**The Cryosphere
Discussions**

Interactive comment on "A computationally efficient model for the Greenland ice sheet" *by* J. Haqq-Misra et al.

Anonymous Referee #1

The manuscript entitled "A computationally efficient model for the Greenland ice sheet" by J. Haqq-Misra and colleagues presents a one-dimensional model of the Greenland Ice Sheet and describes the advantages of using this kind of simple and computationally efficient model to analyze the sensitivity of the ice sheet to a large number of parameters, and their relevance in Integrated Assessment Models of Climate Change.

The manuscript is well written, generally clear, concise and well-structured and the figures appropriate. The manuscript first gives a detailed overview of ice sheet modeling and describes the one-dimensional model they are using. The parameterization and calibration of some experiments are also described. Results and discussion are then presented but are very limited and no scientific conclusion is reached here. The one-dimensional model is improved from an earlier model already published and only its

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parameterization is changed. The authors stress that this kind of model is fast and can be used to run large numbers of simulations but they do not take advantage of this capability to run large ensemble runs and provide some scientific results. Despite all the details provided, some aspects of the model remain unclear (see general comments and specific points below) and the pre-calibration section is confusing.

Overall, this manuscript details an already existing and published model that the authors improved with additional physical parameterizations. They do not include any new method, physical processes or scientific results and do not take advantage of the computational efficiency of this model. I therefore recommend this manuscript not to be published until some additional experiments are made and scientific conclusions are reached and I encourage the authors to improve the description of model set up as well as the discussion.

1 General comments

My main concern about this manuscript is that there are no new scientific results or applications. The model is an improved version of an already existing model, GRANTISM, which relies on Microsoft Excel (http://ftp.vub.ac.be/~fpattyn/grantism/welcome.html). GRANTISM is an extremely simplified ice sheet model. The model presented here is another implementation of same equations, with additional parameterizations. However it does not introduce new processes or major improvements. I understand the importance of improved parameterization and the difficulty of implementing new ice flow models, however, the simulations presented here are based on previous results, limiting the effort needed for both model development and set-up. I am aware of the experiments described in the pre-calibration section, but do not see any conclusion from these experiments other than parameters chosen to configure the GLISTEN model. What do you conclude from the diversity of parameters found for the seven experi-

ments? How do you explain the differences in parameters found with SICOPOLIS? I do not see what information you get from the pre-calibration. I also do not understand why the authors did not run additional examples and draw some scientific conclusions. This is all the more surprising given that the efficiency of this model is emphasized throughout the text, as well as its ability to provide "extensive analysis of the Greenland Ice Sheet behavior across a wide range of relevant parameters". Why not use this capability to obtain some scientific results? Consequently to these remarks, I think the manuscript should be shorter in the general description of ice sheet models (that is very clear and well described) and develop more the results and discussion parts.

In contrast to the rest of the manuscript, the organization of the "Pre-calibration" section is confusing and many details regarding the model set-up and experiments are missing. For example, it is not clear how the model is initialized, how long the runs are, what time steps are used, what the ranges for each parameter are, what values are used for these parameters, how many simulations are done, what is meant by constraints or how these constraints are used. Most of this information is present in the text, but the path followed is not clear for the reader, who gets lost in the section and is not able to understand and appreciate the work being done here. I would therefore recommend a complete rewrite of this section.

Finally, I am very skeptical about the accuracy of a one-dimensional model to represent an entire ice sheet. Many previous studies demonstrated that a flow-line model could provide a good representation of a basin, especially when using the centerline of a glacier but this approach has limitations (see e.g. Sergienko [2012]). However I am not convinced that such a model could represent the variety of situations of the Greenland Ice Sheet, when major features like the Jakobshavn Isbrae or the Northeast Ice Stream are not taken into account and the model does not follow flow-lines of the major basins. How would changes affecting only one part of the continent be represented in such a model (for example warmer ocean or enhanced sliding in the south)? It is also not clear how changes on this one section can translate into volume evolution of all the

Greenland Ice Sheet. For all these reasons I am questioning the ability of a one-dimensional model to provide realistic results when modeling the Greenland Ice Sheet.

2 Specific points

I think describing the model as a "one-dimensional flow-line model" is misleading as the section used does not follow a flow-line but is based on a east-west cross section at 72° latitude. Using "one-dimensional model" would be more appropriate.

p.2753 l.16: Guidelines of The Cryosphere indicate that reference should not be included in the abstract unless urgently required (http://www.the-cryosphere.net/submission/manuscript_preparation.html).

p.2753 l.21: You could also cite Seddik et al. [2012] and Price et al. [2011] that are mentioned later in the text.

p.2754 l.4: Acronym for GRANTISM?

p.2754 l.5: Typo: "model model"

p.2754 l.27: "power-law for fluid flow" is not very accurate, consider rephrasing.

p.2755 l.6/l.10: "solution" is used for both the ice flow equations (approximation) and the mathematical method to solve them (finite elements, finite differences, ...). A clearer distinction should be made between these two aspects.

p.2755 l.26-28: Not very clear, consider rephrasing.

p.2756 l.2: If you want an exhaustive list of processes, you should also mention grounding line migration.

p.2756 l.8: The enthalpy method developed by Aschwanden et al. [2012] in PISM is another possibility to include temperate ice.

p.2757 l.23: Results by Schoof [2010] show in some cases that more melt water leads to more distributed channels under the ice and therefore less sliding.

p.2757 l.24: Are you talking about air or ice temperature?

p.2758 l.4: As mentioned earlier, I would not use the term "flow-line".

p.2758 l.13: Is this the general mass transport equation? In this case you should use vectors (bold characters) for the velocity (same for basal velocity and driving stress later on) and explain how you go from 2d to 1d.

p.2758 l.13: It is generally not clear how you derive the 1d model: there is only one component of velocity but you do not follow a flow-line, so how do you account for changes in the second direction?

p.2759. l.8: You mentioned earlier that the sliding coefficient was temperature dependent. How do you go from temperature to surface mass balance parameterization?

p.2759 l.14: This is the definition of the driving stress.

p.2761 l.22: I would suggest using consistent units for the temperature in all the manuscript, either K or °C. Furthermore, this equation is not very clear, is the thermal response instantaneous?

p.2762 l.5: between → sum of.

p.2764 l.2: What is ϕ in the equation? It was defined as $\phi = 72°$ in eq. (21) and (22).

p.2766 l.15: I am not sure how you go from results on a 1d cross-section to the Greenland Ice Sheet volume? How accurate is this conversion if changes are not uniform through the ice sheet?

p.2766 l.10: What do you mean by constraints? Do you force or impose the model to respect those values or is it just a reference to compare your results?

p.2766 l.11: Not clear what "locations" you are using? Are you referring to basins or

epochs?

p.2766 l.22: You compare results from your model and GRANTISM with SICOPOLIS and conclude that you model has a better fit. But it seems that this was part of your objectives while I am not sure that it was one for GRANTISM. In this case, how can you compare results of those two models?

p.2767 l.8: I suggest defining the loss function here and not earlier.

p.2767 l.9: Does the solid black line represent the constraint as mentioned in the text or GRANTISM result as described in the figure and its caption?

p.2767 l.12: It is not clear for how long and with what time steps you run your simulations. This should be specified before defining your "constraints".

p.2767 l.25: "optimal parameter": How many runs did you do? What range and values of parameters did you use?

p.2767 l.26: "first constraint" → "first set of constraints" (same for second and third constraint on p.2767) as each of those constraints is actually composed of several conditions.

p.2769 l.10: What are the limitations of this model? What is the effect of choosing one particular cross-section? Would results and parameters be similar with another West-East section or with a North-South section? These are questions I would have liked to see mentioned in the discussion.

p.2782 Table 4: What do T and F stand for?

p.2783 Figure 1: I would suggest using "Sliding" and "Internal deformation" instead of "At the bed" and "Near the bed" or something equivalent.

3   References

Aschwanden, A., E. Bueler, C. Khroulev, and H. Blatter, An enthalpy formulation for glaciers and ice sheets, J. Glaciol., 58, 209, 441–457, doi:10.3189/2012JoG11J088, 2012.

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Schoof, C., Ice-sheet acceleration driven by melt supply variability, Nature, 468(7325), 803–806, doi:10.1038/nature09618, 2010.

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