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Discussions

Interactive comment on “Reformulating the full-Stokes ice sheet model for a more efficient computational solution” *by* J. K. Dukowicz

Anonymous Referee #1

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The manuscript presents a new formulation of the Stokes equations that includes only two horizontal velocities as independent variables. In addition to reduced number of independent variables (two instead of original four), this formulation is positive-definite that makes it more attractive from a numerical point of view. This formulation definitely has a merit, however, the manuscript needs substantial revisions in both presentation and the equation formulation before it could be published.

In terms of presentation, the manuscript reads rather as a technical report or in many places as notes with derivations than a scientific paper. *The Cryosphere* encompasses a variety of glaciological subjects, therefore making the manuscript less technical and appealing to wider audience will be beneficial for the manuscript. For instance, the author mentions Brezzi-Babuska condition (line 16) with no explanation what it is, he

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Discussion Paper

also mentions a “good” numerical solution without explaining what he means (line 15). Similarly, in Conclusions section there is a reference to “the JFNK method” without explanation what that abbreviation stands for and why “. . . the JFNK method of Knoll and Keyes (2004) will likely be the preferred solution method. . . ”.

As Jed Brown and Richard C. A. Hindmarsh pointed in their comments, a formulation of Stokes equations where pressure is excluded by veritcal integration of one of the equations is well known in glaciology. Although the presented formulation has a different origin, it would be useful to (a) reference papers where those approaches were developed, and (b) point out how the presented formulation is different from the earlier ones. The same comment applies for formulation of the Stokes equations as a minimization of an action functional. This approach has been used by many authors to formulate the Stokes equations and their various approximations (e.g., Schoof, 2006; Bassis, 2010; Goldberg, 2011, etc).

In several places in the manuscript, the author suggests (or at least those statements make that impression) that any velocity field that satisfies the continuity equation and boundary conditions will be a solution of the Stokes equations. This statement is surprising, because in addition to satisfy the first order continuity equation the velocity field needs to satisfy the second order Stokes equations as well. Does the author imply that for instance a solution of the Shallow Ice Approximation, which satisfies the continuity equation, is a solution of the Stokes equations as well? If this is not what the author suggests, then those statements need to be clarified. Also, to justify the first-order approximation the author often refers to an assumption of the small basal slopes. However, the same effect (i.e. the normal stress is balanced by glaciostatic pressure) could be achieved in locations with large basal slopes but with small or zero vertical shear in ice. Perhaps, it is better to have a more general statement that in the first-order model the glaciostatic pressure balances normal stress.

The presented derivation heavily relies on the no-penetration boundary condition, eqn (12). That limits its applications to circumstances when ice is always in contact with its

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bed. That seems to be too restrictive for a model that aims to be a continental scale ice-sheet model. In addition to having a different formulation for ice shelves (that would account for a free bottom surface), an ice-sheet model based on this formulation, will not be able to simulate cavitation or transitions of ice flow between grounded and floating modes where the no-penetration condition is not satisfied. In many circumstances, the boundary condition at the ice bottom surface is formulated as inequality rather than equality (so-called contact problem). It is unclear whether the formulation of the Stokes equations where pressure is eliminated is appropriate for these situations. As Richard C. A. Hindmarsh pointed out, pressure is singular at the grounded/floating transitions, it seems like a formulation where pressure is eliminated would result in at least discontinuous (if not worse) velocity field, that hardly could be viewed as an advantage of a numerical solution.

An assumption that pressure at the ice surface is zero (eqn 11) is very restrictive. Indeed, it *might* be zero, but in very limiting and, if such a term is appropriate, “boring” cases when the gradients of the horizontal velocities are zero at the ice surface. The stress-free boundary condition *is* a condition for pressure at the surface. If one would consider pressure as a superposition of the glaciostatic pressure and the “dynamic” pressure, then the glaciostatic pressure is zero at the surface, but the “dynamic” pressure is not. It is determined by no-stress condition. The presented formulation needs to be corrected to account for non-zero pressure at the surface to make the formulation useful.

Since a title of the manuscript states that the presented formulation is more computationally efficient, it is somewhat surprising that there are no computational examples to support this claim. Perhaps, few experiments from ISMIP-HOM or other simple examples could be presented (even in a 2D geometry) to demonstrate that the presented formulation is indeed efficient.

To summarize, the formulation needs to be corrected and the manuscript needs to be significantly revised before it could be published.

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