in BESSI and whether the parameter changes used in the BESSI sensitivity studies really representative of the uncertainty in modeling the Greenland snow and ice surface. I would imagine that comparing multiple SMB models across simulations would add to uncertainty in this component. There are processes (e.g. the evolution of bare ice albedo) that are still not well understood or included in models, and which could add uncertainty in future projections. The authors should discuss these potential caveats in further detail especially with regard to their conclusion that the uncertainty associated with the snow model parameters is small.

(2) The authors utilize a single ensemble member from each GCM to evaluate the inter-model uncertainty. The inter-model uncertainty is therefore influenced to some extent by the internal variability of each model. The question this raises is whether the inter-model variability is larger than the internal variability of any one model. The authors' assumption seems to be that the spread of the single ensemble members from each model is indicative of the uncertainty caused by the inter-model uncertainty. It is not clear whether this is the case. For a more definitive result, it would be useful to perform some additional experiments: (1) Forcing BESSI with the ensemble mean from each simulation rather than single ensemble members, and (2) Forcing BESSI with multiple ensemble members from a single model to compare the inter-model vs. single-model spread. It would be useful to have at least one of these additional simulations if the authors think this makes sense.

(3) The authors note that they linearly interpolate GCM input variables onto the 10 km BESSI grid. This seems somewhat problematic, especially in coastal areas where there is a high degree of spatial variability. Because temperature, for example, is dependent on elevation, downscaling methods are often employed to take this into account (e.g. Noël et al…., Fischer et al., 2014). The simple linear interpolation seems likely to lead to biases in the SMB forcing fields. However, as the authors are looking at differences on a broad scale, it might be less important. The authors should discuss the impact that this might have on the results, and if possible test the impact of a different more sophisticated downscaling technique to evaluate the impact on the results.

(4) The authors frequently refer to General Circulation Model/Global Climate Model/ Earth System Models simulations as "climate models". However, Regional Climate Models are also "climate models", and the use of the term "climate models" is sometimes confusing. I suggest replacing the term "climate models" with "GCMs" to make clear that these are global simulations.

Specific Comments

1. **Line 17:** Add "currently" after "(GrIS)" for clarity.
2. **Lines 27-35:** Here, following the first sentence, the authors introduce what is done in this study, but then go back to describing previous evaluations in the next paragraph. BESSI is also mentioned in this paragraph, but then introduced later, in the last paragraph of the introduction. I suggest moving this material in this paragraph and combining it with the last paragraph of the introduction, and making clear the different sources of uncertainty that are being addressed.
3. **Line 37:** Define the term PDD here.
4. **Lines 41-42:** Here the authors should make it clear that RCM simulations are used to dynamically downscale GCM simulations, which often do not have the spatial resolution or detailed physical representation of the ice sheet surface needed to simulate SMB reasonably well.
5. **Line 43:** I suggest changing "evaluating" to "downscaling", after clarifying the reason for using RCMs in the previous sentence.
6. **Lines 44-46:** Here the sentence is a bit unclear. Suggested correction: "In Fettweis et al. (2008), which utilized RCM simulations to project SMB forced with a subset of CMIP3 simulations, a multiple regression for the SMB changes as a function of temperature and precipitation is performed to calculate the SMB changes for CMIP3 simulations that were not used to force the RCM."
7. **Lines 52-56:** In addition to adding in material from the second paragraph of the introduction, I would suggest briefly explaining what BESSI is and why it is advantageous in this situation as compared with GCM or RCM simulations.

8. **Lines 61-62:** Please explain how the layers are adjusted. Are the authors referring to the snowfall amount within the grid cell? Is there a maximum thickness of the snow model? If the simulation were run continuously, areas of net positive SMB the amount of snow represented in the model would continuously increase.
9. **Line 62:** Suggest changing to "15 snow or ice layers" for clarity.
10. **Lines 68-69:** What was the result of this comparison?
11. **Line 71**: Can the authors briefly describe how these parameterizations work?
12. **Line 74:** "ERAinterim" should be changed to "ERA-Interim" here and throughout, and a brief description and reference should be added.
13. **Lines 83-84:** Can the authors briefly explain the Bougamont et al. (2005) albedo routine here or at the start of the paragraph? How is the snow vs. ice albedo treated? This is particularly important because the contrast between bare ice and snow albedo plays an important role in the GrIS energy and mass balance (e.g. Ryan et al., 2019).

    Ryan, J. C., Smith, L. C., Van As, D., Cooley, S. W., Cooper, M. G., Pitcher, L. H., & Hubbard, A. (2019). Greenland Ice Sheet surface melt amplified by snowline migration and bare ice exposure. Science Advances, 5(3), eaav3738.

14. **Lines 89-90:** Suggest changing "whereas the dewpoint is calculated…" to "with the exception of the dew point, which is calculated…"
15. **Line 100:** Add "to perform bias correction" after "ratio of the monthly means" for clarity.
16. **Lines 104-105:** From Figure 1 it seems that shortwave radiation increases, while longwave radiation decreases, contrary to what is said here. (Also see note below about the axis label on Fig. 1e.) Could the authors be referring to the SSP126 simulation? If so it might make more sense to discuss all the simulations shown on the figure. Please clarify and/or revise.
17. **Figure 1:** The axis labels on Fig. 1e seem to be incorrect. I would expect the downwelling SW radiation to have a positive value, and to be of the same order of magnitude as downwelling LW radiation. Also change "Temperature in 2 m" to "Temperature at 2 m" and "Dewpoint in 2 m" to "Dewpoint at 2 m" in the caption.
18. **Line 105:** Suggest changing "the larger the increase" to "the larger the change", as there is a larger change in the higher GHG forcing scenarios regardless of the direction of the change.
19. **Lines 105-107:** Rather, it seems that precipitation shows the least overlap between scenarios, as the model spread is small relative to the scenario spread in that case. Please clarify or revise.
20. **Line 128:** What is meant by the "actual" climate simulation? Suggest changing to read "following the temporal distribution of precipitation in the GCM simulation."
21. **Line 133:** Note which variables were changed here.

22. **Lines 145-147:** This is a bit confusing. I suggest revising to note that the effect of the larger change in the input variables is a larger cumulative effect on SMB.
23. **Line 147:** Change "snow model" to "BESSI" for clarity.
24. **Line 158:** Change "increase of" to "change in".
25. **Lines 157-158:** As mentioned above, I'm confused about the SW and LW radiation changes shown on Fig. 1. The axis labels for SW radiation seem to be incorrect. An increase in downward SW radiation would be consistent with the decrease in downward LW radiation that is shown. However, an increase in precipitation might be indicative of increased cloud cover, which would increase downward LW radiation and reduce SW radiation. Please revise the figure and/or the text.
26. **Line 158:** The SW radiation having little effect on SMB seems contrary to recent studies that suggest recent changes in atmospheric circulation lead to increased downwelling SW radiation and increased melt (e.g. Tedesco et al., 2016; Hofer et al., 2017). However, it could be that the combination of factors and feedbacks, which are not included in these idealized experiments, may play an important role in that case. This could be mentioned in section 3.3. Perhaps it should also be clarified here that SW radiation alone does not influence SMB in the idealized experiments performed.
27. **Line 172:** Is this the decadal running mean of SMB for the entire ensemble? Please clarify.
28. **Line 206:** Is this a seasonally varying mean?
29. **Line 241:** Could the authors explain this a bit further? Is this because bare ice is exposed at the surface, and do model assumptions about the snow/ice profile play a role in this feedback? I suppose this effect may fall under the model parameter uncertainty experiments.
30. **Line 289:** Explain the "multiple regression" a bit further.
31. **Figure 9:** The plots here are a bit confusing because the shading on (a) shows the maximum range, while (b) shows the mean and 25th and 75th percentiles. Would it be possible to also shade the 25-75 range a slightly different color and show the mean on (a)? Or to show the maximum range on (b)?
32. **Line 310:** Please define ETOPO and provide a reference.
33. **Line 325:** Can the authors explain the choice of the 50 m threshold? Is it estimated based on previous assessments?

Technical Corrections

1. **Line 22:** Change "are met" to "were met".
2. **Line 43:** Suggest changing "leaving their use to" to "leaving their use limited to".
3. **Line 74:** ERAinterim should be changed to ERA-Interim
4. **Line 94:** Change "in the period of 1979-2014" to "over the 1979-2014 period".

5. **Lines 109-111:** Suggest adding “(1)” before “The main ensemble…”, and “(2)” before “The ‘single forcing’…” for clarity.
6. **Line 161:** Add “there” after “melt increases considerably” for clarity.
7. **Line 170:** Suggest changing to “climate model uncertainty, climate scenario uncertainty, snow model parameter uncertainty, and internal variability.”
8. **Line 175:** Change “forth” to “fourth”.
9. **Lines 202-203:** Suggest changing to read: “The scenario uncertainty has a similar magnitude as the climate model uncertainty only at the margins of the ice sheet and in the area where the total variance is low.”
10. **Line 213:** I believe the correct term is “desublimation” or “deposition”.