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The Cryosphere
Discussions

Interactive comment on "Effect of uncertainty in surface mass balance–elevation feedback on projections of the future sea level contribution of the Greenland ice sheet – Part 1: Parameterisation" *by* T. L. Edwards et al.

Anonymous Referee #2

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This manuscript presents a new parameterization of the surface mass balance – elevation feedback, which would allow near-term simulation of the Greenland ice sheet using ice sheet models driven by regional climate model projections. Indeed, it is clear that under global warming, more accurate simulation of the SMB will be critical for projecting future sea level rise. This work therefore is a step in the right direction towards direct coupling between RCMs and ISMs. Not only does it provide a useful parameterization for use in ice sheet modeling, but it can help to quantify what effect elevation changes on the centennial time scale can be expected to have on SMB.

I find the manuscript well written and it thoroughly describes the parameterization such that it could likely be applied in other models without too much difficulty. The applied experiments adequately validate the ability of the parameterization to improve the uncoupled model results with respect to the coupled (NonUn) situation. In the larger picture, I am still left with the question of how long such a parameterization can be considered – 100 years, 500 years? I think the authors should add some discussion concerning this point. Other than that I have two concerns that I think the authors should address (below), but overall I think the manuscript is publishable as it is.

The uniform experiments show a wide spread of SMB change basically centered on zero, over two values of elevation reduction (50 and 100m) in Fig. 5. Meanwhile the NonUn experiment clearly shows a rather linear slope in each of the four panels. Thus, I don't see the value of applying the NonUn simulation as a Bayesian update, since it comes from the same model (ie, it's not an observational dataset) and it arguably provides much more context for the estimate of the coefficients. Perhaps it is my own lack of statistical depth, but it seems to me that all the information comes from this last experiment – why not just estimate the coefficients and uncertainty from this experiment directly? Would the posterior pdf in Fig. 10 be much different in this case? The method as is appears to work and produces a nice estimate, but I wonder if the statistical formulation is overly complicated.

The discussion concerning spatial correlation is valuable, and I appreciate the effort to eliminate this as a factor in the estimation of the parameter range. At the same time, the ad-hoc sensitivity tests to cell spacing and cell offset are not very satisfactory. Would it not be easier to make a spatial correlation plot to show the distance over which this dissipates, in order to find the appropriate cell spacing? In addition, this is confounded with the issue of representing the margins well, as it is discovered that cell sampling is important for this too. Why not apply an a priori grid weighting as a function of elevation or distance from the margin, to explicitly give more weight to cells that are clearly more important for the application of this parameterization? Picking the cell spacing that arbitrarily represents the total sea level rise better does not seem to fit with the quite rigorous statistical application elsewhere.

Interactive comment on The Cryosphere Discuss., 7, 635, 2013.