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The Cryosphere Discussions

Interactive comment on "High-resolution simulations of the surface mass balance of Greenland at the end of this century" *by* G. Krinner and N. Julien

Anonymous Referee #2

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General comments

This study is interesting, because it introduces a new type of atmospheric model into the discussion about the surface mass balance of the Greenland ice sheet. The idea of using a GCM rather than a Regional Climate Model is interesting, because it includes the interaction of the ice-sheet with the large-scale circulation around Greenland. By using the model to predict the future climate and the mass balance of the Greenland ice sheet, the article has the potential to be an original contribution.

However, since it is a new model study of the Greenland ice sheet, an thorough assessment of the model performances for the present climate is essential. The authors

compare their results only to some glaciological data and early published model simulations of the SMB. Recently, more accurate estimates are published using RCM's. I recommend the authors to compare their AGCM performances in more detail with recently published papers, like Fettweis (2007), Box et al (2006) and Hanna et al (2005).

The authors downscale the results of the AGCM to a finer grid for the computation of the runoff and melt, but it is unclear what the impact is of this exercise. A comparison between the downscaled melt and runoff from the AGCM results must also be presented. For such an evaluation and interpretation of the presented results, a short description of the snow pack parameterisation in the AGCM and its shortcomings is required.

The used emperical relationship air temperature and melt is very simple and adds further uncertainty to the AGCM errors that arise form its simplistic snow parameterisation (e.g. constant albedo of 0.77). As Bougamount et al (2007) showed, the impact of different model physics on the SMB of the GrIS can be significant. It should also be addressed that parameters tuned to present climate might be not suitable for future predictions.

To resolve the narrow ablation zone (50-150 km) of the Greenland ice sheet, the resolution of the AGCM is fairly poor. Compared to other recent studies of the SMB of the Greenland ice sheet (Box et al, 2006; Fettweis, 2007) I would not call the AGCM resolution 'high', rather 'medium'.

The introduction is not very well structured, slightly misleading and incomplete. It does not comprise references to recent estimates of the SMB components from the ERA-40 dataset (e.g. Hanna et al 2002, 2005) nor of RCM studies (e.g. Lefebre, 2005; Fettweis, 2007; Box et al., 2006) which are validated against satellite measurements. The introduction and abstract suggest that this survey is only on the future climate prediction of the mass balance, while evaluation of the AGCM for present climate is also part of this study.

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Section 3.1 and section 4.1 treat the same subject, namely validation of the AGCM simulations in the current climate. It would be advisable to restructure this manuscript and combine part of these sections, e.g. make one section on the description of the present day simulated climate only, and a second section on a more thorough comparison of the model performances with available data from measurements as well as RCM's.

All 2D-plots described in section 3 and 4 seem to represent AGCM data and not the finer grid, but this is not clear from the text discussing the figures. This is confusing, especially for the interpretation of figure 5, because it is unclear if the melt and runoff in the SMB is computed on a different grid then e.g. precipitation.

The authors put the all SMB components in the unit kg m-2 yr-1, where the unit km3 yr-1 is more common in literature.

Specific comments

Below is a list of more specific comments referring to the text. Since I recommend some major revisions based on the major comments, this list is probably incomplete.

P 352, abstract: The performance of the AGCM under the present climate is not adressed, while this information is essential to be able to evaluate the future climate.

P 354, line 23-25: Considering that this study is focussed on the SMB, it is advisable to explain how the snow surface and its interaction with the atmosphere is parameterzed in this AGCM. This information is essential in evaluating the errors introduced in the estimates of the SMB components in the sections following.

P 355, line 17: For the emperical method the 'air temperature' is used. At what level?

P 355, line 22: For elevation data Ekholm, 1996 is used. Why did the authors decide not to use the digital elevation model data by Bamber et al (2001)?

P 356, line 19, fig 1: Does fig 1 concern the air temperatures on 60 km grid or on finer

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grid? And how do these 2 compare to each other and to the measurements? This would give insight of the effect of the downscaling method.

P 356, line 23, 24: For the interpretation of the bias and its spatial and temporal variation, it would be advisable to provide the range of the bias.

P 357, line 17, fig 4: 'However, LMDZ4 only simulates...' suggests that the melt area in fig 3 and 4 are result from AGCM and compares reasonably well with observations. What about the empircal method, which uses the air temperature and therefore might result in an enhanced melt area?

P 357, line 23-24, fig 5: 'using the ablation parameterisation by Ohmura et al (1996)...' The plotted data seem to be on the streched 60 km grid. How do the authors treat the different SMB components (solid precipitation, sublimation, etc) on the different grids?

P 357, line 24-27: Why referring to IPCC (2001), while there are more updated datasets available, e.g. Fettweis (2007), Hanna et al (2006), Box et al (2007), Lemcke et al (2007)?

P 358, line 11-12, fig 5: Putting the maximum and minimum value in the plot will help its interpretation. The scale is not readable, less levels will do.

P 359, line 1-2: An increase in the number of days during which surface melt occurs, does not have to mean an increase in meltwater production.

P 360, line 1: The comparison is made to Box et al (2004), while an update has been published by Box et al (2006), which actually comprises a large dataset, 1988-2004.

P 360, line 6, fig 6: In this figure the method by Pfeffer et al (2001) is introduced as yet another method of computing ablation and proves to fit the observations better. Why is this method not discussed in more detail? What effects does this parameterisation have on estimates of the total SMB of the GrIS?

P 360, line 22-26, fig 6: The authors give as only reason for the underestimation of the

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SMB the fixed albedo used in the model. Another reason for this underestimate could be the 60 km resolution of the model that is not capable to resolve the narrow ablation zone well.

P 361, line 1-5: 'in fairly good agreement with the measured valued...' What do the authors mean with 'fairly good agreement'?

P 363, line 10-12: I agree with the authors that for their treatment of the SMB it matters if the precipitation is snow or rain, but how do they distinguish between the two? This is essential for interpreting the results.

P 366, line 6: Could the authors give any uncertainty levels to the estimated 0.8 mm yr-1 sea-level rise?

Interactive comment on The Cryosphere Discuss., 1, 351, 2007.