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The Cryosphere Discussions

Interactive comment on "Manufactured solutions and the numerical verification of isothermal, nonlinear, three-dimensional Stokes ice-sheet models" *by* W. Leng et al.

Anonymous Referee #1

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1 General Comments

This article presents the construction of a manufactured solution to the momentum equations of a Stokes fluid with power law rheology along the lines of the works by Sargent and Fastook (2010). In fact - and that is my main point of critics - it is really difficult to be distinguished from the earlier publication. It basically duplicates the derivation of the solution already presented by Sargent and Fastook, without highlighting the new aspects. Especially in view of the initial statement on essential errors within the earlier derivation I would have expected to read more about this. The rest of the article is dedicated to verify the authors' own code with the obtained results. As important this is, verification of code is a routine procedure that should be undertaken by the developers, but in my personal opinion not necessarily makes a major contribution to literature. To cut it short: In my opinion the paper, as it stands now, without highlighting the distinguishing features (in terms of equations as well as results) to the earlier approach presented by Sargent and Fastook, has not enough substance to be a publication of its own. Perhaps the authors could consider writing a comment to the Sargent and Fastook publication, instead.

- *Does the paper address relevant scientific questions within the scope of TC?:* yes.
- *Does the paper present novel concepts, ideas, tools, or data?:* as I see it now, not really.
- *Are substantial conclusions reached?:* with respect to point out the novelty to Sargent and Fastook (2010), no.
- *Are the scientific methods and assumptions valid and clearly outlined?:* no
- *Are the results sufficient to support the interpretations and conclusions?:* no
- *Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?:* no, where are the compensatory terms?
- *Do the authors give proper credit to related work and clearly indicate their own new/original contribution?:* credit: yes, indicate own contribution: no
- *Does the title clearly reflect the contents of the paper?:* yes
- *Does the abstract provide a concise and complete summary?:* yes

- *Is the overall presentation well structured and clear?:* yes
- *Is the language fluent and precise?:* mainly, yes. But sometimes the authors use formulations, such as "some compensatory terms", which for me not really provides good information
- *Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?:* yes
- *Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?:* yes - as said, we would be really in need to get a clear explanation where your approach starts to differ from Sargent and Fastook.
- *Are the number and quality of references appropriate?:* yes
- *Is the amount and quality of supplementary material appropriate?:* no

2 Detailed Comments (sorted by their importance)

- I have some issue with your sliding condition (12) - (13). It appears to me, that for the horizontal directions of your Cartesian coordinate system (although you never define what system your equations are formulated in) you are directly setting

$$\boldsymbol{\sigma} \cdot \mathbf{n} = -\beta^2 \mathbf{v},$$

with $\boldsymbol{\sigma}$ denoting the stress tensor, $\mathbf{n}$ the unity surface normal and $\mathbf{v}$ the velocity vector. Correctly, the sliding law should apply to the tangential components, only

$$\boldsymbol{\sigma} \cdot \mathbf{n} \underline{- ((\boldsymbol{\sigma} \cdot \mathbf{n}) \cdot \mathbf{n})\, \mathbf{n}} = -\beta^2 \left(\mathbf{v} - \underline{(\mathbf{v} \cdot \mathbf{n})\mathbf{n}} \right),$$

whereas in case of no-penetration, the underlined term on the r.h.s. vanishes. So, in your equation (12) and (13) you are missing the underlined terms. The two expressions above are the same, if and only if either the bedrock is a horizontally aligned flat bed, or if you transform the relations into a local normal-tangential coordinate system. The consequence of both situations would be that the surface normal by definition reduces to $(0,0,1)$ and all the derivatives of the function b vanish, leaving r_b to unity value, limiting contributions in (12) and (13) only to vertical shear stresses and, finally, reducing (14) to non-penetration condition $w = 0$. I realized, that you adopted this part from Sargent and Fastook (2010), so there the same problem persists. In any way, you have to exactly define the coordinate system you are referring to and, should you introduce local transformations, define them and use distinguishing notation in order to indicate an altered coordinate system (and remove the vanishing terms). Later in the text (page 2701, line 16) you state that the sliding conditions (12) and (13) in case of a sliding condition (which is kind of a contradiction, because either you apply a sliding condition or not) has to be revised - perhaps the necessity of adding additional terms could be a consequence of the missing contributions?

- I have difficulties to understand Equs. (50) - (52): you claim, that they represent a zero-velocity condition at the bedrock. I disagree. If you take expression (50) and set $z = b$ you get

$$u(x, y, z = b, t) = c_x \left(\frac{s - b}{s - b} \right)^4 = c_x,$$

which is zero only if $c_x = 0$. In this special case, nevertheless, the complete velocity field in x-direction after (50) is identical zero and you would loose your coupling to the temporal evolution in (51).

- Closely linked to the critics above is my difficulty to interpret Fig. 3: First of all, it would be good to annotate the columns and rows. Secondly, you should include the evolution of the free surface s in your results (you could thus omit Fig. 1 , instead). Thirdly, at least for one row you should apply the same range of values in the color bar in order to make it comparable - I for instance cannot interpret whether the value of u is 46 or zero. Due to (50) it should be zero. As it has the same color, I then presume also $v(t \to \infty) = 0$, which would mean that $c_y = 0$, which by page 2705 does not seem to be the case (although with 10^{-9} it is very small). Not having seen your compensatory terms, I do not trust whether your simplifications to (36) - (38) are still consistent.

- Fig. 4: To enhance the understanding, you could show the different components and not just the norm of the velocity.
- page 2695, line 16: *We also point out that, in general,* $b(x, y) \neq s(x, y, t)$ *along the boundary of* Ω_H *so that the ice sheet may also have a lateral boundary* Γ_l *with some appropriate boundary conditions posed there.* In fact, your equations are getting singular if $b \to s$, so this is not an option, but a necessary condition for the construction of the manufactured solution.
- page 2701, line 9: *In order to maintain the equalities of these equations, some additional compensation terms need* <u>to</u> *be added to the right-hand sides of the Eqs. (1)-(3) and Eqs. (8) - (10);*

Interactive comment on The Cryosphere Discuss., 6, 2689, 2012.

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