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**Geoscientific
Model Development
Discussions**

Interactive comment on "Ice sheet dynamics within an Earth system model: coupling and first results on ice stability and ocean circulation" *by* D. Barbi et al.

Anonymous Referee #1

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This manuscript presents the methodology and initial results of coupling an Earth system model with an ice sheet model. This is valuable work that has particular importance now since such coupled approaches are in their infancy. However, I find that the presentation of this work needs significant improvement. Often it seems that the description of procedures is not very precise. Several subtle details (such as ice sheet initialization) are not discussed that are important for understanding the benefits and problems associated with the methodology. The discussion of individual features of the simulations that are deemed important should also be expanded and placed in context (e.g., why are the various oceanic anomalies and feedbacks important?).

Additionally, there seem to be some problems with the downscaling procedure that may require further investigation, or at least explanation. While the downscaled accumulation pattern is much improved compared to the original field, the temperature and ablation pattern look less convincing. The high ablation in southern Greenland appears to be a direct result of large temperature biases. Rather than use a "classical lapse rate correction", the authors employ a temperature redistribution equation (Eq. 3) to downscale temperatures from the low resolution GCM grid to the high resolution ISM grid. While the procedure described in Sect 3.4.3 improves the temperatures somewhat from the original GCM, the temperatures are still much too high. It would be informative to see what results are obtained from the lapse rate correction. It may also be interesting to apply both the procedure and a lapse rate correction to the temperature field, as the former would seem to be intended to account for the distribution of temperatures, while the latter would help correct for actual inherent elevation discrepancies between the model domains. It is not clear though, what justification Eq. (3) has, other than it "gives better results", and this should be further elaborated.

I also find the plots could be more concisely presented. There are many oceanic plots that are discussed only very briefly (sometimes in just a couple of sentences). At the same time, there is not even one plot of Antarctica, which is also simulated here.

Overall, while interesting, it feels like this manuscript is still in a draft phase. Therefore, I would recommend publication only after major revisions, as well as consideration of the comments below.

Comments ==========

English: A native speaker should check this manuscript carefully for grammatical errors.

Page 4, line 10: Which combination of ice sheet model approximations was used for the simulations here (SIA, SSA, Full Stokes)?

Page 6, line 13-14: Statements such as this one should be revised, as they misrepresent the authors' goals. I don't think the downscaling approach was developed to make the ice sheets inherently stable, as it sounds here. Rather, the statistical downscaling approach was used to more accurately reproduce climatic forcing fields for the ice sheet. In turn, this results in stable present-day ice sheets.

Page 6, line 15: Why 999 ppm and not 1000 ppm? The difference is irrelevant, but 999 ppm seems odd. Perhaps say $\sim$1000 ppm?

Page 8, line 7: molten => melted

Page 8, line 24: Is the interpolation method sensitive to the choice of e? A sentence or two should be added to explain this choice.

Page 11, line 5: "Within a box surrounding the grid point x, ..." A box of what size? How was this size determined?

Page 12, line 12: The initial conditions of the ice sheets in the experiments need to be explained in more detail. What was the initial temperature field inside the ice sheet? Normally, an initialization time of about 100 ka is needed to obtain correct internal temperatures of the ice sheet, which in turn strongly affects the stability of the ice sheet.

Page 13, line 17: The fact that more ice is present is most likely due to a lack of ice sheet physics and/or resolution. Is the ice sheet model run in SIA mode?

Page 16, Section 4.2: The presentation of these results is cursory and does not convey to the reader their importance. How sensitive is the coupled model to these changes? How realistic are they?

Interactive comment on Geosci. Model Dev. Discuss., 6, 1, 2013.