

Review of “The impact of rheological uncertainty on dynamic topography predictions: Gearing up for dynamic topography models consistent with observations”

October 2019

The authors have performed numerical simulations of Stokes flow for a density anomaly in the mantle under a variety of different rheological assumptions. These simulations are benchmarked against analytical solutions for some of the simpler model setups. More complex behaviours are then explored, including using a power law rheology for which analytical solutions do not exist. The authors show that the rheological choices can have a profound impact on the observed dynamic topography observed at the surface.

The paper is mostly well written and contains a simple yet powerful illustration of some of the potential pitfalls in modelling dynamic topography. Some of the effects that are highlighted are already relatively well known, but are worth repeating and are useful in combination with the new results for the power-law rheology. My principal issue surrounds the motivation for the study, which is ostensibly concerning the amplitude mismatch between observed and predicted dynamic topography at long wavelengths (spherical harmonic degree ~ 2). However, I think that the model set up means that the main conclusions are probably more applicable for shorter wavelength features, and the significance for long-wavelengths mismatch remains under-explored. Nevertheless, I still think that this is an elegant illustration of some of the caveats associated with mantle convection modelling, and recommend that it be published in Solid Earth Discussions.

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Detailed comments (line numbers from the supplied .pdf):

Main comment:

Discrepancy between observed and predicted dynamic topography: As you explain in Lines 41–55 there is a mismatch between the amplitude of observed residual topography and dynamic topography predicted from simulations. Over the last few years, there has been a general focus on the long-wavelength (degree 2) components, where the driving density anomalies have comparable lateral scales to the depth of the mantle. Instantaneous flow kernels (with no lateral viscosity variations) show that the effect of features such as a low viscosity asthenosphere are less pronounced at the lower degrees than at higher degrees (shorter wavelengths). Thus, I think that the experimental set up that you are using is more suited to comparison with shorter wavelength density anomalies, and the results on long-wavelength dynamic topography predictions could turn out to be less dramatic.

Nevertheless, I think that there is also potentially an issue with amplitudes at short wavelengths. Studies that attempt to include the shallow mantle tend to predict larger dynamic topography than we observe in residual topography (e.g. Steinberger, 2016; Steinberger *et al.*, 2019; Davies *et al.*, 2019). My suspicion is that the conversion between seismic velocity and density structure is largely to blame, but your results show that the rheological assumptions may also be a significant factor. I therefore think that the motivation in your study should probably be more nuanced than it is currently written.

Additional comments:

L15–17 (in abstract): In this sentence, it is unclear that you have shown that using a power law rheology reduces dynamic topography and so potentially helps to explain this discrepancy. Please clarify, particularly the final sub-clause.

L34: “...created by plate tectonic processes.” I think this should be expanded further to improve clarity. Essentially, it is dominated by isostatic topography associated with variations in the thickness and density of sediments, crust and lithospheric mantle.

L34–39: I think that this section is a little misleading. There are two separate types of observation: i) the absolute amplitude of dynamic topography at the present-day and ii) the rate at which it is changing. Measurements of residual topography constrain the former, as you explain in the next paragraph. The couple of sentences here on sedimentary basins are more to do with the rates of change, and in that sense are a little out of context with the rest of the manuscript. I’d suggest either clarifying this issue or removing these sentences.

L43: “...isostatic components...” is a little vague. Specifically we want to remove isostatic topography arising from sediments, crustal structure and the lithospheric mantle if we want to investigate signals arising from deeper mantle convection.

L47: Rather than the accuracy of the measurements, it is more whether the measurements are truly a proxy for deeper mantle contributions that depends upon the factors you highlight here.

L59: Repetition of “*In this paper...*”.

L67: Replace “...lesser magnitude...” with “...lower amplitudes...”.

L85: Replace ρ with $\Delta\rho$ and explain the difference between air and water-loaded dynamic topography.

L95–96: This is a little hard to read and would benefit from clearer grammar.

L108: Replace “...normal total stress...” with “...total normal stress...”.

L109: “...mass anomaly per unit length...” – what length is this referring too?

L111: This needs a lead in sentence. Something like “Total normal stress can be calculated in the Fourier domain according to...”

L122: Start this sentence with a clause like “Although unrealistic for the Earth, under the assumption where...”

L140: What is the purpose of this crustal layer? Is it an elastic lid? Does it have a rheology that deforms during the simulations? Please clarify. It does not show up in the Figure pictures.

L159–160: Does this affect happen in all of your simulations?

L169: Qualify what the asthenosphere here refers to. Is it the whole of the rest of your model domain beneath the lithosphere? How is the asthenosphere defined?

L197–199: Good! This is a very clear and useful explanation of the cause of this behaviour.

L225–227: I did not know that this was generally accepted. Is this an opinion of the authors? Some back up references would be helpful. I agree that larger deviatoric stresses are thought to promote deformation by dislocation creep.

L169: Typo – currently reads “...creep of wet dry olivine...”

Figure 1: I think the y-axis in panel (b) would be better as dynamic topography for comparison to panel (a). Also, the key in (b) is a bit messy... A legend as in panel (a) would be clearer.

Figure 3: These are great, but could do with standardising to make it a truly iconic figure. Could you i) add a line above the surface showing the dynamic topography (or state the peak value), ii) make all streamlines the same colour (either white or black), iii) place the key entries in their true depth order (lith, channel, asthen). I also think it could be clearer that the relative viscosity jumps between layers are what is important, rather than absolute values, but it is fine as is.

References

- Davies, D. R., Valentine, A. P., Kramer, S. C., Rawlinson, N., Hoggard, M. J., Eakin, C. M., & Wilson, C. R., 2019. Earth’s multi-scale topographic response to global mantle flow, *Nature Geoscience*, **12**, 845–850.
- Steinberger, B., 2016. Topography caused by mantle density variations: Observation-based estimates and models derived from tomography and lithosphere thickness, *Geophysical Journal International*, **205**, 604–621.
- Steinberger, B., Conrad, C. P., Tutu, A. O., & Hoggard, M. J., 2019. On the amplitude of dynamic topography at spherical harmonic degree two, *Tectonophysics*, **760**, 221–228.