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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.

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General comments

This paper present a new parameterisation of the surface mass balance over the Greenland ice sheet. The authors use a series of sensitivity experiments using a regional climate model MAR in which they change surface topography under an emission scenario. Using a statistical approach, finally they construct a parameterisation that relates the surface mass balance 'anomaly' to the surface elevation 'anomaly', i.e., the gradient of these two terms, and present best estimates as well as plausible ranges of the gradients, with separating the whole domain into four types.

This paper is fairly well written. Although it may be a little bit long to follow the text easily, it is still reasonable. I think it acceptable with minor revision as follows. I have also a following suggestion (not requirement) to this paper, which may need a major change of the manuscript.

The authors mention about computational costs of regional climate models several times: abstract, introduction, conclusions. I really understand such technical situation as an ice-sheet modeler, but, due to the same reason I get a negative impression from the paper and the new parameterisation. Partly because I start to read the paper from abstract, then conclusion, then introduction, the text about technically challenge is repeated three times in a very short time and may have biased me for such negative impression. Honestly speaking, while I do agree this not a general idea, I think it bad to use ‘low computational costs’ as an excuse of simple parameterisation. If a phenomenon can be simulated only using high computational cost, which is imperative to the target of numerical study, then it means simply we can not simulate it at this time. The low computational cost matter is really an advantage, but it has nothing to do with robustness of the study. If the method fails to extract the essence of a phenomenon due to neglect of some important processes, then it fails to provide robust and complete information about uncertainties in the projection, even if it looks good.

If this paper is a kind of description paper of a new method, then it is acceptable to keep this style. However, as mentioned in introduction and conclusion, the focus of the paper is to present uncertainties in the projection of response of Greenland ice sheet to changes in climate.

If I had same material, I would construct the paper as follows: From the series of sensitivity experiments using a sophisticated climate model (which succeed to simulate the SMB at present-day), the good linear relation between the surface mass balance

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anomaly and the elevation anomaly is obtained. Also uncertainties and plausible range of the linear gradient are presented using a Bayesian framework, which allows us to probabilistic assessments for the effect of elevation feedback uncertainties like its companion paper. Moreover, this feedback process is an essence for ice-sheet climate model coupling at least the target time-scale of this study, and, fortunately, this does not require heavy computation with technical challenges such as full coupling of GCM and ice sheet models.

Again, this is not requirement for acceptance, but suggestion for revision. I really understand those technically challenging matters, but at the same time personally I do not prefer the style and reasoning of this paper. The logic is consistent and fair. I have no doubt that the paper is acceptable with minor revision, without changing the whole style as I suggested. The elimination of the sentence from the abstract is a minimum solution, but I am not sure it is enough to diminish such negative impression.

I think, however, that the paper become more constructive and benefit to community of ice-sheet and/or climate modelers if the author shift the paper basis, from reduction of expensive simulation, to abstraction of important processes and discuss its (physical) advantage and limitations. This can be done without additional expensive computation.

Specific comments

Several matters relating to PDD (Positive Degree-day) scheme.

As far as I understand, in a very specific definition, PDD just compute the melting as a function of temperature. Usually it involves a simple snow pack model, but 'technically' we can use more complicated snow pack model, for example. Typically as the authors mentioned on P640 L7, 'PDD schemes include a parameterisation of the temperature aspect,' but again 'technically' it can be outside of PDD. We can drive PDD model with any temperature field (GCM, observation), as mentioned in the companion paper.

P638 L23: "PDD schemes do not account for variations of ice sheet response in time or (horizontal) location...". We can, in principle. For example, Tarasov and Peltier (2002,GJI) use temperature-dependent PDD ablation factors. The temperature is computed by background climate scenarios and changes in the ice topography (in terms of a lapse rate), which means their PDD DOES account for variations of ice sheet.

Moreover, it is even possible to present new variation of PDD scheme with following the same procedure of this paper, for example, 'PDD factor lapse rate' instead of the SMB lapse rate. I cannot access Goelzer et al. (2013 accepted), but from other papers cited from P638 L26 to p639, the fact that PDD sometimes overestimates while sometimes underestimates the melting than RCM merely seems to reflect the simplicity of the PDD scheme, technically.

I do not mean to say that full coupling with sophisticated climate model is only way to compute reasonable surface mass balance or that PDD is good enough, and do not say that the new method in the present paper is worthless. Either method is good in some sense but bad in other senses, as well as the new method. Although construction of the new method uses RCM results, the parameterisation itself is much simpler than typical PDD scheme. It just adds a correction term, which is a linear function of local height change, to a reference state. Again from my first impression, the simple linearity seems to be the objective of the paper, not a result, in order to reduce the complexity and avoid high computational costs. These matters should be explained better in the paper.

About exclusion of the cell which has smaller topography anomaly.

P649 L21 'Our minimum threshold for the denominator, ...'. The authors exclude the sensitivity at the cell whose surface anomaly is less than 25m (also mentioned in P644 L15). I wonder how much such cells exist and how much SMB anomaly is over such cells. Similar thing in P.644 L15 and others. The authors exclude cells in which the SMB crosses the ELA. How much such cells? Is it really safe to exclude such cells in the analysis? I am afraid that exclusion of such cells mean that we cannot use

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the SMB-lapse rate method for mild warming cases which may occur in early stage of global warming experiment. Since the method in the present paper may be limited to the case which the area of the Greenland ice sheet is not so much changed from the present (because there are no change in the ice-covered area in the experiment), the applicability of the method seems to be limited to from not-mild warming to not-large warming.

I suspect that other effects than the local height change are significant on such cases that of course cannot be represented in the SMB-elevation method. We can say that the focus in the paper is just to extract SMB-elevation feedback processes, but, then the authors have to clarify/discuss how other (non-linear) effects are included. For example, a 'pure' SMB-elevation feedback may be offset by such excluded effects and affect the best estimates. As mentioned above, I am impressed that in order to 'force' to fit the result to the simple linear function, the author neglect other processes without careful interpretation which do not fit to the function (I am not saying the authors DO intent, but I was impressed by the way they write). Although this is mentioned qualitatively in the discussion (P658–9), I think a better explanation should be done for this minimum threshold as well as non-linearity (e.g., albedo feedbacks) and how such exclusion affect (or not affect) the result. Quantitative evaluation may be difficult, but to show the results over ignored cells may help readers to imagine such effects. Also, it may be better to justify why the authors exclude -25m or less while include -50m (fixed experiment) in the analysis. I am not sure how the SMB anomaly changes between these two relatively closed anomaly of the topography.

Minor comments

About experiment naming. There are three types of the boundary condition of the surface topography: -50m, -100m and NonUn. The former two are generally referred to as 'Fixed height change' in the text. Why not use 'Uniform' experiments, rather than 'Fixed' experiments? The 'fixed' experiments in this paper is really Non-NonUn experiments, i.e., Uniform. Moreover, if you use such different terms, we may think it in a different context such as ‘Temporally-fixed’ experiments. For example p640 L6. “Surface topography in RCMs is usually **fixed**”: this ‘fixed’ is not ‘spatially’ fixed but ‘temporally’ fixed. Of coarse we can distinguish them by context, but still confusing to the readers.

P641 L25 ‘Unlike most RCMs it includes the positive feedback..’ Needs reference.

P644 L21 ‘These show positive gradients in the south but tend towards negative gradients in the north...’ It is very difficult to catch such features in Figure 5. Explain better, please.

P646 L27. ‘We choose not to make the gradients a function of grid cell location’. Actually, the separation of a certain latitude corresponds to make the gradients a function of grid location.

Technical corrections

Figure 1 and other Greenland maps. Longitude axis is not consistent with reality. I think the axis in the figure is correct only at a certain latitude (may be the upper range of the figure, 85 degree North). Better to rewrite all the figures either correct longitude/latitude lines or Cartesian coordinates.

Figure 1 right. I do not think it good coloring, it has too many levels. Also, it is better if we can find easily where is positive, where is -50m, and where is -100m and maybe where is -25m, because these height anomalies are used in other experiments or analysis/discussions. We can easily get an image of the relation between ‘NonUn’ and ‘Fixed’ experiments and the region excluded to construct the parameterisation.

Figures 6, 7, 12. Some of the plots exceed the ranges of the vertical axis. Redraw the figure or adjust the axis.

SAITO Fuyuki.

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