

Interactive comment on “Upper mantle structure around the Trans-European Suture Zone obtained by teleseismic tomography” by I. Janutyte et al.

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General comments: The paper deals with studying the upper mantle structure across the TTZ-line from northern Germany to Lithuania by teleseismic tomographic imaging. Seismic tomography is one of the prominent tools in research on the upper mantle structure since it can deliver a rather high spatial resolution over a large area (and depth range) and hence allows the geodynamic interpretation of the results. The authors use one of the commonly used non-linear iterative inversion schemes, called TELINV, for their study. The paper is well organized and written, very well illustrated and makes arguably a strong case for quite large lateral heterogeneities across the TTZ. There is however a number of points which need clarification.

Specific comments, according to text passages: p. 1728: here the authors report
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about a global tomography study of I. Artemieva. What kind of data is I. Artemieva using and what is the spatial resolution of the data used. I think that this is necessary information to assess the quality and to compare with the seismic data reported in this study. p. 1730: The authors talk about using the seismic bulletins of the USGS as well as the ISC catalogue. Were both sources used at the same time, i.e. for some events source (a) and for others source (b). In that case, how was the bias of the two differing solutions handled? Apicking error of approx. 0.2 sec (in general) seems rather large, what was the sampling rate of the mobile stations? I'm not very convinced about the "value" of figure 4 since it does contain no azimuth information and is as such only of limited value -> could be explained in words.... p. 1731: The reference of the TELINV code to Voss et al. (2006) is not appropriate. This particular method has other people who should get the merits (e.g. Weiland et al.,...). Why was the smoothing set to a value of 50? What exactly is the smoothing value for when the node spacing is anyway 50km? The authors claim that they choose 120 as damping value. Looking at the curve provided in figure 5, I would have around 80 found more appropriate. SO what was the reason for taking 120? To be on the conservative side? p. 1732/33: I'm a bit puzzled by the results of comparing the different "crustal" corrections. Obviously EUCRUST07 is too rough and hence not really applicable for this kind of research. But why are the two other results very similar? I also wonder whether there aren't more refraction profile results available to better cover the area. What does the European crustal model organized by M. Grad look like?

p. 1733: The checkerboard test is not fool-proof as it does only work properly in the case that the nullspaces of the inversion of the observed and theoretical data would be identical (see Leveque et al., for this problem). Personally I prefer a spread function analysis to dwell on the resolution available in the data. p. 1734/ Fig9: I suppose that you plotted the diagonal elements of the resolution matrix. However, the values are rather small and suggest a considerable amount of smearing! p. 1735: To my opinion is your reasoning not correct: First of all you've got anomalies of +/- 6.5% for which you need an explanation. And then you may come up with the different factors/effects

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which may contribute to that. By the way, how do you determine that anisotropy does account for 0,5% This is not clear to me?

By the way: did the authors use a flat-earth transformation or not?

The discussion and conclusion sections are fine with me.

Figures: 4: not very relevant 8: did you consider other checkerboard size boxes rather than 200km in horizontal scale?

Interactive comment on Solid Earth Discuss., 6, 1723, 2014.