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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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Many models for marine ice sheets, such as in the MISMIP experiments, use a sliding relation in which no dependency on basal water pressure is included. Such a dependency is likely to occur in real systems, and sliding relations exist which incorporate the dependency (the main difficulty lies in computing the basal water pressure itself). The current study uses one such relation (Schoof 2005), and calculates basal water pressure using the ocean pressure as a function of depth. A tunable parameter is introduced to scale the basal water pressure between depth dependent water pressure and zero. This basal drag law is shown to relax the resolution requirements for simulating

grounding line migration.

It is good to see work done in this area, as pressure dependence for basal sliding has a strong physical motivation and ways to parameterize it need to be considered. However, what is presented as a new parameterization is really just a scaling parameter in an existing basal sliding law. The main point of the paper, that resolution requirements are relaxed by pressure dependence in the sliding law, is not a result of the new scaling parameter, but is inherent in the existing basal drag law. No physical justification for the choice of 'p' is given. Still, the point is worth making, and I'd be happy to see this work published.

The point is made that this approach only affects a region (20km is suggested) near the grounding line, associated with the assumption that H increases steeply inland of the grounding line. Is this really true? What about the Siple coast ice streams, with their gentle profiles, or the Pine Island Glacier, which is pretty deep under the main trunk? Surely the approach taken here will have some impact on sliding far inland?

Page 371. Why must the transition occur over a finite length scale. I am pretty sure that you are right, but what is the argument for this? How can you be so sure that there isn't a step change in basal drag in the real system?

Change domain of fig 1 b,c,d to 0.9km – 1.4km? 0.7-0.9 is not really interesting. And perhaps units should be 10ˆ3 km instead of km?

Presumably the results being analysed are for steady state geometry? Can you state this clearly?

Key point: this is a change to the physics. Glossed over in the conclusions. Not only is this approach easier for models, it is likely closer to the real world.

In discussing the resolution requirements, it may be worth citing the Gladstone 2012 Annals of Glaciology paper showing that buttressing can relax the resolution requirements. Like buttressing, the suggestion here of pressure dependence in the sliding relation is a change to the physical problem that makes it easier for current ice dynamic models to solve. Both studies (Gladstone 2012 and the current study) make the point that the real system may be less harsh on models than the original MISMIP experiments!

Don't get hung up on saying that 1km is the appropriate resolution. Presumably the required resolution is a function of slope of bedrock near the grounding line? Also (see Gladstone 2012) it is likely a function of buttressing. You can say that 1km was appropriate in this set of experiments, but in different real world situations it may vary.

Conclusions section is very long. Suggest you separate out into discussion and a much shorter conclusions section.

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