

Review of GMDD-4-1569-2011  
*Improved convergence and stability properties in a  
three-dimensional higher-order ice sheet model*

13. Sep 2011

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Reviewer: Jed Brown

## 1 General comments

This manuscript presents a finite difference stencil with a different choice of flux points than used by Pattyn (2003) and compares the performance of a certain linear and nonlinear solver choice at a single grid resolution for a few model problems. Although the issues of discretization properties and solver convergence are mixed in the manuscript, but I will discuss them separately here.

The described stencil is a classical method for discretizing nonlinear elliptic terms, therefore it's hard to call "novel". The choice of locations at which to evaluate such intermediate quantities is a topic discussed in most books on finite difference methods, Ferziger and Perić (1996) or LeVeque (2007) for example. More recent work regarding choice of flux points has used the mimetic finite difference formalism to maintain local conservation and add support for non-smooth and unstructured meshes, see Hyman et al. (2002) for an introduction. In any case, the present formulation is a classical choice of flux points (though not usually regarded as the best—face midpoints would typically be preferred for conservation reasons), not locally conservative, and still only second order accurate. It may be better than the unconventional choice used by Pattyn (2003), but it doesn't qualify as "novel". (The approach used by "DIR" is typically dismissed by the numerical methods community due to undesirable accuracy, stability, and sparsity properties.)

If the goal is to compare the performance of two stencils, the accuracy, stability, and conservation properties should be compared in a setting independent of the iterative solver. A sequence of successively refined grids should be constructed and errors evaluated. A problem with a manufactured solution can be used to provide an exact solution or a very high resolution solution computed with a known robust method can be used as a reference. If the authors would like to establish that their choice of stencil is more practical, it would be worth giving special attention to the performance in regions with discontinuous bed slope since basal topography is rough in practice (at the spatial scale of feasible meshes). The method should be evaluated on its ability to produce non-oscillatory solutions, to locally conserve momentum (integrate stress around a control volume, the current formulation appears to be non-conservative due to the transformed coordinates, but I could be wrong), and accuracy (e.g. as assessed by grid refinement).

The iterative solver convergence results appear to be strongly influenced by artifacts of the solution algorithm and what appears to be a flawed methodology for solver tolerance. See my comments for lines 14.23, 15.12, and 19.6 for details. The linear solver especially is not robust for this problem and without further analysis, no significance should be ascribed to its erratic convergence behavior. I am a developer of the PETSc solvers package and we answer questions about similar solver issues every day via our mailing

lists and support email. Such solver-related issues are documented in various books including Saad (2003); Kelley (1995); Smith et al. (1996). While the different convergence behavior for Bi-CGSTAB with each finite difference stencil is perhaps an interesting observation, it does not warrant a paper. The present method uses no preconditioning and I am surprised that the authors were patient enough to wait for more than one million linear iterations. It is extremely common for iterative solvers, especially for nonsymmetric problems, to not converge at all without preconditioning. Regardless, the required iteration count for second order elliptic operators necessarily increases proportional to  $(\Delta x)^{-1}$  under grid refinement. This can be rectified by preconditioning, which is usually mandatory and, depending on the method, can solve the problem in a constant number of iterations independent of grid size. Additionally, the slow convergence of the Picard nonlinear iteration can be overcome by switching to a Newton iteration with grid sequencing for globalization. The nonlinear equations for every model configuration in this paper can be solved to a relative tolerance of  $10^{-10}$  in at most 10 linear iterations independent of grid resolution using a Newton-Krylov-Multigrid method. A demonstration of this using a  $Q_1$  finite element discretization has been available as a tutorial in PETSc (Balay et al., 2011) since early 2010, see `src/snes/examples/tutorials/ex48.c`. I mention this not to cry “you didn’t cite my (unpublished) work,” but rather to illustrate constructively that the iterative solution methods in this paper cannot be considered robust or efficient by any reasonable standard, for example, compared to what is achievable by applying established methods in a straightforward manner.

The paper needs a moderate amount of editing for English grammar.

## 2 Specific comments

- 2.8 ISMIP-HOM offers relatively non-discerning test cases and did not establish the correctness of reported results. For the test cases relevant to the model, the range is reported results is as large as the mean, indicating that there were implementation errors in those submissions, casting further doubt on the significance of statistics like the mean. This sentence seems to suggest an overemphasis on ISMIP-HOM as a “benchmark” and even as sufficient demonstration that the claimed equations are being solved.
- 3.23 Use of “entirely” is not really appropriate here. They solve the Stokes problem without further changes to the continuum model.
- 4.1 While SIA may be “feasible”, this does not imply that it is “suitable”. In particular, the long-term evolution is significantly influenced by flow in regions where SIA is a very poor approximation. That SIA has an a priori assumption of minimal sliding should be mentioned more explicitly in this part.
- 4.29 These equations do not only involve horizontal derivatives, so “horizontal PDEs” is an odd choice of terms. The PDEs are genuinely 3D PDEs, but only solve for two components of velocity.
- 5.1 This paragraph is confusing since it mixes stability and consistency of a discretization with convergence of an algebraic solver for the discrete equations. There is no citation for “smoothing algorithms”, but it is usually the responsibility of an algebraic solver to solve the algebraic equations up to a tolerance at least as small as spatial discretization error.
- 5.13 This sentence is just not true. A discretization may produce algebraic equations for which a certain iterative method performs well, but the spatial discretization in no way provides “direct control” of the convergence properties of the iterative solver.
- 5.20 Mattheij et al is an odd choice of reference for “proposing” a staggered grid finite difference method. Harlow and Welch (1965), for example, would be a more appropriate reference for staggered grid FD for incompressible flow with free surface.
- 5.27 The model is not “validated” or even “verified” in this work, it is only compared to some other models that have not been verified or validated either. “Verification” and “validation” have precise meanings when applied to computation. See Babuška and Oden (2004) or Roache (1997) for details and further discussion.

- 7.16 Remove ambiguity by writing an equation for “horizontal gradients of the vertically directed shearing field”.
- 9.1 In most cases, the actual solutions have only one weak derivative and certainly not two classical derivatives. This is a somewhat pedantic matter because the finite difference method is algebraically equivalent to a Petrov-Galerkin method in which only one derivative is used.
- 10.11 This evaluation of viscosity at staggered points has little to do with the Arakawa B grid which is was originally formulated to address the divergence relation in the shallow water equations. The classification of Arakawa and Lamb (1977) applies more directly to the choice of location for ice thickness nodes relative to velocity nodes. Although the elliptic equations in the LMLa model are of different character, there is still a useful analogy in the divergence operator applied to stress, however compatible (locally conservative) discretizations for this would be based on a C-type grid instead of the B grid chosen here. It would be useful if the authors could justify their rationale for choosing the present formulation instead of the more commonly used C grid.
- 13.9 A negative diagonal and positive off-diagonal is not a “CFL criterion”. I think the authors are trying to invoke the notion of “positivity” as described in Chapter 4 of Wesseling (2001) which notes the discrete maximum principle under the sign criterion above combined with a sum of zero (needed anyway for consistency). A discrete maximum principle is not needed for stability (indeed, it does not exist for high order approximations<sup>1</sup>), but it is a desirable property for a discretizations, especially when solutions are not smooth. This section should use “discrete maximum principle” instead of “stability” and should not use the term “CFL criterion” which refers to an entirely different issue.
- 13.15 This sentence does not make sense. The compact stencil improves matrix sparsity which reduces the cost per iteration. Additionally, the larger diagonal relative to off-diagonal entries makes local smoothers more effective. This property is made more precise by the technique of local Fourier analysis which quantifies  $h$ -ellipticity, a necessary and sufficient condition for the existence of a pointwise smoother, an important property for efficient multigrid schemes, see Trottenberg et al. (2001) for details. It appears that a preconditioner is not used in the present work, but popular inexpensive preconditioners such as incomplete factorization, Jacobi, and Gauss-Seidel are more effective when when the coefficient of  $h$ -ellipticity is larger. Analyzing the present method within this context may explain the observed convergence rates.
- 14.23 The BiCG method is not a monotone method for nonsymmetric matrices, therefore the residual can grow between successive iterations. In fact, the monotonicity of BiCG is notorious and motivated the development (and success) of the stabilized variant (Van der Vorst, 1992) which is also not a monotone method, but is less erratic. Even with a monotone method such as GMRES (Saad and Schultz, 1986) (in exact arithmetic), there is no guarantee that successive iterates should move significantly between iterations until the residual is small. Indeed, it is common for iterates to remain essentially constant for several iterations before converging rapidly. If the convergence test is actually comparing successive iterates as indicated in the text, then it is a fundamental misuse of the method. If instead the norm of the residual  $\|Ax - b\|$  is being used as a convergence test, this should be stated. A combination of relative and absolute tolerances are typically used to define convergence.
- 15.12 Although the term “error” is used, I am once again left with the impression that the norm of the difference between successive iterations is used to define convergence. Although the Picard iteration can be shown to be contractive under modest conditions, there is no guarantee that a small difference between successive iterations implies that the current iterate is anywhere near the solution. Indeed, I have observed this near-stagnation for the same continuum equations as modeled here, but with

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<sup>1</sup>Wesseling (2001) introduces the topic in the context of advection, for which linear nonoscillatory schemes are at most first order accurate (Godunov’s Theorem). All practical discretizations for advection of higher than first order accuracy are nonlinear. The situation is different for elliptic systems, which typically have far fewer problems with oscillations, for which second order linear nonoscillatory schemes are readily available, and for which higher order linear methods are frequently practical.

nonlinear basal sliding. Instead of comparing successive iterates, the residual of the nonlinear equations should be used as a convergence test.

19.6 As mentioned before, BiCG is well-known to exhibit erratic convergence behavior. Divergence of this iteration says little to nothing about the spatial discretization, instead it is an indication that the operator should either use a different Krylov method (e.g. BiCGSTAB, GMRES, or many others) or use a more effective preconditioner. If the authors wish to assert that the DIR scheme is not stable, they can compute the eigenvalues with smallest real part. (This can be done efficiently for large sparse matrices using software packages such as SLEPc Hernandez et al. (2005).)

It is also worth noting that since the continuum equations are self-adjoint and uniformly elliptic, spatial discretizations can produce a positive definite matrix for which the conjugate gradient method can be used, thus also guaranteeing monotonic convergence (in exact arithmetic). This property is “automatic” for Galerkin methods such as the finite element method. Finite difference methods often do not lose symmetry due to boundary condition implementation and the handling of non-uniform grids. This is not fundamental, however, and the mimetic approach (e.g. Hyman et al., 2002) produces finite difference schemes that do preserve symmetry.

25.15 The second term on the right hand side should use  $f_{k-\frac{1}{2}}$ .

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