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

C. L. Vernon et al.

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Anonymous Referee 1

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.

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We agree with the referee that i) the purpose of the intercomparison; ii) its scope and limitations and iii) the outcomes/findings are not clearly spelled out and elucidated in the submitted paper. We have re-written much of the introduction, discussion and conclusion to address these shortcomings and feel that the messages are now much clearer. These changes have been highlighted in the supplement to this response.

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.

The 6m figure was from a few areas of MAR output, not a general result. Statement rephrased to refer to estimated melt in excess of 1 m water equivalent along margins as presented by Fettweis (2007).

Section 2 is currently entitled "Model description", when in reality it describes data as well as models. The title should be changed accordingly.

Agreed. Title changed to 'Model and data description'.

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.

Agreed. This text moved into the introduction and amended.

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?

Agreed. Sentence has been reworded recognising other forcings have been used and stating that for ease of comparison this work limits itself to the four models discussed and ERA-40 forcing.

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Section 2.2, page 4004, line 27: The authors refer to the 'study area'. I think 'model domain' is more accurate.

Agreed. Changed to 'model domain'.

Section 2.3, p4006, line 8: What does CROCUS stand for? The authors should state this here.

Not sure it stands for anything, no longer name is provided by either Brun et al. (1992), Brun et al. (1989) or Reijmer et al. (2012).

Section 2.3, page 4006, line 17: 'The MAR version used here is the used by...' - doesn't make sense. Need to re-word.

Agreed. Reworded for clarity.

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...'

Agreed. Amended as suggested.

Section 2.5, page 4008, lines 6-9: The authors state twice in 4 lines that ECMWFd has the smallest mask - unnecessary repetition.

Agreed. Removed 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.

Agreed. Moved to conclusion.

Section 2.5, page 4009, line 6: 'Approximately 2496 of these grid cells...' seems pretty exact (i.e. not approximate) to me!

Agreed. Removed 'approximately'.

Section 3.1, page 4010, line 11: 'then' should read 'than'.

Agreed. Amended as suggested.

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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...'

Agreed. Amended as suggested.

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.

Agreed. Removed confusing sentence.

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.

Agreed. Word 'stable' replaced with 'reference' period as described in section 3.2.

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?

Agreed and focused on in rewritten conclusion.

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.

Agreed. Comment added, not expected to affect mean seasonal cycle.

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.

Agreed. Amended to make it clear change is w.r.t. 1961-1990.

Figure 9: Top-left-hand corner of SMB plot: '1996-08' should be '1996-2008'.

Agreed. Key changed and new figure 9 provided.

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.

Agreed. Colours changed and new figure 10 provided.

Anonymous Referee 2

Major/General comments

In the conclusions section of the paper the study is summarised, but no thorough discussion of the presented analysis or any real conclusions are drawn, apart from the fact that having models applying the same ice sheet mask and the same grid resolution would make the comparison between the model products more useful. Reader is left with many open questions, the most important ones are: what are the conclusions of this study? And what have we learned from this work?

There are fundamental differences in the models that are compared, for example in the representation of the albedo and how the refreezing processes are included - and possibly is the topography different between the models. These will have a large impact on the surface mass balance, but this fact is not at all addressed in this study. How large differences are there in the values of the albedo in the different models? how does that variation change regionally and seasonally? How large impact will that difference have on the simulated melt? A recent study by two of the co-authors emphasize the importance of the albedo feedback for the Greenland Ice Sheet mass balance (Box et al., 2012) and therefore some consideration of the albedo differences between the models would be appropriate in this study. Similarly, a large difference is presented in the amount of refreezing between the models, but no analysis or attempt to understand the impact that the difference in refreezing implementation between the models will have on the model results.

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To paraphrase these concerns: this referee feels that we do not make substantive conclusions and do not consider, in enough detail, the physics behind the differences between the models. In particular they discuss differences in albedo and refreezing. We agree with these views but are also aware that, in this paper, which is already reasonably long, our primary aim was to assess the degree of spatial and temporal variability between the available reconstructions of GrIS SMB. To add a meaningful analysis of the origin the differences, in terms of model physics, is really part II of this work and would constitute a major paper and analysis in its own right. It would require, for example, examination of all the energy balance terms, the reason for the difference between each term, the parameterizations that are used in each model, the calibration data (where used), the impact of time step, resolution and numerical integrations and so on. Although we agree that this is worthwhile and informative, it goes a long way beyond the remit of this paper. To illustrate this point, we would like to point out that a paper has recently been published that is a comparison of just one component of the SMB, namely refreezing and provides a useful analysis of the importance of different parameterizations (Reijmer et al., 2012). It does not, for example, determine which is 'best' because refreezing is extremely difficult to measure directly in the field. With regard to albedo, while we entirely agree that it is extremely important for the energy balance and hence melt, it would not be particularly meaningful to compare this parameter between the reconstructions. This is because one of them uses albedo observations for calibration (PMM5) and another does not determine or utilise albedo at all (ECMWFd). This leaves MAR and RACMO that determine albedo independently using different schemes based on age and grain growth. Thus a simple comparison of albedo between MAR and RACMO would not really be particularly instructive unless we also examined the radiation fluxes and precip. Which also has a strong influence on the albedo in the models. This goes back to the earlier point, that such a detailed analysis of the energy balance is beyond the remit of the paper. The original topography used by the models is similar but is profoundly affected by the resolution of the model (which is poorer than the original topography) and how the topography was resampled

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or interpolated to the native resolution of the model. There are numerous papers (Mar-siat and Bamber, 1997; Franco et al., 2012) on this topic, which, again, is not part of what we wanted to address in this study. In summary, we agree with the referee that the analysis they suggest would be interesting and informative but consider that it is i) beyond the scope of this study and ii) would constitute a substantive and long paper in its own right.

This intercomparison is clearly presented and a valuable analysis made on the difference between the selected models, but there is a lack of interpretation and discussion of the results (tables (e.g. 3 and 4) and figures (e.g. 5, 7, 8 and 9) are presented but not really discussed) and conclusions of this study are really missing. With clear conclusions and perhaps a recommendation for further work on improving the models and increasing the understanding of their shortcomings this paper would become a valuable contribution.

We agree with this and these comments are echoed by reviewer 1. We have substantially rewritten the introduction, discussion and conclusions to i) make the purpose of the work clearer and ii) to draw out and discuss the key findings such as the importance of differences in land/ice mask, the compensation of errors regionally, and whether the published error estimates are consistent with our findings.

Specific comments

Page 4001 line 13 'runoff' here 'melt' would be more appropriate as runoff includes rain that does not contribute to mass loss

We would like retain the term runoff here. The reviewer is correct to say runoff does include rain but we regard rain as mass input in the in the same way as snow is a mass input. The suggested replacement with 'melt' is problematic as melt includes some liquid which will refreeze, retaining the mass in the ice sheet.

Line 26- line 2 on page 4002 This statement is not correct, there are a number of other reconstructions available (see for example in Rae et al. 2012)

Agreed and amended in line with a similar comment from first reviewer.

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Page 4002 line 20 typo in 'coving' Line 22-23 not clear sentence, rewrite to make clearer, less satisfactory than what? Over which period?

Agreed. Sentence amended to explain that inhomogeneity can occur when two different products are used (first ERA-40 followed by ERA-INTERIM or operational analysis) over the study period (1960-2008).

Page 4003 line 1 'ERA-40 data' - ERA-40 is a reanalysis product based on data, suggest to be consistent throughout the paper (see also page 4003 line 20, page 4004 line 26, page 4006 line 14 and probably other places where it is called ERA-40 reanalysis data, ECMWF re-analysis ERA-40, ERA-40 respectively) and call the fields ERA-40 reanalysis product.

Agreed. Manuscript amended throughout for consistency.

Page 4003 lines 1-3 very long and unclear sentence, suggest to break up and clarify

Agreed. Same material over two sentences now.

Page 4003 lines 3-5 another long and unclear sentence, suggest to rewrite

Agreed. Minor change to increase readability.

Page 4003 line 20 missing reference for the operational analysis data. Please explain better what reinitialising entails, is the snowpack reinitialised? What variables are reinitialised?

We are not aware of a reference for the ECMWF operational analysis data. Papers describing models in more detail than here do not include one. Additional detail on model set up is available in cited papers and we feel it is not required of this paper to restate.

Line 22 - not ideal for what? What kind of inhomogeneities? Line 24 errors in what? Explain better Line 25 Is the precipitation, one SMB component, modelled with an energy balance model?

Agreed. Sentence rewritten in line with Box et al. (2009). Precipitation excluded from EBM.

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Page 4004 line 27 missing reference for the operational analysis. Suggest to replace study area with model domain.

Agreed. Changed to 'model domain'.

Page 4005 line 1 what does 'open fraction' mean? How is it prescribed? Using what? Equation 5 there is inconsistency in the use of M, here it is used for melt energy, but in equation (4) it is the amount of melt, please be consistent with the terms. QH and QE are already defined.

'open fraction' refers to sea ice, text amended for clarity. M for melt energy given subscript ME. Repeat definitions of QH and QE removed.

Line 23-26 This is not correct, in a paper that is reference in this study (Lucas-Picher et al, 2012) analysis of a 5km resolution run with HIRHAM5 is presented.

Clarified statement is with respect to models considered in this work.

Page 4006 line 14 mission reference and explanation for ERA-INTRIM.

Added reference for ERA-INTRIM (Dee et al., 2001) in section 2.

Line 26 what fields?

Added 'downscaled' for clarity.

Page 4007 line 1 suggest to add 'up to' (is the error the same everywhere? How large variability?)

Added 'often' in line with Hanna et al. (2005), there is no information on variability.

Line 19 replace 'runoff' with 'ablation' area

Previous paper describing the model in question (Hanna et al., 2005) used 'runoff' rather than ablation when describing this area, we would prefer to use consistent terminology.

Line 21 suggest to turn sentence, the downscaled data is not independent from observations.

Agreed. Order swapped.

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Line 23-27 this sentence should be somewhere else in the paper not in the description of the ECMWF downscaled product.

We have separated to a new paragraph, but suggest it should remain at the end of sections detailing the four models under investigation here.

Page 4008 line 3 (with fraction grid boxes rounded) what does this mean?

Amended for clarity.

Please clarify Lines 4-8 this sentence is not clear, please edit.

Amended for clarity.

Line 16, what do you mean by stating that common mask is not more accurate?

Sentence amended to clarify that common mask is not closer to reality.

Page 4009 line 1 what is 'the sub-grid issue'?

Sub-grid issue (addressed through fuzzy mask) is explained in preceding sentence (mix of land and ice).

Line 6 replace 'reconstruction' with comparison.

Reconstruction is the right term as this sentence only relates to the PMM5 mask.

Line 12 what is meant by 'fewer of this type' - please edit and clarify.

This refers to stake measurements, as described in the first half of the sentence.

Line 15 do you mean each position? This sentence is not clear, suggest to rewrite and explain what filtering means here.

Reworded for clarity.

Page 4010 line 26 do you mean larger negative SMB? Do you mean common points? (rather than masks)

Sentence amended for clarity. Larger positive SMB, when integrated over the common mask than native masks.

Page 4011 line 13-15 - sentence is not clear, do you mean modelled mass balance

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(not observed)? Please edit and clarify.

Sentence amended for clarity. We are referring to variation in mass balance estimation from observation (altimetry and gravimetry).

Lines 24-25 what is meant here, how is the effect of biases minimised?

Suggest no amendment to text necessary. Any bias between models is removed by comparing the magnitude of their anomaly rather than the model's absolute magnitude.

Page 4012 lines 4-6 replace 'from' with 'in period' the sentence is not clear, please rewrite, what is SMB tracking?

Sentence amended for clarity.

The Note on refreeze is not useful here, some explanation of the different implementation of refreezing between the models would be helpful to present here

Agreed. This section removed, with more comment on refreeze moved into model description.

Page 4013 line 26 'A similar, but different pattern ...' what does that mean? Please rewrite.

Agreed. Replaced with clearer statement: 'Rank order also varies'.

Page 4014 lines 7-9 What does this sentence mean? Is the comparison then not reliable when only total values are compared?

We have not said the comparison is not reliable, it is just the regional analysis suggests compensating errors across regions.

Page 4014 line 20 - and why is this difference? Can you give some explanation?

Added explanation related to the fact that the PMM5 snow model is not fully coupled to the atmosphere so neither melt-albedo feedback, nor the expansion of dark, bare ice is explicitly considered.

Lines 24-25 why is this difference? Some analysis and discussion would be useful here.

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Sentence of discussion added. This is related to the comment above about not being fully coupled to the atmosphere and therefore missing feedback.

Page 4015 line 10-12 some analysis and discussion of this difference would be useful.
A more detailed discussion is included in the cited work of Fettweis et al. (2011).

Section 4 Conclusion is really only a summary of the previous sections, there is missing thorough discussion of the presented intercomparison and conclusions from this study.
Agree. Significant additions made to conclusion.

line 19 'reasonable agreement' what does that mean? Is it regional? Seasonal? Can you quantify?

This is total mass balance, quantified in abstract and section 3.1.

Line 20 What do you mean by 'filtering'? add 'between models' to the last part.
Sentence re-worded for clarity.

Page 4016 line 1 - what was previously thought? Maybe some references could be added here?

This is in relation to the models' estimates and quoted errors, following sentence added to expand on this pointy.

Line 24 what is meant by this sentence? It is not clear. Suggest to end the study with some conclusions that have been drawn from this intercomparison exercise.
Sentence amended for clarity.

Figure 10 what is a 'rank order' - this could be explained better.
Sentence amended for clarity.

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