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

Reply to List of Comments

by J.J. Fürst, O. Rybak, H. Goelzer, B. De Smedt, P. de Groen and P. Huybrechts

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 1:

General comments

This paper presents an advanced numerical finite-difference discretization scheme for the force-balance equation in the higher order Blatter/Pattyn (LMLa) approximation. It is advanced in a technical sense, that for the used solvers the convergence is significantly faster and more regular as for a scheme, that has been suggested by Pattyn (2003) for the ISMIP-HOM benchmark model intercomparison. Furthermore, much more precise solutions can be found, which really is an important issue in ice sheet modeling.

The main difference to the older scheme is, that the viscosity is here defined on a shifted/staggered grid with respect to the velocities. This allows for a smart discretization of the second-order derivatives within the compact stencil and an additional reduction of the truncation error compared to the DIR/Pattyn scheme, that is defined exclusively on a regular grid.

The general idea of defining the viscosity field on a staggered grid and enhancing the coupling by averaging adjacent velocities is not new and has already been used in similar ways, e.g., for the discretization of the force balance in Shallow Shelf Approximation (SSA). But in this specific case it is worthwhile to give a profound description of the exact formulation, which easily can be reproduced. What I really like is the formulation in terms of operators split up in single steps, which helps a lot in creating a clearly arranged structure of distinct cases for a complex set of equations.

From my point of view this paper is ready for publication after some minor revisions. My comments below focus on some aspects, which could be discussed in more detail or where I would wish a more precise reference.

Specific comments

section1:

p1573 l.12: "reduce numerical instability" Does this mean that the scheme is less unstable for a certain set of parameter or does it mean it is stable for a larger range of parameters?

> *Note that this statement does not directly refer to the presented STAG discretisation and should be rather understood as a general comment. The context to this statement is the usage of an optimisation technique to improve numerical convergence of an iterative solver to a general, not closely defined problem. Such optimisation could be capable to reduce the amount of non-linear iterations for retrieving the solution. Consequently errors in the solution can only build up through this reduced amount of iterations and might even be*

attenuated by the relaxation method. In this way, such methods can also have an effect on the solver's stability properties.

Correction.
"Such algorithms [reference to optimisation] should facilitate convergence while the reduction of iterative steps together with the applied solution correction certainly influence and possibly improve numerical stability."

p1573 l.18–19: "Decoupling of the solution in adjacent points using centred differences in the Stokes equation is an understood phenomenon" Could you give some more information for those who are not aware of this phenomenon. Is this discussed in Mattheij et al. (2005), what page?

In general the Stokes equation links the velocity solution to gradients in a pressure field. In a heuristic way using centred differences for the pressure field gradient makes the velocity solution at one point dependent on the pressure at its two neighbours. If there is no strong coupling between adjacent velocities this can cause a decoupling of the velocity solution of adjacent points. In turn one can end up with a solution where each second point is linked but not the direct neighbours. For theory refer to pp.178 in Mattheij et al. (2005).

We argue that such detailed information would exceed the context of an introductory section on ice flow modelling. Such details would also need a more thorough explanation of the underlying equations and their discretisation. For this reason we would like to refer the interested reader to chapters on the Stokes problem in standard literature on numerical mathematics treating the Stokes problem (see Mattheij et al., 2005; Ferziger and Perić, 2002; LeVeque, 2007).

Correction.
Provide additional references in line 18: 'problem (see Mattheij et al., 2005; Ferziger and Perić, 2002; LeVeque, 2007)'.

section2:

p1574 l.21 and following: "The acceleration term in the force balance equation is in general omitted but not, as sometimes stated, because it is negligibly small. On the contrary, accelerations in fact reach large values but the time needed to adjust the velocity field and attain a new balance of forces is small." This is an interesting issue though just mentioned as a side information and not further relevant for the study itself. You argue that the time scale for the adjustment of the ice to accelerations is much shorter compared to glaciological relevant time scales. It think it is not trivial and worth to explain a little more or give a reference.

This side information was meant to indicate the consequences of the fundamental assumptions in the force balance. Glaciers velocities can actually change their magnitudes even on daily basis causing large accelerations. Reasons for this could be found in diurnal lubrication for valley glaciers or tidal forcing for outlets. However one assumes that these accelerations themselves, though they happen, do not trigger internal dynamic feedbacks that influence the overall force balance and in turn the velocity field (in contrast think of water mass acceleration in the Navier-Stokes equations for ocean dynamics). Accelerations mostly are attenuated fast and the velocity field follows accordingly. Consequently one can mimic such fast responses by neglecting accelerations in the force balance and allow the velocity field to adjust to perturbations instantaneously. On top of this, ice behaves not only like an ideal fluid but also fracture dynamics is important definitely when we think of a bergschrund or calving events. In fracture mechanics accelerations temporarily become large. Adjustment to such new situations is also rather instantaneous but also in this case accelerations are normally attenuated quickly. We added the following clarifying sentence:

Correction.
'Moreover, accelerations do not trigger dominant dynamic feedback that decisively influences the overall force balance.'

p1581 l.8 and following: The CFL-criterion is named here as a condition for stability. There are different definitions of this criterion but all of them are related with explicit time-marching

schemes. What exactly is the condition here? Is it about a perturbation of the parameters, which doesn't blow up during the iterations? Is it a norm of the (dissipative) average? A page number of the cited book by Wesseling (2001) could be helpful. In general, stability issues for non-local problems are absolutely not trivial. "Condition numbers" may be useful measures for the sensitivity of the solution with respect to input coefficients and hence of the accuracy of the solution, both for prescribed viscosities and in the nonlinear case.

> *Actually the reviewer is right to ask for details on the CFL criterion. Rethinking this passage we decided to drop the CFL criterion here since it is clearly linked to the discretisation of a 'time' derivative. More naturally for the presented example with exclusively spatial derivatives (as the force balance equation) we focus on invertibility of the discretisation matrix. The resultant matrix for Ω in the STAG scheme fulfils properties that ensure that an inverse can always be found. The matrix is a so-called M-band matrix that has positive coefficients on its diagonal and negative off diagonal entries. For the presented example this ensures that the STAG discretisation always provides an invertible matrix. The DIR discretisation does not guarantee invertibility especially for large grid spacing or high viscosity gradients. The same condition is applicable as already used in the previous version of this chapter. We consequently also adjusted the reference (Berman and Plemmons, 1994) where the reader finds a chapter on properties of M-matrices.*

> **Corrected along this discussion.**

section3:

p1582 l.12 and following: "...to a numerical decoupling of the x- and y-direction of the force balance equation in the nonlinear iterations and consequently reduces the matrix size of the linear system by a factor 4." How does this decoupling look like in detail and what are the consequences? In Eq. 14 one still finds a dependence on both the old u and v-velocity components. Is there a reference, since this could be of common interest for other ice sheet modelers?

> *Indeed there is a reference to Pattyn (2003) which is already present in the text. Therefore we prefer to keep information on the iterative process condensed and did not add additional information. The interested reader is encouraged to consult this publication. However for this revision, we will shortly recapitulate the ideas. In an iterative process that starts from an initial guess, one can always access the solution of the previous iteration. Decoupling of the x- and y-component within one iteration means that one solves for one velocity component by prescribing the perpendicular component using information from the previous iteration. Thus via the iterations the system remains fully coupled but the artificial decoupling allows by far smaller matrix sizes and in turn relates to memory issues for computation. Thus the reviewer notes correctly that for each iteration one still depends on the previous velocity field. But the decoupling concerns the velocity components of the updated velocity field. In other words $^r u$ and $^r v$ do not directly depend on each other. The velocity components link via the iterations.*

> **Reference to Pattyn(2003) made.**

section5:

p1587 l.27 and following "For high precisions, the maximum becomes locally flat and even shows a local depression..." How broad is this depression in terms of grid lengths? As you showed in Eq. 12 there is a strong resolution dependency of the DIR and the convergence is not very regular. It is definitely an interesting issue and emphasizes the quality of the STAG scheme. But regarding the outline of Appendix C it is not clear to me, why this phenomenon is so important for this study. I would guess that for even coarser resolution also the STAG scheme may fail in reproducing ellipticity characteristics of the underlying PDE.

> *The small inconsistency that is observed in the DIR scheme only concerns one grid point. We agree that also the presented new STAG scheme will fail on coarser resolutions or in some highly demanding setups. However, the observed phenomenon appears only for the DIR discretisation while STAG does not show it for the same setup. From this fact we infer a*

generally better numerical representation of the underlying equations. This provides an additional point in the argumentation apart from differences in the convergence characteristics. Appendix C is merely necessary to link extrema in bed topography to velocity extrema for large-scale applications.

Technical corrections

Please consider these to be suggestions. It is not my intent to be pedantic, just to be helpful.

p1574 l.24: "small" -> "short"

> ***Not corrected.*** *The adjective "small" refers to an "acceleration term" and we think that for characterising its negligible value "small" is better suited than "short".*

p1576/77 Eq. 5 or 9: y' and zeta are switched in the last a_x term in one of these equations

> ***Corrected*** *as indicated. We exchanged $\Psi(\zeta y',v)$ by $\Psi(y', \zeta v)$. Equation (5) and (9) then match.*

p1578 l.17–18: "The two first derivatives of the velocity field and the one of the surface elevation…" Maybe reformulate this passage to make sure what exactly you mean. (Similar issue in p1581 l.15)

> ***Corrections.***
> *p1578: "This concerns the finite difference approximation used for the two operators Ω,Ψ but also the three additional terms that show mere first derivatives either in velocities or surface elevation. These three terms have not the same structure necessary for Ω,Ψ and consequently a distinct discretisation is used, which we base on centred differences between the two inline-adjacent grid points."*
>
> *p1581:" This is caused by the appearance of the first viscosity derivative which itself uses centred differences for the velocity gradients. This gives rise to error propagation solving the nonlinear system of equations."*

p1578 l.21: "that" -> "which", there are only two operators

> ***Not corrected.*** *Our understanding of the usage of 'which' and 'that' would suggest 'that' since it is an essential subordinate clause.*

p1579 l.21: "right" -> "relevant", or "suitable" may fit better here

> ***Corrected as suggested.*** *(but I found it at line 3)*

p1580 l.14: "Therefore…" suggests it would generally be a result of the use of staggering and the compact stencil, rather than an effect of the specific STAG scheme.

> ***Corrected.*** *'Therefore …'* → *'In this way …'*

p1581 l.17: ".. to this…" -> maybe drop it or specify what you mean by "this"

> ***Corrected.*** *'In contrast to this, …'* → *'In contrast to DIR, …'*

p1582 l.10: Is there a certain reason for the ntilde over the u? To my understanding, the ˆr stand for the current iteration of u.

> ***Not corrected.*** *We admit that the tilde might be confusing but this should actually indicate that after solving the linear system there exist relaxation methods to improve convergence as suggested in De Smedt et al. (2010). But since we do not use such a correction method it is indeed correct that one can actually omit the tilde. For completeness we wanted to make this distinction.*

p1582 Eq.14: Does the b_x and b_y correspond to those in Eq. A4? If not, maybe rename.

> **Corrected.** *Indeed this represents a double definition of b_x and b_y. We chose to use a capitalised version.*

p1587 l.4: "shows" -> "show"

> **Corrected as suggested.**

p1603 l.10: reference "Press et al" not in order

> **Corrected.** *Added the reference to Press et al. (2003).*