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## ***Interactive comment on “Estimating Greenland ice sheet surface mass balance contribution to future sea level rise using the regional atmospheric climate model MAR” by X. Fettweis et al.***

### **Anonymous Referee #2**

Received and published: 28 September 2012

#### General Comments

In this paper, the authors use the MAR model to downscale output from a number of GCMs and compare the results with simulations forced by reanalysis data to assess the capability of the model in making projections of sea level rise in the future. The combination of a number of different forcing GCMs from the CMIP5 database make this a novel piece of work and the thorough analysis of results by the authors helps to unpick the strengths and weaknesses of the different forcing GCMs. The structure of the paper is relatively easy to follow but unfortunately the mass of detail presented obscures the importance of some of the issues raised by this study which would reward

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further discussion. In addition I miss a strong narrative within the paper in terms of setting out the aims of the study from the outset and then presenting methods and results within that context. The conclusions of this study are scarcely surprising, but the exercise of carrying out these simulations and analysing the strengths and weaknesses of each is valuable in terms of tracing the climate envelope for Greenland and surface mass balance of the ice sheet. A re-write and re-structuring of the paper to reflect these points would be very worthwhile with the emphasis on clarifying and refining the analysis of the results and drawing out the key elements while discarding extraneous and distracting detail that is unnecessary to the narrative.

## Specific Comments

I agree with many of the comments expressed in the other review and rather than simply repeating these I add some of my own.

1) The abstract is overly long and should be shortened to include only the main conclusions and a brief overview of the methods.

2) On page 3106 lines 13 – 16 indicate that when integrated over the ice sheet the interannual variability of the SMB is not affected by different horizontal resolution. In this paper only 25km resolution is used for MAR, but it is compared with RACMO at 11 km resolution. A paper by Lucas-Picher et al., 2012 shows that horizontal resolution is important when looking at local and even regional climate in Greenland. This does have important implications on a larger scale since ice sheet models are very sensitive to the distribution of SMB and this would have a consequential effect on ice dynamics which may also feedback on to the SMB. Some discussion of this point would be useful since it is returned to on page 3112, where it is clear that some of the models (e.g. CanESM2) appear to get good answers (when integrated over the whole ice sheet) for the wrong reasons (geographical variability of SMB in the model does not reflect the actual pattern of geographic SMB variability).

3) It is not clear why the particular GCMs were chosen in this study, reading page 3107

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and 3110 I am left with the impression that it was largely because the output was in the correct format. Is this correct? How is their ability to simulate general circulation over Greenland assessed? And how does this relate to their abilities to simulate circulation in the rest of the world? It is of course entirely possible that Greenland is well represented in these models for entirely the wrong reasons, so much more information needs to be provided here.

4) Also on page 3107, the use of the MAR ERA-Interim simulation as a reference dataset is justified as 'having been successfully evaluated against in-situ weather station data'. This is a bit vague and some detail on what MAR does and does not represent well would be welcome, for instance are there biases in seasonal temperatures? How does this affect how we interpret the simulations forced by GCMs compared to the reanalysis?

5) On page 3108 the period 2000 – 2010 is explicitly excluded because it is a warmer decade than preceding ones. It would be interesting to see output from this period though as it is likely to be more representative of the future climate in Greenland and could have some interesting implications for the evolution of the ice sheet.

6) The decreasing SMB observed over the last 10 years is problematic for this study in the way it is presented here since it appears to show that the models are only capable of simulating one phase of the natural variability of the system but not others. This is however, largely due to the short period of the simulation. In the light of recent (and still tentative) work suggesting that loss of Arctic sea ice cover may lead to changes in the dominant atmospheric circulation, this point needs some further discussion. In fact a better way of presenting this may be to simply state that 20 years is too short to assess the climatology of a GCM. In this context, the performance of a GCM against the 1980–1999 period as opposed to the later 2000 – 2010 period is more or less meaningless since both should be well within the bounds of natural variability but the model is not in the correct mode at the correct time. Even more difficult, just because it performs well against present day data does not mean it will continue to do so. This point should be

made much more prominently, especially given the conclusion in section 3.4 that none of the GCMs can adequately reproduce all aspects of current climate in Greenland.

7) Section 5.2 is very confusing and difficult to follow; I suggest simplifying and rewriting this entirely.

8) The figures are very small and the text practically unreadable in some of them, if possible it would be nice to have them enlarged and put on separate pages.

#### Technical Corrections

Given that most of the authors are writing in a non-native language, there are few major errors in this paper but I would like (as a native speaker) to give a few comments on style.

1) The text is made unnecessarily complex and difficult to read by the use of multiple nouns or adjectives before a noun in a string. While this is not necessarily incorrect use of English, it is not easy to read and makes it harder to grasp the sense of some of the sentences. For example line 1 on page 3105 reads “which aims to improve the projections of the land ice melt contribution to future sea level rise”. It would be easier to follow this clause if it were written “ which aims to improve projections of future sea level rise due to the contribution of melt from land based ice”

2) A number of prepositions are incorrectly applied throughout the text (I realise that this is a very difficult one to get right when writing in a foreign language!), for example, line 19 on p. 3105 should read “it consists of”, I suggest having the manuscript proof read through by a native speaker to catch these minor errors.

3) It's a personal preference but I dislike seeing “impact” used as a verb, (for example, line 19 on page 3103 “Beside impacting surface processes...”). Verbs such as ‘to affect’, ‘to alter’ or ‘to vary’ can be used instead.

4) Typos on page 3108 the 4.5  $\text{wm}^{-2}$  and 8.5  $\text{wm}^{-2}$  units should be  $\text{W m}^{-2}$

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Interactive comment on The Cryosphere Discuss., 6, 3101, 2012.

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