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The Cryosphere Discussions

Interactive comment on "Effect of higher-order stress gradients on the centennial mass evolution of the Greenland ice sheet" *by* J. J. Fürst et al.

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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:

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Interactive Comment

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 relavant 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.

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?

Minor comments:

p. 2965, l. 5-7: Awkward sentence, please rephrase. Maybe "Accounting for pseudo-plasticity 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."?

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

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.

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

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.

p. 2973, l. 20: remove ','

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Interactive Comment

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

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

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

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

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

Interactive comment on The Cryosphere Discuss., 6, 2961, 2012.