Geoscientific
Model Development
Discussions

Interactive comment on "*Albany/FELIX*: a parallel, scalable and robust, finite element, first-order Stokes approximation ice sheet solver built for advanced analysis" *by* I. Kalashnikova et al.

Anonymous Referee #3

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Summary: The first order, or Blatter-Pattyn equations describing the diagnostic solution to ice flow are discretized using the finite element method and solved using modular and highly scalable software libraries. The libraries provide built-in facilities for UQ (uncertainty quantification) and optimization via AD (automatic differentiation). Similar models have been constructed in numerous recent publications (see references in this paper), and its completion is not novel enough to merit publication. However, several interesting applications of the model are performed to explore its numerical properties, making this an interesting paper. The applications are: 1) the homotopy, or a systematic method of reducing a viscosity regularizaton parameter which improves convergence, 2) the manufactured solutions which allow for verification of the conser-

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vation of momentum solutions, 3) application of a multi-level algebraic preconditioner, and 4) exploration of the vertical mesh spacing requirements of the model. Each of the 4 applications have been performed in some way by others, but I am not aware of another paper featuring as systematic and rigorous approach to model testing. Because of this, ice-sheet modelers should find the results interesting and worthwhile to compare their own models to.

Review: The utility of this paper is found in its applications of the new momentum solver. I will treat the novelty and utility of each of the applications, in turn.

Homotopy is cool, but it looks like the stopping value of the regularizaton parameter is close to the value used by many (10^{-10}), which makes it seem more like an additional burden faced by this model, rather than a feature that improves numerical performance, particularly in the numerous models that do not have (or perhaps fail to report on) difficulties reaching convergence. I might find the treatment more satisfying if the sequence of alphas had been continued to make the regularization exactly zero, assuring the choice of parameter does not influence results. I am not clear on how the sequence of values for alpha are selected, this should be clarified.

AMG is nice, but somewhat lacking in terms of details of how it is implemented. The paper provides a good general overview of the method, as well as insight into the particular challenges posed by ice-sheet modeling; low aspect ratios and their impact semi-coarsening techniques. However, the discussion was not useful to a modeler interested in how AMG might be applied to a particular matrix generated by Stokes equations. This comes back to the editor's note about code release and versioning; for the material on AMG to be useful, it needs to be (minimally) accessible in the form of source code, and preferably accompanied with a better explanation. The authors do state that a second paper dealing with AMG is in preparation, and I think that some details can be delayed, but more implementation specifics should be provided now.

MMF Equations 22-23 are the shallow shelf approximation (SSA) equations, right?

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Where are the vertical shear terms (du/dz, dv/dz, see eqns 2-8)? Are these zero in the case where the surface slope is zero? That makes sense, but I wonder if the system has been simplified to the point where verifications is done on a specialized case of the FO equations (SSA) and not the true FO system? In this case, you've lost the most interesting part of the FO eqns; the ability to estimate BOTH vertical shear AND membrane stresses. This is probably the biggest issue with the paper as it is now written.

The exploration of the convergence for different mesh resolutions is sensible and provides clear results. It's a little difficult to relate reported errors to something practical, like; 'how many layers are needed to eliminate errors in prognostic runs of several hundreds of years'. That is probably not an easy question to answer, but if the paper is going to have significant impact, making a clear statement about how important the errors are would be helpful.

Summary Statement This is a long and important paper; providing a new means of solving a complex set of equations, and giving significantly expanded means of verifying the results are correct. There are some matters that have to be addressed before it is publishable. They are: Minor changes in the section dealing with homotopy to explain the sequence of values that are used for alpha. It would be really nice to know if homotopy is needed in time dependent problems, but it seems beyond the scope of the problem. Could homotopy be used when moving from coarser to finer grid resolutions to see if it reduces Newton iterations? Major changes should be made to the section on AMG, providing better insight into the implementation details. This might be most easily done by, Release source code, identify where it can be accessed, and provide versioning, as per the requirements of GMD, Reconsider the approach to manufactured solutions, which in the present form do not appear to be verifying the solution of the FO equations. Provide a roadmap detailing how this advancement in a single component of an ice-sheet model will integrate with other components to be more comprehensive and useful. How will the energy balance be computed? What about ice transport? In-

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tegration with climate models? I understand that this effort is coming from the national laboratories, where there is significant activity on all of these components, but the authors need to demonstrate that this is destined to become more than 'just' another momentum solver with no capacity for prognostic ice sheet modeling.

All of these can be addressed in the context of minor revisions. Unless I misunderstand, the MMF will require serious reconsideration, but there is enough in this paper that without the MMF the paper is still interesting.

Specific, but less significant issues in the text:

Abstract:

Why is "Template-Based Generic Programming" capitalized?

Introduction:

"A primary development focus has been on improving the representation of the momentum balance equations over the "shallow ice" (SIA; Hutter, 1983) and "shallow-shelf" (SSA; Morland, 1987) approximations through the inclusion of membrane stresses over the entire model domain."

Careful, membrane stresses are supported over the entire domain when using the SSA. Wouldn't it be more appropriate to say "through inclusion of BOTH vertical shear AND membrane stresses over the entire model domain" (CAPS just for emphasis).

"allowing for a quantifiably "optimal" match between modeled and observed velocities"

There is no guarantee that this match is globally optimal.

p8083, line 1-5: There is mention of integration into ESMs. It is possibly true that the model can be easily integrated into ESMs, but if how that is done is not discussed in the paper, then it really shouldn't be mentioned in the introduction because the issues is complex enough that in a journal like GMD, such claims can not be made without some supporting documentation.

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p 8087, line 5 not an energy balance but a conservation of energy model.

p 8090, line 11 'horizonal' -> horizontal x2

p 8090, line 26 How is the domain decomposition performed?

p 8091, line 5, Is the viscosity differentiated too, or just the strain rates? That is to say, its the a so called 'incomplete adjoint' (Goldberg and Sergienko 2010), or complete adjoint?

p 8093 line 21-23. OK, I don't really know much here, but I thought the basic idea of multigrid techniques was to use multiple resolutions to speed the rate of information transfer across the domain. It's more difficult for me to understand error capture from a high level.

Eqn 26 contains the strain rates dot_epsilon_1 and dot_epsilon_2. These should be updated to clarify their 2D counterparts.

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