

## ***Interactive comment on “What is the Surface Mass Balance of Antarctica? An Intercomparison of Regional Climate Model Estimates” by Ruth Mottram et al.***

### **Anonymous Referee #2**

Received and published: 27 February 2020

#### Remarks to the Authors

Review of “What is the Surface Mass Balance of Antarctica? An Intercomparison of Regional Climate Model Estimates” by Ruth Mottram et al.

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#### General comments:

This paper presents results from a first intercomparison of polar regional climate models (RCM) applied in the Antarctic Ice Sheet (AIS). The model performances were

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compared and assessed in terms of surface pressure, near-surface air temperature, near-surface wind speed, surface temperature, and surface mass balance (SMB) of the AIS. The models that participate in this intercomparison project are COSMO-CLM2, HIRHAM5, MetUM, MAR, and RACMO. For some models, results from different versions are provided additionally.

My first honest impression after reading through this manuscript is that the current title “What is the Surface Mass Balance of Antarctica?” is a bit misleading, because meltwater runoff is not considered in the most participating models except for MAR and RACMO. It is true that a contribution by runoff to the changes in the present-day AIS SMB is relatively small than contributions from precipitation and sublimation/evaporation. But, runoff in the present-day AIS already cannot be neglected as presented by several studies cited in this manuscript. In the future in a warming world, the contribution by runoff to the changes in AIS SMB will become much higher almost certainly as pointed out by the authors (P. 2, L. 18 ~ 19). Therefore, this reviewer expected that all the models calculated runoff in the present study, and as a result, I was a bit disappointed when I found the relevant description in Sect. 2.2.1.

Related to the point indicated above, the intercomparison procedure for SMB sounds a bit inadequate to me, because the authors employ different definitions of SMB (Sect. 2.2.1). If the authors focus intercomparisons only for precipitation and sublimation/evaporation (in addition to the three surface meteorological properties as well as the surface temperature), it makes sense and highlights key differences in model physics employed by these participating models more clearly. This reviewer recommends the authors to reconsider the title of this manuscript: maybe something like “intercomparison of Antarctic ice sheet surface meteorological conditions simulated by five different regional climate models” would be appropriate.

However, the intercomparison of RCMs performed in the AIS itself is a considerable new challenge, so provides the latest comprehensive information related to these RCMs, which is very informative for readers certainly, so deserved to be published

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in the journal The Cryosphere.

In the following part, this reviewer gives specific comments to be considered before the publication. Please note that page and line numbers are denoted by “P” and “L”, respectively.

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#### Specific comments (major)

P. 2, L. 7 ~ 8: What kind of measurements do the authors think here (observational campaigns)? Maybe it is not necessary to indicate explicitly here; however, please suggest something at least in the discussion and/or conclusion sections.

P. 12, L. 2 ~ 3: What kind of physical mechanisms do the authors think here? Please detail more.

P. 12, L. 5: How large do the authors think the uncertainties are?

P. 12, L. 5 ~ 6: Readers cannot know the difference in turbulent heat schemes, because they are not described in this manuscript.

P. 24, L. 13 ~ 15: It is interesting to see the model-simulated precipitation integrated over the common ice sheet mask by COSMO-CLM2 tends to be lower than that by the parent data ERA. It is because, precipitation in a dynamically downscaled data is higher than precipitation in its parent data in general. Please discuss.

P. 29, L. 27: Please suggest what kind of measurements do the authors think necessary in the “observational campaigns”?

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#### Specific comments (minor)

P. 4, L. 13 ~ 18: Is it OK to understand MAR 3.6 is older than MAR v3.10? If yes, it is a bit confusing isn't it?

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P. 8, L. 6: What do the authors mean by “cloud physics”? To me, it is difficult to understand why “cloud physics” is resolved better in nudged models.

P. 12, L. 24 ~ 25: It is not clear why the authors think so. Please explain more.

P. 12, L. 31: “For the warmer costal regions”: From which data can we see this argument?

Figure 2: This figure is a bit difficult to see. Please provide a table indicating ME, RMSE, and R2.

P. 23, L. 4 ~ 7: What is an interesting point here? I don't think the lower panel of Fig. 8 is necessary; however, if the authors think it is necessary, please discuss more about the figure. Maybe, inter-annual variations of these model results should be discussed more.

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#### Technical corrections

P. 2, L. 1: “compar”: typo

P. 2, L. 11: “a potentially important potential contributor” -> “an important potential contributor”?

P. 2, L. 22: surface mass balance -> SMB; Note this term is already defined.

P. 3, L. 22 ~ 25: This sentence especially after “and better understand drive sea level rise ...” is a bit difficult to understand. Please reformulate it.

P. 5, L. 11: “regional mesoscale model e.g.” -> “regional mesoscale model as presented by e.g.”?

P. 7, L. 14: Define  $SU_{\{ds\}}$  and  $ER_{\{ds\}}$ .

P.9, L. 2: “GrIS”: typo, right?

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P. 10, L. 27: “assessing” -> “assess”

P. 12, L. 7: Indicate the publication year for Zentek and Heinemann.

P. 12, L. 25: “downwelling longwave and surface albedo” -> “downwelling longwave radiation flux and surface albedo”

P. 14, L. 18: “HIRHAM5.011”: typo

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