

Reviewer: Dr. Jennifer Jenkins

Permian plume beneath Tarim from receiver functions

This paper uses receiver function analysis to image the mantle transition zone (MTZ) discontinuities beneath the Tarim basin. While most of the area shows average MTZ thicknesses, several regions show anomalously thin or thick MTZ. The authors focus on one of these regions in their interpretation which shows a thinned mantle transition zone which they suggest is due to a depressed 410 caused by a hot temperature anomaly. They note that this region it is co-located with Permian basalt deposits at the surface and suggest that these formed as a result of the underlying temperature anomaly – which is interpreted as a remnant of a mantle plume. Simple temperature modeling suggests that a cooling remnant plume would have sufficient temperatures to cause the currently observed depression on the 410. The key claim of this paper is that since the eruption of these basalts, the Tarim region has been tectonically displaced by 2000 km as based on palaeomagnetic reconstructions. The authors argue that since the underlying temperature anomaly is still located beneath the erupted basalts, this suggests that the upper mantle down to 430km has translated with the overlying plate, following the tectnosphere model of Jorden, (1978).

I would recommend this paper for publication after some minor corrections/comments are addressed. I have outlined my major suggestions/questions below and also made a list of minor spelling/rewordings.

General comments

Overall I find the paper provides a reasonably convincing observation with a very interesting interpretation. Many of my comments relate to what tomographic models of the region would add to this study, whether they can improve methodology and/or support the interpretation. My other main comment is that the authors present two options:

- The whole of the upper mantle has been translated beneath Tarim, OR
- Palaeomagnetic reconstructions are wrong in this area by an order of magnitude

I feel I would like to see more details on the practicalities of how the first option would work if it were the case, and an indication of how reliable palaeomagnetic reconstructions are. I would like to get a sense of which of these interpretations the authors support, because both are big claims.

Question about Tectnosphere model

In the introduction (lines 28-46) the authors suggest that the Siberian LIP may show evidence for a coupled tectnosphere based on observations of:

- depressed 410 discontinuity
- Slow velocities 350-410km
- Low vel. layer 460-500km

This would suggest a tectnosphere extending to depths >500 km to explain the low velocity layer, but in the conclusion, based on the observations of the current study, the authors cite a translation down to depths of 430km (line 187)? So how deep would this tectnosphere extend? Do the authors suggest it could be different depths in different locations? And is there any more background to this theory of a coupled tectosphere? By what mechanism would this large scale deformation take place?

Would we expect to see an observable feature at the base of the coupled section? Is there other ways we should be trying to test this hypothesis?

Line lines 77-83

The authors discuss using differential arrival times because P410s and P660s are equally affected by velocity heterogeneities above 410 km. What about velocity heterogeneities extending through the MTZ? I realize these are likely to be smaller magnitude at depth, but if you are looking at a region where you are arguing for the possible presence of a plume, you might expect velocity anomalies extending deeper than 410 km. Using your methodology such anomalies would effect only P660s phases and would not be accounted for, effecting estimates of differential time. Can the authors justify why this would not be problematic in their specific region, or why they deem it not necessary to consider? What do tomography models of the region look like? Do they suggest strong heterogeneities are restricted to < 410km?

More generally do tomographic models agree with their observations of a depressed 410 in this region (e.g. a slow velocity anomaly)?

Lines 84-96

This section describes stacking based on pierce points – how are these calculated? Are they just based on the IASP91 model? Again what do tomography models of this region look like? Is it reasonable to use pierce points from a simple 1D model? Or would tracing through a 3D model, offset the pierce locations significantly?

Stacks are based on 535km pierce points, which justifies the use of differential time measurements as corrections for upper mantle velocity heterogeneity. But as the authors themselves point out, the pierce points between P410s and P660s for the same recording are 1-2 degrees offset, on the order of a few 100km. So the differential time measurements relates to an offset thickness of the TZ. This would prove problematic if a velocity anomaly effecting both discontinuities was observed by at 410 pierce point location, but the 200km offset 660 was not effected by it. Do this authors think this could cause a problem for their results?

Line 103 – “*optimum size of stacking boxes was found through trail and error*” – could the authors be more specific what they mean by this? What made them use these specific dimensions?

Line 105 – “*sufficient for a robust detection*” - what quantitative criteria are used to define a robust detection?

Line 111 - “*The resulting anomalies of thickness of the MTZ for a, b and c are +15 km, -11 km, and -15 km, respectively*” – based on what velocity model? IASP91? Is a 1D model sufficiently accurate in this area?

Line 116 – “*The further analysis (Kosarev et al., 2018) demonstrates*” – what further analysis was conducted? How did Kosarev et al. determine that the thickening or thinning of the TZ was caused by one discontinuity or another? Since the interpretations of this paper hinges on some of the conclusions of the Kosarev paper (e.g. that in b and c thinning is due to a depressed 410 but stable 660), I think it is important to state how this was determined.

Lines 114-124 – I am interested here why of the 3 anomalous regions that the authors identify only one seems to justify a full discussion and interpretation. Areas a and b are given only a single sentence of interpretation here.

Lines 120-121 – “*The elevated temperature in b may be related to a plume which is responsible for small-scale basaltic volcanism in the Tien Shan from 72 Ma to 60 Ma.*” – I feel like this statement could do with a reference? What is this interpretation based on?

Line 134 – “*a striking spatial correlation of the depressed 410-km discontinuity and the Permian magmatic province in Tarim, with implication of a causal relation between them*” – I take your point but as all good scientists know correlation does not equal causation! Maybe rephrase somehow? I also note that while stacking region c sits in the centre of the magmatic province, from what I can tell it also seems to extend to the areas covered by the stacking regions to both the north and the west, neither of which appear to be significantly effected. Maybe the distribution of pierce points in these regions do not densely sample this effected area?

Lines 139-140 – “*The depressed 410-km discontinuity and the stable 660-km discontinuity are typical for hotspots and plumes (e.g. Du et al., 2006)*” – I don’t know that I would agree that the conclusion of the Deuss paper is that hotspots “typically” show a depressed 410 and an unaffected 660. Certainly it is not unusual, but neither would it appear to be typical.

Lines 141-143 – “*The stable depth of the 660-km discontinuity is either the result of a zero temperature anomaly at the base of the MTZ or an effect of two phase transitions at nearly the same depth but with opposite Clapeyron slopes (Hirose, 2002).*” - I don’t know that the Hirose paper suggests that the presence of 2 different transitions would cancel each other out to produce a null average observation. The recent paper of Liu et al. (2018), also seems to suggest we might expect to see double discontinuities in the very hot plume centre or one phase transition dominating another, such that the one peak observed is biased towards one transition or another if it is too low freq to capture both peaks, but – anomalous in either case. In any case the appearance of a garnet transition is thought to become significant only at the higher end of plume temperature estimates, so may not be relevant in this cooling remnant plume scenario.

I also note that for the authors interpretation of a translating tectonosphere a flat 660 would be expected anyway, so there is no need to invoke complex phase transitions in this case.

Ref: Liu, H., Wang, W., Jia, X., Leng, W., Wu, Z., & Sun, D. (2018). The combined effects of post-spinel and post-garnet phase transitions on mantle plume dynamics. Earth and Planetary Science Letters, 496, 80-88.

Line 173 – “*On the assumption that Tarim and the Siberian craton were parts of the same continental plate in the past 300 Myr, 2000 km can be used as a rough estimate of the shift of Tarim.*” – Is this a common and justifiable assumption? What if you don’t make that assumption, how would that effect the relative motion of Tarim?

Line 175-178 – “*The spatial correlation between the anomaly in the MTZ and the basalt eruptions in Tarim (Fig. 4) in spite of the shift of the Tarim craton to the east and north-east by a few thousand kilometers is possible if the layer which translates coherently with the plate includes the top of the MTZ*” – If this is the case then presumably offset 2000 km to the west is the remnants of the deeper section of this hypothesized plume. Is there any evidence for this in tomographic images? As mentioned in my previous comments – this may be a good point to explicitly say this interpretation would suggest no temperature anomaly at 660, which fits with your

observed average depth 660 discontinuity.

Line 179 – “*Alternatively this is possible without the recourse to the deep tectosphere, if the available palaeo-reconstructions for Asia are too rough and the actual shift of Tarim is by an order of magnitude less than predicted.*” – as before estimates of error, or the reliability of reconstructions would help here. Is it likely that reconstructions could be off by such a large amount? Or is it relatively robust, and this really is not a viable alternative explanation, providing strong evidence for your tectosphere interpretation?

Figure 1 – Maybe include a large scale inset map of regional location – so readers can relate its relative location to other major tectonic regions discussed (e.g. Siberian LIP etc.)

Figure 2 – The string of numbers at the top of the figure seems to relate to stack location, no. of RF, times of 410 and 660? In sec? etc. etc. – While it is possible for the reader to try and work this out themselves, why not make it clearer by reformatting the figure with this information more clearly presented? The caption of this figure and axis describe “trial depths”. I assume this relates to some kind of move out correction given an assumed depth? This is not mentioned anywhere in the methodology or main text. Maybe add in some where or add a reference it for the reader can find what you mean by this.

Figures 3 and 4 – I feel that these figures could easily be combined into one. Just transfer the outline of the Permian basalts onto figure 3. I also feel that plotting on pierce points used to determine stack would help give an understanding of the data distribution – I know this information is given in Table 1 but it is much easier to have it visualized.

Minor corrections/questions

Line 69 - The 64 stations used – were these part of a net work? Should a paper or data doi be referenced acknowledging it? How long were the stations operational for? How many RF were produced from this data set, and after quality control how many were used? Is the data open source? Will the data products be made open source? If at all possible this is something that should be aimed for by all authors where possible, for future studies and reproducibility checks. I believe this may also be in the journal requirements

Line 70 – “PRFs” – make sure to define this somewhere before using the acronym

Line 148 – “*Q is constant*” – Maybe Q is a constant? What is this constant?

Line 167 – “*Tarim might be attached to Eurasia since the Late Paleozoic time*” – Tarim may have been ...

Line 174 – “*2000 km can be used as a rough estimate of the shift of Tarim.*” - direction of shift? Relative to..? Are there any error estimates on this distance? How rough is a rough estimate?