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Solid Earth
Discussions

Interactive comment on "Increased LLVP density recovered by seismologically constrained gravity inversion" *by* Wolfgang Szwillus et al.

Anonymous Referee #2

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In this article, the authors perform inversions for the Earth's mantle density structure using satellite gravity data. To take into account the effect of the lithosphere, they define 2 different models, "isostatic residual" (assuming that continental topography is compensated at depth) and "crustal residual" (which uses a recent crustal thickness model to estimate the effect of the crust). The distribution of density anomalies is constrained by vote maps of seismic tomography models. Crustal residual inversions fit data less well, but inversions with both methods show density excess in the Large Low Shear-wave Velocity provinces observed by tomographic models, supporting a thermo-chemical nature for these structures.

This paper could be an interesting contribution to the debate on the thermo-chemical structure of the deep mantle. However, my overall feeling is that it is poorly written

and lacks of clarity. In addition, the assumption that CMB isostatic and dynamic topographies are equivalent is, I think, not correct. Most importantly, the way dynamic topography is calculated oversimplifies the influence of the flow on topography, potentially introducing a bias on the amplitude of density anomalies. Solving these issues requires some substantial revisions and polishing.

1. While the authors take into account both isostatic and dynamic effects on CMB topography, there is a lot of confusion about interpreting or estimating these terms. The assumption that dynamic and isostatic topographies are similar (see for instance lines 62-64) is, I think, not correct, as demonstrated by numerical simulations (Deschamps et al., 2018), and may impact the authors results (see below). More specifically, this assumption is correct only in the purely thermal case. In that case, dynamic topography (i.e., accounting for both isostasy and dynamic effects) is fully correlated to isostatic topography (hot material is less dense and, because it rise, stretch the CMB upwards), but has more amplitude. In the thermo-chemical case, things are more complex. Because they are hot, piles of dense material (here, LLVP) have their proper dynamics, and the dynamic effect (which is also to stretch the CMB upwards) opposes the isostatic effects (related to the fact that piles are globally denser). If material is dense enough, the dynamic compensation is limited, but topography has less amplitude. If dense material is only slightly denser than surrounding material (typically, around 60 kg/m3), dynamical effects are stronger and isostatic topography can be nearly fully compensated. In addition, patterns are not perfectly correlated. This was detailed in Deschamps et al., GJI, 212 (2018) (see their section 5.3 and Fig. 13). In addition to this, the dynamic topography calculated by the author does not take into account lateral variations in viscosity (as models of convection do), and may then be substantially biased. Effects of lateral viscosity variations (due to both thermal and compositional changes, and potentially to post-perovskite) are important, as shown by Lassak et al., EPSL, 289 (2010) and Deschamps et al. (2018). A possible consequence of oversimplified dynamic topography and/or assimilating dynamic and isostatic topographies is that the amplitude of CMB topography beneath LLVP may be overestimated. The total

gravity signal (accounting for density excess in LLVP and CMB topography) would then be underestimated, i.e. the inverted density excess may also be underestimated. In that sense, the density excess estimated by the authors are lower bounds.

2. The impact of post-perovskite on the authors' interpretation is treated very briefly (1 sentence in the conclusion), and should be discussed more in details. These impacts include the influence of post-perovskite on shear-wave velocity and on density, but also the possibility that post-perovskite has a low viscosity, which may locally impact the CMB topography (see Yoshida, G3, 9, 2008, and Deschamps and Li, JGR, 124, 2019).

3. Overall, I find the manuscript not clearly written. There are many shortcuts in reasonings, and points that need to be clarified. For instance, lines 26-28, it is very difficult to understand that the authors mean that LLVP may have remain stable for a long period of time, and that the hints for this come from the reconstruction of plumes location (I would also avoid the adverb "tectonically" here). Line 223 "A long-wavelength error in the upper mantle . . . incorrect estimate of the density of the LLVP". Can the authors detail what they mean here? Lines 289-294 are extremely confusing and should be detailed or clarified. The authors first discuss surface topography, then turns to CMB topography without transition. It is difficult to follow their point here. In many places, units are missing, for instance line 231-232 (kg/m3, I guess), and again, lines 255, 258. Regarding CMB topography: do I understand that positive topography means deflection in the core?

4. Introduction (lines 55-60). It is true that CMB topography is difficult to estimate from seismological data, in particular because it trades off with structures on the core side. However, it is not true to say that there is no constraints. Many studies addressed this problem. In addition to Tanaka (2010), studies on this topics include Morelli and Dziewonski (1987), Doornbos and Hilton (1989), Sze and vand der Hilst (2003), Koper et al. (2003), Colombi et al. (2014), etc . . . . The problem is that these models and constraints do not lead, so far, to a consensus, but are very different both in amplitude

and patterns. Numerical modelling of convection also addressed this problem (see references in previous points) and provided valuable information to understand the link between the CMB topography and the models properties (e.g., rheology) and thermo-chemical structure.

5. CMB topography. There is a (very strange) strong topography appearing beneath Australia on models for crustal residual inversion. This needs to be commented, and probably further rules out the crustal residual approach.

Minor comments.

- Abstract, first line: add a "s" to "remain".

- Abstract, first line: replace "They" by "These structures".

- Abstract, line 6: replace "of" by "with".

- Abstract, lines 7-8. Remove "Therefore, the geometries . . . in most tomograpjies": this doesn't belong to an abstract.

- Line 31. Regarding estimates of density anomalies, please also refer to Trampert et al., Science, 2004.

- Line 80. Remove "any" between "propagates" and "errors".

- Line 104. Replace "what" by "that".

- Line 196: add "is" between "This" and "likely".

- Line 208. Remove ". . . both in an absolute and a relative sense".

- Line 250. Add "to decide" after "criteria".

- Line 316. The figure number is missing (I guess Fig. 9d)

- Line 331. Replace "upper Earth" by "Earth's upper mantle".

- Line 345. Replace "stayed" by "remained".

- Line 346. Replace “an old feature” by “old features”.

- Lines 367-373. The authors’ conclusion regarding MORB as a component of LLVP are similar to those in Deschamps et al. (2012). It is worth mentioning it. Also worth mentioning is the fact that MORB are not well defined in terms on oxides composition. There is a strong dispersion (in particular in the FeO content), which potentially results is a large uncertainty on the seismic signature of high-pressure MORB.

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