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Discussions

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

Anonymous Referee #2

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This paper presents a modified version (GLISTEN) of an existing flowline model of the Greenland ice sheet (GRANTISM) to make ensemble runs of the Greenland ice sheet over the last glacial/intergalcial cycle. The GRANTISM model was originally a classroom model meant to explain the mass balance - elevation feedback of ice sheets and the resulting hysteresis (Pattyn, 2006). It was based on sound physical principles, approximating the Stokes flow through the Shallow-Ice Approximation (SIA); ice-temperature coupling was introduced in an ad-hoc way. The GRANTISM model was calibrated against a few model runs (Lettreguilly, 1991) and the classroom exercises, as presented in Pattyn (2006), existed in determining ice sheet response to perturbations in atmospheric temperature. Therefore, mass balance was crudely parameterized based on the aforementioned study.

GLISTEN is a modified version of GRANTISM. It does not touch upon the physics and numerical implementation of GRANTISM, but focuses on adapting the boundary conditions, especially surface mass balance, where surface melt is introduced through the use of PDDs. This is a major improvement and adds realism to the model. Another major improvement is a thermomechanical coupling scheme that is based on more sound physics. The englacial temperatures are represented by a thermodynamic calculation of a time-dependent diffusion equation for englacial temperatures. Besides this, some adaptations are made to the basal sliding parameterization (see also remarks below). However, the purpose of GLISTEN has changed completely compared to GRANTISM. This is Âănot a classroom model anymore, but a scientific tool meant to determine ice volume variations and sea-level contributions on glacial-interglacial timescales, and this shift of scope changes the way the model results should be evaluated.

Besides the improvements in boundary conditions, the GLISTEN model is still a one-dimensional flowline model on a coarse grid size along and East-West section of the Greenland ice sheet, roughly situated along the 72°N parallel. The model is therefore unable to capture any spatial variability of the extension of the Greenland ice sheet over glacial-interglacial cycles, and to overcome this, the authors match the parameter settings in the model (free parameters related to uncertainties in basal sliding prescription, flow law, surface accumulation, etc.) to match the volume variation curve obtained with a 3D model simulation of the whole Greenland ice sheet, based on SICOPOLIS. They obtain a good match by doing so and compare these results with the original GRANTISM settings, forced by the same environmental record. The results are striking different, but the main reason why the results are so different is not further elaborated upon by the authors. Pattyn (2006) introduces a different parameterization with respect to sea level. This is an ad-hoc representation of grounding line migration as a function of paleo sea-level stands. It permits the ice sheet to expand laterally along the continental shelf during glacial periods (as a classroom example). However, the volume changes this represent are not scaled up according to the scaling with the SICOPOLIS model, which makes a direct comparison irrelevant. The lateral expansion also explains the large jumps that can be seen in Figure 2 (lower panel). Nevertheless, if the authors would have turned this option off, the discrepancy between GLISTEN and GRANTISM could be different.

The major advantage of using the GLISTEN model is its calculation speed. The model is very fast and numerically coded in such a way that it permits the use of large time steps, similar to GRANTISM (Pattyn, 2006). However, there are a number of issues that makes it unclear whether such simplifications are really needed and whether they are valid where they are applied. The paper does not explain adequately how many model runs were needed to determine the optimal parameters according top the SICOPOLIS run and the 7 experiments, nor how much time it takes to do just one run. With the current available computer power, the need for such simplicfication could easily be outrun by the time it takes to run a simplified, similar 2D ice sheet model. It is not clear what the scope is of the paper. After the 7 runs in which the best fit is found according to several limiting conditions, a brief discussion is given followed by a conclusion that does not support the results that were presented in the paper. At least, it is not possible to make these conclusions hard based on the experiments carried out (or it is not clear to the reader) As far as i can get it from Figure 5, GLISTEN uses a rather crude representation of the bedrock topography, which is not the same profile as depicted in the cross section given in Figure 1. Is such a flowline representative for the Greenland ice sheet? What would be the result if the bedrock was taken with some more detail? How to represent latitudinal variations in ice extent, which are much more important oer Greenland than laongitidinal variations (remember, GRANTISM is a classroom model)? Would a simple 2D model not be better? Of course the numerical scheme should be adapted, but a 2D plane-view model based on SIA could also run fast. Although some improvements are made to the dynamical control (sliding, see remark below), the main driver within the model remains mass balance (accumulation and ablation) and the mass-balance/elevation feedback. Results should therefore be interpreted in that context and don't expect to get something different out. The response of the Greenland ice sheet to climate (mass balance change) is largely a function of the applied forcing.

Forcing with a different atmospheric model will yield different results, as shown by many studies in the literature, and such spatial difference cannot be captured by the flowline model? A good fit/tuning with SICOPOLIS does not mean that the model can represent the paleo record well. SICOPOLIS is also an SIA model with simplified dynamics at the edge (what about sliding, calving ?), and as mentioned above, it depends on the way the forcing is introduced. If it is done in the same way (without the use of atmospheric model coupling), it will probably yield a similar result. There is a lot to do about the so-called Zwally effect. Recent literature (not mentioned in the paper) have shown that the Zwally effect may well play a minor effect in the dynamics of the Greenland ice sheet with local speedups in summer. Studies, such as Sundal et al. and Schoof emphasize the importance of basal hydrological characteristics in temporary speedups, which are not simply related to the amount of meltwater that is produced at the surface and can therefore not be directly linked to surface melt. Furthermore, the incorporation of the Zwally effect, by increasing basal sliding by 10% in the ablation area (Equation 4) may well reflect a different process than initially meant to and just highlight the difference between accumulation and ablation area: summer melt is relatively unequivocal on Greenland, but results in a runoff at the surface in the ablation area and a refreezing in the accumulation area (reducing the mass loss there). The parameterization may therefore reflect this process instead of local speedup due to basal lubrication. It may also be a hidden way to represent calving processes. Therefore, the parameterization is not a clear representation of the Zwally effect, but a simple way of enhancing ice flow in the ablation area due to a series of processes that are not further detailed here. Based on these comments, the paper should be seriously reworked before any resubmission. GLISTEN maybe a computationally efficient model, but maybe not be representative for the Greenland ice sheet (this has not been shown). It is fast, but it has not been demonstrated why it needs to by that fast. Below, i listed a number of remarks that may aid in improving the manuscript.

Further comments

The introduction is too long. This is not a brief overview of ice sheet models, on the contrary. There is no need to go in detail over the recent (sic) developments in ice sheet modelling and the use of higher-order physics. Moreover, there is a serious bias in the literature, thereby hardly pointing to model studiees and numerical model development with respect to the inclusion of processes such as basal sliding and calving, as well as with respect to operational models according to different approximations to the Stokes flow. The authors should directly try to answer the question: despite the fact that IPCC 2007 states that current ice sheet models lack a lot of physics and appropriate processes, why can we continue anyway with a model that lacks the physics? The statement on p2753, line 22 is not an answer to that question, although it is put forward that way. I don't mean that IAMs are not useful, they are not a cover for the lack of representation of a series of physics.

The temperature calculation is based on a diffusion equation. Horizontal ice advection is not taken into account. A discussion should be given on how valid this assumption is. Phi in Equation 28 is not defined. Is this in the vertical or horizontal (should be vertical coordinate, but this contradicts with what is written on p2764, line 8).

Data: not clear what flowline data are used. I would recommend to use most recent bedrock elevation data (Bamber). Data are reposited.

Pre-calibration: It is unclear how this calibration is actually done. For people that are not well familiar with loss functions, this part is written with a lack of detail (contrary to the section on the ice sheet model, where a lot of detail is given). For instance, it is not clear how the volume of ice in the flowline model is upscaled to the SICOPOLIS model. There is a comparison with GRANTISM, but are free parameters in that model tuned as well to yield an optimal result? It should be, if a comparison is made.

The experiments 1-7 are only briefly described, and it is not clear what their purpose is. The reader is quickly (too quickly) brought to the discussion and conclusions, which are very brief (not worth a discussion).

Appendix: GLISTEN does not include the Zwally effect. There is no reason to believe that the parameterization represents this effect. I wonder what the effect in Tms is over the Greenland ice sheet following the remark p2771, line 8). Does a truncation of a 0.0036°C make a difference in the response of the ice sheet? The conclusion of that same section cannot be drwan based on that information. If GRANTISM is tuned the way GLISTEN is tuned (even with less sophisticated boundary conditions), one could have different results. This is comparing apples and pears.

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