Author's reply to RC2

Anonymous Referee #2:

Authors Rubey et al. presented a global geodynamic prediction of dynamic topography from 200 Ma to the present. They used a forward modelling approach by mostly considering the effect of sinking slabs. The results compare relatively well to other studies including those using inverse approached based on seismically converted present-day mantle density structures. I find the global framework of dynamic topography history provided in this study to be especially useful for further references in similar modelling work. I do have some minor suggestions for improvement (as discussed below). Overall, this work is solid and reasonable, and I would suggest publication with a minor/technical revision. The topic of dynamic topography has generated enormous amount of discussion and debates recently. Part of this is due to different (sometimes erroneous) understanding on the principles of mantle dynamics, and most is due to the quantification of mantle dynamic properties such as viscosity and density profiles of the mantle.

The authors did a good job laying out this evolving discussion, especially the contrasting views on dynamic topography from numerical modelling and geopotential field studies. However, I think the paper could be further enhanced by providing more analyses on the differences between different dynamic models, especially over geographic regions where many debates exist. For example, the authors cited two earlier papers (e.g. Lithgow- Bertelloni and Silver, 1998; Gurnis et al., 2000) for African topography, but there works (especially the former) suggest much large dynamic uplift over south Africa, compared to the authors' result.

A key reason, according to my understanding, is that these earlier works underestimated/neglected the compositional nature of the LLSVP below the region that has be progressively established since then (Isshi & Trump, 1999; Masters et al., 2000; Ni et al., 2002; McNamara & Zhong, 2005; Simmons et al., 2009; Liu & Zhong, 2016).

I consider the current paper potential to serve as a cornerstone to establishing a framework on this important topic of dynamic topography, so some discussions like the over outline above would significantly strengthen this manuscript.

We are thankful for this motivating statement and accounted for all comments of Referee 2 below and in the revised version of the manuscript.

Minor comments:

1. P 2, L5: Geoid is a result of deep density anomaly and/or surface topography. E.g., in theory, an isostatically balanced mass anomaly in the lower mantle sitting on the CMB may generate geoid without exiting dynamic topography. It is thus not entirely fair to use geoid as a direct constraint on dynamic topography.

   We agree and removed this statement.

   The sentence in section 1 now reads: "Dynamic topography generates significant surface deflections in both continental and oceanic regions (e.g. Ricard et al., 2006)."

2. P 3, top line: The cited work by (Glišovic and Forte, 2017) is a quasi-reversibility method, instead of an adjoint method.

   Corrected.

The revised sentence in section 1 is: "These studies examined the temporal evolution of dynamic support by forward modelling (e.g. Ricard et al., 1993; Zhang et al., 2012; Flament et al., 2013), backward advection (e.g. Conrad and Gurnis, 2003; Moucha et al., 2008; Heine et al., 2010), adjoint schemes (e.g. Bunge et al., 2003; Liu et al., 2008; Spasojevic et al., 2009; Colli et al. 2017), or a back-and-forth iterative method (Glišović and Forte, 2016, 2017)."

3. P 5, end of parag. 10: from the description, it is clearly that the presented model assumes no compositional anomaly for the LLSVPs on the CMB. While a forward model with a pure thermal mantle likely would predict similar dynamic topography to those inverse models (based on tomography) assuming a thermal-chemical origin of the LLSVPs, it would be worth clarifying on this, so that readers will not be confused/baffles by the apparently different modelling approaches but with similar results. This would be a good place to clarify on these existing confusions in the field.

   We agree that there is an ongoing controversy regarding the thermo-chemical nature of LLSVPs and in the revised version we explicitly clarified that we are using a purely thermal model without any compositional heterogeneities.

   We added this clarification in section 2.1: "Note that for simplicity our models assume no chemical heterogeneity. The distribution of heterogeneity is solely controlled by the imposed plate motion histories, the material properties of the model and the heating mode. Purely thermal models as in the present study have previously been shown to provide a good match to seismic observations (Schuberth et al. 2009; Davies et al. 2012; 2015) and dynamic topography (Colli et al. 2017)."

4. P 7, L 10: The prediction over South Africa implies little absolute dynamic topography (~200 meters), and even less dynamic uplift since the Mesozoic (Africa is a stable platform). This is indeed in contrast to earlier models that suggest large amplitude uplift (e.g., Lithgow-Bertelloni and Silver, 1998). The likely reason is that the earlier model assumes a pure thermal origin of the LLSVP and takes the geometry of the LLSVP from tomography, leading to a much larger dynamic uplift signal. Again, this should be discussed and clarified.

   The models of Lithgow-Bertelloni and Silver are very different to ours. They are instantaneous in nature, have a simplified depth-dependent rheology and the buoyancy field is derived from tomography (with seismic anomalies converted to density anomalies using a constant scaling factor which is oversimplified). Our model also assumes a purely thermal origin for heterogeneity, but the distribution of heterogeneity is controlled by plate motion histories (see point above). Furthermore, we incorporate a temperature dependent viscosity and also account for mantle compressibility, which will both lead to differences. We emphasize that whilst our models do yield different predictions to the earliest models by Lithgow-Bertelloni and Silver, they are generally consistent with more recent models by, for example, Flament et al. (2013).

5. Final remark: I really like the cluster analysis in this paper, and this is partly why I think this work could be used as a framework on the concept of dynamic topography.

   Once again we would like to thank Referee 2 for his/her constructive comments.