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Interactive comment on "Parameterization of basal hydrology near grounding lines in a one-dimensional ice sheet model" *by* G. R. Leguy et al.

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General appreciation

This novel contribution explores new and physically-based parametrizations for representing grounding-line dynamics in large-scale ice sheet models. While the results are presented for a flowline case, the authors demonstrate that the same functions can easily be transferred to three dimensions. The concept is twofold. First a new sliding law across the grounding line is presented, taking into account both the effect of basal hydrology and its connection with the ocean. Both aspects guarantee a smooth transition of basal drag across the grounding line. A similar mechanism was presented

by Pattyn et al. (2006), but the advantage of the new approach is that it is physically consistent and not an ad hoc parametrization (i.e. one does not need to specify the width of the transition zone). Second, a grounding-line parametrization through linear interpolation is introduced, similar to the work by Gladstone. The combination of both factors greatly improve the reversibility of grounding line response on linearly sloping beds under steady state conditions.

The modelling presented here in sufficient detail is novel and may have a big impact on future modelling initiatives, especially with respect to improving large-scale ice sheet models to cope with grounding line migration. Furthermore, it is very well written and leads the reader through the model and experiments in a clear way. The conclusions, however, are way too long and should be cut up in a section 'discussion' and a short section 'conclusions', taking up the major findings of the study.

The analysis does present a couple of flaws that should be rectified, or at least put in the right context. The new parametrization as a function of p introduces faster flow at the grounding line with larger values of p. This results in different sizes of ice sheets. For the linear bed slopes, this does not introduce a bias, but it is important in comparing results for the nonlinear beds. Part of the different response across nonlinear bed slopes may be due to the fact that the ice sheet size is influenced by both the value of A and the choice of p. The reversibility results are also better for large values of p, but in that case it is not possible to compare the model directly to the boundary layer solution due to Schoof. this comparison is only possible for $p = 0$.

The reason why the reversibility (even for larger grid sizes) is better for $p = 1$ is given by the fact that the transition zone at the grounding line is better. It has also been shown by several authors (e.g. Pattyn et al., 2006) that larger transition zones show a better reversibility, but this is because the underlying physical model is different than the physical model for $p = 0$, as used by Schoof. While in Nature, the smoother change in basal conditions across the grounding line is probably more common than a sharp transition, it is not possible to directly address the result to a novel parametrization that gives a better reversibility. It is a different physical model that in any case will assure a better reversibility because of the faster flow (hence response times) at the grounding line. It does not show that the numerical problem is solved at a higher spatial resolution. If MISMIP would have run with a higher sliding parameter, the reversibility for the ensemble of models would intrinsically be better.

I would suggest that the authors try experiments with a significant higher spatial resolution, comparable to the one that is used by other authors (e.g. Cornford et al. who descend to 200m) for the different physical models, including $p = 0$, which would prove this point. It would also put the results in a broader perspective, i.e. spatial resolution is an important factor, and lesser constraints on this could be achieved by using a different physical model of sliding at the grounding line (however, with the consequence that the reproduced ice sheets are different in size). Given the fact that it is a vertically integrated flowline model taking up little computational cost, this shouldn't be a problem.

In essence, the results of the experiments should be independent of the numerics, i.e., spatial resolution, so that the effect of the two parametrizations can be clearly identified. So instead of focussing on numerical errors in reversibility on the linear sloping bed, the authors would do a better job in a priori defining a error margin and then repeating the experiments with increasing resolution until the result is within the defined error bars. the output would then be 'spatial resolution ' as a function of p and with/without GLP.

Another factor is that the paper is only looking at steady states. Since the authors have a moving grid model at hand that gives a good match with the (steady state) boundary layer theory of Schoof, why not looking at the transients as a function of p and GLP compared to the moving grid model? Or is this for another paper. I think that this would be very interesting, and probably more important to look at IPCC time scales. Subsequent research efforts in model intercomparison, such as MISMIP3d, focus on such time scales. So the paper does not clarify how good the model performs on such

small time scales, especially since very large perturbation (in A) are used, which are physically unrealistic on shorter time spans.

Minor remarks

p376, Line 24-27: to 'within millimetres'. Is this really necessary as a measure. It conflicts with a statement later in the manuscript on p379 where it is stated that 'The grounding-line position in our Chebyshev simulation differs from that of the boundary-layer solution by less than 1.2 km', which is not millimeter.

p378, line 26-27: rephrase this without the terms within brackets.

p382, line 5-6: the bias is still a function of spatial resolution, bed slope is an aspect, but resolution issues are still dominant.

p385, top para: this is an over-interpretation of the results: reducing p increases the transition zone, hence makes the ice sheet smaller and faster, and will lead to different results because of a different model.

p386, line 10-14: p and GLP are two different things; the first one alludes to a different basal slippery model, the second one to a numerical interpolation technique. A high value of p does not mean that you have a small error. Its numerical behaviour remains a function of spatial resolution. At high values of p you can obtain reversibility at coarser resolution. That's all. GLP is a numerical aid that produces reversibility in steady state (but may hamper the transient response - the latter has not been investigated in this study).

p387, line 10-15: similar remark - see also general appreciation.

Figure 6: use 'without GLP' instead of 'No GLP'; explain the dashed line in the figure caption

Figure 5 and 7: a higher resolution experiment would be better, because reversibility holds. If it doesn't, the model is not ok.

Figure 8: explain dashed line

1. Does the paper address relevant scientific questions within the scope of TC? yes

2. Does the paper present novel concepts, ideas, tools, or data? yes

3. Are substantial conclusions reached? yes

4. Are the scientific methods and assumptions valid and clearly outlined? yes

5. Are the results sufficient to support the interpretations and conclusions? not entirely

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? yes

8. Does the title clearly reflect the contents of the paper? yes

9. Does the abstract provide a concise and complete summary? yes

10. Is the overall presentation well structured and clear?yes

11. Is the language fluent and precise? yes

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? yes

14. Are the number and quality of references appropriate? yes

15. Is the amount and quality of supplementary material appropriate? yes

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Interactive comment on The Cryosphere Discuss., 8, 363, 2014.