

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

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Summary of the manuscript: The motivation for this manuscript is to improve the testing of 3D numerical ice sheet models that describe the ice by the isothermal, Glen law Stokes equations. This manuscript constructs exact solutions to a generalization of the Stokes equations, in the sense that the momentum equations and the surface kinematical equation have extra terms so that the artificially constructed functions (“manufactured solutions”) really are solutions of these modified Stokes equations. The specific construction here corrects the construction in a closely-related work (Sargent & Fastook, 2010). Then the manufactured solutions are used as verification tests for the

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parallel, finite element Stokes solver built by the same authors (Leng et al., 2012 JGR), and convergence under grid refinement is shown for their model.

Major comments and conclusions: This paper both corrects errors in the Sargent & Fastook paper and actually demonstrate convergence of a numerical model under grid refinement, which was not done by Sargent & Fastook. Because the correction of errors is an important aspect of this paper, the claimed errors in Sargent & Fastook should be stated specifically. The reader needs to know where in Sargent & Fastook is the first error, and what equations are in error. The construction of the manufactured solution would be much clearer if errors in equations (23) and (50) in this manuscript are corrected, and certain odd aspects inherited from Sargent & Fastook are corrected (esp. wrong sign on pressure). The presentation of convergence is minimal, and figures are not used effectively to show the locations and/or character of the largese errors. More significantly, much effort is made to manufacture time-dependent solutions with moving surface, but then grid refinement beyond the initial time (i.e. at some $t > 0$) is not shown, and the reader might wonder if the model achieves it. It is not clear (at least in the manuscript) whether the code which demonstrates grid refinement is open source, which would be advantageous for claims of grid refinement. The best motivations for publication of this manuscript are that it corrects errors in a Cryosphere publication, and that grid refinement of a Stokes solver is actually shown. However, it is only appropriate to publish a carefully-revised form of the manuscript which addresses these several flaws.

Specific and constructive comments:

title: The title is too long. “Manufactured solutions and the verification of three-dimensional Stokes ice-sheet models” would be long enough.

abstract lines 7-8: The phrase “and other model parameters” is either worth expanding, or it should be removed. Perhaps “the geometry of the ice sheet, basal sliding parameters, and the ice softness.”

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abstract lines 9-10: Break run-on sentence. Perhaps "... tests. The upper surface is altered ..."

abstract lines 12-14: Note "excellent agreement" is in the eye of the beholder. No argument is made that it is "excellent" in the paper. What is shown is more important anyway, namely convergence. So I suggest: "Results from the computational model show convergence under grid refinement using the manufactured analytic solutions."

abstract: Is well-written and clear.

page 2690, lines 16-23: Fine V&V summary.

page 2690, lines 24-26: This sentence citing Alley and IPCC is unnecessary, as no Cryosphere reader has missed these ideas.

page 2691, lines 6-7: The phrase "is generally accepted as the gold standard for the modeling of" is silly. The reader who is interested in anisotropy or fracturing or etc will disagree anyway. And it is common that simpler models explain more in some situations, too. The phrase "is the standard non-shallow description of", or similar, is surely sufficient.

page 2691, lines 6-24: This is a reasonable summary of the situation.

pages 2691-2692: This may be the most important sentence in the current manuscript: "However, due to essential errors in their solution method for this key part in the three-dimensional case, the Sargent and Fastook (2010) manufactured solutions .. for the 3-D Stokes model are incorrect." If this is an accurate assessment of the Sargent and Fastook (2010) solutions then the current manuscript needs to be published, even as an expanded erratum. Thus the authors *owe the reader a more precise statement*! What is wrong (i.e. which equations in Sargent & Fastook) and which equations here are corrections?

page 2692, lines 17-18: The ending "... in a low Reynolds-number flow" is redundant and unnecessary; a Stokes flow is the zero Reynolds-number limit.

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page 2693, equations (1)-(3): These equations appear to be inherited from Sargent & Fastook, and I think they have a nonstandard sign on pressure. E.g. compare Pattyn et al 2008. In particular, $\sigma_{ij} = \tau_{ij} - p \delta_{ij}$ is, I believe, the standard sign convention. Thus the trace of σ_{ij} is $-3p$ so that compression (i.e. diagonal of σ_{ij} are negative numbers) is a positive pressure. If this is indeed the nonstandard choice it should be clearly stated. Better, it should be fixed to match the standard literature.

page 2693, equation (5): This equation seems also to be inherited from Sargent & Fastook, and it is not obviously correct. Now, it *is* correct, because of a particular method of using incompressibility, but why not write it so it is manifestly the matrix norm $|A|^2 = 2 \text{trace}(A^T A)$, i.e. the second invariant? Why confuse readers? The form in Leng et al. (2012; JGR) is correct and manifestly so.

page 2693, last line: This defines Ω_t at a fixed time t , and it should define a subset of \mathbb{R}^3 (i.e. space only, not space-time). If I am correct then the definition of this set should *not* have " $t \in [0, t_{\max}]$ " inside the curly brackets. The condition could be stated as "at each time $t \in [0, t_{\max}]$ we define the set ..." in the preceding sentence.

page 2694, equations (8)-(10): Up to the sign convention on pressure p , previously mentioned, these equations are correct. Are they stated with the $1/r_s$ factor because that is the way the equations are scaled numerically? More generally, how are the equations scaled before numerical solution? (This may be addressed somewhere in Leng et al. (2012; JGR), which would be fine.)

page 2696: It looks to me that equations (15)-(17) are correct. Have Sargent & Fastook made an error at this point, or does it occur later?

page 2697, equation (23): This is either wrong or makes the assumption that the surface $s(x,y,t)$ and bed $b(x,y)$ elevation functions do not depend on y . This would seem not to be true of the solution shown in Figure 1. What's going on? I think (23) is in

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error.

page 2701: State that the manufactured solution is available in C form in the supplement.

page 2701: This long paragraph is the high point of the paper! So give some real information on the manufacturing, such as the size of the compensatory terms relative to the other terms in the equations that they balance. Use Figures effectively here, instead of the mindless "simulation results" which appear instead.

page 2703, lines 8-9: I think the phrase "we specify $\gamma_1 = 0$ and $\lambda_1 = 4$, in which case that integral becomes ..." is carrying too much load. Please say something straight like: "Set $\gamma_1 = 0$ and $\lambda_1 = 4$. Then equation (37) simplifies to ... and Equation (38) simplifies to ..."

page 2704, formula (50): This must be in error. If I follow the earlier formulas, this one must be: $u(x,y,z,t) = c_x \left[1 - \left(\frac{s-z}{s-b}\right)^4\right]$. With this correction, lines 8-9 on the same page ("The solution (u, v, w) defined by Eqs. (50)-(52) satisfies a pure zero-velocity boundary condition on the whole bedrock surface ...") makes sense because the velocity is actually zero at the base.

page 2705, "4.1 Model convergence": Grid refinement is shown at $t=0$. How about at later times? This is presumably very closely-related to the point of manufacturing a time-dependent solution. Aren't we trying to simulate the co-evolution of the velocity field and the surface elevation? Isn't that the point? Why go to all this work if the outcome is so weak? At very least, what is the surface elevation at later times? (That error can be compared to shallow theories for the same problem.)

page 2710, Table 1: The meaning of the numbers in this table needs to be greatly clarified. First, is the velocity error relative or absolute; what are the units? (Presumably m/a.) Same question for "Pres. error", i.e. pressure error. How are the convergence rates calculated? (Between consecutive pairs or using all data "so far"?) Presumably

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the function which is claimed to be a power law, to compute such convergence rates, is the numerical error proportional to a power of the number of DOF, but even that is not clear! Maybe numerical error depends on element diameter? This table and the related text needs to be better written and more informative.

pages 2711-2712, Figures 1 and 2: Because of the convergence under grid refinement, this Figure presumably both shows simulation results and the manufactured ice sheet geometry, at least up to "screen accuracy". This may be worth saying in the caption.

pages 2713-2714, Figures 3 and 4: The text on these figures is unreadably small.

pages 2711-2714, Figures: The authors should be much more selective about what is shown in the four figures. What does the reader need to know about these manufactured solutions? Note that there is no good reason to show "simulation" results once you know you have several digits of accuracy under grid refinement, as already asserted.

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

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