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The Cryosphere Discussions

Interactive comment on "The effect of more realistic forcings and boundary conditions on the modelled geometry and sensitivity of the Greenland ice-sheet" *by* E. J. Stone et al.

Anonymous Referee #2

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1. Does the paper address relevant scientific questions within the scope of TC?

The paper makes a substantial contribution to the scientific fields of glaciology (as well as numerical modelling and remote sensing) by examining the dependencies of parameterisation and abstraction in numerical ice sheet modelling to data quality and resolution. Applying "improved" datasets for the modelling of the GrIS and comparing the results with those of previous work as well as observations, assumptions that are widely being used across the ice sheet modelling community can be tested, and the suitability of these assumptions, parameterisations and boundary conditions are tested. This paper can therefore be considered as an valuable and essential contribu-

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Interactive Discussion
Discussion Paper

tion to research in the field of glaciological modelling.

2. Does the paper present novel concepts, ideas, tools, or data?

The paper uses a new and potentially improved dataset of Greenland bedrock and ice surface elevation, and applies sophisticated climate data in combination with a well tested ISM (Glimmer). Results are compared to existing model results from older elevation data (using both Glimmer and other ISM) as well as observation data for ice thickness)

3. Are substantial conclusions reached?

Consistent and helpful conclusions are reached. However the fact that using "improved" datasets result in considerably "worse" simulations of today's GrIS strongly implies that for these datasets, some of the assumptions and abstractions applied in ISM (Glimmer and others) might not hold, for example the lack of higher order physics to represent fast flowing ice, the lack of basal melting or sliding, the limited resolution modelling the ablation process, etc. While the authors mention these points, I feel that the consequences might be more important and far-reaching, and should be explored more elaborately.

4. Are the scientific methods and assumptions valid and clearly outlined?

All methods are clearly defined and well documented. Considering the length of the paper, I would be tempted to reduce the description of the Glimmer ISM as well as the EISMINT experiment setups even more, as they have been subject of a number of papers which are readily available to the reader.

5. Are the results sufficient to support the interpretations and conclusions?

Results are in general sufficient to support the stated interpretations and conclusions, in fact I find the conclusions to be a bit too brief and superficial. While the approach using LHS to determine the best-fit parameter set is valid and innovative, the results suggest that ISM such as Glimmer, that use a number of abstractions and simplifica-

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tions and run on relatively large scales, might not be a valid choice for the modelling ice sheets at the given spatial and temporal resolutions. While the shortcomings of these models are acknowledged, alternatives are not being discussed, and it is only suggested that with increasing computational capacities models using higher order physics, which can be run at higher resolutions and capture fast flowing ice, will solve the problem. A number of alternatives, such as finite element modelling, subgrid modelling for calculation of ablation, methods of calculating mass balance other than using PDD etc are not being discussed. The fact that assumptions used in current models result in pronounced deviations with observation in the field might also hold consequences for scenarios and forecasts made in the past using these models. Additionally, it could be concluded that the results presented in this paper show that topography and it's inherit uncertainty appear to have a significant effect even on large scale models of considerable abstraction. It should be discussed whether the use of more "realistic" topography and climate data is sensible, where the degree of abstraction (in terms of ice dynamics and topographic resolution) of a model is rather high .

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes

8. Does the title clearly reflect the contents of the paper?

I would personally change the title to reflect the topic and results of the paper a bit more concisely

9. Does the abstract provide a concise and complete summary?

The abstract is very well done and should only be adapted where the findings and

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discussion of the paper will hopefully be extended.

10. Is the overall presentation well structured and clear?

The paper is well written and structured, but the overall length is relatively large, which makes it difficult to follow in parts. I would prefer it to be more to the point. An overview of the conducted experiments, for example in the form of a table, would probably make it easier for the reader to anticipate the content. This might also help to cut down the overall size of the paper a little. I would suggest revising some of the more elaborately phrased sections to be more concise. The conclusion and discussion section is a relatively short and repetitive, because results from the different experiments are subsumed. Consequently, the actual yield of the discussion of the discussion is below my expectations. Are rather long paragraph of section 3 (datasets) is devoted to explaining why the newer parameterisation of climate date by Fausto et al 2009 has not been used. I would move this explanation to the discussion.

11. Is the language fluent and precise?

In some places, I would prefer the wording to be more concise.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

As mentioned above, I think description of model physics and EISMINT setups could be shortened. Consider revising the captions of figures 8-10 to make them even clearer to the reader. Please make sure that all maps in Fig 12 are using the same projection and are scaled alike. (Fig 12a appears “squeezed” compare to Fig 12b-f)

14. Are the number and quality of references appropriate?

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Yes

15. Is the amount and quality of supplementary material appropriate?

Yes

Additional questions: Are you sure that the variables (e.g. parameters) used within LHS are independent of each other? Values are chosen independently, but in reality PDDs for ice and snow and lapse rate might not really be independent of each other...

Interactive comment on The Cryosphere Discuss., 4, 233, 2010.