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The Cryosphere Discussions

Interactive comment on "Towards direct coupling of regional climate models and ice sheet models by mass balance gradients: application to the Greenland Ice Sheet" *by* M. M. Helsen et al.

Anonymous Referee #2

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General comments

This article proposes a new approach toward coupling surface mass balance (SMB) results from climate models and ice sheet models. It focusses on the altitude dependency of the SMB. To do so, the authors develop a statistical scheme to calculate the vertical gradient of SMB, taking into account the fact that best regressions are different in the ablation zone and accumulation zone.

The method is well presented and I think it is a rather technical but useful paper. It could be improved by having more discussions on the general points below. And finally I still regret that the method has not been tested against RCM runs on a different

topography. I know that the authors estimate that this is beyond the scope of the paper, but this would have been a real achievement.

1- I have difficulites to see in which framework this method will effectively been used.

âĂć next centuries simulations: a full coupling should be possible which is better than using statistical parameterization of the SMB "lapse" rate. May be the method presented here could be useful for sensitivity studies to avoid running the RCM too often

âĂć I am a bit sceptical about the use all along a glacial-interglacial cycle as it is given in the article. I aknowledge that the problem is strictly the same with a PDD method but for me it is just one more sensitivity study. I think the main difficulties are linked to changes in circulation and orographic precipitation.

âĂć The use of this method in long term simulations with asynchronous coupling is more convincing and more detailled explanation should be given on how this could be done. How to decide the asynchrounous time step ? is is possible to derive a quality score of the method by using two successive topographies ? how the RCM will be given lateral boundary conditions ?

2- I appreciate that the results are all along compared with the PDD method which was the standard method up to now, however, it would be better to explain early in the article (before the discussion) which variety of PDD was used. I am also a bit surprised that a significant advantage of this approach over the Pdd is not well highlighted: In this method, the day to day variability is implicitely taken into account because the climate model does it by cumulating SMB terms (precipitation, melting, ...) all over the year. It requires only the exchange of one field per year (or even several years) while with annual fields, the Pdd method has either to relies on assumptions such as periodic (sine) variation of temperature or exchange many more fields between climate model and ice sheet model.

3- The same philosophy as for SMB is applied to the refreezing and here I regret that

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the results have not been compared with the other refreezing schemes (Reeh and Janssens and Huybrechts for instance).

4-The method seems to require a lot of neighbouring points, ie. a climate model with a very fine grid (RCM as proposed in the article). I wonder wether this method is still robust with a GCM with a coarser grid. A test could consist in undersampling the RCM results and compare the reconstructions.

Detailled comments.

p. 2121 top, Mb max should be explained at this level because there is a reference to figure 2 on which it appears and check the notation because it is SMBmax in equations and MBmax in figures.

p. 2122 equation (3) please define beter what are the various terms (SMBpos, SMBref)

p. 2124 is the 26.6 simply related to the latent heat ? It would be better to give the real equation.

p. 2124 line 16-17. Explain more physically why a diferent sign is demanded for the gradient on R depending on ablation/accumulation zone.

p. 2127 line 20, experiment with no refreezing. Two variables are usually affected by the refreezing: the SMB itself and the ice temperature. Is it the case in this experiment and if yes, that means that the impact on temperature is more efficient than the one on SMB.

p. 2131 line 16. The present topography at the end of any simulation is also dependent on the enhancement factor used in the ice sheet model. This parameter is usually introduced to calibrate the ice sheet model so the fact that the simulated surface elevation is in good agreement with the observed one is not that significant.

figure 8. How is calculated the calving ?

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