

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

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Review of Permian plume beneath Tarim from receiver functions by Vinnik, Deng, Kosarev, Oreshin, and Makeyeva.

In this paper, the authors expand on the discussion presented in Kosarev et al., 2018 by focusing on the depressed 410km discontinuity under the Tarim Basin. Correlating this depression with the location of  $\sim 300$ Myr old basalts, they argue for a tectosphere style interpretation in which the upper  $\sim 400$ km translate coherently over  $\sim 300$ Myr and 2000km. This represents a controversial view as typical thought is that the mechanical lithosphere is limited to  $\sim 100 - 250$  km.

The additional evidence, beyond the seismic data of Kosarev et al., 2018, they provide is a pair of simple models for 1D and 2D heat diffusion from a plume, suggesting the interpreted temperature perturbations are consistent with 300Myr of cooling from a

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300€Ž plume.

The primary weakness of the paper is the reliance on a spatial correlation between basalts and seismic observations of a warm upper mantle and I don't feel they've done a rigorous enough job of evaluating and eliminating alternative hypotheses. A secondary weakness in the paper is the frequent use of approximate phrases where a more specific quantification is warranted. For example, line 68 they state that about 100 broadband stations were used rather than listing the actual number of stations. I assume these shortcuts were taken as the interested reader can see the Kosarev et al., 2018 reference, but much of the method section uses the same loose language in Kosarev et al., 2018.

There are no major technical data or method concerns specific to this paper. I do worry about the validity of the interpretation, however, I feel having the idea out there in the discussion is valuable and may spur future works, such as 2D or 3D numerical models exploring under what mantle conditions this type of translation could occur. I recommend minor revision to quantify various aspects in the method section and a few additional citations are required.

Line by line: Line 37: "Siberian LIP drops by a few percent": quantify "a few"

Line 37-39: Expand on evidence used for "partial melt" and alternatives. Not all low velocity mantle anomalies are partial melt.

Line 68: "about 100 stations": how many were actually used?

Line 69: "corner at around 6s": what were the actual period limits?

Line 83-86: Why was this stacking method used rather than the more traditional CCP? Kosarev et al., 2018 does briefly discuss it, but rephrasing it here would be useful for analysis of the data presented here.

Line 87-88: What is the event coverage? Perhaps reproduce Kosarev et al., 2018 figure 2 with figure 1 or 2 here.

Line 89-90: How is a confidence interval of 66% determined? Most importantly, is it based on waveform variability or measurement variability?

Line 91: Citation for IASP91 model? Also, I assume the thickness perturbations are based on that same model?

Line 93-97: Somewhere in here, it would be useful to label the anomalous boxes (a,b,c) for referencing.

Line 113: Citation needed for high heat flow and uplift during the Permian.

Line 115: “Coherence” should be “correlation” (also, see elsewhere in the manuscript where coherence is used instead of correlation).

Line 124-126: The heat diffusion relation may be well known, but a citation is needed for the equation and choice of parameters.

Line 137-138: What is the relationship between increased Mg content and partial melting (citation)?

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Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2018-41>, 2018.

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