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Geoscientific
Model Development
Discussions

Interactive comment on "Derivation of a numerical solution of the 3D coupled velocity field for an ice sheet – ice shelf system, incorporating both full and approximate stress solutions" *by* T. J. Reerink et al.

Anonymous Referee #1

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In the current study, a numerical method is presented which aims at overcoming the problem of separate ice-sheet vs. ice-shelf dynamics by using an approximation that is valid for both regimes. The method is based on finite differences, and the sigma transformation is applied for the vertical direction, which maps the ice column on the unit interval. A further aspect is the introduction of "simplification coefficients" which allow to switch off terms in the momentum balance, so that several approximations (up to the shallow ice and shallow shelf approximation) can be realized.

Due to the review policy of GMDD, I have already seen the review of Anonymous

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Interactive Discussion

Discussion Paper

Referee #3, and essentially I agree with this assessment. In particular, I think the paper should not be published without a demonstration that the model works. This does not have to be a real-world problem, but the least thing I'd expect is an application to an ISMIP-type problem.

I think that the authors underestimate the problem of grounding line migration. From reading the paper, one gets the impression that this problem vanishes into thin air just by employing a common set of field equations for the ice sheet and the ice shelf, but this is not true. There is still a discontinuity in the basal boundary conditions, and the problem of determining the position of the grounding line as a function of time remains. The floatation criterion (3.1) which the authors use to distinguish between sheet and shelf domains is only fulfilled in a sufficiently large distance from the grounding line where shelf-type dynamics is fully realized, but it is not very good in the immediate vicinity of the grounding line. Thus, in a higher-order model which claims to deal with grounding line migration in a proper way it should definitively not be used. A more appropriate alternative is the formulation as a contact problem by Durand et al. (2009; Ann. Glaciol. 52., in press), which is based on (i) the topological condition that the ice base cannot penetrate the lithosphere surface and (ii) the comparison of basal normal stress in the ice and basal sea water pressure.

Another critical point that requires attention is the formulation of the boundary conditions at the atmosphere-ice and ice-water interfaces. The authors say that they are both taken as free (I assume "stress-free" is meant) surfaces by imposing Eq. (3.29), which simply states that the vertical derivatives of all three components of the velocity vector vanish. For the horizontal velocities, this is only correct if you impose the shallow ice/shelf approximations, but you won't get it in the higher-order (hydrostatic) approximation. For the vertical velocity, it is simply plain wrong (no vertical compression at the surface of an ice sheet...???).

I'm not convinced by the concept of simplification factors. Of course, it is an elegant way for the mathematical formulation, but when it comes to the concrete, numerical

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Interactive Comment

solution, I anticipate problems, especially when different simplifications are used for the sheet and shelf domains. It is naive to expect that a scheme which works for the full problem will also work smoothly for a simplified problem with efficiently different field equations in the two domains. I won't mind to be proven wrong, but this would require an application.

To sum this up: The authors describe a promising approach for a new ice-sheet/ice-shelf model which may produce interesting results in the future. However, I've got the impression that the work is not mature yet, and publication should be delayed until the remaining issues are fixed and it can be demonstrated that the model works.

Interactive comment on Geosci. Model Dev. Discuss., 2, 81, 2009.

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