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The Cryosphere Discussions

Interactive comment on "The stability of grounding lines on retrograde slopes" *by* G. H. Gudmundsson et al.

G. Jouvet (Referee)

guillaume.jouvet@fu-berlin.de

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General Statement:

This study addresses the problem of the existence of a steady Grounding Line (GL) on retrograde slopes in two horizontal dimensions (2HD). In the 1HD case, GLs on an upward-sloping bed are well-known to be unstable to small perturbations, this statement being supported by theoretical arguments. However, in the 2HD case, the same argumentation does not hold anymore because of possible buttressing effects, leaving open the existence of such stable GL. This paper provides an example of stable GL that is partly lying over a retrograde bedrock. To build such an example, the authors consider a channelized bedrock. On the sides, the GL – localized on an upward-sloping bed – is stable and sufficiently well-affixed to sustain the GL over central part (the

channel) stable even if the bed is downward-sloping in this area. The authors use two different models (including the most accurate one, Stokes) to verify that the solution is not dependent on the model.

This is an interesting and well-written paper which presents original results. I recommend to accept this paper for publication after addressing or answering the listed points below.

Specific comments:

- The chosen channelized geometry shows very sharp transitions of the bedrock along the y-axis. Taking a smaller f_c would change the channel into a V-shaped valley and smooth the bed in y. In that case, I expect less buttressing and then a narrower range (or maybe empty) of w_c (like on Fig. 5) that allows for a stable GL that is partly lying on a retrograde slope. It would be interesting to add few runs to study the effects of parameters f_c and d_c since there are directly related to the "level of ice-shelf buttressing at the GL" (line 276).

- I believe that a part of the model is missing: nowhere it is written that the Weertman sliding law (Eq. (9)) applies under the grounded part only, and that a perfect sliding applies under the floating part (Eq. (9) in Pattyn and al, 2012). Did I miss something ?

- Since both models have been already published (lines 157 - 158), I wonder if they need to be re-described with this level of accuracy. Number of equations could be efficiently replaced by words and references. Also, it would be clearer to decouple/separate the full Stokes and the vertically integrated models by describing them successively, and not in mixed way.

- Lines 134 - 136 : I would have expect the vertically integrated ice flow model to be naturally coupled to the vertically averaged mass conservation equation

$\frac{\partial h}{\partial t} + \nabla \cdot (uh) = a$ and not to the local mass conservation equation (kinematic boundary condition, Eq. (13) in Pattyn and al, 2012). Was this choice made for the sake of the comparison between both models ? Do you have a reference for such coupling ?

- Lines 205 - 206 : The model comparison would be even more convincing if Elmer and Úa's runs would have been performed independently. Indeed, starting one model from an other one might influence the results and skew the comparison. Moreover, multiple steady state states are not excluded since no theory exists in 2HD. Do models agree even if both were run by starting with an initial constant ice thickness ?
- Lines 207 - 209 : The profiles across the central section agree well, but do the whole GLs and velocity fields also agree ? Additional data would be valuable for comparison.
- Lines 222 - 226 : Is the GL "protuberance" the consequence of the very abrupt channel walls, does it vanish when smoothing the bedrock in y ?
- Line 230 : Why is the ice thickness forced to be slightly positive and not positive ? Otherwise Eq. (5) degenerates ? (or for Stokes, you need to restrict the domain of computation only where the thickness is positive ?). Could you, please, give details the "numerical reasons" ?
- Lines 231 - 257 : This paragraph (even if of interest) looks beside the point or at the wrong place. Indeed, a substantial part concerns the model description. I encourage the authors either to restore the first part (line 231 - 240) into the dedicated part "Numerical model" or to postpone the whole paragraph in appendix since it is not essential for the paper.
- Lines 256 - 258 : Regarding to the example of steady ice sheet you have built, it

would be of great interest if you could give rise to "longitudinal stresses decreasing with respect to the ice thickness" in a figure, if this is possible.

Technical comments:

- Line 99 : "a full Stokes model" $\Rightarrow$ "a three-dimensional full Stokes model" to contrast with the vertically integrated model.
- Line 105 and later : Is Úa an acronym ? if yes of what ? if not, could you please use a more intuitive model name ?
- Line 106 and later : "obatained" $\Rightarrow$ "obtained"
- Line 153 : One can add that this model is often named "Shallow Shelf Approximation".
- Line 171 : "elemetns" $\Rightarrow$ "elements"
- Line 186 - 187 : I would remove this sentence since it does not bring much to the article ?
- Line 200 : Largest ice thickness is indeed visible on Fig. 3, but not highest velocity.
- Line 210 - 211 : I don't see in the text below any reference to the limiting case : $w_c = 0$ and $w_c = \infty$.
- Line 211 : "By taking a model geometry ...", since the bedrock changes, what is the geometry, the surface ? or the ice thickness ? please, clarify.
- Line 212 - 213 : This sentence is redundant, I would skip it.

- Line 227 - 229 : This sentence is a bit long and would be more efficient if split in 2 parts ?
- Line 253 : Could you, please, cite the exact equation number ? I could not find it.
- Line 270 : "one horizontal dimension" $\Rightarrow$ "1HD".
- Figure 1 : Could you, please, draw (with a dashed line) the profile of the bedrock also out of the channel, for instance at $y = -100$, this would improve the overview of the domain geometry. "colourbar" $\Rightarrow$ "colorbar" ?
- Figure 2 : Since these velocities derive from the Úa model which is vertically integrated, the horizontal velocities do not depend on z ? There are not then only surface velocities ?
- Figure 3 is hard to read. Perhaps, it would be better not to color it with the ice thickness but with a constant color (including shading effects if the visualisation tool allows it). The GL and channel boundaries lines should be even more stressed.
- Several time in the text (in Table 1 for instance), "parametre" $\Rightarrow$ "parameter".
- Somewhere in the text, there were "Figure" and somewhere else "Fig.", please check the consistency if this was not already done.

Interactive comment on The Cryosphere Discuss., 6, 2597, 2012.