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Solid Earth
Discussions

Interactive comment on "Mantle roots of the Emeishan plume: an evaluation based on telesismic P-wave tomography" *by* Chuansong He and Madhava Santosh

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The tomographic image of the Emeishan LIP in southwestern China, is explained by "melting of delaminated lower crustal and/or lithospheric components and associated plume-like upwelling from the mantle transition zone". In other words, a shallow origin. Such a surprising conclusion requires a scrupulous argumentation, based on verifiable predictions and hard evidence. I am afraid I do not quite see that in this paper, which presents the results of a new tomographic study with some differences with earlier images (that seems to have used more stations than this study). The argumentation is less than compelling, it does not clearly argue how today's tomographic images can give relevant information for an eruption that occurred more than 250 my ago. Laurasia

at the time was certainly not at the same location, between 200-160 my ago it moved rapidly northwards, its earlier history is not solidly documented as far as I know (see Seton et al., Earth-Science Reviews 113:212–270, 2012). Anything visible below the lithosphere at its present location is therefore irrelevant for the interpretation of the Emeishan LIP.

This is my main objection to this paper. Let me try to pose a number of other questions and give some comments.

On page 2 the authors do not clearly distinguish between the causes of seamounts and LIPS, which is confusing. Whereas seamounts are often thought to be caused by secondary or shallow, lasting plume activity, the volume erupted in the short time span observed for LIPS are probably caused by the impact of a large head from a massive (and supposedly deep) plume, not the plume tail. The volume of a LIP is of the order of 10ˆ6 kmˆ3, very much larger than that of a seamount, their causes must thus be very different. Eimashan is characterized by a rapid, large volume (0.25*10ˆ6 kmˆ3) eruption, typical for a LIP. Being accessible by land-based seismic stations, it could thus be a prime candidate for testing of the plume head impact hypothesis if one can clearly link this to present day lithospheric structure. This missing link might be provided by geodynamical calculations leading to predictions for the present day, but that obviously introduces a whole new class of degrees of freedom, so it is not obvious how to interpret even the shallow tomography in terms of the causes of an ancient LIP.

The authors use the stations from the CSN, but not of the Chin array project or other temporary deployments, for reasons not discussed. The span of the array is thus comparable to that used by Huang et al. (2015), but the station density is inferior. The span is not very large, it covers an area of about 600x800 km. Not surprisingly the resolution is therefore limited to the upper mantle (fig 5), as was the result by Huang et al. (2015). There are a few small but interesting differences between the two solutions, for example at 500 km depth where He finds a strong low velocity anomaly beneath the Jiangnan orogenic belt, unlike Huang. And at 700 km Huang finds a plume-like anomaly beneath the Yangtze block that is absent in He's analysis. But here we are at the limit of resolution, so one wonders if these differences are significant. And, as I argued before, the deeper structure at this location cannot be linked to the LIP because that event must have taken place at a much lower latitude.

Unfortunately, even the resolution test leaves questions. Were any errors added to the synthetic data? If not, the test is too optimistic and the level of 10% adopted for whitening out of the solution (figure 8-10) is probably far to low. If they did include errors in the test, what standard error was adopted for the delay times? Figure 4 seems to indicate that an error of about 0.4s is realistic.

The discussion of the mechanism for the LIP formation is not very transparent and mixes several possible scenarios. The absence of evidence for an uplift and for underplating is used to question the plume hypothesis. This is the closest the authors come to formulating a testable prediction.

Both studies detect high velocity zones near 300 km depth. He et al. argue for a role of delamination in the Eimashan eruption. However, this leaves me again with a question about timing. If the delaminated slab has anything to do with causing the LIP 260my ago, it would by now surely have sunk deep into the mantle further south. He et al. speculate that a water-rich transition zone might stop it, but (1) the high velocities are shallower and (2) wouldn't water lower the density of the surrounding mantle and have a slab sink even faster? Slabs normally dehydrate when sinking, and surely the water cannot diffuse back so quickly into the slab that it lowers its density? And (3) it is at the wrong location.

In summary, it does not become clear why these tomographic images should be preferred to those of Huang (where they differ). And one cannot link images of the transition zone with the Eimashan.

A final, serious, remark: the number of references is very excessive (more than one hundred!), and this smells suspiciously like manipulation of the citation indices. It can

be reduced substantially. Just to give one example, the authors state (line 104) that the velocity is determined by linear interpolation among eight nodes (more accurate would be to say "trilinear interpolation"). This is such an elementary operation that it could be stated with one or even without any reference, but it is followed by no less than ten references. And each of these references is to the same person. . .

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