Improved convergence and stability properties in a three-dimensional higher-order ice sheet model

Reply to List of Comments

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First of all we want to thank all three reviewers for the critical and therefore useful comments they gave on the presented document. All comments are considered and helped to improve the quality of our work. In the following the responses to the reviewers comments are denoted in italic.

Review 2.

General commments

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 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).

> ***Corrected as suggested.*** *Now the basal boundary condition and the linear friction sliding are presented together with the force balance.*

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 ui for instance would fall somewhere between Hi and Hi+1. Right now, one may have the impression that ui+1/2 is the mean of ui and ui+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.

> *As noted correctly by the reviewer the usage of a staggered grid actually introduces computational nodes but in our case for η and the $(\eta\,\partial u)$. For both terms the computational grid depends on the specific derivatives calculated in Ω and Ψ. For details Appendix B should be consulted since it describes in detail the discretisation. Therefore we avoided any corrections.*

Reference to Appendix B.

Concerning colour issues: indeed the used green and orange for the two discretisations showed a similar value when converted to a grey palette. We kept the light green but changed the colour for DIR from orange to blue. We hope that this increases the contrast sufficiently and helps to easily identify the two discretisations.

Colour choice adjusted.

Specific comments

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.

The abstract was adjusted but we can offer a shorter version without loss of content. Together with the latter remark on frictional sliding at the base, the description of the ISMIP HOM setup could be shortened and it consequently gained in clarity.

Partly corrected.

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

We appreciate that the reviewer agrees with our formulation on the superposition of two velocity fields. However the statement is embedded within a general description of dynamical processes rather than a model overview. Since the reference is made later we argue that the latter reference is sufficient and more appropriate when model approaches are discussed.

No additional reference placed.

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.

We agree with the reviewer about shortcomings of the SIA in such integral regions for ice sheet evolution. However, the SIA combined with a sliding law has proven to be a good approximation for models with more comprehensive dynamics. Set aside issues emerging close to the grounding line which also require shelf dynamics, we think that the SIA has shown its capability to capture long term evolution of large-scale grounded ice sheets. The question remains if on short time scales more comprehensive dynamics is capable in causing fast signal transmission that can span the distance from the margin to the interior in order to significantly alter the ice sheet evolution.

Corrected *as follows: 'Apart from clear deficiencies in capturing full ice dynamics, the SIA has proven to be a feasible approach for modelling the evolution of large-scale ice sheets on long time scales.'*

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.

Corrected *following the suggestions.*

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.

__Corrected.__ This comment is in line with the first remark in the general appreciation. Introducing the frictional sliding law at this point assures that our discretisation will be applied for experiments with sliding. In addition the ISMIP-HOM experiment E on Arolla glacier is a combination of sliding together with a rugged topography. We also want to use the opportunity to mention that the model has been successfully applied on an alpine, sliding valley glacier, the Morteratsch glacier in Engadin Switzerland (Zekollari et al., in preparation).

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

__Reformulation:__ 'Among all matrix coefficients, at most 19 out of N are non-zero in each matrix column.'

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.

We want to reformulate the reviewer's comment and emphasise its two main aspects: (a) generally convergence is obtained when using a true Picard iteration and (b) introducing under- and over-relaxation in the true Picard iteration hardly facilitates convergence.

Regarding point (a), we confirm that for experiments conducted so far with the true Picard iteration in another presented implementation of a finite-element discretisation (I2 in De Smedt et al., 2010), a converged velocity result is obtained. But also shown in this work is that for a finite difference scheme the true Picard iterations can be hampered by oscillations in the iteratively determined velocity field. Our work confirms that the DIR scheme shows divergence in some experimental setups of the ISMIP HOM benchmark using true Picard iterations. With respect to point (b) for the presented set of experiments we did not test the influence of the proposed relaxed Picard iteration by De Smedt's (2010) on STAG and DIR. We can only refer to the work itself (De Smedt et al , 2010) and references therein where the authors state that relaxation is beneficial to convergence rate. Therefore we would rather omit the suggested statement for section 3.3. in our publication.

Not corrected as discussed.

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 ISMIPHOM 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, 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.

We want to thank the reviewer for the detailed information provided with this remark. Indeed our argumentation, which makes use of deviations from the mean solution, loses its base when one assumes that the E2 experiment is ill posed.

__We suggest the following correction:__ 'In the combined geometric and basal boundary problem of test E2, the difference between the STAG and DIR schemes is largest while DIR exceeds the rms deviation, which is large by itself. The fact that solutions for E2 show such a high variation is attributed to the fact that the setup with a zone of no friction might actually be ill-posed (see Pattyn et al. 2008). In this light, a direct comparison of the solution is possibly inappropriate. In contrast to some model participants of the E2 experiment, both discretisation schemes suggest smooth solutions suppressing build-up of instabilities where the base supports no resistance.'

Technical corrections

Page 1585, line 22: curiously

> ***Corrected.***

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

> ***Corrected.***

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.

> *We appreciate that the reviewer emphasises the fact that the setup of experiment E2 is highly controversial. We accounted for this in the text but at the position commented here, we actually refer to experiment E1 which has no sliding at the base. However to avoid any confusion, we decided to reformulate the passage as follows:*
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> ***Corrected.*** *'… . The diverged solution exhibits jagged structures that indicate numerical instability, which occurs preferentially for sliding experiments that apply Neuman boundary conditions at the base, but also for a setup without sliding as soon as a realistic and ruggged geometry is assumed (compare experiment E1). …'*

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 ax0 , but in A2 ax is defined.

> ***Corrected.***

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

> ***Corrected.***