

Interactive comment on “Mantle roots of the Emeishan plume: an evaluation based on teleseismic P-wave tomography” by Chuansong He and Madhava Santosh

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In this paper, the authors use teleseismic tomography to investigate the upper mantle beneath the Emeishan LIP in southwestern China. Based on the imaging results, a number of inferences are made with regard to the mantle origin of the volcanics. I have a number of comments, both minor and major, which are listed below:

(1) Line 16-17: “Our results do not provide any conclusive evidence for upwelling mantle plume rooted in the CMB beneath the Emeishan LIP” - given that the model region extends down to 800/900 km depth and the mantle layer is ~ 3000 km thick, there is no prospect of conclusive evidence for a plume rooted in the CMB, irrespective of what the results show.

(2) In general, the written English is ok, but there are a number of places where the sentences don't quite make sense (e.g. Lines 62-65).

(3) Lines 74-76: If the LIP formed a quarter of a billion years ago, present day mantle dynamics are unlikely to be able to help shed light on its origin.

(4) Line 91: "velocity structure or mantle dynamics" - if you apply travelttime tomography, then there is no choice but to recover velocity structure. You can only make inferences about the mantle dynamics from these results.

(5) Lines 102-103: What 1D velocity model is used, and what is the "fast raytracing technique"? I know that references are provided, but a one sentence summary would be useful.

(6) Lines 104-107: Is this a regular grid in spherical coordinates? And linear interpolation is not possible in 3-D when the velocity field is a function of 8 surrounding control points (in general, gradV will not be constant inside a cell). I think most people refer to it as pseudo-linear interpolation. This parameterization is fairly standard, so why are so many papers by Zhao cited in lieu of a definition?

(7) Lines 111-112, and Figure 2 caption: It is stated that events between 30-85 degrees angular distance are used, but the plot shows events out to ~100 degrees. Azimuthal coverage is very varied too, with most events from the south and east. In order to improve coverage from other quadrants, is there any prospect of employing other global phases, such as PP, SKP, Pdiff etc?

(8) Line 115: Does capping the maximum residual mean that larger residuals are due to noise or an inability to model signal? In practice, this difference is perhaps not worth dwelling upon, but I always find it interesting when influential data (large residuals demand more significant model perturbations) are ignored.

(9) Line 118: But at least in Figure 6 (for example), the model seems to extend in depth to 900 km, not 800 km.

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(10) Line 119: The contribution of the crust to the pattern of teleseismic arrival time residuals can be quite significant, particularly in regions with large changes in elevation. More information about how this correction was made and which model was used would be useful. Looking at the results (e.g. Figure 9), shallow high velocity zones tend to be associated with regions of low elevation, where one would expect thinner crust, and hence a negative contribution to the arrival time residual compared to where there is thicker crust. Hence, if the crustal correction doesn't adequately take this into account, it may result in artefacts in the upper mantle velocity structure.

(11) Lines 122-126: Why only quantify (and illustrate) the damping vs. data fit trade-off if smoothing is also applied? Also, is the tomography iterative non-linear, or just linear (rays only traced through reference model)?

(12) Paragraph starting line 128: "...tracing the actual rays through a synthetic structure..." What is meant by the "actual rays"? Are these the ray geometries from the initial or final model of the observational study? In other words, is the checkerboard test a purely linear inverse problem? At the risk of blowing my own trumpet, I suggest reading the paper:

N. Rawlinson, W. Spakman; On the use of sensitivity tests in seismic tomography. *Geophys J Int* 2016; 205 (2): 1221-1243. doi: 10.1093/gji/ggw084

which outlines some of the pitfalls of using a synthetic recovery test with a relatively tight pattern of uniformly-sized anomalies. I think in this case the quality of the recovery overstates what can actually be achieved with the real data. Apart from data noise, which I doubt is accounted for here, this kind of test is strongly preconditioned to produce favourable results. For instance, there is very little evidence of smearing, yet the use of teleseismic body waves generally results in some near-vertical smearing. Finally, lines 135-136 simply repeat lines 117-118 for no good reason.

(13) Lines 154-155: Given that this is a teleseismic tomography study based on relative arrival time residuals, velocity perturbations can only be discussed in a relative rather

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than absolute sense, unless constraints from elsewhere are applied.

(14) Line 157-159: The recent study by Huang et al (2015) is frequently referred to, and indeed one lingering question is what the new teleseismic tomography study brings to the table that the 2015 study does not. A quick comparison of the arrays used show that Huang has a much denser station network, but in this study the array extends further north and east. The implications of these differences should be discussed somewhere. Also, Huang jointly inverts a large database of local earthquakes along with the teleseisms in order to constrain crustal structure, which ostensibly is an advance on what is done in the current study. Therefore, some discussion and perhaps justification of the current study relative to those that precede it is probably warranted.

(15) Line 197: "...with crust and (or) lithospheric delamination." - It would be more correct to say "mantle lithosphere" rather than just "lithosphere".

(16) Line 225: How does a receiver function study demonstrate convective circulation in the mantle? A few more details of this study would be useful to include.

(17) Line 248: Following from the above, how do the receiver functions identify felsic lower crust?

(18) Lines 260-264: Getting back to an earlier point I made, I would be very careful about associating what you see at 500-600 km depth with a LIP that formed a quarter of a billion years ago. I'm not saying that there cannot be an association, but it would be good to see some independent evidence e.g. from geodynamic modelling. If the delaminated lithosphere is distributed as inferred by this study, how long would it take to go from initial separation to this state, and given plate motion over time, is it located where you would expect? And when might the volcanism occur? While undertaking modelling of this type is clearly beyond the scope of this study, I still think that the readers of the journal will need a bit of convincing that what is proposed is actually possible.

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(19) The Conclusions are rather brief, and would benefit from being fleshed out a bit.

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