

Interactive comment on “An efficient regional energy-moisture balance model for simulation of the Greenland ice sheet response to climate change” by A. Robinson et al.

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We would like to thank this reviewer for the thoughtful and constructive comments. Here we describe the general changes we will apply to the manuscript to address reviewer criticism. We will also correct or modify the text of the manuscript and the figures according to the specific comments of the Reviewer.

(Reviewer comments are numbered, responses follow)

1. 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

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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 of the Greenland ice sheet. This also implies new analysis and a considerable rewrite, my major conclusion is therefore major revisions

We agree and following the Reviewer's recommendation, we will skip the discussion of multiple equilibria. Since this is a rather interesting but complex problem, we will address it in more detail in a separate paper.

2. 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.

The motivation for comparing our model results with EISMINT stems from the fact that this parameterization is widely used in modeling of the GIS response to climate change. By comparing our results with the EISMINT parameterization, we would like to show that for present-day conditions REMBO performs with comparable skill to the EISMINT parameterization, but has an obvious advantage that it can also be applied for a broad range of climate conditions and to a GIS geometry completely different from the present one. At the same time, we fully agree with the Reviewer that our paper will benefit from comparison with the most recent detailed simulations of GIS mass balance and this comparison will be presented in the revised manuscript.

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3. 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.

We agree with the recommendation to perform a more detailed comparison with modeling and empirical data. We have obtained SMB data from RACMO and PolarMM5 and will include a comparison of mass balance vs elevation. Furthermore, since the initial manuscript submission, we have been able to improve the performance of our model significantly, which eliminates this strong over-estimation of melt. We will show some difference plots but due to a large special heterogeneity of the difference fields, they are not always informative.

We agree with most minor remarks and will change the text appropriately. Below we address only a few of the Reviewer's most critical 'minor' comments.

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.

The Reviewer is right – this approach does not permit us to properly account for the inversion, which is strong during winter season. However, our primary interest is in the simulation of surface mass balance and most surface melting occurs at low elevation and during the summer season, when inversion is absent or weak.

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.

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We find that either function would be appropriate, since both can be set to give the same results – especially since they apply within a fixed temperature width. For simplicity, we kept the sine function.

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.

The latitudinal dependence of the diffusion coefficients reflects the empirical fact of reduced synoptic activity from the middle to high latitudes. A dependence on elevation can be justified by increasing wind with elevation. We found that the elevation dependence of the temperature diffusion coefficient is necessary to produce the seasonal cycle of temperatures correctly.

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?

We agree that this is a very crucial and a rather crude parameterization. However this parameterization, unlike most similar studies, at least in the first approximation, explicitly accounts for the surface and planetary albedo changes related to changes in climate and GIS geography, which is crucial for the simulation of the long-term GIS evolution. It is, of course, useful to check these results against more complex climate models. The fact that simulated temperature changes over ice free Greenland are consistent with GCM results indicates that our approach is not so bad.

Page 741. It is remarkable to not 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 strength of the model!

Testing of the Milankovitch theory is one of the principal activities of our research team

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and we have already published several papers on this subject using a Northern Hemisphere ice sheet model and rather sophisticated surface EBM model (Calov et al., 2005, *Climate Dynamics*; Ganopolski et al., 2009, *CPD*). As far as the paper under consideration is concerned, the goal is to present a model primarily designed for simulation of the long-term future evolution of GIS. However, we completely agree with the Reviewer that paleoclimate simulations are very useful for testing the model and constraining the model's parameters. In particular, transient simulations of GIS evolution during the Eemian interglacial will be discussed in a forthcoming paper. However, we find Reviewer's suggestion to compare PDD and ITM for Eemian conditions very interesting and we will now include a comparison of surface mass balance change simulated for a fixed-geometry ice sheet using PDD and ITM under temperature and insolation changes. This should help illuminate how the two mass balance models will perform for greenhouse gas induced warming, and for Eemian conditions.

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.

We have largely resolved this discrepancy, through several structural improvements to the model. We will show in the final submission that we are able to produce the partition of mass balance for the GIS within the range of RCMs.

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.

We appreciate the skepticism, and found there was a problem in the model, which has now been fixed. Nonetheless, we still contend that given the state of current shallow ice approximation ice sheet models, it is not possible to tune the mass balance model with a fixed present-day ice sheet geometry and correctly simulate the present day volume and area.

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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.

We agree that it sounds trivial. The question is how different are the schemes. Since most previous studies of GIS response to global warming were performed using PDD, using the two different schemes at least allows us to assess possible uncertainties related to the parameterizations of the surface mass balance. Nonetheless, this section will be modified greatly.

Interactive comment on *The Cryosphere Discuss.*, 3, 729, 2009.

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