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Dear Dr. Larour,

Thank you for sending our manuscript on a simplified model of the Greenland Ice Sheet out for review. The reviewers' comments have helped us sharpen our presentation of the material in the paper. We have revised the manuscript according to their suggestions, and we hope that the updated manuscript will be suitable for final publication in The Cryosphere.

The revised manuscript 1) more clearly acknowledges our debt to the earlier GRANTISM model by Frank Pattyn, 2) shortens the introductory material on ice sheet processes and integrates it with the model description, 3) provides a better description of our model calibration procedure, and 4) explains why this work is needed, both for integrated assessment modeling and in the context of ongoing ice sheet model-based sea level rise projection projects (e.g., seaRISE; Bindschadler et al., in review 2011, Journal of Glaciology). These changes are direct responses to the reviewers' comments.

Our precalibration experiments show, perhaps for the first time, that a one-dimensional ice sheet model reproduces the aggregate behavior of a three-dimensional model (SICOPOLIS by Ralf Greve; sicopolis.greveweb.net), and matches data on the ice sheet's past behavior. The calibration experiments required about 10^9 model years each, emphasizing that GLISTEN provides a fast tool for parameter sensitivity studies in integrated assessment models. Moreover, GLISTEN may be the only ice sheet model that is intended for future projections of Greenland Ice Sheet mass loss and has been explicitly calibrated against data on the ice sheet's evolution over time. Most ice sheet models are calibrated solely against the modern state of the ice sheet, and this practice raises questions about whether these models give reasonable results when they are forced away from the present climate. Thus, our results bear on the ongoing discussion about how best to spin up and calibrate ice sheet models. We argue that these results constitute a publication-worthy scientific contribution.

Please note that the title of the paper has changed. The title of the discussion paper was, "A computationally efficient model for the Greenland Ice Sheet." The new title is, "Pre-calibration of a simple Greenland Ice Sheet model for use in integrated assessment studies." The title change helps give credit to Frank Pattyn, whose model we largely copy. It also emphasizes our real contribution, which lies in calibrating the model against time-dependent data.

Thank you again for considering our work for publication in The Cryosphere.

Sincerely,

Patrick Applegate
and co-authors

The reviewers' comments follow. Specific points within the reviewers' comments that require a response are highlighted. Our responses are indented and shown in blue, as in this paragraph.

Reviewer #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 onedimensional model is improved from an earlier model already published and only its 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.

> The reviewer correctly notes that the discussion paper did not explain the calibration experiments clearly. We have rewritten this section.
>
> The reviewer also argues that we do not take adavantage of the updated model's speed in this paper. We disagree, but acknowledge that the discussion paper did not clearly explain the nature of our contribution. The revised text notes that the calibration experiments required 8,000-11,000 model runs covering 125,000 years of model time apiece (so, more than 1 billion model years per calibration experiment). Carrying out this number of model runs with a three-dimensional ice sheet model would require years to decades (Fig. 1 in the revised text), whereas GLISTEN performs these calculations in just a few hours.

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 experiments? 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.

> The reviewer inquires, quite reasonably, why Table 1 in the discussion paper shows such different parameter results for the different pre-calibration experiments. In looking at the discussion paper, we concluded that the seven different experiments made the pre-calibration section needlessly confusing. We now show just two of the experiments from the discussion paper. The first of these two experiments demonstrates a good agreement between GLISTEN and SICOPOLIS (bottom panel of Fig. 2 in the revised text). The second experiment (Figs. 3-5) shows that GLISTEN can reproduce assessed ice volume changes from Alley et al. (2010), historical mass balance data from Rignot et al. (2008), and the modern ice profile (Letreguilly et al., 1991).
>
> As we now explain in the text, there is no reason to expect the parameter values for precalibration to SICOPOLIS to match those from calibration to the ice sheet's past behavior, as we know it from data. The particular ice sheet model run that we use (#29 from Applegate et al., 2012, The Cryosphere) was chosen solely based on its good match to the estimated volume of the modern ice sheet; thus, this SICOPOLIS run agrees with data on the ice sheet's past behavior only "coincidentally." This calibration procedure is broadly similar to that used in the seaRISE assessment (Bindschadler et al., in review 2011), which also calibrated its ice sheet models only against the modern ice sheet. Moreover, the work of Applegate et al. (2012) shows that broadly similar modern ice sheets can be obtained with very different parameter combinations, due to tradeoffs among parameters. We explain these complicating factors in the revised text.
>
> As the reviewer asked, we shortened the general discussion on ice sheet processes and integrated it with the model description.

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.

> As the reviewer says, the details of the precalibration experiments were not clearly described in the discussion paper. We have revised this section completely. All of the information that the reviewer asks to see is now in Section 3.1 and Table 1.

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 onedimensional model to provide realistic results when modeling the Greenland Ice Sheet.

> The reviewer correctly notes that a transect through the ice sheet is not necessarily representative of the whole ice body. But, the match between the profile model GRANTISM and a full, three-dimensional model is fairly good, even during transitions between climate states. This correspondence suggests that an appropriately-chosen transect can describe the full ice sheet's behavior rather well. We plan to experiment with different transects in the future, to see if other transects provide an even better representation of the full ice sheet, and if so, what the significance of latitude for this representativeness is.

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.

> We have deleted the offending terminology from the text.

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).

> Done.

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

> Done.

p.2754 l.4: Acronym for GRANTISM?

> Clarified. We now explain GRANTISM at its first occurrence in the text.

p.2754 l.5: Typo: model model

> Fixed.

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

The revised sentence reads, "Ice deformation is proportional to the third power of the applied stress (Glen, 1957)," which should be accurate.

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.

The word "solution" now appears just once in the text, so its meaning should be unambiguous.

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

Fixed through rewriting.

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

This process is now listed in section 2.2.5.

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

Done. See section 2.2.3. We included a citation of Aschwanden et al. (2012)

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.

Yes. We now give more background on glacial hydrology in the text, including a reference to Schoof and preceding work.

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

We replaced instances of "temperature" with "surface air temperature" where that wording was appropriate. The revised text should be much clearer in this regard.

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

Fixed. This terminology has been removed from the paper.

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.

This equation is no longer in the paper. Because the ice flow treatment in GLISTEN is largely the same as in GRANTISM, we now refer interested readers to the Pattyn (2006) paper.

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?

Again, this material is no longer in the paper. See Pattyn (2006).

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?

> We rearranged the model description to have a more logical order. See section 2.2, which clearly explains the relationship between temperature and surface mass balance.

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

> Yes. The text now reads, "The driving stress tau_d is defined as..."

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?

> We sympathize with the reviewer's desire for consistent units. However, degrees Celsius are standard in meteorology, whereas Kelvins are used in materials science; as an interdisciplinary science, glaciology uses both. We found the paper became more confusing when we tried to carry out this suggestion.
>
> Section 2.2.2 now states clearly how each of GLISTEN's heat transport treatments handles the time-dependent behavior of heat transport. Temperatures within the ice body change instantaneously in response to surface air temperature changes; temperatures at the base of the ice sheet evolve diffusively.

p.2762 l.5: between ! sum of.

> This sentence was unclear in the discussion paper. The new sentence reads, "... the surface mass balance is the difference between the rate of mass addition by snowfall and the rate of mass loss from melting, sublimation, and wind erosion."

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

> In reviewing this expression, we are also uncertain what phi stands for here. We have deleted it from Equation 3 in the revised text.

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?

> Section 2.2.6 now describes this conversion explicitly. We use a simple proportionality of transect ice area to ice volume. As we now state in the text, this approach is probably OK for small changes in the ice sheet, but we expect it to break down for large changes. Given the good matches to three-dimensional ice sheet models and paleo-data in Figs. 2 and 3, however, this approach may be fairly robust.

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?

Section 3.1 now describes our calibration approach in much more detail than we gave in the discussion paper. We adjust model input parameters until we find a combination that minimizes the distance between model output and observations, as calculated using a penalty function.

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

The word here refers to "locations in time," which is confusing. We removed this language from the revised text.

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?

Both reviewers argued that comparing GRANTISM to GLISTEN and SICOPOLIS is not reasonable, so we deleted this comparison from the revised manuscript.

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

This section has been completely rewritten.

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?

In the revised paper, the black line in Fig. 5 definitely represents the gridded ice thickness data from Letreguilly et al. (1991). We acknowledge that this point was unclear in the discussion paper.

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 .

Done. See Section 3.1.1 in the revised text.

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

These points are now much clearer in the revised text. See Section 3.1.

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.

We have been careful to use the word "constraint" in line with the reviewer's wishes in revising the paper.

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.

Unfortunately, we have not yet investigated how the choice of model transect, or grid spacing, affects the results we obtain. We mention these points in the discussion as avenues for future work.

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

T and F stand for "incorporated as a constraint" and "not included as a constraint," respectively. Because we now have just two experiments, this table is not included in the revised ms.

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

We deleted this figure from the revised manuscript, because not all the processes described in the manuscript are reflected on the figure.

Reviewer #2

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); icetemperature 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 not 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 onedimensional 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.

> As noted above, we removed the comparison between GRANTISM and GLISTEN in the revised manuscript.

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.

> We now report clearly how many model runs are required per calibration experiment (8,000 to 11,000, covering about 125,000 years per model run). The time per run is quite short, just a few seconds for a single run covering 125,000 years.

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 noted above, we have rewritten the latter half of the paper, including the discussion, which is now much longer. We feel that the discussion is now better supported by the material that comes before.

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)?

> The reviewer is correct, much smaller grid spacings could be used in the model. We have listed this item as a point for future work in the discussion.

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.

> Naturally, a map-plane, vertically-integrated model could have important advantages over the profile approach that we use here. We would expect such a model to be substantially slower, just based on the number of grid cells; with a 36-km grid spacing, a map-plane model for Greenland

would have about 77 grid cells in the north-south direction, based on the extent of the SICOPOLIS domain. Thus, we expect a map plane model to be about 77 times slower than a profile model, again assuming a grid resolution of 36 km. The finer the grid becomes, the greater the profile approach's speed advantage becomes over a map-plane model with the same spacing.

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?

The new discussion acknowledges that the ice flow treatment used in GLISTEN is highly simplified, and that the model primarily represents changes in the ice sheet's surface mass balance. We do not use a climate model, but we are well aware of earlier studies that investigate the sensitivity of ice sheet model results to temperature and precipitation fields from different climate models. The classic reference is Pollard et al. (2000, Global and Planetary Change).

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.

The reviewer's statement is true, but we also match the model results to estimated ice volume changes during the Eemian and the Last Glacial Maximum. The good agreement between this data and the calibrated model is stronger evidence that the model reproduces the paleo-record well. We acknowledge that our justification of this work in the original paper was lacking, and we hope that the updated version will provide a better explanation of why a match to SICOPOLIS is desirable in the context of integrated assessment modeling.

There is a lot to do about the socalled 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.

Yes, this is true. We include a fuller description of the Zwally effect and the literature surrounding it in the revised text. We also acknowledge in the text that our parameterization is really a qualitative representation of many processes that might accelerate ice transport near the margins and that cannot be described by shallow-ice approximation methods.

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.

We shortened the introduction, based on comments from both reviewers. This revised introduction provides a more complete justification for the work we do here.

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.

We have attempted to provide a summary of the literature as we understand it, but we have undoubtedly missed some studies. We would be grateful if the reviewer would point out the papers we have not included.

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.

As we make clear from the title, "Pre-calibration...," this manuscript represents a first step in the use of GLISTEN for integrated assessment studies. We simply do not know yet how important higher-order ice flow dynamics are to the behavior of the ice sheet. We will not know the answer to this question until we have a model with the appropriate dynamics that has been calibrated against time-dependent data on the ice sheet's behavior, not just a single time slice representing the present-day. To our knowledge, no such results from a well-calibrated ice sheet model have been published. Moreover, when such a model becomes available, it will be too expensive to run in an integrated assessment context -- but GLISTEN, or a similar model, can be calibrated to the complex model's output. Thus, GLISTEN can and should develop in parallel with three-dimensional, higher-order ice sheet models.

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.

Done. See section 2.2.2 in the revised text.

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).

> The other reviewer noted this problem, also. We have fixed it in the revised text.

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

> We now make clear that we are using the old, Letreguilly bedrock elevations and ice thicknesses. This is an obvious weakness of our modeling effort, one that we will rectify soon. In the meantime, the model still emulates SICOPOLIS (which uses the updated bedrock topography) and matches data on the paleo-behavior of the ice sheet.

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).

> We have rewritten and expanded the whole latter half of the paper, and we believe that our description of the pre-calibration experiments, and the discussion, are now more satisfactory. A new section, 2.2.6, explains how we scale from the profile area to the ice volume.

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.

> We have largely addressed these comments above.