

Interactive comment on “Global patterns of Earth’s dynamic topography since the Jurassic” by Michael Rubey et al.

Anonymous Referee #1

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In their paper "Global patterns of Earth’s dynamic topography since the Jurassic", Rubey & co-authors used forward models of thermal convection in earth’s mantle to infer time-dependent changes in dynamic (surface) topography over the last 200 million years. They also performed k-means clustering to address how the long-wavelength dynamic topography in continental regions is affected by subduction zone. While I am somewhat convinced that the cluster analysis is worthwhile publishing, I am very critical to other aspects (i.e., the methodology for modelling dynamic topography, the validity of thermal convection models, the absence of mantle plumes, etc.). I formed the impression that the study do not provide any advance in our understanding of flow-induced dynamic topography. Also, it is important that the limitations and hypothesis are more clearly presented. I think a major revision of the manuscript is needed before it can be accepted for publication. My comments are as follows.

C1

1. The authors stated that their models are Earth-like in terms of surface heat flux (page 1, lines 10-15). However, they didn’t provide a global value for the heat flux at the surface obtained by their simulations. What is more important, a successful forward model of mantle convection must satisfy the energy balance (e.g., Kellogg et al., Science (1999)). Authors didn’t demonstrate this fundamental constraint for any thermal convection model, i.e., that the difference between the surface heat flux and the core-mantle boundary (CMB) flux is equal to the internal heating (22 TW according to Table 1?) over a period of 200 million years.

2. The authors imposed the past plate velocities, derived from a global plate kinematic model, as a surface boundary condition (page 3, lines 5-10). I understand this is one of modelling techniques for the surface boundary condition, but this is also a non-physical procedure - the mantle buoyancy should drive the plate motions not the other way around. The authors should discuss this.

3. Why did the authors apply "a 70 Myr long pre-conditioning stage"? Can they demonstrate that their simulations reach a (quasi-)steady state in 70 Myr?

4. In modelling dynamic topography, the authors employed an approach in which the effects of lithospheric buoyancy structure on dynamic topography are discarded. It is hard to comprehend this approach if we know that dynamic topography which arises from all density anomalies in the convecting mantle (including the lithosphere) provides the best fit to an independent, reference model of dynamic topography estimated by removing isostatically compensated crustal heterogeneity, described either by CRUST1.0 or CRUST2.0, from the observed present-day surface topography (see Forte et al. in Treatise on Geophysics (2015)).

5a. Additionally, a layer above 225 km depth encompass both lithosphere and asthenosphere (see e.g. the PREM model). The lithosphere and asthenosphere are closely coupled to the large-scale flow patterns generated much deeper in the mantle. Anyone who knows that the dominant control on the evolution of temperature anomalies in

C2

the mantle is from advection - velocity \times grad (temperature) - should understand that velocity is critical and this velocity (including in the shallow mantle) is produced by density anomalies at all depths in the mantle, and at long wavelengths this velocity thus depends on very deep density anomalies. So, one cannot "zero" out shallow density anomalies and pretend to model time-dependent convection correction.

5b. It is also enigmatic (no single explanation is provided by the authors) why did they use a constant value of 2150 K to replace the model's thermal structure above 225 km depth (and why this depth)? After all, what is the point of re-running the model with a 225 km thick layer of a constant temperature?! This approach in modelling dynamic topography is not Earth-like.

6. Why didn't the authors compare their predictions of the present-day dynamic topography to a crust-corrected dynamic topography which is an independent prediction? These independent reference models are also published by Forte et al. in *Treatise on Geophysics* (2015). Additionally, Glisovic & Forte, *JGR* (2016) have recently published the predictions of the present-day dynamic topography obtained by the forward integration of tomography-based, Cenozoic reconstructions.

7. It looks like their numerical models of thermal convection in earth's mantle do not produce a single plume (i.e., a hot upwelling) that rises through the mantle (see Figure 1). In other words, their model design and approach are not well suited to take into account the important effects of mantle plumes on dynamic topography (e.g., Moucha & Forte, *Nature Geoscience* (2011); Glisovic & Forte, *Science* (2017)). Therefore, the authors should clearly state that their predictions of the global pattern of Earth's dynamic topography since the Jurassic are solely subduction-driven. I even suggest to the authors to change the title in order to reflect this fact.

8. Furthermore, the absence of mantle plumes in simulations presented here implies that the CMB flux is very low. For example, the recent estimates of the CMB heat flux are about 15 TW (e.g., Pozzo et al., *Nature* (2012)). The low heat flux across

C3

the CMB prevents the development of a strong thermal boundary at the bottom of the mantle (e.g., Stacey & Loper, *Phys. Earth Planet. Inter.* (1983)) which means that deep buoyancy forces associated with active, hot upwellings are largely dismissed in this study. Also, how well does the CMB heat flux fit into the energy balance?

9. Accordingly, the authors cannot define "'geodynamic rules' for how different surface tectonic settings are affected by mantle processes" (page 1, lines 15-20) if they employ numerical models of thermal convection that do not resolve all buoyancy forces (hot upwellings and cold downwellings) throughout the mantle. These 'rules' are also well-known relations, for example, the negative topography below the eastern and western margins of the Pacific has been already interpreted in terms of present-day and Cenozoic subduction history. Therefore, authors may choose to remove or tone down on this statement, as well as to cite related work.

10. I also strongly call into question the statements like this one: "Our models provide a predictive quantitative framework linking mantle convection with plate tectonics and sedimentary basin evolution, thus improving our understanding of how subduction and mantle convection affect the spatio-temporal evolution of basin architecture". As I mentioned above, thermal convection models are solely subduction-driven, thus they can address only subduction effects on continental regions (and this only to some extent, because models do not include topographic contributions from lithospheric buoyancy).

11. Rubey & co-authors stated, "In contrast to previous models, our model M1 agrees well with this suggestion, mapping deep mantle return flow above the Pacific and African LLSVPs at amplitudes not exceeding ~ 1 km", but it seems they missed to explain that this finding is strongly biased by the model 'assumptions'. Namely, their result (Figure 5f) is very similar to dynamic topography predicted on the basis of density anomalies below 200 km depth given by Forte et al. in *Treatise on Geophysics* (2015) (see Figure 21b).

12. Perhaps, authors should consider comparing their results for Africa to those ob-

C4

tained by Moucha & Forte, Nature Geoscience (2011).

13. It seems 3D mesh is uniformly spaced (page 3, lines 15-20)? If this is true, then I am wondering, do the authors experience any numerical instability across the thermal boundary layers?

14. I appreciate the 3D view of model's temperature in Figure 1, but perhaps it would be more informative for a reader if the authors decide to show isotherms based on lateral temperature variations.

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