**The Cryosphere
Discussions**

Interactive comment on "Climate of the Greenland ice sheet using a high-resolution climate model – Part 1: Evaluation" *by* J. Ettema et al.

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Reply to the reviewers' comments on our manuscript:

"Climate of the Greenland ice sheet using a high-resolution climate model – Part 1: Evaluation"

First of all, we would like to thank Gudfinna Adalgeirsdottir and Xavier Fettweis for their constructive and valuable comments, which will certainly improve the manuscript. In this response we address their comments point by point.

Answers to comments of referee 1: G. Adalgeirsdottir

1. I find missing, however, discussion and presentation of temporal evolution of the

variables discussed in the paper. We choose deliberately not to discuss the temporal evolution of the variables over the 51-year period, but focus this manuscript on the comparison of model output with in-situ observations. In our opinion, including the temporal evolution of these near-surface variables would make this manuscript too lengthy and less focused. We tend to write a separate paper on the temporal variability and evolution of lower atmosphere over the period 1958-2008. The spatial distribution of the temporally averaged variables is discussed in the second part of this tandem paper.

2. Is the model evolution similar as the available data? The first few decades of the model simulation cannot be evaluated due to the lack of observations. The dataset of the DMI stations starts in 1973 to 2005, GC-net data covers the period 1998-2001, and the K-transect data is available for the period August 2003 – August 2007. This meta-information will be added to Section 3. For the period 1973-2005, we added a comparison of measured annual mean temperatures at a few DMI stations with the model output in Section 4.1. The modeled temporal evolution over this period is in good agreement with the observed evolution. The biases found are constant in time, which confirms that the model is not drifting.

3. Can the model simulate the observed variability? See reply to comment nr 7 of Referee 2, where we discuss the findings on interannual variability or standard deviation in temperature.

4. Is there a climate signal in the model similar to the observations? Discussing the modeled and observed climate signal would require including a description of climatic changes over the ice sheet and thus a refocus of this manuscript. We intend to write a separate paper on potential climate signals in the 51-year model simulation in the near-future. Here, we have added a sentence to the conclusions that RACMO is a good and valid tool to study the recent climate changes over Greenland.

5. Specific comments

a. The paper presents a validation of the model rather than evaluation Based on definition of both words, this paper should be considered as evaluation. As Orekes (1998, Science, 263, p 641-646) stated "Verification or validation of numerical models of natural systems is impossible." and "Models can only be evaluated in relative terms, and their predictive value is always open to question."

b. Consistency in writing the name of the model The regional climate model RACMO2 is also used for other climate studies over Antarctica and Europe, so we refer to the basic model as RACMO2. RACMO2/GR refers to the model version including the described snow model which was especially developed for modeling the Greenland atmosphere and snow pack. This naming is now consistent throughout the manuscript.

c. Should solar radiation be replaced with shortwave radiation? All shortwave radiation is solar radiation but not all solar radiation is shortwave radiation. For consistency we replaced all solar radiation in the text by shortwave radiation. We have extended the definition of the abbreviations SWnet, SWâĘŞ, SWÂňâĘŚ, LWnet, LWÂňâĘŚ and LWâĘŞ in the text companying Equation 3. We made sure that they are used consistently throughout the manuscript, including the figures and captions.

d. The use of capital letters in Greenland Ice Sheet, GC-Net and van den Broeke (and other Dutch names) We used capital letters according to the English grammar rules. In case of the Dutch names of persons, we adopted their names as they are stated on the papers referring to.

e. The model initialization is not clearly described We rewrote this paragraph taken these comments into account.

f. It seems that two different model runs are used to create ignition conditions for SIF on one hand and for firn initialization on the other hand. Why not use the same source for initial values used in both cases? We choose to use data from two earlier runs of different models to initialize the 51 year simulation, because both individual model runs were inadequate to give all initial snow conditions realistically. We would have liked to

use the snow/firn profiles from an earlier 16 year integration of RACMO2, which was initialized by the offline simulation with the snow model. Due to progressive knowledge of the snow model configuration, e.g. optimal thickness and pore capacity, data from the 16-year run could not be used for setting all initial snow/firn conditions, nor for publication. Because RACMO2 is computationally very demanding, we decided to use the offline model of Bougamont et al (2005) to compute realistic vertical density and thickness profiles. As this offline model does not allow interaction with the overlying atmosphere, the resulting firn temperatures and SIF or melt rate were not of satisfactory quality. Therefore we decided to use Reeh (1991) for temperature profiles and to rerun RACMO2/GR 3 times the same year (September 1957-September 1958) to check for any spin-up problems. We have added this information to the manuscript.

g. The GC-net have been operating after 2001, why do you not use more recent data? Over the period 1998-2001 this dataset is of a high quality (Box and Rinke 2003) and quality checked (Jason Box, personal communication); biases are removed and necessary corrections were applied. The latter is a requirement before using measurement data for model evaluation. . We decided not to extend the GC-net and DMI datasets, as the quality of this later data could not be guaranteed.

h. Can you give an estimate of the errors involved in the 2m T transformation? The T2m in the model output does not come with any error estimate. The accuracy of measured temperatures at approx. 2 and 6 meter height at K-transect is 0.3ïĆřC as stated by Van den Broeke et al (2008c). The transformation to 2 m will involve some errors. As the transformation is only done when both temperature measurements were available and by applying the bulk method, errors are reduced in the transformation as much as possible.

6. Technical corrections The proposed technical corrections have been changed in the text.

a. From which data or observation are the open sea surface temperature and sea ice

fraction, ERA-40 as well? Yes, we have added this information to the manuscript

b. How can shelf ice and multi-year sea ice impact the ice sheet area? The ice sheet mask is used of Bamber et al (2001), which is kept constant during the model simulation. In the sea ice data field from ERA-40 no distinction is made between ice shelves, one-year sea ice or multi-year sea ice.

c. How is the roughness length over bare ice changed and what is changed? We realized that this suggested bare ice roughness length is not incorporated in the model version for the 51-year simulation. We removed the sentence referring to this adjustment.

d. Is the cloud correction applied in the RACMO2/GR? Yes. This is added in the manuscript.

e. How many layers are in the snow model? We have added 'composed of a maximum of 100 layers' to line 1 at page 567.

f. Is the accumulation rate, a, in equation 2 kept constant throughout the simulation? Yes, we changed its definition into 'annual mean accumulation rate' to be more clear. This is conform the definition by Herron and Langway (1980).

g. It is not clear from the text what the direction of the fluxes is positive, upwards or downwards? By using this generally accepted SEB equation, all atmospheric fluxes are positive when directed downwards, whereas the subsurface heat flux is positive when directed upwards, i.e. both are positive when transferring energy towards the surface. As non-meteorological readers might find the plus-signs in the equation misleading, we decided to state this definition also in the text.

h. Is it snow surface-atmosphere interaction? Not necessarily, equation 3 is also applicable to the snow-free tundra area, sea ice or open ocean.

i. Page 578, line 20 do you mean skin temperature? The skin temperature is introduced for modeling purposes and is defined as the temperature of an infinitesimally thin layer

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at the surface that has no heat capacity and responds instantaneously to changes in energy input in RACMO2/GR. Surface temperature is the temperature of this surface layer, so this term is applicable both to observations and to model output.

j. Pages 579 and 580 suggest to write out shortwave and longwave rather than use abbreviations In our opinion the use of abbreviations and arrows improves the readability of the manuscript. Instead of using terms as emitted longwave radiation at the surface, incoming longwave radiation at the surface, net longwave radiation, incoming shortwave radiation at the surface, outgoing solar radiation at the surface and net solar radiation at the surface, the use of the abbreviations LWâĘȘ, LWâĘŚ, LWnet, SWâĘȘ, SWâĘŚ, and SWnet shortens the sentences, improving readability.

k. Page 582, line 9 "deposition" do you mean condensation? When the surface temperature is below freezing, only rime formation (i.e. deposition) occurs.

l. Comments on figures Y-labels and captions of figures are changed to enhance readability.

Answers to comments of referee 2: X. Fettweis

1. Sec 4.0: The authors should insist that the model is not reinitialized or post-calibrated during the simulation We have followed this suggesting by adding the sentence "During the 51-year simulation, no model parameters were re-initialized." to Section 2.3 pg 569 line 26, and also "For this analysis, the model output is has not been post-calibrated." to Section 4 pg 573 line 17, and finally "This makes RACMO2/GR a suitable and valid tool to study climate changes over the Greenland ice sheet." at the end of the conclusions in Section 5.

2. Sec 4.1: Analysis of the annual mean temperature could mask seasonal biases. A table showing the biases for the main observational sites and for the 4 seasons should be useful here. The variability of the seasonal means should also be validated. We calculated the seasonal biases and RMSE based on daily means for the 3 stations of

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the K-transect. Here is a table with the seasonal and annual bias, RMSE and standard deviation of the observations.

T2m	S5			S6			S9		
	bias	RMSE	std	bias	RMSE	std	Bias	RMSE	std
DJF	-4.1	4.9	7.8	0.8	2.7	8.3	-0.2	3.0	8.4
MAM	-2.3	3.3	8.2	0.9	2.3	8.5	0.9	2.3	8.4
JJA	-1.2	1.4	1.7	0.7	1.2	1.7	0.7	1.5	2.6
SON	-2.8	3.7	6.6	0.9	2.3	7.3	0.5	2.3	7.7
Annual	-2.6	3.5	9.3	1.1	2.2	9.8	0.5	2.3	10.2

Only S5 shows a seasonal cycle in the bias, as could also be concluded from figure 4b. For S5 the RMSE is only 1°C larger than the bias, indicating that the error is systematic as also seen in fig 4b. For all stations and all seasons, the bias and the RMSE are smaller than the standard deviation of the daily mean observations. This confirms that RACMO2/GR is well capable of simulation the near-surface temperature variability. The seasonal correlation between the observations and model output exceeds 0.95 at all stations based on comparison of daily means, except for the summer months when due to melting the surface temperature is limited to the freezing point. We will add a table with the seasonal bias and standard deviation for all 3 locations and a discussion of the variability to Section 4.

3. Sec 4.1: A map showing the temperature bias at each observational site will help to better interpret the validation and to understand the comments written pg574 lines 10-24 Such a map gives a rather distorted view of the land/ice bias, because of the irregular distribution of the stations over Greenland. For a better interpretation we decided to change pg 574, line 10 into: "Figure 4a shows for the entire ice sheet (green and red dots) and the surrounding tundra (black dots), the simulated climatological

values. . ."

4. Page 574, line 21: the authors should insist that the surface scheme used over tundra and ice sheet is not the same. The proposed additional information is added to the manuscript after pg 574 line 21: "In RACMO2/GR, tundra and ice sheet are considered as different surface tiles with specific characteristics, such as albedo, thermal skin conductivity, and vegetation type. The calculation of the surface fluxes is done separately for these different surfaces, leading to different solutions for the SEB equation and skin temperature even if the overlying atmosphere would be identical."

5. Sec 4.1: the 2-m temperature could be compared to the GrIS temperature parameterization from Fausto et al (2009) allowing a validation at the scale of the whole ice sheet Comparing model output with the parameterization from Fausto et al (2009), gives no additional information about the performance of RACMO2/GR over Greenland, because both are parameterizations. In our opinion, the quality of the model can only be evaluated by comparing model output against observations. Therefore we decided not to include the suggested comparison in this paper.

6. Sec 4.1: If the K-transect measurements are available at higher temporal resolution than daily value, a validation of the simulated daily cycle/amplitude in summer should be shown. The modeled 2m-temperature is available at 6-hourly resolution, which is not high enough for a thorough evaluation of the daily cycle. Therefore, we decided to focus the evaluation on daily, monthly, and climatological means.

7. Sec 4.1: A plot showing both modeled and observed time series of annual mean 2m-temperature at some DMI weather stations over 1958-2008 should be shown here to validate the interannual variability in the model The standard deviation of the modeled annual temperature is for 3 out of 4 DMI stations smaller than the standard deviation in the observations. This means that although large biases (ranging from -4.4 to 0.8°C) are found they are rather constant in time. This is confirmed in the figure below with the modeled and observed temperature for 6 stations spread around the ice sheet. We

will add this new information to the manuscript, including the figure.

Caption Comparison of simulated (dashed lines) and observed (solid lines) annual mean 2 m temperature anomaly [K] with respect to their mean value (1973-2004) for 4 DMI locations spread around the ice sheet a) Thule, b) Tasiilaq, c) Sondre Stromfjord, and c) Julianehavn.

8. Fig 4b, Fig5-fig 6b: If the authors have the data, it should be interesting to add on these plots the ECMWF reanalysis outputs for showing the interest of this RCM compared to the reanalysis The underlying ECMWF model for ERA-40 has the same physical parameterizations as RACMO, but the surface model is crude with regard to the ice sheet. In addition, the resolution of ERA-40 is much coarser, which would lead to a larger bias with the measurements compared to the RACMO2/GR model bias. Furthermore the actual ERA-40 dataset is already extensively evaluated for Greenland in other publications. Therefore we decided not to include this suggestion in the manuscript.

9a. Fig 5, 8, 9, 11, and 13: These figures compare RACMO2 to measurements only during 2004. Why is this year chosen? The year 2004 was not an exceptional year within the 51-year simulation. Similar results are found for the model evaluation of other years.

9b. I think that similar figures for other years and S9 could be included as supplementary material if the comparison is different. For the albedo, the general conclusion does not change when looking at the other years: the albedo drops too early in spring, and increases too late in autumn. Of course the timing of the start of the melt and the fresh snow in autumn does change for the different years, but the time bias between the model and the observations is similar. Comparable biases are found for the other surface energy fluxes. We deliberately choose to show daily values at S6 as the conditions at S5 are not representative for the ablation zone in general and S9 is located on the equilibrium line altitude, where slight deviations in surface fluxes may result in large

biases due to strong feedback mechanisms. Therefore we decided to show monthly means for general evaluation and daily means at S6 for a more detailed evaluation. To make this more clear we have added a similar comment to the new text.

10. Sec 4.2 As for temperature, a table listing wind speed seasonal biases for the principal weather stations of fig 6a should be useful here For the K-transect, analysis of the daily mean values with respect to the model bias and RMSE, is complementary to fig 6a. At S5 and S9, the bias for all 4 seasons is negative and smaller than the RMSE and standard deviation of the observations. At S6, the seasonal biases are close to zero, except for JJA. The seasonal RMSE (on averaged 1.5 m/s) and bias (on average 0.5 m/s) are considerably smaller than the standard deviation of the observations (on average 2.5 m/s). We decided not to include these wind statistics in the manuscript as they are consistent with the conclusions drawn from fig 6b.

11. Pg 576 line 8: the authors describe the seasonal cycle of wind speed along the K-transect, but no figure showing this cycle is shown. Perhaps, fig 6b, instead of showing the wind bias, should show the real values of both modeled and observed monthly mean wind speed The description of the seasonal cycle is given here to allow a better interpretation of the model evaluation with respect to wind speed in fig 6b. The findings of a seasonal cycle in wind speed is not new, Van de Wal et al (2005) already described that the katabatic forcing is strongest in winter based on observations along the K-transect. Therefore we decided not to replace fig 4b with plots showing observed and modeled wind speed at 10 m

12. Sect 4.3: Why do the authors not show a figure similar to fig 4b and 6b with specific humidity, allowing a validation for S5 and S9, in addition to S6 We decided not to show such a figure as the model bias is rather consistent throughout the year for all stations. We have changed pg 578, line 4 into: "The monthly bias is rather constant throughout the year (-0.1 g kg-1) for all three stations along the K-transect".

13. Pg 578 line 13: the authors found a bias in the simulation of RH. Is this bias

repeated in the other years than 2004? Yes, we added the following sentence on the other years to the manuscript (pg 478, line 15: "This discrepancy is also found for the observational years 2005 and 2006."

14. Sec 4.4: a table summarizing the biases in the seasonal mean energy fluxes at S5, S6, and S9 should be useful here as complement of the text. A table with the DJF, JJA and annual mean bias will be added as complement of the text. The conclusions from this table remain the same.

	S5			S6			S9		
	DJF	JJA	Ann	DJF	JJA	Ann	DJF	JJA	Ann
SWnet	-0.2	25.9	8.8	0.6	23.6	10.7	0.6	27.3	9.3
LWin	-25.6	-13.6	-18.5	-19	-3.1	-9	-30.5	-5.6	-14.6
LWnet	-2.8	-11.4	-5	-15.9	-3.6	-8.7	-23.1	-7.2	-12.9
NetR	-2.5	14.5	3.7	-15.3	19.9	2.1	-22.5	20.1	-3.6
SHF	10.3	-4.5	2.6	22.7	20.1	15.9	21.8	4.9	10.5
LHF	3.3	-3.9	1.9	-3.8	-4.3	-4.7	-3	-4	-3.6

We have added a few references in the text to the table, where appropriate in Section 4.

15. Sect 4.4.1: Fig 9 shows too early decrease and a too late increase of the snow albedo. Are these delays repeated in other years? Yes, we have added a comment on the other years to the manuscript.

16. Sect 5: a table summarizing the main biases averaged over all weather stations available is needed here as reference for future improvements in the RACMO2/GR model As we decided to include a table with biases for 2 m temperature and the surface energy fluxes, this is no longer necessary in our opinion.

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