

Interactive comment on “Permian plume beneath Tarim from receiver functions” by Lev Vinnik et al.

Lev Vinnik et al.

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In this paper Vinnik et al. discuss the results of Kosarev et al. 2018 that show there is a relatively thin mantle transition zone (MTZ) beneath part of the Tarim Basin that is coincident with the presence of 290-260Ma basalts, thought to be plume related. They calculate that a plume present ~ 300 Ma could still leave a thermal signature on the MTZ. They argue that these results support a tectosphere model: that the layer that translates coherently with the continental plate extends to the top of the MTZ at around ~ 410 km depth, although they do mention that if the rate of plate motion is an order of magnitude less than predicted by plate reconstruction models this may not necessarily be the case. It is an interesting possibility, however I feel that the manuscript would benefit from the authors addressing several weaknesses prior to publication. Thanks for the constructive comments, which have greatly improved the manuscript.

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A major weakness I found with this manuscript was that in order to understand much of the detail of the data and results it was necessary to also read (and thus have access to) Kosarev et al. 2018. One such example is the choice of 2 degree boxes for stacking the receiver functions, which is explained in the earlier paper, but not in this manuscript. I appreciate that the authors may want to avoid republishing the same information multiple times, however including details such as a figure of piercing points and/or a table showing the number of receiver functions stacked in each box would be very valuable. Some of this could go in supplementary material, but I do think it is needed somewhere. We have explained the choice of the degree 2 boxes in lines 99-106 and present Table 1. Other problems of data processing are discussed in lines 84-96.

The manuscript would benefit from the authors discussing alternative possibilities as to the cause of the relatively thin MTZ in their results in this part of the Tarim Basin. It would be good for them to try and find stronger evidence for them being both being caused by the same plume, and to discuss possibilities for them being unrelated. We have addressed this problem in lines 133-138.

It concerns me that, according to the figure in Kosarev et al. 2018, the number of piercing points, and so presumably seismograms in the stacks, is lowest in this region. We have addressed this question in lines 113-115.

Further, it would be useful for the authors to try and find other evidence that may indicate whether the tectosphere extends to over 400km depth. While it may not be conclusive, and I'm not sure if appropriate data exists in this instance, it would interesting for them to look at the strength and direction of azimuthal anisotropy in the 100-400km depth range to investigate if there is evidence for flow related to absolute plate motion shallower than the MTZ. It is difficult to address these questions with the available seismic data. The Tarim basin is the largest desert area in China, practically devoid of seismograph stations, but our group already deployed several stations. We will use these data to discuss the issues as you're mentioned.

The manuscript is generally well written and the figures are mostly clear, however there are a few issues that need to be resolved:

Line 37: How much is a few percent? We have changed it to 4-5% (line 38).

Line 68: “around 100 broad-band stations” - how many was it exactly? Precise number is 64 (see line 69).

“around 6 s” What exactly was the corner frequency? We have removed “around”.

“their depths are sensitive to the temperature” - it would be good to also mention that they are sensitive to composition here. We added “composition”. See also the lines 157-162.

“on the order of several hundreds” - how many was it exactly - include a table of the numbers. We have added Table 1 with the numbers.

Lines 109-111: citations needed for the discussion about the Permian basalts. We have added reference to Wei et al., 2014 (line 129).

Lines 122-133: This section needs rewriting to make it clearer. Why/how did you choose a diffusivity value of 32 km²/m.y? By reduced twice do you mean halved? We used the standard value of diffusivity with reference to Morgan and Sass (lines 150-152). We have changed “reduced twice” to “halved”.

Figure 5: Given the discussion is of something that occurred 300ma, I think this figure would benefit from showing the curve for 300 m.y. Is R in the figure the same as described in lines 122-133? In this figure, to avoid any confusion, we replaced R by r. We tried to replace 400 m.y. by 300 m.y. but this change failed to produce positive effect.

Is there any paleomagnetic data that the authors can find for Tarim that describes how far it may have moved? We have provided reference to Zhao et al., 1996, which contains other useful references (line 166-169).

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