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The Cryosphere Discussions

Interactive comment on "Benchmark experiments for higher-order and full Stokes ice sheet models (ISMIP-HOM)[1]" *by* F. Pattyn et al.

Anonymous Referee #2

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General Comments: This paper provides a useful service for the community. Inter-model comparisons are critical as we move glaciology forward toward a predictive discipline, and the results reported in this paper will undoubtedly serve to improve models and our understanding of ice flow. Thus, overall I think this paper should be published, with some minor revisions.

Specific Comments: Word choices can be extremely difficult, and I think this paper has a particularly important example. It is not appropriate to use "accuracy" to describe the degree of inter-model agreement with the groups of models. In all but one benchmark there is no analytic solution, so there is no way to establish accuracy. I wish I had

[1]Ice Sheet Model Intercomparison Project for Higher-Order Models; http://homepages.ulb.ac.be/~fpattyn/ismip

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a good replacement word to suggest, but I would hope that the authors can adopt different language to discuss the spread of the results within various model types. Perhaps "consistency" instead of "accuracy"?

On page 114, line 16, and on page 115, line 22, the use of the word "linear" is potentially confusing. Linear Stokes problems are NOT what we are dealing with here; save for the final benchmark we are dealing with nonlinear Stokes problems. And while we are indeed modeling a form of the linear-momentum equations (as opposed to angular-momentum equations), we are not dealing with "linear momentum balance", i.e. without the hyphen the reader may think we are dealing with a linear problem, when it is most definitely nonlinear for the majority of cases.

Lastly, on page 126, line 4-5, it is claimed that higher-order models assume hydrostatic pressure. That is manifestly false. They assume that there is a hydrostatic component, and a dynamic component, to the pressure. The vertical normal stress IS assumed to hydrostatic, but the pressure (the sum of the normal stresses) departs from the hydrostatic pressure by the longitudinal deviatoric stress. This is why the horizontal stress equilibrium equation ends up having a 2*sigma_x' term in it once the pressure is eliminated. What is missing from the pressure, in these so-called higher-order models, is the integrated horizontal gradient in vertical shear stress, and the inclusion of these components in full-stokes models certainly may contribute to differences in the results for sudden changes in the basal boundary condition.

Minor Technical Issues:

The paper is sloppy with the subscript i for the density of ice - I don't care if you keep the subscript or not, but just be consistent.

Figure 1 is useless. Coordinate system illustrations without labeled coordinates are hard to interpret.

For the figures comparing model results, it would be really useful to have the legend

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abbreviations explained once; I can guess what L1* means, but I shouldn't have to.

Are figures 9 and 11 switched? The caption for 9 mentions top and bottom, and 11 has two panels...

Figure 2 is incorrect, with values that are too low by roughly 400 meters. The sense of the topography is fine, but the magnitudes are off relative to equation (14).

The betaˆ2 values (on order 1000) seem low, if they are in Pa s/m. I'm not saying the results are fine, but most glaciologically relevant scenarios I've seen would put betaˆ2 in the 1e7 to 1e9 range, in Pa s/m. Then again, we are never told the units.

Interactive comment on The Cryosphere Discuss., 2, 111, 2008.