

## ***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.***

**A. Robinson et al.**

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First, we want to express our thanks to the reviewer for the constructive and useful comments. Below, we describe the general changes we will apply to the manuscript to improve the paper. We will also correct or modify the text of the manuscript and the figures according to the specific comments.

(Reviewer comments are numbered, responses follow)

1. Comparisons are made between results provided by the REMBO approach and the EISMINT parameterization. However, the importance of this comparison is not mentioned. While reading this manuscript my feeling was that the work would have

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been self-consistent with the presentation of the only REMBO results.

The reason for including this comparison is that EISMINT has been generally accepted as the standard approach to obtaining forcing for ice sheet models. 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 be also applied for a broad range of climate conditions and to a GIS geometry completely different from the present one. We will make this point more explicit in the manuscript.

2. It should be interesting to give some details about the surface interface module in the model description section. In particular, I feel that the variables which are passed from REMBO to the interface module are not clearly indicated in the text ...

We agree and will improve model description in order to make these points more plain to the reader.

3. It does not seem straightforward why REMBO explicitly accounts for the continentality and orographic effect. Could you please specify through which modeled processes these effects are accounted for ? In REMBO, no indication is given on how (or if) the cloud cover is taken into account. Could you please add a comment about this ?

The continentality effect is explicitly accounted for in REMBO because simulated temperatures in the Greenland interior have a certain degree of freedom and show large seasonal variations compared to the prescribed coastal temperatures from ERA-40. The orographic effect is accounted for, both for temperature (via the lapse rate) and precipitation (via the effect of surface slope on precipitation). Changes in cloud cover are not explicitly accounted for. We will add sentences to address these points.

4. A few words about the spin-up procedure of the REMBO model should be added. Since references to calving are made in the text and in table 2, it should be explained how calving is treated in SICOPOLIS.

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Due to the low thermal inertia of the atmosphere, the REMBO model already reaches quasi-equilibrium after one year of integration. A simple snow pack model used in the mass balance scheme requires a much longer time (on the order of 100 years) to reach an equilibrium state. In SICOPOLIS, “calving” is treated as the ice discharge into the ocean calculated by the ice sheet model directly from the divergence of ice volume fluxes, assuming that all land ice which flows into the ocean is melted after a certain time. We will address these issues in the manuscript.

5. The model is forced with the ERA-40 lateral boundary conditions. This data set spans from 1958 to 2001, and the results are compared to a compilation taken from several sets of observations : 1) Cappelen et al. 2001 which provides long-term means of various climatic variables and 2) GC-Net program. First I would like to know what “longterm” means in this case. Secondly, since the model is forced with the ERA-40 lateral boundary conditions, the consistency between model results and the comparison with data only available since 1995 may be questionable. Actually GC-Net data are likely much more affected by the global warming trend than the ERA-40 data set and also by the decadal variability. I understand that authors can only compare their results with best available observations. However, this comparison requires at least an additional comment.

We completely agree with the existence of this caveat and will discuss it in the paper. Of course, it would be better to use longer and more consistent data sets, particularly for the GIS mass balance, but we can only use what is available.

6. Is it possible to find arguments explaining the so huge differences between PDD and ITM melt schemes are produced under ice-free state conditions ? To my knowledge, there is only a very few number of studies that compare the PDD with other more physically-based approach. One of this study is that from Bougamont et al. (2007) who find a higher sensitivity of the PDD scheme to climate warming than the energybalance model they used. Although both approaches are not fully comparable, my feeling is that this paper should be at least mentioned in the introduction. It should be even better to

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discuss the differences between the main findings of the present work and of the study of Bougamont et al. (2007).

After performing a suite additional experiments, we found that these differences are not as great as indicated by simulations of the GIS using ice-free initial conditions. We found that using slightly different sets of model parameters can result in multiple or singular GIS equilibrium states under present-day climate conditions, for both thePDD and ITM schemes. We will discuss these multiple equilibria in much more detail in a separate paper (in preparation). In general, we found clear differences between PDD and ITM but they are not as strong as those reported in Bougamont et al. (2007). One reason is perhaps related to the different treatment of refreezing between Bougamont et al. (2007) and in our model. Another is that we are primarily concerned with the equilibrium response, while Bougamont et al. (2007) studied the transient reponse of the surface mass balance to temperature rise. We will include the latter two points in the discussion section of the present paper.

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

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