

Interactive comment on “Improved convergence and stability properties in a three-dimensional higher-order ice sheet model” by J. J. Fürst et al.

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General appreciation

The paper describes improvements of the numerical implementation of the Blatter-Pattyn model, following its initial description in Pattyn (2003). The improvements consist of a novel coding of the numerical solution on a staggered grid in three dimensions, which was not employed originally. The new implementation has a much better and faster convergence rate. The paper is well written and the experimental results are adequately described and presented. The detailed description of the numerical technique in the appendices is greatly appreciated. Rewriting the original equations in a form given in equations 8 and 9 is a novel, and the numerical difference between the STAG and the old DIR model are nicely illustrated by eq. 10 that is based on the

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previous equations. This is clarity is particularly rewarding. Although the precision in the convergence is largely increased, the resulting velocity field remains in agreement with the DIR model and the ISMIP-HOM benchmark. Apart from some relatively minor remarks listed below, I find the paper acceptable for publication.

A first remark is that the authors mention initially that they use Dirichlet boundary conditions at the base and set the velocity components to zero. This is indeed a very simple case that does not require a higher order model. However, later in the paper the basal sliding condition is invoked. For the readability, it would be better to state the basal condition right away, as even a sliding function is valid for non slip conditions by setting the friction parameter to infinity. In that case, no slip conditions become a special case of the basal sliding condition. It would also render the section on boundary conditions more conform, evoking both the upper stress free surface and the lower surface condition (see also Pattyn, 2003).

Secondly, the authors use a particular way of representing staggered grids, in which the velocities are still defined on the H grid points, but then also interpolated in between them to work out the computational node. Staggered means that the grid is not aligned and that u_i for instance would fall somewhere between H_i and H_{i+1} . Right now, one may have the impression that $u_{i+1/2}$ is the mean of u_i and u_{i+1} .

I would also put a bigger contrast in the colours of the line corresponding to DIR and STAG, which renders identification difficult for colour-blind people.

Detailed remarks

The abstract can be shortened, by for instance merging phrases. The description of the ISMIP HOM tests and the increase of velocity field accuracy described further down can be merged together.

Page 1571, Line 4: I agree that superimposing both deformational and sliding components cannot be done, but reference should be made to the combined SSA-SIA

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approach, as for instance given by Bueler and Brown, 2009. This reference is given later in the text anyway.

Page 1572, line 1: SIA is not valid for large scale ice sheet modelling. However, it has been extensively used for that purpose, but that does not legitimize its use. SIA is valid for large aspect ratios and ice sheets on flat beds without much sliding. It is however NOT valid under ice divides, in areas of fast flow and near grounding lines and ice shelves. These areas are integral part of large ice sheets, but cannot be represented by SIA.

Page 1575, line 13: the singularity that occurs is due to the nonlinear flow law ($n > 1$) and infinite viscosity underneath an ice divide when basal sliding is zero.

Page 1576, line 14: One should be able to demonstrate that the discretization works for models that include sliding and/or rugged topography. Otherwise a SIA model could do the trick and you don't need to develop a higher order model for that. It should therefore be mentioned that the experiment considers a rugged topography which requires the development of longitudinal stresses, and that in another experiment very low basal friction is tested.

Page 1582, line 20: what is meant by a percentage of 19/N is non zero? Is that 19/N

Page 1583: it could be mentioned in section 3.3 that De Smedt et al. report that generally the Picard iteration holds and that the under- and over-relaxation in the unstable manifold iteration is very rarely invoked.

Page 1584, bottom: in the analysis of the performance of the STAG model compared to the DIR model for the Arolla sliding experiment some more reference to the ISMIP-HOM paper is needed. Indeed, since the spread of numerical solutions of the participating models was very large compared to other experiments, it is not clear whether a model result that lies well within the error bounds should be a correct solution for non full Stokes models. Also full Stokes models were the majority used staggered grids,

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showed a much larger spread in their solution compared to experiments A to D. We reported in that paper that the large spread was probably linked to the ill posed problem of having a sudden jump in boundary conditions (no slip towards full slip) that could be regarded as singularities, especially on a low resolution grid. This means that in such a sudden jump the boundary layer may not be resolved. In that sense, one needs to be careful in addressing this experiment as being a challenging experiment for models. It may say more about the experiment than about the models or the model accuracy.

Page 1585, line 22: curiously

Page 1587, line 4: does show or shows, but not does shows

Page 1587, line 10: as stated above, care should be taken with the Arolla sliding experiment, because of the sudden slip condition on a low resolution grid.

Page 1590, appendix A: all these functions were initially reported in Pattyn, 2003 which should be referenced here. The use of coefficients a, b and c to set up the equations was done in that paper. A1 mentions the coefficient $a_{x'}$, but in A2 a_x is defined.

Page 1606, fig 2: the reference to appendix A should be appendix B.

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