Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-194-AC3, 2021
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Geoscientific Model Development Discussions

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

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Received and published: 12 February 2021

We thank the reviewer for their comments. As we have indicated in our response to the first reviewer (Mohr), we agree that more references should have been included to reflect more recent studies with analytical Stokes solutions in shell domains. We have addressed this in a revised manuscript by adding references to these more recent publications and by adding a new section that discusses the relation between the solution sets in this paper and those publications. We thank the reviewer for highlighting this shortcoming of our original manuscript.

Regarding the novelty of the paper, the mathematical techniques that were used in the paper are classical and have been used elsewhere, e.g. to derive the solutions in

the propagator matrix method that is used in the Zhong '08 benchmark. The solution for that case corresponds to one of the eight cases that we present in the paper, and the solution coefficients for these have appeared elsewhere, e.g. Ribe '09, which we cite. The solutions for the other seven cases we have not seen derived and presented elsewhere. Although the solution coefficients are simply given after our summary of the classical theory to derive these, the actual derivation is laborious and error-prone. We therefore believe these to be of significant value for the geodynamic modelling community, in particular in combination with a software implementation in the form of the assess python package. We note that reviewer 1 and Cedric Thielot, both world leading geodynamical modellers, agree with this viewpoint.

In addition, although the techniques for deriving analytical Stokes solutions in spherical domains are more familiar in this community, the equivalent derivations in 2-D cylindrical shell domains are less well known. The mathematical techniques for deriving these are not new, but we believe there is value to the community in our comprehensive overview. Finally, we discuss an issue that has been overlooked in all recent global mantle convection validation papers, which is that models based on a continuous pressure discretisation have a very poor convergence for cases with a delta-function forcing in the interior of the domain, which will not be noticed if one only examines the surface response.

Of the additional references you provide, [2] appeared just before we submitted our paper. In our response to Marcus Mohr, one of the co-authors of the paper, and in the new discussion section at the end of our revised manuscript, we have indicated how our solutions relate to theirs. The ASPECT paper [4] uses the same Zhong '08 test case we already mentioned. We have now added this paper [4], in our discussion alongside three other papers that have used the same approach (CitcomS, Rhea and TERRA). References [1] and [3] indeed, contain cylindrical test cases for Stokes flow, but [3] only contains a Method of Manufactured Solutions (MMS) case, the limitations of which we have discussed in our paper; [1] appears to contain a test case with a

driven cavity-like solution in an annulus segment, however it does not actually present the analytical solution, other than in a figure, or how it has been derived. Nonetheless, for completeness we have added references [1,3] to our modified discussion section.

Additional suggestions:

1. *The abstract and introduction can be improved by including self-explained sentences and letting citations only for verification purposes. In particular, the sentences "Computational models of mantle ..." in the abstract, and "3-D spherical geometry is implicitly required to simulate global mantle dynamics" in the introduction must be complemented with a brief explanation, from the physical and numerical point of view, of the loosing when considering a cartesian model of the globe.*

   We have re-read the abstract and introduction and disagree with the reviewer here. The first sentence of the abstract is concise and to the point - with further information provided in the introduction. Furthermore, we do not think that a qualifier is necessary for the second suggestion. The mantle is a 3-D spherical shell: simulations of 'global' mantle dynamics therefore cannot be undertaken in Cartesian domains. We are also unclear what the reviewer means by "cartesian model of the globe". Perhaps the reviewer is referring to structured, "cubed-sphere" approaches? Regardless of the numerical approach, the physics of global mantle convection takes place in a 3-D spherical-shell geometry. We have rephrased the second sentence to make it more clear that we are referring here to the step between processes in a Cartesian physical domain, and global mantle convection models.

2. *I strongly support the suggestion of considering a more deep literature review. In particular it is also missing a discussion of the already existing benchmarks in the FE community for Stokes equations in smooth domains (e.g., [1,3]).*

   Discussed above and addressed in our revised manuscript.

3. *Please review the punctuation of the entire document. In particular, equations must be treated as part of the text. For instance, equations (6), (9), (10)(this is a typo), (12), ... must be ended with a "dot".*

   We have reviewed and corrected the punctuation of all equations as suggested. Thank you for highlighting this issue in our original submission.

4. *In line 20, page 4 add "s" to relation*

   Corrected.

5. *In line 17, page 5 (and in the rest of the article when it corresponds) add "coefficients" after "solution". i.e., write "solution coefficients"*

   We have followed the reviewer's suggestion throughout.

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