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Geoscientific Model Development Discussions

Interactive comment on "Accuracy of the zeroth and second order shallow ice approximation – numerical and theoretical results" *by* J. Ahlkrona et al.

Anonymous Referee #1

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General Appreciation

This paper attempts a difficult exercise, the expansion of the shallow ice approximation (SIA) to higher order. It seems from the results of the paper that there are good practical reasons that the SIA should not be so expanded, and that alternative approaches based on the Muszynski and Birchfield approaches are to be preferred. I have made several comments about how the reader could be convinced that the problems of SOSIA have been correctly identified; have the authors made algebra error, are their inconsistencies in their asymptotic expansions. I think that readers need to be convinced further, especially as the SOSIA might be a good preconditioner for higher-order methods. The essentially negative message about the SOSIA is a valuable lesson for ice mechanicians, provided it is correct. I urge the authors to have a think about demonstrating that the problems are fundamental, and do not arise from causes listed below. The most work is presumably involved in checking that the error scaling is correct for a Newtonian rheology; demonstrating this would add to the message very strongly.

Major Points

1. Schoof and Hindmarsh expand in two parameters – aspect ratio and 'traction number' – which indicates how slippery the bed is. This distinction needs to be drawn in the introduction – it looks as though the SOSIA deals with the case of 'small sliding' – at the base, $v_x \ll O(1)$.

2. I am rather disturbed by the absence of t_{xx} in the creep response function at higher order, and wonder how it comes about. Intuitively, including stretching at the surface at higher order would would remove the singularity for all cases but uniform flow on the infinite plane and remove a lot, possibly all, of the difficulties.

3. The algebra is very complex in all these higher order asymptotic expansions. The authors attempt tests by seeing how the error scales with the aspect ratio, but owing to the complexities associated with the presence of the McMeeking-Johnson boundary layer, do not obtain definitive results. Validation of the theory and the algebra could be accomplished by looking at solutions for a Newtonian viscosity, where it is generally agreed that the error is uniformly ϵ^2. This would convince readers that the problems are associated with the nature of the asymptotic expansion and not some algebra error. I haven't checked the derivations.

4. I beleive that the approach of Soucek and Martinec (2008) Journal of Glaciology, 54, 188, 812-822 is a numerical implementation of higher-order expansions to the SIA, and should be discussed here, especially as their results seem superior to the SOSIA. Is this because of their different treatment of the viscosity near the upper surface.

5. Hindmarsh (2004, JGRF) considers something similar to the SOSIA, the L1S1 and L1S2 approximations (distinct from the L1L1, L1L2) where the SIA velocities are used to compute the longitudinal stress correction – numerically. The point here is that he found them to be dynamically unstable; do you know whether the SOSIA is dynamically stable?

6. The authors might usefully address the question of whether there is potential for the SOSIA as a pre-conditioner for more complex numerical methods.

Minor Points

1. 4286L8. Longitudinal stresses were only really used in plane flow situations to refer to t_{xx}.

2. 4289 Where are the statements of the upper surface boundary conditions? In particular, I am wondering about the one including $t_{xz(2)}$ and $t_{xx(0)}$. Some comment is needed here since we can anticipate that $t_{xx(0)}$ is singular (or requires regularization) at the upper surface.

3. 4289 Where are the statements of the creep response function at the various orders? I get the major surprise on 4299L18 that t_{xx} is neglected in the creep response function in the SOSIA? How does this come about? Isn't this the cause of all the sensitivity to the regularization parameter?

4. 4290L3 Fast sliding can be dealt with by the dual expansions e.g. Schoof and Hindmarsh etc.

5. 4291L21ff. Who says the McM-Johnson BL is thin – it's $\epsilon^{1/3}$ thick which is typically 15% of the ice-sheet.

6. 4292L21-23. Baffling sentence. What is non-ideal about the choice of the values of slope?

7. 4292L21 Presumable α is measured in degrees and not radians?

8. 4293L14. I missed the statement of the boundary condition that the basal velocity is zero; is that the case here?

9. 4296Eq(23) It strikes me that the singularity in $t_{xz(2)}$ is a bit artificial – you could have rotated the coordinate system to ensure that the shear stresses were zero at the surface. Could this have improved your solutions by eliminating a potentially problematical numerical complication?

10. 4297L20 These expressions are complex, but a simple a posteriori check is to insert them into the appropriate balance equations using a symbolic algebra program and prove that 0 == 0. Can this be done?

11. 4301L11 Why does stability enter into consideration – you aren't dealing with time-dependent problems as far as I can see?

12. 4301L26 Presumably the critical shear rate restriction refers to Elmer/ICE only?

13. 4303L10-15. You should cite the value of ϵ used in these error estimates.

14. 4309L10. This is too easy a comment – first one would have to check it can represent the grounding-line boundary layer accurately.

Interactive comment on Geosci. Model Dev. Discuss., 6, 4281, 2013.