The Cryosphere Discuss., 7, C792–C795, 2013
www.the-cryosphere-discuss.net/7/C792/2013/

The Cryosphere
Discussions
Open Access

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.

Anonymous Referee #2

Received and published: 14 June 2013

This companion paper provides an application of the SMB elevation feedback parameterization explained in part 1, as well as probabilistic estimates of the SLR contribution from the Greenland ice sheet in 2100 and 2200. I believe that the parameterization as applied is reasonable and well-tested, even if the separation between North and South is somewhat arbitrary. The results of this work will be valuable to the glaciological and larger Earth Science community and should be published. However I think the presentation and discussion of the methods and results could be improved.

A lot of emphasis is put on the statistical uncertainty ranges extracted here. Such a transparent and robust methodology is indeed important to use and quantify. In the end, the 95% credible intervals are rather narrow relative to the total ice sheet contribution to SLR. This is an important result. However, the ice sheet and climate (read snowpack) model parametric uncertainty is completely ignored here and is likely to have a much stronger effect on predictions of future mass loss. The discussion mentions that future work could include this, but I think it should be made more clear that this is a key uncertainty that needs to be quantified, as it will likely result in a much wider range of estimates than that presented here.

Along the lines of Reviewer 1's comments, I would like to see a more systematic explanation of the initialization procedure(s). The methods are usually well described and helpful, giving the reader a broad picture of how the initialization worked. However, some justification of why certain steps were performed is lacking. For example,

- Page 683, line 11: Why was relaxation time given to some of the ice sheet models but not others, especially given that a synthetic SMB correction is applied afterwards? How was the relaxation time decided (55 years or 145 years)?

- Page 684, lines 1-7: Why was the temperature and bedrock calculation disabled? This seems like an odd thing to do, even if the effect is expected to be small. As mentioned later, one cannot always know a priori what the effect will be. But disabling these two processes will certainly result in a narrower uncertainty range for the SLR contribution estimates.

I find the same issue with regards to discussion of the resulting probabilistic distributions of SLR contributions. Is it more useful for predictions to have a symmetric PDF? Does it become easier to make simplifying assumptions elsewhere? Such clarification is missing for the reader. For example on Page 689, lines 13-17: Why are the different shapes of the density gradient distributions important?

I would suggest slightly reorganizing the formulas, in order to show the different SMB contributions that were applied. Namely, it would help the reader to see that the to-

TCD
7, C792–C795, 2013

tal SMB forcing field is a combination of four distinct terms: the climatological SMB (S^{init}), the 'flux correction' synthetic SMB (S^{syn}), the RCM anomalies and the elevation feedback correction, as

$$S = S^{init} + S^{syn} + S^{RCM\prime} + S^{h}, \tag{1}$$

where

$$S^{RCM\prime} = S^{A1B} - S^{20C,A1B} \tag{2}$$
$$S^{h} = b_t \left(h^{ISM} - h_0^{ISM}\right). \tag{3}$$

In some cases, S^{syn} is set to zero, but to be consistent I think it is more appropriate to include this term in the equation (especially since later it appears in the text). To maintain consistency with S^{adj} in the companion paper, I changed the superscript here to S^{h} for height. Then on Page 685, each simulation can be described by the combination of forcing terms used (eg, control used $S^{init} + S^{syn}$, with $S^{syn} = 0$ for Elmer/ice and GRISLI). Then as per reviewer 1, a table could provide average magnitudes of the various components to see their importance. The above is only a suggestion for clarity – at a minimum the term S^{syn} should appear somewhere.

Finally, while the discussion is quite thorough, the conclusions should be strengthened. Particularly the last paragraph is very abstract. There is a fitting opportunity to suggest "where future research should be directed" rather than just say that it could be suggested using this work. For example, it seems to me that one conclusion of this work is that for centennial time-scale predictions, full coupling between RCMs and ICMs is perhaps not warranted given the effort and resources required. This is worth stating here in explicit terms (with appropriate caveats, etc).

Minor comments

Page 689, line 8: The "likely" here seems strange, as no other uncertainty distributions are included in the calculations. Perhaps "largely" fits better?

TCD

7, C792–C795, 2013

Page 693, line 1: “our uncertainty” is hard to understand – could you rephrase this sentence?

Page 694, line 10: Delete the word “forcing”, as it is the SMB that is decreasing, not the forcing.

Page 694, line 23: “practicable” => “practical”?

Figs. 12: Why are different line styles used for each model? Since they appear in different labeled columns, solid lines would be fine, right?

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