

## Interactive comment on "The impact of rheological uncertainty on dynamic topography predictions: Gearing up for dynamic topography models consistent with observations" by Ömer F. Bodur and Patrice F. Rey

**Bernhard Steinberger (Referee)** 

bstein@gfz-potsdam.de

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While I think the general idea of the paper – to study how much lateral viscosity variations (LVV) due to temperature and strain rate dependence may help to explain the discrepancy between the (higher) amplitudes of dynamic topography inferred from mantle flow and the (lower) residual topography estimates based on observations is useful in that it addresses an unresolved problem, and I also appreciate the relatively simple setup, which should help with gaining a qualitative understanding, I think the current paper suffers from several shortcomings, which limit its usefulness.

C1

Firstly, the parts without LVV add nothing new to what is already known. Of course, I realize that these are mainly meant for comparison with the later results with LVV. But that a contrast between low-viscosity mantle and high-viscosity lithosphere leads to increased dynamic topography, and the topography gets higher the stronger and/or thicker the lithosphere is, and that an asthenospheric low-viscosity channel leads to reduced topography can all be inferred from topography kernels (see e.g. my papers from 2001 doi:10.1016/S0012-821X(01)00229-1 Fig. 2 and 2016 doi:10.1093/gji/ggw040 Fig. 3), for a broad range of depths and size of anomalies (corresponding to spherical harmonic degree). In contrast, your results are just for particular anomaly depths and (rather small) size compared to what is seen in tomography. I think for such small scales the effect of a low viscosity asthenosphere channel are stronger than for the larger scales seen by seismic tomography. E.g. in my 2016 paper Fig. 9a I find that one needs a very strong reduction in asphenosphere viscosity in order to get an appreciable reduction in topography, if anomalies are inferred from tomography. So I think the comparatively strong reductions in topography you show for a low-viscosity channel are partly misleading. Also, in my Tectonophysics paper (doi:10.1016/j.tecto.2017.11.032) I find that the largest discrepany by more than a factor 2 is at spherical harmonic degree two, whereas the discrepancy is much smaller at higher spherical harmonic degrees (i.e. smaller scales). It seems that your results could mainly explain a discrepancy at small scales, whereas the real discrepancy is at very large scales, and your results cannot explain this.

Secondly, I think the usefulness of the models with LVV is severely limited because of the limitation of viscosities to the interval 10\*\*19 Pas to 10\*\*22 Pas. In this way, model 4b is almost the same as model 1 with constant viscosity (and giving very similar amplitude), model 4a approximately corresponds to the 2-layer model with topography accordingly increased, and model 5b has a low-viscosity channel with topography accordingly reduced. What I am puzzled about, though is that case 5a gives almost the same topography as 4a although it is also two-layer (although with thinner lithosphere). I think this limitation kind of beats the purpose of introducing a realistic

rheology, because models essentially turn out to be a more complicated implementation of the easier models without LVV above. Also, I expect that without a cutoff, lowering activation enthalpy would not only lead to overall reducing viscosity, but also reducing viscosity contrasts. So, in contrast to your results I would expect a weaker lithosphere-asthenosphere contrast, and hence reduced dynamic topography for the lower activation enthalphy.

In the following are a few more consecutive comments: 1.54: as said, this large descrepancy is at the very largest scales, much larger than your model. I. 220 this equation could actually be quite simplified. Because grain-size exponent p=0 the factor d\*\*(p/n) is equal to 1 and therefore disappears. In each case, A\*\*(-1/n)\*f\_H2O\*\*(-r/n) is just a given number so you could simplify the equation in this way. I. 223 should be "volume and energy" (i.g. the other way round) I. 273 should be "wet olivine" (remove "dry"). I. 321 I don't know where I would have said in that paper that the misfit demands a scaling factor  $\sim$ 0.35, It it true that one needs to downscale shallow seismic anomalies, but I believe this has nothing to do with viscosity structure; it is rather because the thermal anomalies and corresponding seismic anomalies in the lithosphere are largely compensated by chemical anomalies, with a much smaller seismic signature. Fig 1 a: Why the results for Morgan Hard Sphere and Molnar Hard Sphere are different? I think they are both analytical results, so they should be identical. Fig. 7c: Viscosity 10\*\*20 Pas or 10\*\*21 Pas at the lithosphere-asthenosphere boundary both seems much too low to me. Table 1: For better comparison with text and eq. 5, you could also include the symbols (in those cases where you have defined them) in another column. I think the units for the pre-exponential factor should be MPa\*\*-ns\*\*-n (not -1)

minor comments: I. 28: write "from the surface" I. 65: better "dependence ... on" ?

СЗ

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