

4 November

Robinson et al. present an alternative mass balance model for the Greenland ice sheet. The scope of the paper is either the presentation of a new mass balance model or the idea of multiple equilibria for similar climate states. Reading the manuscript from the first point of view leads to the conclusion from the authors that they have provided a new and good mass balance model. As a reviewer I would like to see a more critical attitude towards their own results as expressed below. Reading the manuscript from the second point of view no real conclusions are reached as admitted by the authors. Given the title of the manuscript the authors prefer the first point of view as the second point does not come back in the title. My main suggestion is therefore to leave out the meager section on the multiple equilibria and focus more on the new mass balance model, which could be deepened according to the suggestions below. This is a completely valid approach as mass balance modeling including widely different geometries is an open question in the modeling the Greenland ice sheet. This also implies new analysis and a considerable rewrite, my major conclusion is therefore major revisions.

The paper leans on the comparison with the old EISMINT parameterizations. This is reasonable, but there is more nowadays. There are classical energy balance models to compare to and more importantly Regional Atmospheric Models like MM5 and RACMO. The second paragraph on page 731 needs to be rewritten after digesting those results.

Suggestion for a more thorough analysis of the mass balance performance are mass balance as a function of elevation at several location. Plotting the accumulation with on the background the observations (see Ettema et al. 2009). Showing the importance of radiation changes. A better physical justification of several of the assumption in the model or a demonstration that these assumptions are not critical. A better explanation of the overestimation of the melt by 50%. Show more difference plots rather than field plots to indicate the magnitude and localization of the differences.

Minor remarks.

Line 6 the ice sheet model is just at standard resolution not particularly high

Line 24: It seems more appropriate to refer to the AR4 report here.

Line 25: the three different methods, Radar, Insar in combination with mass balance and Grace should be mentioned explicitly and appropriate references should be given at least for each of the methods one.

Page 731 line 2 e.g. (Van de Wal and Oerlemans 1994)

Line 4-7: remove unnecessary phrase “a certain threshold, which is probably just” and In view... will be used”

Line 9 replace and by which.

Page 734 In view of the remarks in line 5 it would be good to refer to a paper by Van de Wal 1996 who compares an energy balance approach and a degree day approach and argues that despite a comparable performance for the present-day climate a large difference in sensitivity occurs for the two models. This is precisely illustrating what you want to say here.

Page 735 line 22. What is the physical justification of the fact that the vertical structure of temperature and humidity remains constant under a changed geometry? To my knowledge the strength of the inversion depends on the climate state. This might have important consequences.

Page 737 line 7. Rather surface slope changes than elevation changes.

Page 737 line 13. An Arctan function should be more appropriate than a sine function given the derivatives of these function once you approach the limits.

Page 737: What is the physical justification of the fact that the diffusion coefficients are latitude dependent and height dependent for DT, which is not mentioned.

Page 738: A crucial parameterization is how the albedo is parameterized. Is there a physical argument to assume that this is a time invariant parameterization under very different geometries and climate states?

Page 738: the appropriate reference for the albedo parameterization seems to me Oerlemans 1991.

Page 738 line 22-24 poor justification there are weather station data to test your albedo parameterization. This is not tested thoroughly enough.

Page 740. The introduction deserves explicit reference to the work by Braithwaite (choose an appropriate one yourself) for local glaciers and the general formulation by Reeh 1991 (already in the ref. list of the paper) rather than the long list of applicators for ice sheet models.

Page 741. It is remarkable to note that the ITM, which contains shortwave radiation, is used in this paper to simulate long time scales without application of the Milankovitch theory. This is maybe half the signal of change in the mass balance forcing. You really need to do some experiments for e.g. Eemian conditions to show how this works for your model. This is one of the possible strengths of the model!

Page 741 see also Konzelmann et al. 1994 for a parameterization of the transmissivity.

Page 743. Line 14- 18 Rephrase the part on the performance by REMBO and Hanna et al. The coastal stations are part of the retrieval included in ERA-40 so the good performance is a circular argument.

Page 744: I think you should compare your results to the work by Ettema et al. 2009. But not only compare it to their global estimates of the accumulation over the entire ice sheet, but also make a plot like their figure 1c. The figures you show mask the differences between the observations and the model performance. Showing height profiles of the mass balance at a few locations and comparing those to the observations might reveal better how good the model really is.

Page 744. I am worried enormously by the fact that your estimated ablation is 50% higher than the IPCC estimate this implies your model is not that good as you pretend it is. It is honestly to mention this flaw, but it point to a flaw in the melt model.

Page 746. You really need to add changing radiation conditions if you wish to maintain this section.

Page 747 line 13. I miss the point here. Do you really believe that poor calving in your model explains the problems? In view of the above there seems ample room for shortcomings of the mass balance model as well.

Page 748 line 1-3. Here you reach your conclusion with respect to the use of the different mass balance models. But isn't it a bit trivial in the sense that the use of two different models always leads to two different results. Listen to your conclusion as you formulate it "a strong dependence of results on the chosen melt scheme" To a reader familiar with ice sheet models this sounds rather trivial, summarized the results depend on the forcing.

Page 748: I am not convinced that you really proved that accumulation and temperature fields agree well.

Page 748: You argue that REMBO results are consistent with GCM results. Did you make a proper comparison with any of the GCMs available? You might be right, but it is not where the paper is about and also not based on the results you presented.

Page 748. The preference for the ITM needs to be justified by showing the performance with changed radiation conditions.

Page 749. Your final statement needs to include the combination of melt scheme and initial conditions.

References:

- Ettema, J., M. R. van den Broeke, E. van Meijgaard, W. J. van de Berg, J. L. Bamber, J. E. Box, and R. C. Bales, 2009: Higher surface mass balance of the Greenland ice sheet revealed by high-resolution climate modeling, *Geophysical Research Letters* 36, L12501, doi:10.1029/2009GL038110.
- T. Konzelmann, R.S.W. van de Wal, W. Greuell, R. Bintanja and E. Henneken, and A. Abe-Ouchi (1994). Radiation measurements and a parameterization of radiation for the Greenland Ice Sheet. *Global and Planet. Change*, 9: 143-164.
- J Oerlemans (1991): The mass balance of the Greenland ice sheet: sensitivity to climate change as revealed by energy-balance modelling. *The Holocene* 1, 40-49.
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