

Modelling the climate and surface mass balance of polar ice sheets using RACMO2, Part 1: Greenland (1958-2016)

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This paper presents an updated version of the polar version of the RACMO2 regional climate model (RACMO2p2), evaluated over the Greenland ice sheet against various observational datasets. Model updates include changes to the concentration of impurities assumed to be deposited onto the snowpack, the grain size of snow that has experienced meltwater refreezing, the albedo of superimposed ice, and the saltation coefficient for drifting snow. These changes generally result in an improved agreement between modeled and observed atmospheric variables, radiative fluxes and SMB. Some biases persist and can be corrected by future improvements to model physics and parameterizations and/or downscaling of model outputs.

General Comments

The paper is well written and the presentation is generally clear. The paper is not novel in the sense of presenting new model physics or parameterizations, but the changes to RACMO2 that are presented seem to have a significant impact on representation of the Greenland ice sheet surface and the agreement between modeled and observed SMB. A detailed validation of this updated version of the model has been conducted. The paper is therefore likely to be of interest to the cryospheric and climate modeling communities, and is important in providing details about and validation of a new version of RACMO that will be used for future studies of Greenland mass balance. I feel that the paper should be published in the Cryosphere after some relatively minor concerns are addressed below.

1. One general concern is the use of net surface energy balance to indicate energy available for melting. If the snow temperature is below 0°C, this energy must first be used to warm the snowpack before contributing to melting. In the ablation zone during summer where temperatures are close to freezing, most of the net energy goes to melting, but some of it must go into warming the snow/ice. The authors should revise the manuscript to refer to the net energy balance rather than melt energy, or explain why they can assume that the net energy balance can be considered melt energy.
2. Regarding comparisons between RACMO2.3p1 and observations for some of the plots, it would be useful to see how much RACMO2.3p2 improves on the previous version with respect to meteorological variables. Adding RACMO2.3p1 to Figs. 3, 4, and 5 could potentially make them difficult to interpret, but I think that the authors should at least provide some statistics with regard to the meteorological comparison (corresponding to statistics for Figs. 3 and 4). It would be nice to see the corresponding figures for

RACMO2.3p1 in the supplementary material as well. For RACMO2.3p1 outputs corresponding to Fig. 5, there is a similar figure in Noël et al. (2015) as the authors mention, and I think the tables provide enough information to understand the improvement.

3. In Section 3.3 and Tables 1-5, the signs of biases for upward and downward fluxes and interpretation in the text are confusing and sometimes inconsistent. It seems that the biases are generally considered with respect to the absolute value of fluxes (e.g. a negative bias for an upward flux is an underestimate of the upward flux) but this is not always the case. The calculation of net fluxes is also inconsistent. Mostly, the upward flux bias is subtracted from the downward flux bias, but not always.

The authors should make sure that the signs for all biases are consistent, and that the text interprets the direction of the biases correctly. The authors could use the same conventions for both fluxes and biases, but this should be made clear in the figure captions and in the text, to remind the reader that a positive bias for an upward flux is an underestimate of the upward flux. For example, if SW_u (Obs) is -70.9, and the RACMO2.3p2 bias ($RACMO2.3p2 - obs$) is 4.5, the authors can indicate that the magnitude of SW_u is underestimated by 4.5 in RACMO2.3p2.

Alternately the authors could define upward fluxes as being positive, which is more intuitive, and Eq. 1 could be changed so that upward fluxes are subtracted rather than added.

4. The section on the Northeast Greenland ice stream seems a bit out of place with respect to other sections. The authors should provide some more background at the beginning of Section 4.2 discussing how the ice discharge measurements can be used to evaluate SMB. The discharge measurements should also be mentioned in Section 2.5. Also, the authors should provide a rationale for their assumption of equilibrium between SMB and discharge for the northeast Greenland basin for the 1958-2015 period. The better agreement with discharge measurements suggests an improvement to SMB, but doesn't necessarily prove that SMB is accurate. This should be clarified in Section 4.2.

Specific Comments

1. **P. 1, Line 10:** Be more specific here. How are the patterns “better resolved”?
2. **P. 1, Line 13:** “future climate scenario projections” is unclear. Do the authors mean “projections of GrIS climate and SMB in response to future climate scenarios”?
3. **P. 2, Line 19:** Change “model simulations” to “RCM simulations”

4. **P. 2, Lines 25-26:** This phrase is confusing. Also, I don't believe the Box (2013) approach used data assimilation. Suggest revising "and data assimilation ... accumulation measurements..." to read: "and reconstruction of SMB obtained by combining RCM outputs with temperature and ice core accumulation measurements..."
5. **P. 2, Lines 27-28:** Add reference detailing CESM future simulations (Vizcaíno et al., 2014):
Vizcaíno, M., Lipscomb, W. H., Sacks, W. J., and van den Broeke M.: Greenland surface mass balance as simulated by the Community Earth System Model. Part II: Twenty-first-century changes, *Journal of Climate*, 27, 215-226, doi: 10.1175/JCLI-D-12-00588.1, 2014.

The authors might consider citing this study that presents a simulation of SMB from the GEOS-5 model:

Cullather, R. I., Nowicki, S. I., Zhao, B., and Suarez, M. J.: Evaluation of the surface representation of the Greenland ice sheet in a general circulation model, *Journal of Climate*, 27, 4835-4856, doi: 10.1175/JCLI-D-13-00635.1, 2014.

6. **P. 2 Line 49 – P. 3 Line 1:** There are likely also improvements that could be made regardless of resolution, that a high-resolution simulation could not fix. Perhaps mention this also.
7. **P. 3, Line 56:** Could the authors provide a reference for RACMO2.3p1?
8. **P. 4, Lines 82-84:** As noted above, the net energy absorbed by the snowpack must be used to raise the surface temperature to the melting point before it can be used for melting. "M" should therefore be changed to " E_{net} " and this sentence should be revised accordingly.
9. **P. 4, Line 87:** Add "net" before "sensible and latent turbulent heat fluxes" for clarity.
10. **P. 5, Lines 109-111:** The corrections that have been made also can affect the ablation zone, though they probably have less of an impact there. Were similar biases found in the ablation zone previously?
11. **P. 5, Lines 116-118:** If possible, can the authors provide evidence that supports decreasing the size of refrozen snow grains?
12. **P. 5, Lines 136-137:** Can the authors be a bit more specific about the levels or height at which upper atmosphere nudging is applied?
13. **P. 5, Lines 139-141:** Provide a few more details about this. What are the "best" profiles and how are they derived?
14. **P. 6, 149-151:** Are fractional areas of ice vs. tundra allowed in a RACMO grid box? If so, it would be useful to have this information here.
15. **P. 6, Line 156:** Please specify the version number. Is this version 5 or version 6?
16. **P. 6, Lines 157-160:** These sentences are a bit unclear. I think the authors are saying that MODIS values for bare ice albedo below 0.3 are replaced by a value of 0.3, and MODIS values above 0.55 are replaced with 0.55. Any grid

cells without a valid MODIS estimate are assigned a value of 0.55. Please clarify.

17. **P. 7, Lines 201-202:** Are these biases statistically significant? It might be useful for the reader to have this information.
18. **P. 7, Lines 209-210:** Can the authors be sure that the LW_d underestimation leads to the LW_u underestimation?
19. **P. 8, Line 214:** Can the authors elaborate here? Is there a difference because of heterogeneity in fresh snow distribution leading to differences between the model estimate and local measurements?
20. **P. 8, Lines 229-231:** It's a bit unclear that the values in parentheses are biases and not absolute magnitude of the quantities. Clarify here and where applicable elsewhere in the text, e.g. "... between overestimated SW_n (bias of 16.2 W m^{-2})".
21. **P. 8, Line 231:** Make clear whether SW_u is over- or underestimated. I believe it's underestimated. (See general comments.)
22. **P. 8, Lines 236-237:** I believe the newest MCD43A3 product includes a correction for sensor deterioration, but if v5 is used here, this still applies. Perhaps clarify with "underestimated surface albedo for the MCD43A3 v5 product"
23. **P. 8, Lines 238-239:** Again this is confusing because of sign conventions. If the signs of the biases follow the conventions, the net bias should be -23.9 W m^{-2} and not 0.3 W m^{-2} .
24. **P. 9, Line 252:** Also add reference to Table 2 here.
25. **P. 9, Line 253:** Clarify under- vs. overestimated, use " $\sim 4 \text{ W m}^{-2}$ " to indicate that the value is approximate.
26. **P. 9, Lines 269-270:** According to most of the previous calculations of net flux, these terms don't compensate. There is underestimated downward flux and overestimated upward flux, so the net flux is underestimated.
27. **P. 9, Line 271:** Add reference to Table 4.
28. **P. 9, Lines 272-273:** Again, here the biases have been added rather than subtracted to get the net flux, in contrast with calculations for other sections.
29. **P. 10, Line 289:** Here the SW_u bias, shown as positive in Table 1, is referred to as negative, which would make sense if conventions are followed everywhere, but is not consistent with earlier discussion (e.g. p. 9, line 264, where a positive bias for SW_u is referred to as an overestimation).
30. **P. 11, Line 316:** The increase in refreezing is attributed to an increase in precipitation, but along the west coast, there is a decrease in precipitation in some areas. Perhaps another factor could be persistence of snow cover as a result of reduced melting.
31. **P. 12, Lines 353-354:** Show numbers for both model versions for comparison.
32. **P. 12, Lines 358-362:** Is this correction applied to the values in Fig. 9?
33. **P. 13, Lines 402-405:** Provide some numbers to illustrate that the new version performs as well as the previous version.
34. **P. 13, Lines 419-420:** What are the new values for RMSE, bias, and error at QAS_L?

35. **Tables 1-5:** The term ME is used here for melt energy, but the term M is used in the text. These should be consistent. As noted in the general comments I believe this should really refer to the net energy balance. Captions for Tables 2-5 can be reduced to “Same as Table 1 for Station...”
36. **Figure 11:** The red points in (a) indicate something different from (b) and (c). I feel that the authors should include RACMO2.3p2 for (a), (b) and (c), and use the same color scheme. A different color could be used to show the measurements from QAS_L. Units for statistics should be the same for all figures if possible, and should correspond to the units in the text. Also, I believe the third line of caption: “version 2.3 (red)” should read “version 2.3p1 (red)”.

Technical Corrections

1. **P. 1, Line 10:** Change “than the previous model version” to “compared with the previous model version”
2. **P. 2, Line 29:** Change “to explicitly resolve” to “of explicitly resolving”
3. **P. 2, Line 33:** Change “evaluate and improve” to “evaluating and improving”
4. **P. 2, Line 45:** If the authors are still referring to previous versions of the model, change to “is underestimated” to “was underestimated”.
5. **P. 2, Line 50:** Change “near-kilometre” to “near-kilometre-scale”
6. **P. 3, Line 55:** Change “all over Greenland.” to “across the GrIS.”
7. **P. 5, Line 132:** Add “an” before “11 km horizontal...”
8. **P. 5, Lines 134-135:** Mention that the model domain is shown in Fig. 1 to make clear what Fig. 1 is showing.
9. **P. 7, Line 191:** Change “of 23 AWS” to “from 23 AWS”
10. **P. 7, Line 192:** Change “output” to “outputs”.
11. **P. 7, Line 194:** Add “and” after “10-m wind speed,”
12. **P. 7, Line 201:** change “with a small negative bias” to “with the model exhibiting a small negative bias” for clarity.
13. **P. 8, Line 231 and Line 239:** Change “too low cloud cover” to “underestimated cloud cover”
14. **P. 8, Line 241:** The van den Broeke (2008) reference seems to be missing from the reference list.
15. **P. 9, Line 258:** Change “too large SHF” to “SHF to be overestimated”
16. **P. 10, Lines 295 – 298:** The language could be improved here. Suggested revision: “In Section 3, we discussed the overall good ability of RACMO2.3p2 to reproduce the contemporary climate of the GrIS, which is essential for estimating realistic SMB patterns. Here we compare SMB from RACMO2.3p2 and RACMO2.3p1 over the GrIS. For further evaluation, we focus on three regions where there are large differences in SMB between the two versions.”
17. **P. 11, Line 318:** Change “very GrIS margins” to “extreme margins of the GrIS”
18. **P. 12, Lines 360-362:** This sentence is a bit wordy... suggest changing “decreasing the bias...” to “decreasing the bias by 260 mm w. e. yr⁻¹ to -40 mm w. e. yr⁻¹ and the RMSE by 200 mm w. e. yr⁻¹ to 210 mm w. e. yr⁻¹.”

19. **P. 12, Line 378:** Change “3 months” to “3 month”
20. **P. 14, Line 426:** Add “an” before “11 km resolution”
21. **P. 14, Line 434:** change “narrow ablation zones” to “the narrow ablation zone”.
22. **P. 14, Line 444:** Change “to capture” to “to capturing”.
23. **P. 15, Line 455:** Change “cryoconites” to “cryoconite”.
24. **P. 15, Line 467:** Change “proves to accurately capture” to “accurately captures”
25. **Figure 2:** Although it is not necessary since the caption provides a description, a legend on Fig. 2a would be useful for the reader.
26. **Figure 5:** The dashed lines on the legend are hard to distinguish from solid lines.
27. **Figure 9:** The black and blue colors are a bit hard to distinguish. Can the blue color be made slightly brighter? In caption, remove “the” in “for the S5”
28. **Figure 10:** The yellow line is difficult to see. The color could be made slightly darker. Add “and” after “a) daily snow albedo”
29. Fix references to follow format for *The Cryosphere*