

Interactive comment on “A linear algorithm for solving non-linear isothermal ice-shelf equations” by A. Sargent and J. L. Fastook

Anonymous Referee #3

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In this paper, the authors introduce an algorithm to solve the isothermal Shallow Shelf Approximation (SSA) ice flow model in the frictionless case, as for floating ice shelves. By decoupling the second-order elliptic problem into a system of first-order problems, the authors build a two-step algorithm that is cheaper than the original non-linear algorithm in some cases. Even if the method that consists of reducing one n -order ODE into a system of n first-order ODEs is not innovative (it is a well-known textbook method), to my knowledge it was never applied to any ice flow model. It is always interesting to attempt new algorithms and to evaluate their applicability in real cases of modeling. However, no conclusion can be formulated concerning this applicability, mainly because i) the model and the solutions considered are very restrictive and ii) the numerical methods chosen to solve the first-order problems do not suit for leading a fair comparison study (e.g the FFT, see below). As another general remark, a lot of space

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is taken by unnecessary technicalities like the adimensionalization, computation steps, RHSs, Gauss-Seidel scheme, ... (see below).

Major comments:

- The choice of the manufactured solutions appears to stem from a misunderstanding of the model. The manufactured solution is constructed to satisfy "the mass conservation" or (15), which rewrites

$$\frac{\partial}{\partial x}(uh) = 0,$$

i.e. one forces the flux to be constant. Could you motivate this choice? To me, "the mass conservation" can be understood as being either i)

$$\frac{\partial h}{\partial t} + \frac{\partial}{\partial x}(uh) = \text{mass balance},$$

however, so far, no time was involved (the SSA being stationary) or ii) the incompressibility condition

$$\frac{\partial u}{\partial x} + \frac{\partial w}{\partial z} = 0,$$

which can be used *a posteriori* to reconstruct the vertical velocity w from the horizontal one u by vertical integration. Neither i) nor ii) need to be considered since neither the time t nor the variable w appear in the SSA. In the 2D case, the last equation of 1844 should be

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = 0,$$

while the first equation of page 1845 is incomprehensible.

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- p 1845: The idea developed in this paper consists in transforming a second-order problem into first-order ones. In the 2D case, the authors finally differentiate to go back to a second-order problem (linear here) again, a Poisson problem. The reason given in the text to overcome solving the first-order system is not convincing. There must be a deeper reason why (29)-(30) can not be easily solvable with one of the numerous methods for such common types of equations. At that point, further evidences/arguments are needed. It would also be useful to cite other works in which other elliptic equations also took benefit from the same type of decoupling.
- As said in the text, the FFT approach for solving the block tridiagonal can be only applied to very restricted geometries (rectangles). Since this study aims to evaluate the applicability of the order reduction method, one should not use such restrictive method for a comparison study. There are many other (exact or iterative) methods for solving efficiently block tridiagonal systems.
- In the paper, it is said that "the assumption of the ability of boundary conditions for the stresses" is the main limitation of the method. The original frictionless SSA also needs those Boundary Conditions (BC). Of course, BC are no longer needed at the grounding line if one considers the SSA over the entire ice domain (grounded and floating), except at the calving front. So should the limitation not be rather stated as follows: "the main limitation is the absence of friction term, that makes the model usable only for the shelf parts"? Adding a friction term from a sliding law would introduce a zeroth-order term in the SSA making the order reduction trick still possible, but with a "further coupled" resulting system. This issue / method extension should be investigated since the SSA with friction is of much higher interest. Additionally, the paper does not show that the method applies to more complex geometries than simple segments or rectangles. It sounds to be another crucial limitation, but this can be overcome by choosing another method than the FFT.

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- Making the equations dimensionless is usually motivated by an analysis that follows (like a scaling analysis). However, here, the method does not require to use the equation in a dimensionless form. The paper would clarify its target by removing the entire adimensionalization procedure.
- Number of variables are not rigorously defined, in particular, all approximation variables are not defined, e.g. the mesh and the nodes should be correctly defined: e.g. let $\{x_i, i = 1, \dots, N\}$ be a uniform discretization of the interval $[0, 1]$ with $\Delta x = x_{i+1} - x_i$ as cell spacing. Also, there is a confusion between the continuous variables s, h, \dots and their approximations $\{s_i\}_{i=1, \dots, N}, \dots$ so it is necessary to state somewhere that s_i is (by definition) an approximation of s at x_i .
- Equation (17) is not a discretization of (7), but of (12). The expression of the evaluation of the derivative of s at the last point of discretization is maybe second-order (I haven't checked the coefficients) but I don't understand how the scheme for τ can be second-order with a first-order finite difference to approximate the first-order derivative of τ ? Also, should $\frac{\dot{\tau}}{\Delta x/2}$ be $\frac{\dot{\tau}}{\Delta x}$?
- p 1848: Writing the common Gauss-Seidel algorithm is useless.
- Appendix B: as said earlier, the FFT is not appropriate.
- Some proper names are miswritten : Gauss-Zeidel should be Gauss-Seidel, Newman should be Neumann, Furier should be Fourier, ...
- page 1835: For the calculation of the RHS — to Equation (16), it is useless to write the derivation of u, h and the RHS since these expressions do not bring anything to the paper. Anyone who wants to implement these solutions can recompute the RHS using a formal computation tool like in Maple or Matlab. The appendix A should be removed as well.

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- Equations (37) - (45) should be removed since any reader can easily verify that (46) - (47) indeed satisfy (34) - (35).
- beginning of 1845: Fig. 4 , Fig. 5 , Fig. 6 are never discussed.
- The bibliography should be extended.

Minor comments:

- page 1831, line 12-16, the same sentence is repeated twice, both must be merged to render the text flower in style.
- Equation (19), this scheme is well-known in the literature, references must be added (e.g. Bueler,...).
- three-diagonal \implies tridiagonal
- p 1837 : "ice constants" and "The ice bed ... " must be rephrased.
- p 1838 : "it is due accumulation"
- Equation (23), I don't understand the notation $n = n^x : \dots$
- Eq. (29) (30) : z_s or s ?
- Equation (48)-(49), and elsewhere, variables are introduced for a single use, e.g. k_1 and k_2 can be directly substituted in (48).

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