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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 2: Projections" *by* T. L. Edwards et al.

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This paper enlists a number of ice sheet models to explore the coupling between changes in Greenland's ice elevation and surface mass balance. The surface mass balance is parameterized, using the results from part one of this two part paper. The ice sheet modeling is similar to what has been done in recent SeaRISE and ice2sea initiatives. In this case, the parameterized surface mass balance coupling to elevation complements the forcing of a regional climate model that is run over a constant elevation ice sheet.

I like the paper because it provides a clear quantification of the size of the coupling effect: ∼4% increase in sea level contribution over 100 years, and ∼10% over 200. It also provides some reasonable statistics about the confidence intervals of the results.

I only have two concerns that might prevent publication of this paper. First, I have not studied the companion paper, and I do not have the expertise to assess how reasonable the mass balance parameterization made there are. Superficially, it seems plausible, although having the Greenland partitioned in a north-south rather than an east-west manner would seem to ignore the obvious trends in accumulation. In any case, successful peer review of part one is absolutely a requirement for successful publication of this paper, part 2. It seems that much of the statistical methodology appears in part one. That will have to be closely scrutinized as well.

Secondly, I am concerned that the synthetic surface mass balance patterns that some of the models apply may be significantly larger in magnitude than the modeled mass balance patterns that are applied. If this is the case, I have to wonder if we are seeing the parameterizations magnify a synthetic signal, or a real one? This needs to be addressed by providing figures comparing the SMB synthetic and the SMB modeled for several models.

Provided that part one passes peer review, and that SMB synthetic is much less than SMB modeled, I recommend a few minor changes that should make the paper more engaging. Overall, the paper is well written and free of grammatical errors. Most of my comments have more to do with clarifying the modeling process, especially in regard to initialization, physics, and boundary conditions.

p.678 lines 7-9: I disagree with this. Given infinite computation resources, the ultimate model is not the one that incorporates the most physical processes. Each physical process necessarily introduces more unknown parameters, each of which represents an internal degree of freedom in the model. These internal degrees of freedom are then adjusted in order to maintain fidelity to what (little) data is available. The end result is the modeling version of the statistical phenomenon of ‘overfitting’. Like using an 8th degree polynomial to fit 9 data points. It’s not at all surprising it works well, but consider the result of adding more data. The best model is the one with the simplest physics, the lowest number of parameters, and the best fidelity to the data. The trade off occurs with regard to ‘fidelity to data’ vs. ‘lowest number of parameters’. I’m ranting, but I hope you take my point.

p. 681 line 3: justify this ‘climate’ of looping the final decade 10 times. Why not continue the MAR run?

p 681 line 8: This sounds like your sub-grid parameterization of MAR physics. Can you provide more detail about how this works, it isn’t clear to me from this brief description.

p. 681 line 18: add ‘finite element, partial differential equation solving’ between open-source and parallel. That’s the part that makes it a good platform for ice-sheet modeling.

p. 682 lines 17-24: Consider rewriting this paragraph. It’s not very clear how the processes described in each step relate back to the idea that the first two give the shape of the ice sheet.

p 683 line 3: Throughout this section ‘we’ is used in a confusing way. I think it would be easier to understand if you simply wrote things like “the MPAS modeling group”, “the GRISLI group”, etc. It would help the reader know which model is being referred to, and give insight into how the work was carried out.

p 683 LINES 23-25 This can’t be good...

p 683 General comments on initialization:

The experiments are conducted with the same level of care that other ice-sheet model intercomparisons have been done in recent years. Which is to say, I am troubled by the lack of a consistent model initialization procedure, and a consistent set of forcings or boundary conditions, but until the ice sheet modeling community can agree on model physics, boundary conditions, and initialization procedures, this will have to be how studies are done.

I'd find a more structured description of the initialization procedure helpful. It should come down to initialization of the mass, energy, and momentum balances in the models. Each should have a set of clearly defined boundary conditions. The paper as it is now does not have sufficient information about all the boundary conditions. Additionally, a table would be helpful to consult, in order to quickly assess the differences in each model's physics, initialization procedure, and boundary conditions.

p 684 line 1: bedrock elevation is not a boundary condition, it is a geometric constraint.

p 684 lines 10-15 is it significant that you are using the mean from 1989-2009, a period marked by a significant upward trend in temperature?

p 684 line 19: what is meant by 'the mean SMB changes'. Is this the variance?

p 685: This page is the crux of the matter, is this defensible? If so, then it's publishable, if not...

p 686 line 2: did I miss the definition of PPE?

p 688: The importance attached to the fractional uncertainty is high, considering the relatively small number of models participating.

p 698: A general comment on the results is that they are presented without offering the reader much insight into why the results are what they are. The authors are mostly commenting on what they see, rather than speculating on why they see what they see. I'd have enjoyed reading more about the cause of things, recognizing that it is not easy to have intuition about such complex couplings as those explored here.

Tables: Sea level contribution due to ice dynamical component should be recorded in parenthesis after each value, so the reader can understand the importance of SMB vs ice dynamics.

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Figure 2: This doesn't offer much beyond the table. Consider dropping one or the other.
Figure 4: I never quite understood which parameters were being varied in these plots.

Interactive comment on The Cryosphere Discuss., 7, 675, 2013.

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Interactive Comment