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Interactive comment on "Updated cloud physics improve the modelled near surface climate of Antarctica of a regional atmospheric climate model" *by* J. M. van Wessem et al.

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Summary

RC: This is a well-written article assessing the changes in RACMO2 surface fields over Antarctica after a major update of its atmospheric physics package. figures, tables and legends are clear and precise. It shows that the major changes in RACMO results concern downward longwave radiative flux and the sensitive heat flux, inducing reduced negative biasses in surface snow temperature compared with the previous RACMO version. Other components of the surface energy balance remain almost unchanged, together with surface wind fields. Contribution of the work RACMO2 has been widely

used and evaluated for polar climate, and its results are recognised by the scientific community to be very valuable, with a wide range of applications. It is thus of importance to be informed of the latest updates of the model in order to inform the community of the corrected and remaining biasses of the model and the possible consequences on the conclusions of previous studies. However, I detected several major issues when reviewing the manuscript. These are outlined and explained below. In general, I would recommend publication after major revisions have been performed.

AC: We thank the referee for the clear, helpful and detailed review. We will address all mentioned points one by one in the document below. The revised manuscript is added as a supplement.

Major comments

RC: 1. Observation dataset (sections 2.2, 3.2 and 3.3) [1.1] Explain why you chose these 9 AWS.

AC: We added a sentence (p3236,l2) explaining why: "These AWSs were selected because they measure all four radiation components as well as humidity, and therefore enable a reliable closure of the SEB. "

RC: [1.2] For V10m and T2m, you can use the full READER dataset. It is more representative of the whole ice-sheet.

AC: We have looked at the READER dataset but do not feel the need of showing these results, as is noted in p3239,l9, because the results give the same conclusions as the 9 AWSs do. This way we keep the analysis transparent and consistent throughout the entire manuscript.

RC: [1.3] For the radiative fluxes, is there no other AWS data available elsewhere in Antarctica ?

AC: Only a few other AWSs measure radiation, but often only one component directly and with numerous data gaps. As a result, they are not suitable for this study.

RC: [1.4] Your analysis on AWS 4, 5, 6 and 9 is interesting as it allows to better understand the biasses in function of the local settings. However, in section 3.2 and 3.3, you mix the interpretation of biasses for all AWS with those of the 4 AWS, which can be confusing. For example : - in section 3.2, (p3239,l6-8) : ÂńThis improvement occurs year-round for the AWSs except for AWS 4, where the representation was already good due to the overestimated wind speed.Âż > We don't know if ÂńexceptÂż concerns only AWS 4, 5, 6 and 9 or all the AWSs - in section 3.3, (p3240,l8), I guess that Âńall siteÂż should be changed by Âńthe 4 sitesÂż (?) Please separate more clearly the conclusions obtained by analysing the 4 AWSs from the conclusions obtained by analysing the whole dataset.

AC: We agree that it might be confusing in certain parts and have corrected both suggested sentences (and other parts of the text) where necessary.

RC: 2. Organisation of the results (section 3)

As model updates are on atmospheric physics, it would be more logical to: (i) present the atmospheric changes between the new and old version, without comparison with observation (cloud cover, downward radiative fluxes, temperature, wind, humidity, precipitation), as for fig. 9 (ii) present their impact on the modelled surface climate, including the comparison with observations : surface energy balance (surface radiative fluxes, surface turbulent fluxes), surface temperature, and SMB (missing in this version of the manuscript).

AC: We reorganized the manuscript as suggested. Furthermore, we have not included the SMB (and precipitation) analysis in the manuscript as we believe it to be beyond the scope of this study and we wanted to solely focus on the SEB.

RC: Concerning the atmospheric changes : [2.1] You present LWnet and SWnet, but as changes are in the atmospheric physics and not in the snow physics, it would be more interesting to see the changes in downward radiative fluxes instead of net radiative fluxes.

AC: We have added LWdown and SWdown to Fig. 6 and included a new analysis in the text, and substituted LWnet in Fig 9 (which is now moved to section 3.2) with LWdown.

RC: Concerning the surface climate : [2.2] As you show that surface wind is almost unchanged with the update, you can remove the comparison with observations for this field and concentrate on the fields which are significantly affected by the update.

AC: Agreed, but we prefer to keep the comparison in the manuscript as the seasonality in the wind speed bias is extensively used in other parts of the manuscript.

RC: [2.3] I insist that you should present the impact of the update on the simulated SMB, since it is one of the major applications of the RACMO2 model. You say in section 2.1, (p3234,l26-28) that the update may have an impact on precipitation but you don't show results related to this statement.

AC: To retain the manuscripts focus on near surface climate, we decided not to address the SMB in this manuscript, but will do in more detail in a forthcoming manuscript. To that end, we have also removed the part about precipitation in section 2.1, which is not important for the results presented in the manuscript.

RC: [2.4] You say (p3236,l12) that LWd and SWd are measured. It would be more interesting to compare these fields than the net radiative fluxes with observations (see comment [2.1]).

AC: See answer to previous comments.

Minor comments

RC: a. Introduction (section 1) a1. Put more emphasis on applications of the RACMO2 model for Antarctica by adding more references showing that RACMO2 is a reference model to study the Antarctic climate. This will highlight the interest of presenting the new updates and their consequences on RACMO2 results (which are the fields of interest in RACMO studies ? Can the conclusion of previous studies be modified by the update ?)

AC: We added an additional RCM (MAR (Fettweis 2007)) and RACMO for Greenland ,(Ettema 2010)) to the introduction, to put more emphasis on regional climate models being used for research of the polar regions, as well as a rephrased explanation of Antarctic applications. See lines 22-30 (first paragraph introduction) of revised version of manuscript.

RC: a2. first sentence of the introduction : How can we “improve our understanding of atmospheric processes” in “areas where few observational data are available” ? Does it mean that we improve our understanding based only on what models predict ? What can be our confidence on model outputs without a thorough model evaluation ?

AC: What is meant is that observations are basically used to verify model predictions, which are then used to extend this knowledge to where no comparison with observations can be done. We rephrased this part of the introduction (See lines 22-30).

RC: b. figures b1. I suggest to show biasses (model-obs.) as a function of the surface elevation (or better but a little more complicated : of the distance from the cost) for an easier interpretation of the plots (Fig 3, 5 and 7)

AC: While this would be a feasible thing to do for the numerous 10 m. snow temperature data, this cannot be done for the scarce AWS sites. We are of the opinion that by showing the evaluation of the 4 AWSs separately, we have demonstrated and explained the effects of surface elevation. As a result, we would like to keep Figure 7 as is.

RC: b2. Section 3.3, (p3240,l12-20) : the explanation is difficult to follow. It will certainly be clearer with the figures changed as suggested above.

AC: See our answers to previous comments. By including the three main zones in Fig. 7 we think that the elevation and continentality dependence have been clearly outlined.

RC: c. Statistics c1. Add at least the rmse, as the correlation coefficient r does not indicate which of the newer or elder modelled data are closer to the observed data.

AC: We have added rmsd and standard deviation of the bias to Table 2 and removed

the mean of the model (mean of the Obs and bias provides sufficient information).

RC: c2. Use r squared in place of r, as r2 can be interpreted as the proportion of response variation explained by the regressor in the linear model.

AC: Changed.

RC: c3. Add the number of points used in the statistic in the figures.

AC: We added the total and available (used) amount of months to the two tables. For all statistics all months are used, the available amount of good quality months is only provided to give a sense of data quality.

RC: c4. Add statistical tests to assess whether the differences in mean biasses are significant or not.

AC: We added the standard deviation sigma_bias in Table 2 and included it in the text. This shows the statistical insignificance of the improved bias, a direct result of the noise in the data comparison. However, by discussing the physical principles behind the change in bias, we feel that the model improvement is clearly supported.

RC: d. (p3233, l14) : tem perature > temperature e. (p3233) : change the title : “2.1 RACMO2” by something more explicit like “2.1 RACMO2 physics update” f. (p3235, l2 and l6) : “short-“ > “shortwave” g. (p3238) : Title “Simulation of wind speed ...” > “Simulation of near-surface wind speed ...”

AC: All corrected as suggested.

RC: h. (p3241,l4-14) : explain at the beginning of the paragraph why you show the potential temperature (“to compensate for elevation differences ...”).

AC: Based on the other review we have decided to remove these figures from the manuscript.

RC: i. (p3243,l3-4) , fig10 : “averaged over the period 2007–2010 (representative for

the entire simulation)" > Why don't you average over the full period (1979-2010) ?

AC: This timespan overlaps with the CALIPSO satellite timespan, which we performed a comparison with. However, this analysis was omitted in the manuscript due to problems with the interpretation of the data, since both methods (model and obs) provide different cloud cover/content indications. The full timespan would show the same plot (we confirmed this by looking at a lower resolution cross-section) and decided to keep the figure as is.

RC: j. (p3242,l15-18) : Are the changes in SWdown and SWup significant between the 2 versions of RACMO at the scale of the ice-sheet ?

AC: The changes in SWdown are fairly insignificant for East Antarctica ($\sim$2%) but are significant for the coastal margins (and West Antarctica) (which can now be seen in Figure 6) where SWdown is now overestimated more. We have included an analysis of this result in the text.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/7/C1918/2013/tcd-7-C1918-2013-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 7, 3231, 2013.