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 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 1:

In this paper, the authors use a 3D thermo-mechanically coupled ice sheet model to investigate the effects of higher order stress gradients on the centennial mass evolution of the Greenland ice sheet. The main purpose of the paper is to investigate the inland signal propagation to perturbations at the ice margin. Three idealized experiments are conducted in which the basal sliding velocity is doubled via a step-like perturbations. All experiments are performed with five different approximations to the Stokes equations on 20, 10, and 5 km horizontal grid resolutions. The main conclusion of the paper is that membrane stresses modulate the inland signal propagation of a stress perturbation, however, the mass evolution on a centennial time scale remains dominated diffusive surface elevation adjustment. Within the limitations of the studies, the authors suggest that Stokes models may not be needed to investigate the mass evolution of the Greenland ice sheet on a centennial time scale.

In my view the manuscript will be very useful for ice sheet model developers and users to guide further model development and application. The authors pay meticulous attention to detail and show convincingly that their conclusions are robust with respect to grid resolution and initial states. Their conclusion that models including membrane stresses are an acceptable compromise between required ice dynamical complexity and computational costs comes at a time of a Stokes model hype. Nonetheless the authors carefully discuss the limitations of their approach, and do not exclude the possibility of setups where solving the Stokes equations is essential. In particular, the authors mention horizontal grid resolution as a candidate, as their study is limited to a finest horizontal grid of 5 km, and many outlet glaciers have features in bed topography that might be missed at this resolution. This would be indeed interesting to test, as it is my main concern. The really interesting questions is how the different stress balance approximations alter the mass evolution once we start resolving such fine-scale features. Of course, to answer this question, we not only need models capable of dealing with grid resolutions needed to resolve these features, but also the bed topography must be well resolved.

The authors provide a thorough analysis of the effects of ice dynamical complexity on mass evolution in a well-structured manuscript. I find the following analysis methods particularly useful: 1) decomposition of the ice discharge into three components, namely differences in ice thickness evolution, in velocity evolution, and a combination of both (Fig. 7); 2) the spatially averaged velocity response (Fig. 8), and 3), the reaction times (Fig. Fig. 9). In summary I recommend to publish this manuscript almost as is, and I have only a few comments below.

General comments

The term "dynamic discharge" is used throughout the manuscript. I understand what the authors mean with the term, but I think it should not be used as it is somewhat meaningless. First, it implies that there is also a non-dynamic discharge. Second, discharge is a flux through a plane, and therefore the plane should be defined. In most glaciological applications, this is the grounding-line. In other words, most of us glaciologists think of ice discharge as the sea-level relevant ice discharge. How about using "ice discharge" instead of "dynamic discharge"? It would probably suffice to introduce it at the beginning of the manuscript as "ice discharge through the grounding line", and later refer to it only as "ice discharge" for brevity. However I am open to suggestions and comments.

We confirm that the expression 'ice discharge' is more commonly used in glaciology and therefore we decided to follow the suggestion of the reviewer. Any reference to 'dynamic discharge' or 'dynamic export' have been removed and replaced by 'ice discharge' or simply 'discharge'. ***Corrected.***

Fig. 5 and 6 are a little hard to interpret at first, as absolute differences are shown, it took me multiple readings to understand what the authors are trying to say. Would relative differences be a better choice?

We understand that this figure is at first difficult to interpret but since it is the central figure of our study we tried to present these results in various ways and the finally presented absolute thickness differences seemed preferable. Relative differences had the disadvantage that they would amplify interior regions where the reference thickness changes are small. Therefore small deviations from these changes become very prominent in a 2D contour plot though not having much physical meaning. For these reasons we refrain from changing figures 5 and 6. ***Not corrected.***

Minor comments

p. 2965, l. 5-7: Awkward sentence, please rephrase. Maybe "Accounting for pseudoplasticity of of ice and for non-linear sliding, the effective longitudinal coupling length is expected to increase, with values of about 40 km (Williams et al, 2012) for typical Antarctic ice streams."?

Corrected *by reformulating:*

'The effective longitudinal coupling length increases when accounting for the non-linear character of ice creep and basal sliding(Kamb and Echelmeyer, 1986; Price et al., 2008). For a typical fast flowing Antarctic ice stream, it can reach up to 40 km (Williams et al., 2012).'

p. 2966, l. 1: it should read (SIA; Hutter, 1983)

Corrected.

p. 2966, l. 25: I don't find any hints in Huybrechts et al. (2011) on how the bed elevation data from Bamber et al. (2011) has been modified. Please clarify.

The specific passage on the adjustments on the geometric data set is indeed described in Huybrechts et al. (2011). Refer to page 403. The passage reads as follows: 'The grids correspond to those discussed in Huybrechts and Miller (2005) and include modifications in marginal ice thickness around Greenland margins to remove known artefacts when subtracting an ice thickness field constructed for a more limited mask than the actual ice sheet surface elevation. Overdeepened fjord beds of important outlet glaciers were added manually when absent from the interpolated fields, both for Antarctica and Greenland.'

Not corrected.

p. 2969, l 21: change to: We conduct three experiments that...

Corrected *as suggested.*

p. 2970, l. 26-27: From looking at Fig. 1, I am not able to see that modeled ice volume and extend are close to observations. Please clarify.

The reviewer is right in stating that the initialised geometry is not particularly 'close' to present day observations. This word was chosen having in mind glacial/interglacial *transitions where the ice volume undergoes changes of several ten percents and geometries are substantially different from the presently observed one. Accounting for this comment, we decided to use the word similar instead.* ***Adjusted wording.***

p. 2973, l. 20: remove ’,’

Done.

p. 2978, l. 15: change ’SRHO’ to ’SR HO’

Corrected.

p. 2983, l. 18: change ’deployed’ to ’imposed’

Corrected.

p. 2983, l. 25: change ’Gravensen’ to ’Graversen’

Corrected.

p. 2984, l. 23: change ’allows’ to ’allow’

Corrected.

p. 3010, Fig. 9: Please increase label font size

Corrected. *The sizes of both the labels and the used markers have been increased to make this plot more readable.*