

## ***Interactive comment on “Modelling the climate and surface mass balance of polar ice sheets using RACMO2, Part 1: Greenland (1958–2016)” by Brice Noël et al.***

**Anonymous Referee #2**

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This is a very nice well-written and detailed paper describing the revised RACMO2.3p2 model and the effect of improved model tuning on the Greenland ice sheet surface mass balance estimates. This kind of paper is extremely important for users of SMB data to read and digest in order to understand the likely biases and uncertainties within model output and the thorough analysis, while not really presenting much novel scientific research is an important addition to the scientific canon. It is very well structured and easy to read and the authors are to be congratulated on a thorough overview. That said I have some issues, which I feel should be addressed before final publication. Points for consideration

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1. In section 2.3 Model Updates, it is noted that there have been some changes to the cloud scheme but these are not discussed in much detail and it is not clear how the large adjustments to the llcrit in mixed phase and ice clouds were reached. This is a pretty serious adjustment of the model as many of the other model parameterisations in the radiation scheme are likely to be tuned to these kind of values, possibly giving erroneous results or different compensating biases. However little detail is given as to how or why the particular values for these adjustments were chosen. Nor are the effects of this adjustment alone described in any detail – for instance on lines 398 to 400 reference is made to a precipitation bias in the SE compared to observations, but it is not clear if this bias is reduced or increased from RACMO2.3p1 and if this is a result of the cloud scheme changes or for example the change in topography caused by moving to the GIMP DEM. More information on how this change in cloud parameterisations has altered precipitation in particular would be helpful, as the differences in the topography shown in figure 2 seem also to be related to the change in distribution of precipitation, at least in some locations as shown in figure 7.

2. On a similar theme, I note that the small improvement in LWd and SWd on the K-transect is reasonably attributed to the change in the cloud scheme. It would be very interesting to see if this improvement is consistent across Greenland at stations other than the K-transect. There is some reason to believe that western Greenland is often modelled well but in other regions RCMs do a less good job of reproducing observed climate variables, possibly due to biases in cloud schemes as well as the complex topography in other areas. As there is now a fairly large amount of data available from Promice stations it would be nice to see some geographical spread in the figures presented in figure 5 and tables 1-5, perhaps limited to maybe 3-4 extra stations in north, south and east Greenland to determine if the positive results from western Greenland are replicated elsewhere.

3. Upper atmosphere relaxation is mentioned on line 136-7 but no details are given. I would like to see this expanded with details on which fields are nudged and at which

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levels in the atmosphere as this is important for interpreting the atmospheric model output.

4. The authors acknowledge that boundary forcing is important for results (line 20) but the differences between results from ERA40 and ERA-Interim forced years are not explored at all. It would be helpful to have a time series of SMB and the components for the full 1958-2015 averaged over the full ice sheet for the full period. This would show if, for example, there is a marked change in precipitation or melt potentially resulting from the switch in boundary forcing in 1979 is visible in more detail. It would also give a better sense of the interannual and decadal scale variability in SMB of Greenland. Plotting these with model version p1masked with the same ice mask would also allow us to assess the differences in SMB over the full ice sheet that result from the improvements introduced here.

5. I am not quite clear if the improvements to the snow module are part of the online RACMO model or the offline firn model – I assume the former, but please clarify this in sections 2.1 and 2.3

6. On line 216 you note that AWS data is sometimes spurious, Ryan et al 2017 in GRL also showed that the siting of stations (for very good reasons!) also leads to spurious underestimation of albedo – this should probably be referenced.

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-201>, 2017.