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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.

Anonymous Referee #1

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The paper presents an energy-moisture balance model which simulates seasonal variations of temperature and precipitation over Greenland. The model is coupled to the 3D polythermal ice-sheet model SICOPOLIS. In this paper two methods to compute the melting rate are used: the positive-degree-day method widely used in the glaciological modeling community and relating the potential melt rate to the daily temperature, and the “Insolation-Temperature Method” which accounts for the relationship between surface air temperature and absorbed solar radiation. The REMBO model provides results in a reasonable agreement with empirical data. The two melt schemes are tested for present-day climatic conditions with the present-day Greenland topography or with an ice-free Greenland where the bedrock is uplifted. The parameterization of the melt

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scheme is a recurrent problem in glaciological modeling. Glaciological modelers often use the PDD method which gives reasonable results for past periods. However, there are only a very few number of constraints of the free parameters. Moreover this method is not really physically based and does not account of the dependency of the melt rates with surface conditions. This paper does not solve the problem of whether the ITM method is better than the PDD one, but it aims to compare their performances in two radically different contexts. This work is a good contribution to better constrain the surface mass balance of the Greenland ice sheet and address relevant scientific questions within the scope of The Cryosphere journal. The paper is well written and structured and the results are clearly exposed. Therefore I recommend the publication of the paper in TC after some minor revisions that I address below.

General comments

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

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 (sea-level temperatures ? snow accumulation ? total precipitation ? humidity ?). Is the downscaling procedure from REMBO to the surface interface scheme is the same as the one used to interpolate ERA-40 dataset onto the REMBO Cartesian grid ? How the dependency of precipitation and temperature with altitude is accounted for ?

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 ?

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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.

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 “long-term” 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.

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

Bougamont et al, Impact of model physics on estimating the surface mass balance of the Greenland ice sheet, *Geophysical Research Letters*, 34, L17501, doi:10.1029/2007GLO30700, 2007.

Figure 4: The comparison of REMBO and REMBO ice-free with data would be eas-

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ier additional panels indicating the ratio of model results and Bales09 data for both precipitation and accumulation.

Figure 5 : Add also a figure indicating results PDD/results ITM.

Figure 7: Same as Figures 4-5 : Add panels indicating differences between 1) EIS-MINT,PDD and Bamber01, 2) REMBO,PDD and Bamber01 and 3) REMBO,ITM and Bamber01.

Specific comments (in order in which they occur in the text)

Abstract : line 9 : the basis of the “more physically-based alternative” should be explained line 15 : please specify what does “the conventional approach” mean. It is not obvious

page 731-line 16 : “rather coarse resolution climate models” should be replaced by “rather coarse resolution GCMs”.

Page 732-line 1: Here the conventional approach seems to be referred to the forcing method (i.e. perturbation of the modern climate with the anomaly temperature field) and not to the “conventional approach” mentioned in the abstract. This should be clarified.

Page 732-Lines5-9 :I disagree with the statement “Since most precipitation . . .for modeling the surface mass balance”. On the contrary, the albedo effects are strongly different depending on whether solid precipitation (fresh snow) falls in winter or in summer. Therefore the surface mass balance of the ice sheet is likely to be critically dependent on the seasonality of precipitation. Moreover I do not understand what the authors mean with “which may offset the effect of a general precipitation increase”. This is too vague. These both assertions should be corrected and clarified.

Page 732-Lines 9-10 “Present day precipitation over the GIS is, to a large extent, topographically controlled”. Was it different in the past ? Please add appropriate references to justify this statement or modify it accordingly.

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Page 733, 1st paragraph : it seems to me more appropriate to deal with “the effect of albedo on surface melt” rather than “the effect of insolation”. Same remark for “the relationship between temperature and insolation will be different under global warming”

Page 733-Line 10 : Please remove “was”.

Page 734: Although the shallow-ice approximation (SIA) is well known from glaciological modelers, I am not sure that people from climate or data communities are familiar with it. It may be useful for some readers that the basic principles of the SIA are mentioned.

Page 740 : Please clarify : What is the P_{MAX} factor ?

Is there any physical reason to choose $h_{s,max}=5m$ rather than another value ?

Equation (1) : the melt rate is noted as “M_i”. It appears as “M_s” in the following.

Equation (16) I guess that L_m is L_w (equation 1)

Modeling results: It should be interesting to remind here the basis of the EISMINT parameterization (i.e. parameterization of climatic fields as a function of latitude as an example) and explicitly mention a few references (not only the EISMINT report) based on this parameterization

Figure 2b : Do you have an idea why the EISMINT temperatures are overestimated at low elevations, and underestimated at higher altitudes during the winter season?

Page 744 : Could you please remind to which period corresponds the empirical estimate of 300Gt.

Page 745 : EISMINT parameterization: “higher lapse rate used in winter” : this should be mentioned earlier in the text.

Page 747 : lines 14-16 : This statement is a bit misleading. Toniazzo et al. or Ridley et al. considered an ice-free Greenland to examine whether the entire melting of GIS (that

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might occur under global warming) would lead to the recovery of the ice sheet once the climate would return to conditions similar to modern ones. Besides, I think that one of the interest of the present work (within the context of the potential total decay of GIS) is precisely the comparison between present-day topography and ice-free conditions. This could be mentioned in the introduction for example.

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