

Interactive comment on “Practical analytical solutions for benchmarking of 2-D and 3-D geodynamic Stokes problems with variable viscosity” by I. Yu. Popov et al.

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Received and published: 6 February 2014

The authors present here a nice piece of work and elegantly show how to obtain families of Stokes flow analytical solutions in two and three dimensions. They also go one step further by showing numerical results (error convergence) based on these solutions and also provide the matlab codes they have used. After a careful read, here are my comments.

First of all, I believe that this article needs to be partially rewritten and/or corrected by a native speaker. As it is, I find it difficult to read and its overall quality would improve by straightening the grammar.

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I understand that the list on page 2205 isn't exhaustive, but a few articles could (should?) be added to it: - 2D & 3D shear band formation & plasticity implementation (Lemiale et al, PEPI 2008 ; Kaus, Tectonophysics 2010; Thieulot et al, JGR 2008) - Busse et al, 3D convection at infinite Prandtl numbers in cartesian geometry, Geophys. astr. Fluid Dyn., 1993 - Kronbichler et al, GJI 2012 contains examples of SolCx, 2D circular inclusion, Blankenbach et al, and Busse et al. - (Gerya and Yuen, PEPI 2007), (Thieulot , PEPI 2011), and (Gerya 2010) should be added to the numerical sandbox experiment item - Duretz et al, Gcubed 2011, showcase SolCx, SolKz, and R-T instability. - falling block (Gerya and Yuen, PEPI 2003, Gerya, 2010, Thieulot PEPI 2011)

Sections 2&3 contain the real body of the research and they are rather detailed and easy to follow. Nice analytical work. Since the SolCx and SolKz experiments are now more or less standard in the community, it would be helpful if the authors could somehow cast these in the framework of their analytical work and explain how their solutions were arrived at. Also, may be the authors could comment on whether similar derivations could be done for compressible fluids for instance.

In section 4 the authors compute the solutions obtained with a FD code and compare them to the analytical solutions presented in the previous sections. Various error norms are then used to look at the measured error as a function of the grid size. Many aspects of this section need to be revised:

- the authors do not provide the reader with enough background theory nor literature examples regarding the expected error convergence. Given this particular type of Finite Difference stencil, is there an analytical prediction of the expected error convergence ? Given that the code they have used is most likely similar to the one in Duretz et al, G³ 2011, they should mention beforehand what these authors reported so that the sentence "The calculations show that one has good convergence of the numerical scheme for small viscosity contrast, but it is not so for high viscosity contrast " makes actual sense. Also, "good convergence" without a reference point is meaningless. - 18

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tables are shown but never discussed (the tables are barely mentioned in the text: line 15 page 2227 and line 16 page 2228). Are they all necessary ? Does the use of three norms bring something more than using only one ? - 28 figures are shown and barely discussed. Are they all necessary ? - The convergence plots (figs. 3, 6, 9, ...) do not have axis labels. - Why do the authors restrain the (2D) grid size to such low values and to such a low ratio of variation (typically testing 12x12 up to 48x48, i.e. $\log_{10}(h)$ ranging from -1.08 to -1.68) ? Duretz et al show results with grids ranging from 41x41 up to 1001x1001, for instance. - All the figures pertaining to 2D simulations show a caption referring to the x and y axis, and the velocity components are accordingly referred to as v_x , v_y . What is the z coordinate mentioned above every colour plot ? - It is a very positive point that the authors provide the matlab scripts that they have used, but the labelling of the files is misleading (they all have a name containing '3D' while half the simulations are carried out in 2D). A short text in the appendix should explain the content of these files and which one was used in which section.

Section 5 is the conclusion section In absence of a discussion section, the conclusion is too succinct and it certainly should be expanded.

I believe that this article must be rethought. After all, the real results are the families of analytical solutions to the Stokes equation in 2D and 3D, with large viscosity contrasts. Such solutions are indeed useful to benchmark existing or new codes, and I believe the analytical work alone deserves publication. However, the numerical section tends to overshadow the analytical work by showcasing so many tables and figures. Since the authors do not expand on the code they have used, and since they do not put their results into context, this section only illustrates the fact that with increasing viscosity contrasts in a system the quality of the solution decreases (i.e. the convergence rate decreases). This is however not new, see Fig. 14 of Duretz et al, 2011.

I would suggest the authors to either greatly reduce the numerical part while giving more context, or split the article in two distinct articles. In the latter case, the first article would be about the derivation of the family of solutions, and could propose/label

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a few carefully chosen ones. The second one would require additional work, such as running these experiments on a variety of codes (either through collaboration or by acquiring freely available downloadable codes, such as MILAMIN or ASPECT and make a thorough analysis of their performance when the viscosity ratios are progressively increased.

Interactive comment on Solid Earth Discuss., 5, 2203, 2013.