The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-76-RC2, 2018

The Cryosphere Discussions
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Interactive comment on "Estimation of the Antarctic surface mass balance using MAR (1979–2015) and identification of dominant processes" *by* Cécile Agosta et al.

Anonymous Referee #2

Received and published: 26 June 2018

Remarks to the Authors

Review of "Estimation of the Antarctic surface mass balance using MAR (1979-2015) and identification of dominant processes" by Cécile Agosta et al.

The Cryosphere Discuss. Manuscript Number: tc-2018-76

General comments:

This paper presents performance of the polar regional climate model MAR applied in the entire Antarctic Ice Sheet (AIS) for the first time. MAR has been applied and val-

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Discussion paper

idated in the Greenland ice sheet (GrIS) for a long time, and it is widely recognized as a useful and reliable tool to understand the polar climate system. In the present study, the authors follow basically the same MAR model configuration developed in the GrIS. In addition, they decrease horizontal and vertical resolutions (due to the AIS's much larger area than the GrIS), use a boundary relaxation of upper air temperature and wind speed, employ an optimized fresh snow density parameterization for the AIS, and utilize a dynamic parameterization for the aerodynamic roughness length. This reviewer finds that these modifications are reasonable to conduct this kind of study. The model forced by the European Centre for Medium-Range Weather Forecasts (ECMWF) Interim reanalysis (ERA-Interim) is evaluated in terms of surface mass balance (SMB) using the in-situ data obtained during 1979-2015. In this process, the authors also refer to model simulation results by another polar regional climate model known as RACMO2 (horizontal resolution is 27 km) forced by the same reanalysis data to identify important physical processes that influences the AIS SMB simulations. The authors find that both models tend to accumulate too much snow on crests, whereas not enough snow in valleys. Here, the authors attribute the main reason for this discrepancy to the insufficiency of drifting snow-induced erosion-deposition process modeling in both models. When calculated SMBs by MAR and RACMO2 are integrated over the AIS, no significant differences are found between these two results; however, geographical SMB patterns for both models differ significantly, which suggest that there are many things to do to develop a truly reliable polar regional climate model for the AIS. In valleys, RACMO2-simulated precipitation is larger than that by MAR: it is mainly attributed to a difference in modeling approach for sublimation in unsaturated katabatic layer. On the other hand, larger precipitation in the inland AIS is simulated by MAR, because of the difference in horizontal resolution set in both models, which significantly affect orographic impacts on the simulated precipitation rates.

Overall, this paper is well written and can be informative for readers who are interested in the AIS climate system; however, this reviewer thinks that some discussions are not deepened sufficiently and suggests the following points to be considered before the

publication. In the following part, this reviewer gives specific comments. Page and line numbers are denoted by “P” and “L”, respectively.

Specific comments (major)

P. 9, L. 3: What do the authors mean by “shift” mentioned here? Why is this procedure needed here? Please explain more about the procedure.

P. 11, L. 1.: The authors mention that near surface atmosphere is simulated to be drier in MAR compared to RACMO2. How large is the difference? Please quantify and discuss why the difference was made.

Sect. 3.3: Using MAR, can the authors perform a model sensitivity test where sublimation in unsaturated katabatic layer is not allowed? If results from this sensitivity test are provided, the argument by the authors in this section would become more convincing.

Sects. 3.3 and 3.4: In Sect. 3.4, the authors point out the importance of orographic effects on the precipitation simulations in areas centered on crests. It is interesting the authors don’t mention orographic effects on the precipitation simulations at valleys (Sect. 3.3). Do the authors think that considering the process for the low-level sublimation in unsaturated atmosphere (at especially valleys) is more important than setting a higher horizontal resolution to obtain realistic SMB at valleys by a model?

P. 13, L. 33: Can the authors perform a MAR model sensitivity test where the horizontal resolution is set to be 27 km (same as RACMO2) or higher? I know it is computationally demanding, but, results from such a sensitivity test for even only several years would be informative for readers.

Specific comments (minor)

P. 2, L. 4: Regarding the “several approaches”, please list up and explain these ap-

proaches briefly here. I believe the information are very informative for readers.

P. 3, L. 18: Why did the authors set the horizontal resolution to be 35 km for MAR in the present study? To perform detailed and solid comparisons between MAR and RACMO2, setting the same horizontal resolution is very ideal.

P. 5, L. 10: Figure 1 basically presents simulation results from MAR, therefore, referring Fig. 1 in this sentence is a bit strange (MAR simulation results don't reproduce the reality, although I agree it certainly does a good job.).

P. 6, L. 8 $\sim$ 10: I could not follow the explanation here. Could you please detail more?

P. 6, L. 23: For me, it is not easy to understand the authors' intension regarding "oscillates" mentioned here. Could you please reformulate it?

P. 6, L. 23 $\sim$ 24: In Sect. 3, the authors present the performance of modeled SMB by MAR. They also perform detailed comparisons between simulation results from MAR and RACMO2. In this context, I think it is better to denote the performance of RACMO2 in terms of SMB here in the same manner as MAR (please indicate mean bias and RMSE for RACMO2).

P. 7, L. 10: It is not easy to understand the meaning of "oscillations" mentioned here. Could you please rephrase it?

P. 13, L. 4 $\sim$ 14: Do the authors mean that the MAR-simulated precipitation at valleys is more realistic compared to the RACMO2-simulated precipitation at valleys? Please describe more clearly.

P. 14, L. 3: "wind glaze area": Please detail more about its definition here.

Technical corrections:

Figure 1: Please explain red circles in Figs. 1a to 1c in the caption. It is also the case

for Figs. 4b and 4c.

P. 9, L. 5: “wind speed” -> “10 m wind speed”?

P. 13, L. 22 $\sim$ 23: In Fig 5b, no description on the altitude of the AIS is provided. Please check it again and revise it.

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