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Geoscientific Model Development Discussions

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Interactive comment on "Analytical solutions for mantle flow in cylindrical and spherical shells" *by* Stephan C. Kramer et al.

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General Comments

The paper at hand derives families of analytic solutions for the incompressible Stokes problem with constant viscosity for 2D (annulus) and 3D (thick spherical shell). In each case a family of solutions is constructed for the case of a forcing term arising from a smooth density perturbation and an infinitely thin (delta-function) perturbation.

Based on these solutions numerical simulations are performed using different stable Finite Element pairs using the code Fluidity. The findings of these tests are discussed, providing especially a detailed analysis of convergence behaviour for the delta-function

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case by means of Finite Element theory.

The reviewer wants to start by stating the he definitely enjoyed reading the paper. The mathematical derivation is sound and interesting. The numerical tests demonstrate the usefulness of the derived solutions for verfication of simulation software for global convection models, i.e. the choice of valid algorithms for discretisation and numerical solution of resulting linear systems as well as their correct implementation.

While the chosen model (incompressible, constant viscosity) is, naturally, not the most interesting one from the geodynamics perspective, solutions for this case are still valuable, as they allow to verify correctness for this baseline scenario.

The reviewer also especially liked the enlightening discussion of the convergence in the delta-function setting as this is a commonly used test scenario in publications in the field.

The only point of criticism is the following. While a large number of references are given for classical benchmarking studies that compare different codes against each other, there are only two citations, Zhong (1993), Kramer (2012) given involving analytical solutions. However, in recent years there have been several attempts to derive (semi)-analytic solutions for the simple and more intricate settings in both cartesian as well as cylindrical and spherical coordinates, see e.g. [BMP16,HMB20,PLP+14,Thi17], while a manufactured solution in spherical coordinates (for a ridge-like model) is also given in Burstedde (2013), cited in the paper. Thus, a brief explanation how the authors' contribution fits into these various approaches would be appreciated.

Specific Comments

- Equation (65) for computing the relative error: You are working with two different domains, Ω being the physical problem domain, and Ω_h the computational domain for the finite element method. I assume that the computation of the error

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happens w.r.t. Ω_h. In the isoparametric approach that does not make a significant difference, but for the affine mapping approach, Figure 5, I guess it would be better to make that distinction explicit.

- You treat the case of both boundaries being either zero-slip or free-slip. However, aren't many simulations run as a 'mixed' case, i.e. with Dirichlet boundary conditions for velocity (from plate reconstructions) on top and free-slip conditions at the CMB? In that sense, would it make sense to add (maybe in an appendix) also the coefficients for such a mixed case?

- p. 6, line 9 and equation (21): Shouldn't this read $R_- < r' < R+$ instead of $R_- \leq r' \leq R+$ and the piecewise ψ be formulated for the two parts $R_- < r'$ and $r' < R_+$ only? Just to be mathematically more precise?

- Equations (25) and (26): To me the transition from (25) to (26) seemed mathematically quite involved, e.g. if one was to evaluate the integral in the right-hand side of (25) one would get a zero. Maybe you could add some additional details on this transition?

- p. 7, line 16: You are making use of the Mie representation of the velocity field. The necessary condition for this to have the form (27) is that the velocity field is *solenoidal*. For R^3 this is equivalent to u being divergence-free. However, in the case of the target domain, the thick spherical shell, the two properties are not the same. You get that u is solenoidal from u being divergence-free and the fact the you have no outflow in the boundary conditions you consider. You might want to reformulate the sentence in this respect.

- If I have not missed it, you do not explicitely specify how r' was chosen in you numerical tests. I assume that this is an interface between layers of the mesh already for the coarses mesh resolution used, isn't it? Could you please add that detail from completeness.

Out of curiosity, would you expect to observe oscillations in the convergence behaviour, if r' was not a layer boundary on the meshes? I remember that in dipole modelling (geoelectricity and EEG simulation) people sometimes resort to special discretisation approaches for the δ-function, e.g. St. Venant's principle, to avoid such issues.

- Sec. 3.4: The problem matrix representing the discretised Stokes system is singular independent of the type of boundary conditions due to the pressure only being determined up to an additive constant, isn't it? The free-slip conditions enhance the kernel of the matrix significantly leading to higher numerical effort (performing step (64) in each iteration seems required, while pressure needs only be adapted following (62) once in the end); you might want to make that clearer in line 15.

Suggestions

- p. 4, line 19 and p. 9, line 9: A reference for the biharmonic equation resp. its solutions might be helpful for the general audience; it is not quite as common knowledge as the harmonic equation and its solutions
- p. 4, line 14: You might consider changing 'top and bottom' to 'inner and outer' for the cylindrical domain
- p. 6, line 18: Shouldn't the strip be defined as $(r' - \epsilon, r' + \epsilon) \times (0, 2\pi)$?
- p. 9, line 3: The standard definition of the term domain in calculus is *an open and connected set*, so *connected domain* sounds like a pleonasm ;-)
- p. 4, equation (19) and line 19: I must admit that as I reader I very much behave like a one-pass-compiler, as soon as I encounter something I cannot follow I stumble. For the sake of people like me you might consider moving that sentence

which explains why there's only a $\cos$ term in (19) up front. Also you might state that you consciously neglect other harmonic functions (such as $\ln(r)$), for similar reasons as given in line 13.

- p. 1, line 2: IMHO 'within' does not sound quite proper here?
- In your paper you are using the term 'natural' boundary condition. If I understand correctly, you mean 'inspired by nature'? I am asking as in classical FE analysis there is that distinction between 'natural' and 'essential' boudnary conditions, so I was at first glance a little confused. Maybe change it to 'physical', if that still fully expresses what you want to convey.
- p. 1, line 24: Aren't 3D global mantle convection models being simulated routinely today, and not only *becoming more common*? I mean, you site references from the last 35 years ;-)
- equation (A7): Is there any specific motivation for defining the 2D polar curl in this way? It seems to be just the negative of what one would obtain by constantly extending a 2D field in z-direction and taking the z-component of the curl in 3D cylindrical coordinates?

Questions out of curiosity

- I found your discussion of the reason for the reduced convergence rates in the δ-function case with the $P_2 - P_1$ Taylor-Hood element very interesting. As (68) only contains the H_1 semi-norm of u, is there an easy way to see from (70) why we get a similar $\frac{2}{3}$ order reduction in the H_0 norm of velocity as we do for pressure? Maybe I am missing some standard FE-analysis argument?
- Do you have any (speculative) idea why that convergence issue is not observed when one only examines surface quantities?

- In your 3D test cases you always select two similar combinations of degree and order (ℓ, m), which is $m = \ell$ (sectoral) and $m = \ell/2$ (tesseral); I was a little surprised that the errors seem to be fully identical for the two choices, because the number and direction of nodal lines differs. Are the differences just too small to be visible in the figures? Can you comment on that?

Technical Corrections

- equation (14): please check sign, I might have miscalculated, but I think it should read $H_n = + \ldots$
- p. 6, line 15: 'expect a continuity' $\rightarrow$ 'expect continuity'
- p. 21, line 12: solution $\rightarrow$ solution(s)
- equation (A3): $\hat{\varphi} \cdot \nabla \hat{\varphi} = \frac{1}{r} \frac{\partial \hat{r}}{\partial \varphi} \rightarrow \frac{1}{r} \frac{\partial \hat{\varphi}}{\partial \varphi}$
- equation (A12): spurious $+$ near end of equation
- reference Hernlund, Tackley, 2007: IMHO that should be 2008

References

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