Effect of higher-order stress gradients on the centennial mass evolution of the Greenland ice sheet

Reply to List of Comments

by J.J. Fürst, H. Goelzer and P. Huybrechts

First of all we want to thank the reviewer for the critical and useful comments he/she gave on the manuscript. All comments are considered and helped to improve the quality of our work. In the following the responses to the reviewers comments are denoted in italic and are indented.

Review 2:

General comments

Fürst et al. summarize an ambitious effort to implement a number of different lower order approximations to the nonlinear Stokes equations for ice flow within a single modeling framework, and use those different models to simulate mass loss from the Greenland ice sheet for several dynamic perturbation experiments. The resulting spatial and temporal patterns of mass loss are then compared and contrasted between models, and for different grid resolutions, in order to better understand the differences in ice sheet evolution that result from the different dynamic approximations and grid resolutions used. There are two main conclusions from the paper, (1) models with increasing levels of sophistication at including membrane stresses increase the rate at which marginal perturbations propagate inland (while decreasing the overall timescale for the ice sheet to adjust to the perturbation) and, (2) overall, the long-term (100 yr timescale) mass loss from the ice sheet predicted by models that include membrane stresses differs only slightly from the mass loss predicted by models that do not include membrane stresses. The first conclusion is probably not that surprising, as this has been pointed out as a deficiency of standard "shallow-ice" models numerous times during the last ~5 yrs (e.g., in the last IPCC report). The latter conclusion, which the authors attribute to the fact that the ice sheet response to dynamic perturbations is dominated by the diffusive inland propagation of changing ice sheet geometry, is a bit more surprising and potentially of more significance (e.g. with respect to current model development efforts that are inching continually closer to using Stokes models as the norm). In general, this is an interesting and worthwhile study with conclusions that are relevant to the large-scale, prognostic modeling of ice sheets (e.g. it begs the question, are shallow-ice models "mostly" adequate for simulating the large-scale, centennial scale evolution of ice sheets?).

There are a few technical issues of concern, which are discussed further below. These can probably be addressed by the authors during the revision stage. Of somewhat more concern to me is that the paper is quite long and difficult to follow at times, containing repetition and what seems to be a lot of unnecessary equivocation. The writing is often very verbose, obscuring the meaning of a sentence. For example, from the conclusions section:

"The inclusion of membrane stress gradients in various ways in fact reduces the volume response by 20% at most from different inland propagation of marginal perturbations."

This is followed by,

"Models that include direct horizontal coupling are capable of attenuating perturbations faster by instant upstream propagation."

> ***Corrected.*** *Removed this example of repetitive style.*

I understand the second sentence, but not the first. Is the first just another way of stating the second? If so, is the first even needed? There are numerous similar examples throughout the text and after a while it became a bit maddening to try and sort out what was important to read from what was not.

The paper is not poorly written, but it does seem to be "over" written in many places. My suggestion for fixing this would be to have a native English speaker go through the paper carefully and subject it to some very heavy-handed editing. I suspect the length could be reduced by 20-30% while simultaneously making the paper much clearer and easy to read.

The manuscript was subject to intense editing by the authors with special focus on avoiding repetitions and removing secondary information. With this the main text could be reduced by another 13%. The reviewer expected a higher reduction because he/she also envisioned reduction of the amount of presented model versions, which was not adopted here (reasons for this are explained later).

I found the way that the different models were categorized confusing and non-intuitive. For example, the short-hand labels (DR SIA, ME HO, etc.) seemed backward to me. Shouldn't the overall stress approximation used by the model come first, followed by the basal boundary condition approximation (e.g. SIA DR, HO ME, etc.)? More importantly, some of the combinations seemed nonsensical to me. I believe that the "SR SIA" variant includes the shallow-ice approximation within the ice but uses a sliding law requiring the full 3d, higher-order solution. Certainly one *could* do this, but why would one want to? Similarly, one *could* simply use the driving stress in the basal boundary conditions for a higher-order model (DR HO), but if you've gone through all the trouble of creating a higher-order model, why would one then skimp on the basal boundary conditions? One could argue, as in the paper (although not very clearly), that the motivation is to parse out the different aspects of the horizontal coupling (i.e. how much is due to coupling in the sliding vs. how much is due to coupling in the internal deformation), but my impression was that the conclusions when trying to do so were a bit muddled.

Concerning the notation, we opted to state the approximation of the basal condition first since our results indicate its dominant role in the ice sheet response. Differences arising from the approximation used for ice deformation are secondary and thus got the second position. In addition, section 4.3. on the 'decomposition of mass loss' quantifies the effect from longitudinal coupling either in the basal layer or via ice deformation. Though this quantification was not conducted in terms of total total mass loss, it gives a handle on their relative importance and highlights the sensitivity of the dominant sliding adjustment to direct horizontal coupling.

I would have been happy to see the discussion limited to models that are actually used in practice, which I believe are DR SIA, ME SIA, and SR HO. This would also greatly simplify the terminology, since one could refer to the models as simply "shallow ice", (something like) "Bueler and Brown" (ME SIA), and "first-order" (or "Blatter-Pattyn"), which are the more commonly used descriptions for these same models.

The reviewer has a point that most readers will be interested in the results from the model versions commonly in use. Therefore we already tried to put the focus on these models as more than half of the figures (i.e. Fig. 1, Fig. 2b,c,d, Fig.3, Fig.6, Fig.7) exclusively state results from these. We think that the document already concentrated on the three most commonly used models and after substantial editing of the manuscript text, we hope that this focus becomes even more evident. However we refrain from removing the other two more exotic models because they are essential when separating effects arising from dynamic differences in internal deformation or basal sliding. The reason why we did not choose the somewhat common labels 'shallow ice', 'Bueler and Brown' or 'Blatter-Pattyn' is that they did not allow us to get a handle on both the treatment of the basal boundary condition and the internal force balance approximation.

I am confused about the lateral boundary conditions implemented in the model and in the experiments discussed. My understanding from what is written is that, for all mod els, the velocity at the margin of the ice sheet is specified according to a shallow-ice like solution (that is governed by the local geometry only). This is explained as an attempt to maintain a similar boundary forcing for all models. But having spent too much of my own time implementing more complicated lateral boundary conditions in models (i.e., that for a grounded or floating marine margin), I'm immediately suspicious that this choice was made simply because the alternative can be quite difficult (and in some cases, quite touchy and non-robust for simulations using real geometry data). Further, I am concerned that the somewhat similar model responses shown here could be an artifact of this overly simple treatment of the lateral boundary conditions; the velocity at a grounded calving front or freely floating ice tongue is going to be dominated by horizontal normal stresses, not vertical shearing stresses. I can imagine how the latter might "damp" the response of a marginal perturbation unrealistically. For example, if sliding is increased near the margin by reducing the basal friction, the ice at the margin will accelerate, thin, and flatten. The latter two effects will greatly reduce the marginal velocity calculated using a shallow-ice approximation, but they would not necessarily reduce the velocity by the same amount for ice at a marine margin, which is deforming largely as a result of horizontal normal stresses. I'm concerned that in their attempt to compare "apples with apples", the authors really are comparing apples with apples by having "turned off" one of the important mechanisms that differentiate models with membrane stresses from those that do not (i.e. horizontal stress transmission at and across lateral boundaries). A simple set of test case simulations would put my mind at ease about this issue. That is, one could compare a prognostic simulation using DR SIA and SR HO (or ME SIA) for which the boundary velocities were specified from the shallow-ice model (as is done in the paper if I'm understanding correctly), with a similar simulation in which SR HO (or ME SIA) use a more appropriate lateral boundary condition, for which horizontal normal stress gradients are applied (e.g. the standard stress boundary condition applied at the front of an ice shelf). If the concern is that the two boundary velocities do not initially match one another closely enough, an additional "backstress" term could be applied to the shelf boundary condition to bring the two marginal velocities into closer agreement. Note that this could all be done for a synthetic and idealized computational domain (e.g. 2d vertical slice with flat bed, simplified ice geometry). Because the majority of the dynamic discharge from the ice sheet in these simulations is through marine terminating outlet glaciers, it seems appropriate that at least some of the perturbation experiments apply a lateral boundary condition appropriate for that physical setting. Either that, or the reader should be convinced that the more simple boundary condition applied gives similar or reasonable results (e.g. through some kind of simple comparison like that outlined above).

The reviewer is right in pointing out that it is the ice sheet margin where one expects the horizontal membrane stress gradients (horizontal gradients in normal stresses) to have the largest impact on the modelled velocity. Therefore we understand the concerns that ice evolution might look very different in a model allowing for direct horizontal coupling. These concerns are reasonable and we can give the following answers. Given that even the maximal 5 km resolution is not sufficient to capture the complex force balance situation near the ice sheet margin, we think that none of the model versions is preferable over another at the first ice sheet point. They all are perturbed by the detailed numerics that are used to solve the stress balance. Numerically all force balance approximations are very different. In order to avoid differences from numerics rather than physics enter the comparison, we opted to use the most simple boundary condition for all of the model versions and focus on the physical differences arising from the distinct treatments of horizontal coupling. However, the MarAsl2 experiment was initially conducted using a full SR HO model also for the margin but the inherently new lateral boundary condition increased the mass loss by 17%, which hampers a clean comparison between the five model versions (same order of magnitude). For completeness we repeated the MarAsl2 experiment on 20km using the SR HO lateral boundary condition while the interior velocities were chosen according to the local DR SIA solution. Comparing the full SR HO response with DR SIA results confirms the differences seen in the comparison using the DR SIA model as a lateral boundary condition. Again the full SR HO model version produces a reduced mass loss and the 2D inland propagation of ice thickness changes compares to the one in the original document even if the overall mass loss is increased. By this we hope we could convince the reviewer that the exact form of the boundary condition does not affect the outcome of our inter-comparison study. In summary, the interest of our study has always been whether a given marginal perturbation is differently transmitted inland for various model versions and by this gives rise to significant differences in the volume evolution of an ice sheet on a century time scale.

SPECIFIC COMMENTS

Page 2963, line 14-18: sync. speed up vs. regionally linked vs. more erratic behavior – these all seem mutually exclusive, but the way this is written it sounds like they ALL apply. Re-write more clearly?

> ***Corrected as follows:*** *'Although outlet glacier thinning and retreat is observed all around Greenland (Thomas et al., 2009), the temporal and spatial picture for the associated speed-ups is far from homogeneous. For some coastal areas, observations point to regionally linked accelerations (Howat et al., 2008 ; Howat and Eddy, 2011) while for other areas, a more erratic behaviour is found (Howat et al., 2010 ; Joughin et al., 2010 ; McFadden et al., 2011, Moon et al., 2012).'*

Page 2964, line 19-20: I think it is safe to say that long. stress grads. are not suspected of leading to efficient horiz. coupling. I think it is safe to say that this is known at this point (from numerous studies over the past ~5 yrs).

> ***Corrected.*** *Reformulated this passage avoiding a comment on the coupling efficiency.*

Page 2964, Line 20-23: Are you confusing "plane-flow" with "plane strain" or "plane stress" here? I don't find "plane flow" anywhere in the literature, but plane stress or plain strain have specific meanings. Which one is it?

> ***Corrected.*** *The expression was replaced by 'flow band'. The word 'plane' rather evokes confusion with the stress terminology as the reviewer correctly states.*

Page 2965, line 5-6: ": : :long. coupling length is expected to increase : : :." Provide some refs for this statement? e.g. Joughin et al. paper on diff sliding laws applied to Pine Island Glacier, Price et al. paper on inland propagation of outlet glaciers and ice streams?

> ***Corrected as suggested.*** *Included two references to one theoretical study from Kamb and Echelmeyer (1986) and one application from Price et al. (2008).*

Section 2.1: This section is a bit wordy/long. I think it could be trimmed down quite a bit.

> ***Corrected as suggested.*** *This section could be reduced by avoiding unnecessary terminology.*

Page 2966, line 11-14: Not sure I follow this or why it is relevant here. Why not just state farther up that you are using a Cartesian coord. system.

> *The statement on the coordinate system should make the reader aware that if one deals with very steep and complex ice geometries, the suggested stresses terminology along the axis of a rectangular coordinate system might not be favourable. It is more a note than essential to the understanding of the presented results.*

Section 2.2: Is the model serial? Parallel? Solution methods? Does it pass standard tests, benchmarks? Some of this could probably go in the appendix if necessary.

> *Most of these questions are addressed in the given reference to Fürst et al. (2010) where the numerical details are presented. Indeed the dynamic core of the model is parallelised for use on several computational cores.*

Page 2969, line 2-4: ": : : assessment of the range of dynamic mass loss in the future." Clarify what you mean here, the actual expected range or the range due to the use of different model approximations?

> ***Corrected*** *as follows: 'These experiments serve mainly as an intercomparison study for the five model versions with different dynamic complexity but they also give indications on the dynamic ice loss within one century.'*

Page 2969, line 6: The first sentence is not actually a sentence.

> ***Corrected*** *as follows: 'An interglacial equilibrium state serves as the standard initial condition.'*

section 3.2, page 2969, line 24 -: Not clear how the spatial extent for the sliding perturbation is applied. Is it a box? An elevation contour? A speed contour?

> *The perturbation is applied in grid boxes neighbouring an ocean box. Independent of the resolution, grid boxes up to 40 km from the marine margin are forced with the same sliding amplification.* ***Adjusted in the manuscript.***

Page 2970, line 3: "signal transmission" is awkward and used throughout the paper. Be more specific about what you mean here.

> ***Corrected*** *by rephrasing the sentence as follows: 'The dynamic complexity of our model versions is expected to influence the inland propagation of geometric adjustments to such a marginal perturbation.'*

Page 2972, line 3-5: I think we need some more information for how surface mass balance is incorporated. Is it a spatially varying field that is held fixed in time? Is it a function of ice sheet surface elevation (e.g. through lapse rates)? SMB feedback is mentioned, but it is not clear how or why there should be a feedback between SMB and elevation.

> ***Added additional information.*** *The reviewer is correct in stating that the mass balance model was not described in detail. Therefore we decided to put an additional explanation in the model description section 2.2. stating:*
>
> *'The surface mass balance model is based on the widely used positive-degree-day/ retention method. As input serve a generic temperature field, which depends on latitude and surface elevation, together with a temperature scaled precipitation field (Huybrechts, 2002).'*

Page 2972, line 11: doesn't "s.l.e." usually refer to "sea-level equivalent"?

> ***Corrected*** *as suggested.*

Page 2972, line 13-15: Because the forcing scenarios are rather arbitrary, I don't think one can really say whether or not the resulting sea level rise is small or not.

> *Removed this comment on judging the magnitude of the volume response in the presented perturbation experiments. Later in the text the volume loss is anyway compared to present increase in ice discharge rates and to results from other studies that performed similar experiment.* ***Corrected.***

Page 2972, line 19-22: I don't see where the 66 mm s.l.e. in 100 yrs comes from. In equilib. You have 0.66 mm s.l.e. / yr, so multiplying that by 100 yrs gives 66 mm total in 100 yrs. Where does the "doubling" come in? Something is not being explained correctly here.

> ***Adjusted*** *formulations to make the link to the perturbation experiments. The passage reads as follows:*
>
> *'The modelled net accumulation for the IS equilibrium geometry is equivalent to 1.5 mm s.l.e./yr. It is balanced by surface runoff and ice discharge at rates of 0.84 (55 %) and 0.66 mm s.l.e./yr (45 %), respectively. Doubling of the sliding coefficient in DR SIA almost doubles the ice discharge and would therefore extrapolate to a dynamic mass loss of about 66 mm s.l.e. within hundred years in the case such rates could be sustained.'*

Page 2973, line 21-23: Be clear here; more thinning leads to more runoff? "changes" is ambiguous.

> ***Corrected as suggested.***

‘Differences in ice dynamics could increase thinning rates and together with the elevation feedback increase surface runoff’

Page 2973, line 27-29: These last two sentences are confusing and awkward.

***Corrected** by removing passage and reformulating as follows:*

‘Differences in this feedback [height-SMB] between model versions are not consistent neither over the various resolutions nor when allowing for direct horizontal coupling. Therefore it is excluded to be decisive here. Differences in mass loss rather arise from reduced dynamic discharge at the marine ice front.’

Page 2974, line 2-3: This is a strange statement to make since it doesn’t sound like you are applying a boundary condition that is appropriate for a calving front (i.e., the hydrostatic stress from the water column, as opposed to an SIA velocity, which I believe you apply). This leads to additional confusion about the lateral boundary conditions being applied.

***Corrected** by reformulating entire passage. We are however a bit confused by the reviewer’s statement since the lateral boundary condition was broadly described earlier. For the reformulation of the manuscript, we however tried to avoid any reference to more sophisticated boundary conditions. It reads as follows:*

‘The modelled reduction of dynamic discharge at the marine margins for MarAsl2 depends on the details of the upstream propagation of the perturbations.’

Page 2975, line 18-21: Again, you are not applying a “marine” boundary condition here (or at least it doesn’t sound like you are), so the discussion about “marine terminated periphery” and “calving export” are confusing. If you are using SIA to specify ice flux across a “marine” boundary, you need to show/argue why that is a reasonable thing to do.

***Corrected** by reformulating as follows:*

‘Though not decisively altering the mass loss response on centennial time scales, the additional marginal decrease in ice thickness reduces the ice export and thus provides an explanation for the decreased mass loss (see Fig. 1).’

Page 2976, line 20-21: “Direct far field signal transmission : : :”. I’m not sure what this statement means. What is the meaning of “far field”?

***Corrected.** The reviewer is right by stating that the term ‘far field’ is not well defined in the presented work. Therefore we reformulate:*

‘Significant effects on geometric adjustment from direct horizontal stress transmission cannot be confirmed even in the case of full non-linear sliding.’

Page 2979, line 2-3: I’m not sure you’ve clearly explained why membrane stresses lead to faster attenuation of the perturbation. Intuitively, this is because membrane stresses distribute the perturbation over a wider area, and thus the perturbation can propagate farther and faster than if it does so just through geometry adjustment, which is slow and limited by the viscous deformation of ice.

We thank the reviewer for this constructive comment since it is a direct outcome of the entire publication up to this point. Therefore we added an extra sentence to clarify this reasoning. Note that we refrain from the reviewer’s usage of ‘geometric adjustment’ purely in the sense of diffusive propagation of thickness changes. Here this term is used generally and also implies effects from horizontal coupling.

***Corrected** by: ‘A common characteristic for all marginal perturbation experiments is the reduction of the reaction time when direct horizontal coupling is active in the basal layer (Fig. 9). The reason is that the instant velocity response exceeds the area of direct* *perturbation and thus allows geometric adjustment in a wider area than in DR SIA, resulting in faster attenuation.'*

Page 2979, line 24-25: "Yes the response behavior is rather prone to : : :" Wording choice is very vague. Be more specific here. More sensitive? Less sensitive? What?

The entire paragraph was restructured and the passage reads as follows.

Corrected *by: 'Yet the response behaviour is rather sensitive to the chosen grid resolution and reaction times vary by up to 40 %.'*

Page 2980, line 1-17: I'm confused about the resolution discussion. Perhaps a summary would help. Does it matter or not? Surely it must on some level.

Corrected *by reformulating. Re-reading this section we have to agree with the reviewer that this paragraph was rather confusing. We restructured and summarised the passage to focus on the main issues and to make it clearer. Passage reads as follows:*

'At a numerical level, our choice of grid spacing remains an optional candidate that alters or potentially inhibits direct horizontal coupling. Since we do not find a strong grid dependence of the total mass loss, it remains possible that direct horizontal coupling is not resolved properly on any used resolution. However, on 5 km resolution, the initial speed-up in a 40 km vicinity (Fig. 4) is resolved and in agreement with theoretical estimates (Kamb and Echelmeyer, 1986). We are therefore convinced that membrane stress activity is captured in an acceptable way in our large-scale ice flow model. Except for resolving successively more geometric details, we do not expect that further grid refinement would drastically alter model differences in the spatially integrated mass loss. Underlining the consistency of our results, all of them are robust under different initial conditions (Fig. 2b).'

Pages 2980-2983: The discussion section is very long. I think it could be considerably shortened, especially if it sticks to just covering topics discussed in, and relevant to, the material presented in the paper (e.g. the discussion about the details, differences, and failings of various sliding laws doesn't really add much, nor does the discussion of grounding line retreat, which this model can't do anyway because of the coarse horiz. resolution). Overall, parts of the disussion seem to veer off topic and removing them could make this section quite a bit more concise and relevant to the rest of the paper.

This comment of the reviewer was also very helpful and we tried to restructure and shorten the discussion section significantly. The discussion section ultimately was reduced by 40% according to the suggestions. This allowed us to focus more on the relevant discussion points for this particular experimental setup.

Corrected as suggested.

APPENDICES

Equation (A1-A2) should be "del dot u" rather than "del u"; same for "del σ" (divergence not grad)

Corrected *as suggested. Though the previous notation was already self-consistent, the added mathematical sign emphasises that the* ∇ *signifies the divergence operator here.*

Page 2986, line 20-21: This is the 3rd time you mention something to the effect of "deviations from hydrostatic pressure cause deformation."

Corrected. *Removed the repetitions here.*

Page 2987: Somewhere here, state more clearly where the nonlinearity comes in; the strain rate dependence of the viscosity.

Corrected *as suggested. Inserted a statement clarifying this non-linearity.*

Page 2988, Line 1-13: Add some refs for papers where the background for the “1st-order” approximation is layed out in more detail (e.g. Schoof and Hindmarsh, QJMAM; Dukowicz et al., J. Glac.)

Adjusted. *Added both references.*

(A9) This is NOT the most general form for the basal resistance, this is a 1st-order accurate version (i.e., this is not a Stokes form of the basal bc, which would be the most general).

Corrected *by removing the statement on its generality.*

(A9) Have all of these terms been defined, e.g. Tau_bi, Tau|| ? Have you told us somewhere earlier on what “s” and “b” are in all of these equations?

Corrected. *An effort was made to explain all the indices and symbols which were not directly referred to in the previous version.*

(B3) clarify where this sliding law comes from. Also, more undefined terms introduced here (e.g. u_b, u_bi).

Corrected. *Defined the unknown variables in the text. But since the sliding law is historically framed in this passage, we believe that it is needless to expand further on it as the reviewer suggests.*

APPENDIX Somewhere in the appendix, the lateral boundary conditions, briefly discussed in the paper, should be discussed in more detail.

Corrected*. Added a short passage explaining the used lateral boundary conditions. However the specific boundary condition based on the DR SIA model version for the first grounded point remains in the main text as it is essential for the understanding of the experimental setup.*

FIGURE / TABLES

Table 2: shouldn’t the combinations that are not used in the study (e.g. ME HO) be noted here somehow?

An attempt to visualise the not used model version, we put ME HO in brackets in this table. The caption however always stated that this version was not used. ***Corrected.***

Figure 2: The dashed blue lines are not clear in the figure. As noted in the general comments section above, I’m not sure if/why some of these model options need to be shown. I think it might make more sense to limit the figures to model combinations that might actually be used in real life.

Adjusted accordingly. *Changed the dashing style of the blue and green dashed lines in the inlet of figure 2 and in figure 3 to make them more recognisable. However, removing further model version especially the one mentioned here involves a substantial loss of information. Therefore and according to the answer on one general comment, a focus on the most used model versions was not adopted.*

Figure 3: Caption and vertical axis label – confusing here about whether the mass balance is increasing or the mass “loss” is increasing in time. Clarify the wording.

Corrected *by using the following label ‘Cumulative mass loss [mm s.l.e.]’.*

Figure 5: Just show the first three rows of figures (i.e. ignore the SIA SR and DR HO combinations)? Consider showing the spatial pattern of thickness change as normalized to the pattern from the SIA model (to make the differences more clear)?

Not adjusted. *Reasoning is given above as one of the answers to the general comments.*