

## ***Interactive comment on “Surface mass balance model intercomparison for the Greenland ice sheet” by C. L. Vernon et al.***

**Anonymous Referee #1**

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### *Review of Surface mass balance model intercomparison for the Greenland ice sheet by C.L. Vernon et al.*

The paper presents an intercomparison of Greenland ice sheet surface mass balance for the historical period, from three regional climate models (RCMs), together with downscaled reanalysis data.

The paper is for the most part logically structured, and is mostly well-written, with a

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good standard of English. The results are interesting, and are presented clearly and concisely. However, I felt the authors could have spent more time putting their work in context by discussing the reasons/motivation behind this study, the relevance of previous work, and the relevance of the results.

**I recommend that the paper should be published in *The Cryosphere* after revisions are made to address these concerns.**

#### **Major points:**

I believe that the authors should make the purpose of this study clear at the outset, in the abstract and introduction. Why did they perform this study? Was it an intercomparison/validation exercise to determine the models' suitability to be used for future projections when driven by boundary conditions from GCMs? Or was it to determine the relative importances of different accumulation and ablation processes for SMB? Or to study regional effects? Or perhaps to identify sources of uncertainty? Or (more likely) a combination of these? Whatever the motivation, it is important to state it at the outset.

As I mentioned above, I believe that the authors should put their work in context by discussing, and referencing, previous work, in the introduction. The penultimate paragraph of the introduction (p4002, lines 22-29), in particular, contributes to this impression, stating "Until recently SMB was estimated through the interpolation of in situ measurements and automatic weather stations ... The release of high-quality and consistent historical weather reanalysis data has made possible the modelling of the processes controlling SMB over the whole ice-sheet". To me, this paragraph suggests that until now the only way to estimate SMB was via interpolation of observations, but that recently reanalysis data has allowed SMB to be modelled directly. While I am sure that this suggestion is unintentional, I believe it is incorrect - In fact I would say that the opposite is true. There have been many modelling studies over the past decade or so, using both statistical downscaling and RCMs; meanwhile, until data from PROMICE

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became available recently (van As et al., 2011), the only in-situ data available were those from the K-transect (van de Wal et al., 2005), which cover only a very small geographical area and cannot be interpolated to cover the whole ice sheet. I believe the authors should discuss previous work in more detail than they have done, in order to put their own work in context. For example, they should mention the method of statistical downscaling employed by, for example, Huybrechts et al. (2004), and Gregory Huybrechts (2006), to produce high-resolution output from low-resolution GCM fields. This is probably also the point to cite the previous work by Hanna on downscaling ERA-40 data. There have been many previous RCM modelling studies for Greenland, including several using the RCMs used in this study, and covering the same period with the same forcing data, many of which the authors cite later in the paper (e.g. Ettema et al. 2009 with RACMO, and various publications by Fettweis with MAR). The authors must put their own work in context by referencing these studies in the introduction, and discussing how the present study relates to, and differs from, them. Later in the paper, the authors cite the study performed with HIRHAM by Lucas-Picher et al. (2012); they should cite it at this point in the introduction too. They should also cite the recent study by Rae et al. (2012), which, although it focuses primarily on 21st century projections, also has an analysis of model output for the recent past, using two of the models used in the present study, and is therefore of relevance. The sentence "To date four such reconstructions of GrIS SMB....." (p4002, lines 27-29) implies that no other studies have been done and is therefore untrue (see, e.g., Lucas-Picher et al. 2012). In the light of my comments above, I would ask that the authors change this sentence to "This study examines four such reconstructions of GrIS SMB.....".

Having made clear in the introduction the purpose of the study, and cited other relevant work, the authors can then use the conclusion to discuss the significance of their results in this context, rather than simply re-stating the results of earlier sections. The authors present some interesting results, such as the influence of the choice of mask, the regional variations in SMB components in different models, changes in the physical drivers of SMB over time, and the comparison with observational data, which is done

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over a longer period than that of Rae et al. (2012). If presented in the proper context, with a clear purpose and discussion of relevant previous studies, the results in this paper would represent a valuable contribution to the literature.

### **Minor points:**

- Section 1, line 5: "...surface melting is already intense (above 6m per year) along its margins". The authors should provide a reference for this statement.
- Section 2 is currently entitled "Model description", when in reality it describes data as well as models. The title should be changed accordingly.
- Section 2, p4002 lines 6-26 and p4003 lines 1-14 (i.e. everything in Section 2 before the beginning of Section 2.1): I felt that these paragraphs didn't really belong in the "Model description" section, as they are more general than that. I would prefer to see them moved to a reworked/expanded introduction.
- Section 2, p4003, lines 12-15: "Other reanalysis products are available, but model runs using these forcings are not available for comparison". This is not true - MAR has been run with ERA-Interim forcing (Franco et al., 2012, which the authors cite elsewhere). So I'm not entirely sure what the authors mean by "not available" here. Perhaps this sentence needs to be re-worded?
- Section 2.2, page 4004, line 27: The authors refer to the "study area". I think "model domain" is more accurate.
- Section 2.3, p4006, line 8: What does CROCUS stand for? The authors should state this here.
- Section 2.3, page 4006, line 17: "The MAR version used here is the used by..." - doesn't make sense. Need to re-word.

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- Section 2.4, page 4007, line 4: “Runoff occurs when...”. It would be more accurate when talking about a particular model/technique to say “Runoff is assumed to occur when...”
- Section 2.5, page 4008, lines 6-9: The authors state twice in 4 lines that ECMWFd has the smallest mask - unnecessary repetition.
- Section 2.5, page 4008, line 19: “In future it would be helpful if modelling studies could use a common mask”. This sentence belongs in the conclusions (after discussing the results and the benefits of the common mask), not in the model description section.
- Section 2.5, page 4009, line 6: “Approximately 2496 of these grid cells...” - 2496 seems pretty exact (i.e. not approximate) to me!
- Section 3.1, page 4010, line 11: “then” should read “than”.
- Section 3.1, page 4010, line 11: “meaning this relatively low altitude region, not included in the common mask, has a...”. What relatively low altitude region? No region has been referred to previously. Presumably this is supposed to read “meaning *the* relatively low altitude region...”
- Section 3.1, page 4010, line 15: “Considering the same analysis for components of SMB is less clear” - doesn’t make sense. Need to re-word.
- Section 3.4, page 4013, line 21: “during the relatively stable (Fig. 5) period 1961-1990...” Looking at Fig. 5, I wouldn’t say the period 1961-1990 was stable - between about 1965 and 1972 there’s a big dip in both precip and SMB, followed by a recovery up to about 1980. I think the authors need to re-word this, either to remove the term “stable”, or to explain what they mean by it.

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- Section 3.4, page 4014, lines 7-9: “These regional variations suggest spatially compensating errors are leading to the appearance of greater agreement over the whole ice sheet than the localised process modelling is able to reproduce.” - I think this is actually a key result. Could more be made of it, e.g. in the conclusions?
- Section 3.5, page 4014, discussion of Fig. 9: It would be good if the authors could mention briefly what effect, if any, they expect the “dip and recovery” in SMB between 1965 and 1980 to have on the mean seasonal cycle for 1961-1990.
- Section 3.5, page 4014, last paragraph: The discussion of SMB, precip, melt, etc., being “increased” in 1996-2008: I would guess this means increased with respect to 1961-1990, but the authors should make this clear.
- Section 4: I have resisted making detailed/minor comments on the conclusions section, as I suggested under “major concerns” above that the authors should re-write it, putting the results in the context of the motivations of the study, and of previously-published work.
- Figure 9: Top-left-hand corner of SMB plot: “1996-08” should be “1996-2008”.
- Figure 10: Similar colours are used for PMM5 and MAR, which I found made the plot difficult to read. I would recommend using black for PMM5 as in previous plots, and a completely different colour for the observations.

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