Manuscript prepared for The Cryosphere
with version 4.2 of the LaTeX class copernicus.cls.
Date: 30 September 2013

Updated cloud physics improve the modelled near surface climate of Antarctica of a regional atmospheric climate model

J.M. van Wessem et al.[1]

[1]Institute for Marine and Atmospheric Research Utrecht, Utrecht University, Utrecht, Netherlands

Correspondence to: J.M. van Wessem (j.m.vanwessem@uu.nl)

General comments RC: The paper describes the effect of the implementation of a new physics package on the performance of the RACMO regional model in Antarctica, with a focus on the surface climate. This physics update results in increased moisture and clouds over the Antarctic continent, which has a positive impact on the surface radiation budget and surface temperature but little effect on the surface wind field. While not particularly original in its design, the study certainly fits well within the scope of The Cryosphere. It is in my view worth publishing because 1) it sheds further light on the skill of a regional model used in many prominent Antarctic studies; and 2) global models and even regional models still often struggle to properly reproduce some fundamental aspects of Antarctic climate.

Therefore, anything that can be learned about the model physics most appropriate for Antarctica is of interest to the Antarctic modeling community. I found the analysis sound and well supported by tables and figures (Fig. 8 being one exception). The physical processes are properly described. The text is overall well written although its clarity could be enhanced in a number of places. My three most important concerns (why I am asking for major revisions) have to do with the structure of the manuscript, the method used to select the model data, and the excessively short introduction. Details about these concerns (and a few others) are given below, followed by a list of more minor corrections. Recommendation: Publish with major revisions.

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.

Specific comments RC: 1. Overall structure of the paper: My first comment is about the logical organization of the manuscript, which is also reflected in the abstract and the conclusion. Since the study focuses primarily on the effects of the changes in the model cloud physics, the first thing I would expect to see, right after (or perhaps as part of) the description of the model and its physics update, is the actual impact on the model clouds (what currently makes up the first part of the Discussion). What should logically follows is a description of the resulting effects on the surface radiation budget. Then and only then should the effects on the wind and temperature be discussed. In addition, it would make more sense to combine "temperature" (end of section 3.2) and "3.4 Spatial variations in Ts" in the same section, or at least discuss them in two consecutive sections. Finally, the "Discussion" section certainly doesn't look like a discussion but rather a mislabeled integral part of the results. As I suggest above, consider moving the text dealing with the clouds to the beginning of the manuscript and placing the remaining text on effect of the changes in the surface boundary layer scheme under a new section.

AC: First: We agree and changed the organization of the manuscript as suggested by the reviewer. We have also added Figure 8 (and the corresponding text) to section 3.2 (about cloud changes), as suggested by the other anonymous referee.

Second: We agree that the Discussion section is mislabeled, also after the reorganization. We have retitled it: "Impact on SHF regimes".

RC: 2. Abstract: My first recommendation is not to start the description of the results in the abstract with "Significant biases remain". I would expect to find this statement near the end of the abstract, something like "Significant model biases remain, however,..." followed by a sentence or two discussing the issues not addressed with the new physics package. Also, along the same line as my comments about the overall structure of the paper, I suggest moving the sentence describing the effect of the physics update on the moisture and clouds to near the beginning of the abstract.

AC: We have moved "significant biases remain" to the end of the abstract and added the following sentence: "However, significant model biases remain, partly because RACMO2 at a resolution of 27 km is unable to resolve steep topography."

RC: 3. Introduction: The first sentence is obviously quite general and can apply to about any atmospheric model (global or regional). Consider revising it. In the text that follows, all publications refer to work done exclusively with RACMO. If the authors want to keep the scope of the first paragraph general, I suggest including at least a few references to studies that are based on other regional models. Another option is to introduce RACMO earlier on in the text and consider the references currently listed in the first paragraph as applications of RACMO. One other deficiency of the introduction is the utter lack of background about the strengths/weaknesses of RACMO in Antarctica. What were some known issues that could be (could have been) addressed with the new physics package? What were the authors' expectations before conducting the study?

AC: First: We have rephrased the first sentences as follows: "Regional atmospheric climate models

(RCMs) are important tools to improve our understanding of atmospheric processes and their relation to climate change. They provide a physically coherent representation of the climate in areas with a low spatial and temporal coverage of observations. RCMs are also capable of resolving detailed features that are not captured by global circulation models (GCMs). ”
Second: We have added a reference to the MAR model (Fettweis 2007) used for Greenland (and recently for Antarctica as well) and a reference to RACMO2 used for Greenland (Ettema2010).
Third: We have added a paragraph to the introduction explaining the model deficiencies we hoped the model update would solve.

RC: 4. Parameterization of autoconversion (p. 3234 4th paragraph): First, I don’t find the change in the parameterization of *convective* clouds very relevant to Antarctic climate, given that convection is a rare phenomenon in Antarctica. Is it really worth mentioning this among ”the updates that have the most impact on Antarctic applications”? When comparing the IFS documentations (Part IV: Physical Processes) for CY23r4 and CY33r1, I noticed that some text about the parameterization of ice-snow autoconversion was added in the more recent version of the document. A 2006 ECMWF Progress Report (http://www.wmo.int/pages/prog/www/DPFS/ProgressReports/2006/ECMWF.pdf) also states that ”a new autoconversion parameterization was added to convert ice to snow”, along with a new parameterization of supersaturation. This, in my view, looks more relevant to Antarctica. Using the equation for the ice-snow autoconversion coefficient, c0, listed on p. 90 of the CY33r1 Physics Processes documentation, I found that this coefficient decreases with the temperature: 0.001 at 0 degC, 0.0006 at -20 degC, and 0.0004 at -40 degC. So my question is: has the use of this new parameterization resulted in a *decrease* in the autoconversion in Antarctica, thus quite the opposite of the increase mentioned in the manuscript? Some clarification would be appreciated.
AC: First: Since we decided not to discuss SMB (and precipitation) in this manuscript, it is no longer necessary to describe the ice to snow conversion. Second: We agree that the explanation in the manuscript was not clear, as it involves more than just one coefficient. Due to both reasons we have therefore removed the part about the convection and auto conversion and have only kept the part about the super-saturation in the manuscript.

RC: 5. Model versus AWS comparison: First, the manuscript states that ”the [AWS] datasets differ in quality, due to instrumental problems”, without further explanations, which makes one wonder whether some of the AWS records used for the model evaluation may be unreliable. Please clarify. As Table 1 shows, for some AWSs, the model elevation differs quite significantly (by 100m or more) from the actual, observed elevation. As does the slope. As I understand, the model data are taken from the grid point nearest to the observations (p. 3237 l. 4) and are not adjusted for model versus observed differences in elevation or slope. This, in my view, weakens the results of the evaluation.

I suggest including some kind of adjustment of the model data, be it a correction of the temperature assuming a certain lapse rate, or a selection of the most optimal grid point rather than the nearest one. For example, Reijmer et al. (2005) used ”the closest grid point with a reasonable correspondence in elevation and slope is chosen for the comparison with observations instead of the grid point closest to the observation site”. In several places, the authors invoke the overestimated or underestimated slope as the main reason for the model bias. This implies that a higher-resolution version of RACMO (with everything else unchanged) would exhibit smaller biases. Is there any evidence to support this?

AC: First: We have changed the part about the AWS' reliability : ”Due to instrumental problems and icing of the sensors some months of the data are of lower quality”

Second: We fully agree that an adjustment can be made to account for the differences in elevation and slope. However, we decided not to perform this correction, since the main comparison here is done between RACMO2.3 and RACMO2.1, both suffering from the same differences in elevation and slope.

Third: Yes, we expect that a higher resolution simulation will have smaller biases in slope, elevation and climate. (see J. T. M. Lenaerts, M. R. van den Broeke, C. Scarchilli and C. Agosta, 2012: Impact of model resolution on simulated wind, drifting snow and surface mass balance in Terre Adlie, East Antarctica. J. Glac., 58, 211, 821-829, doi:10.3189/2012JoG12J020)

RC: 6. Correlations: Is the annual cycle removed before calculating the correlation coef- ficients shown in Table 2? There is no mention of it in the manuscript so I tend to think that it isn't. As a result, the very high correlation (and significance level) between model estimates and observations for certain variables may simply reflect the fact that RACMO is capturing the annual cycle well. Please clarify and change the correlation calculation if necessary.

AC: It is nearly impossible to remove the annual annual cycle from our data because some months are lacking and not all data-sets start at the beginning of the year (or end at the end of the year). Moreover, the manuscript is about an intercomparison of RACMO2.3 with RACMO2.1 and not primarily about the actual performance of RACMO2. We have changed all correlation coefficients to r^2 and have added standard deviation and root-mean-square difference to Table 2 to make the statistics more complete and to give an indication of the significance of the results.

RC: 7. Merits of Figure 8: I don't find these two maps (8a and 8b) paramount to the paper. They add very little, if anything, to Fig. 7. Looking at the maps, I also find it virtually impossible to tell the extent to which the model agrees with/differs from the observations or, for example, to verify that ”in West Antarctica and the coastal margins, Ts is underestimated the most”.

AC: Fig. 8 was originally included to give the reader a general overview of how temperature varies over the AIS; we have now removed this figure, as we believe that the text already sufficiently explains the temperature patterns.

RC: 8. Changes in precipitation/SMB: Given the importance of RACMO's estimates of Antarctic snowfall/SMB in the recent literature, I suggest adding one section assessing the effects of the new physics update on this variable. However, I would understand if the authors were to consider this topic as beyond the scope of their study.
AC: We consider this topic to be beyond the scope of this study and would reduce its focus. In a forthcoming paper we will describe a thorough and extensive analysis of the SMB.

Minor corrections:
RC: p. 3232 l. 3: Consider "consists of" (or equivalent) instead of "constitutes".
AC: Corrected.

RC: p. 3232 l. 4-5: Isn't "the inclusion of a parameterization for cloud ice supersaturation" implicitly included in "a changed cloud scheme", thus making the text redundant?
AC: Corrected to: "The update primarily consists of an improved turbulent and radiative flux scheme and a changed cloud scheme that includes a parameterization for ice cloud super-saturation."

RC: p. 3232 l. 26: Consider "in combination with" instead of "to support".
AC: Corrected.

RC: p. 3233 l. 3: Despite the title of the paper by Shepherd et al., their mass balance estimates were *not* reconciled. I would describe the study as "a synthesis of mass balance estimates".
AC: Corrected.

RC: p. 3233 l. 5: Change "that" to "which".
AC: Corrected.

RC: p. 3234 l. 2: "the significant complexity of the Antarctic climate" sounds a little overstated (one can actually debate whether the climate of Antarctica is more complex than that of other (vegetated) regions of the world). I suggest removing the portion of sentence starting with "in order to".
AC: Corrected.

RC: p. 3234 l. 21: Consider "better global distribution" or "improves the global distribution".
AC: Corrected.

RC: p. 3234-3235: Be consistent in the spelling of updraught (Brit) / updraft (US).
AC: Corrected in respective places.

RC: p. 3235 l. 18: Move ”Especially for...” to the end of the sentence.
AC: Corrected by moving ”especially” to the end of the sentence but not ”for”.

RC: p. 3236 l. 27: Consider ”a small part/limited sector of East Antarctica”.
AC: Changed to ”limited part of Antarctica”, as the core measurements extend it to (a limited) part of all of Antarctica.

RC: p. 3237 1st paragraph: Consider the following changes: ”net radiation budget” rather than ”radiation budget”; ”prevails” rather than ”dominates the SEB”; ”is balanced” rather than ”has to be balanced”; ”low humidity” rather than ”low temperatures”
AC: Corrected.

RC: p. 3237 l. 22: Consider ”interactions” or ”interactive processes” (or equivalent) rather than ”chain of events”.
AC: Corrected to ”To illustrate these interactive processes”

RC: p. 3238 l. 2: Consider ”upward longwave emission” rather than ”longwave cooling”.
AC: Corrected to ”upward longwave radiation”

RC: p. 3238 l. 9: Figs 3a and 3c do *not* show correlations. They show model values plotted against observed values. Also, a reference to Table 2 only appears in Section 3.4. This table is obviously also relevant to Sections 3.2 and 3.3 and therefore could be introduced earlier.
AC: Corrected and we have added an extra sentence in section 3.2: ”The figure also shows correlation coefficient r^2 and average bias b, also denoted in Table 2. ”

RC: p. 3238 l. 14: First, this part of the sentence is grammatically incorrect. Second, the only statement about the model surface wind field in Lenaerts et al. (2012b) is that ”Lenaerts et al. [2012a] showed that RACMO2.1/ANT is capable of realistically simulating the near-surface temperature and wind climate of the AIS”. Lenaerts et al. (2012a) themselves do not actually evaluate RACMO surface wind field against observations. To my knowledge, such evaluation was only done by Reijmer et al. (2005).
AC: Corrected and removed the part where we referenced Lenaerts 2012b as the reader can make his/her own conclusion about how well the model generally resolves near surface winds.

RC: p. 3239 l. 1: It turns out that, unless you are referring specifically to AWS 9 in RACMO2.3, the biases in Ts and T2m are not necessarily ¿0 when and where the temperature inversion is underestimated by the model. Therefore, saying that ”the bias in Ts is more positive than the bias in T2m” is incorrect.

AC: We have rephrased the sentence: ”The surface temperature inversion, defined here as Tinv = T2 m ? Ts , is underestimated when wind speed is overestimated (AWS 4 and 9) (Fig. 8d), which is intuitively expected.”

RC: p. 3239 l. 9: The proper reference for READER is: Turner, J. et al., 2004: The SCAR READER Project: Toward a High-Quality Database of Mean Antarctic Meteorological Observations. J. Climate, 17, 2890-2898.

AC: Corrected.

RC: p. 3239 l. 17: ”representation” is vague. Consider ”but the correlation remains high”.

AC: Corrected.

RC: p. 3239 l. 25: Consider ”is responsible for” rather than ”triggers”.

AC: Corrected.

p. 3242 3rd paragraph: Change ”three regimes” to ”four regimes” (l. 23) and consider renaming the regimes, starting with regime I and ending with regime IV.

AC: Corrected.

RC: Tabl2, caption: I assume that the significance level refers to that of the correlation coefficients. Please clarify.

AC: That’s correct and corrected.

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