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The Cryosphere
Discussions

Interactive comment on "High sensitivity of tidewater outlet glacier dynamics to shape" *by* E. M. Enderlin et al.

Anonymous Referee #1

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The authors, using a simple numerical ice flow model, explore the sensitivity of a synthetic outlet glacier to its width and the description of bedrock topography. They show that wider glaciers are more incline to retreat following a front perturbation and that slight mistestimation in the bedrock elevation (i.e., in the range of observational uncertainties) may lead to substantial differences in the predicted behavior, ranging from moderated retreat to large unstable recession. The paper is well presented and organized. I am always a bit reluctant to see flowline models discussing lateral effects as it is strongly hampered by the hypothesis included to average the lateral dimension. However, results remain sensible and fairly argued but not surprising: different glaciers respond differently to similar perturbations.

I have three main criticisms:

(i) as mentioned above, discussing lateral effect with a flow line model is a risky task. You may imagine that some of your simulations may lead to a stable position over the retrograde slope when using a two horizontal dimension model. Limitations and robustness of the approach is poorly discussed. This should be improved in the introduction.

(ii) in the conclusion it is argued that “the dynamic response of glaciers under a given perturbation at the ice front is highly sensitive to along-flow variations in shape” (p561, l20). However it is previously mentioned (p559, l4) that “the initial thickness profile determines the mode of response to the perturbation”. Because the prescription of the width is different, initial geometries are different. It therefore turns into a chicken and egg problem that the proposed results may not give an indisputable answer. I would also add that it is well known that an unconfined outlet (i.e., $W = \infty$) is insensitive to a perturbation of its floating extension which sounds in contradiction with some part of the discussion. My feeling is that experiments does not compare simulations presenting different widths with all the other parameters being similar, strongly complicating the interpretation. I would suggest to tone slightly down the parts where the direct impact of the width is discussed.

(iii) the sensitivity experiments proposed are all close to a marine ice sheet instability configuration. In other words, the initial geometry is close to a typing point so that it is not surprising that a slight change in the geometry may lead to two distinct behaviors. By choosing a monotonously downsloping bedrock the responses of the model would have been unimodal and the spread of ice discharge probably much smaller. Not all the outlet glaciers are so close to a MISI configuration. I therefore believe that the conclusion regarding the caveat on the measurement accuracy (“data precision... must be able to resolve differences in ice thickness on the order of 10’s meter”, p562, l5) is not strongly funded. At least, this level of accuracy is not required everywhere.

Minor comments:

- I would suggest to the authors to read and maybe mention the following paper, where a similar topic is discussed: Durand, G., Gagliardini, O., Favier, L., Zwinger, T. & le Meur, E. Impact of bedrock description on modeling ice sheet dynamics. Geophysical Research Letters 38, L20501 (2011).

- section 2.2 p556, l20. Description of the evolution of the basal drag along the profile is not clearly explained. How is it decreasing to zero when approaching the grounding line? Is it different from one simulation to the others? Is it evolving with time? Then, there should be some feedbacks, a retreat decrease the basal drag, accelerating the flow, enhancing further retreat? This somehow get back to my main comment (i)

- section 2.2 p557, l5. To my knowledge this model has been participating to MISMIP intercomparison. This may be mentioned.

- section 2.4. I think that the discussion on the perturbation can be improved. I am not sure what the authors exactly did. As far as I understand, the perturbation is a function of the difference in the hydrostatic pressure between ice and water, and therefore a function of the ice thickness at the front? So the perturbation is dependent of the geometry and is changing with time? And different width of the glacier leads to different geometry and different amplitude of the perturbation? If it is the case, I think that arguing that the width of the glacier drive the response becomes weaker (again main comment (ii), various parameters are different, not only the width).

- section 3, p559 l 16-23. I do not find this result particularly astonishing or unexpected. The grounding line can stand close to a MISI configuration but the glacier may be insensitive to any perturbation. In opposition, the grounding line can stand further away from the retrograde slope area and may retreat easily following a small change in the front condition and reach a MISI situation. It sounds pretty obvious that the location of the grounding line is not enough to predict the stability of an outlet glacier. This is even more obvious when considering the fact that steady position can be found on a retrograde slope in a 3D configuration.

- section 3. In the description of the model results, it would help the reader if direct links to the panels of figure 2 were made.

- Figures. Lots of informations are superimposed on the figures, particularly figure 1. They may be simplified. For example, I am not convinced that experiments shown on the second raw of figure 2 add substantial informations compared to what is presented on the first raw. Furthermore, extensive use of blue and red on figure 1 probably renders the figures hard to follow for colorblind readers (10 % of the population).

Interactive comment on The Cryosphere Discuss., 7, 551, 2013.