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Interactive comment on "Modelling the climate and surface mass balance of polar ice sheets using RACMO2, part 2: Antarctica (1979–2016)" *by* J. Melchior van Wessem et al.

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The updated simulations presented in this paper are interesting and worth publishing. While in general, I would not support that a new paper is written after simply adjusting some model parameters, this paper has value because it clearly indicates, where current models need to be improved and where processes such as cloud dynamics are incompletely understood. The numbers presented are the current state of the art and the basis for other researchers working on Antarctic mass balance questions. Nonetheless, I would like to make some major suggestions to implement before publication. What is lacking in the paper is an in-depth discussion of the generality or universality

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of model assumptions and choices. For example, if RACMO were to be run over the Himalayas with the updated cloud parameterization, what would happen there? Also, the critical model parts need to be better described such that a self-contained paper results. In particular:

1) The main adjustment made to the critical cloud water and cloud ice content parameters appears massive and is not discussed in terms of what it means. While I can see that a full presentation of the cloud model is beyond the scope of the effort, to present the main features of the cloud model and the role of the parameters should become an (adequate) part of the methodological description.

2) Since some important improvements in the model results are caused by the newly introduced upper air relaxation process, this should also be introduced in a methodology section. In particular, if there are any interactions to expect from this assimilation with the cloud parameter changes, this should be discussed in detail.

3) A similar comment applies to the discussion of the drifting snow parameterization. While strict validation of the model is difficult, a more in-depth discussion of available measurements and model results is required. In particular, I would request that comparisons are made to the (limited but existing) Antarctic snow transport results from other groups (e.g. Amory et al., 2015 and references therein). It could be also helpful to discuss similar efforts made in Alpine terrain (Vionnet et al., 2014; Zwaaftink et al., 2013) and to comment on potential errors in the parameterization of drifting snow sublimation in light of newer results (e.g. Dai and Huang, 2014).

4) One other major request is related to data publishing: While the authors say that the model results are available, the observational data should also be published in the form they have been used for the validation. The value of the paper relies heavily on the comprehensive data collection the authors have been able to accomplish. It is very valuable for the scientific community to get these comprehensive validation data along with the paper. Otherwise the data collection effort has to be repeated by authors who

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want to do similar validation work but maybe with different models. I understand that some data sets may not be available for publication if the permission of the original data collector is not given. But I would expect that this is not the case for a majority of the data used for validation and thus those data sets can be made available on a common data platform (e.g. Pangea, NSIDC). Some of the data sets have been processed by the authors (e.g. the accumulation radar data if I understand correctly) and would not be available to the scientific community if not published along with the paper.

Detailed Comments: In general, form of presentation and language are very good and the paper reads very well. A few smaller things are noted below.

Figure 5 and corresponding discussion: Looks like the improvement in bias and RMSE is only because a few rather warm temperatures have been simulated better but this in a temperature range, in which the data show a lot of scatter. However, the new simulations clearly show a larger bias at low temperatures and this may be worth mentioning?

Can you give a reason why you do this elevation binning in Figure 8 instead of showing a map?

Section 3.5.4: Appears that you already start the comparison between coarser and higher-resolution model runs here; somewhat confusing since this is revisited in Section 4?

References: Amory, C., A. Trouvilliez, H. Gallee, V. Favier, F. Naaim-Bouvet, C. Genthon, C. Agosta, L. Piard, and H. Bellot (2015), Comparison between observed and simulated aeolian snow mass fluxes in Adelie Land, East Antarctica, Cryosphere, 9(4), 1373-1383, doi:10.5194/tc-9-1373-2015.

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