

Interactive comment on “Mantle lithosphere transition from the East European Craton to the Variscan Bohemian Massif imaged by shear-wave splitting” by L. Vecsey et al.

K. Liu (Referee)

liukh@mst.edu

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The manuscript reports over 1000 pairs of SKS splitting measurements from a mixed broadband-short-period seismic network from the East European Craton to the Variscan Bohemian Massif. The standard minimization of transverse energy technique was used to produce the results, which suggest that at depth the craton extends toward the southwest, into the Phanerozoic terrain.

The scope of the seismic network is extensive, and the number of good SKS splitting measurements is adequate for the aim of the study, although the azimuthal coverage of the events is not ideal. I also have a few concerns regarding the analysis and

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interpretations of the SKS splitting measurements.

The first concern is that there is a lack of an in-depth investigation of complex anisotropy, which is characterized by systematic azimuthal variations of the splitting parameters (Silver and Savage, 1994). Although the azimuthal coverage is not ideal, one can still plot the measurements against the azimuths (or their modulo-90 or modulo-180 degree) to see if the variations are systematic or not, and if there are systematic variations, are the periodicity 90 (indicating multiple layer of anisotropy with a horizontal axis of symmetry) or 180 (tilting axis) degrees? It seems that some stations do display such variations (e.g., the two stations to the NE of the TESZ in the central profile).

My second concern is related to the display of the splitting results. The arrows plotted in Figure 6 and several similar figures start at the ray-piercing point at 80 km deep, and point to the fast direction. I was confused by the statement of “point in down-dip directions”. I do not think it is necessary to plot the results with arrows. Instead, it is easier to understand if the bars are centered at (not start from) the ray-piercing point. The location of the ray-piercing point relative to the station gives the back-azimuth.

The third main concern is about the interpretation of the results. The authors attribute all the observed anisotropy to the lithosphere, and did not discuss the possibility of a significant asthenospheric contribution at all. The spatially slow variation of the splitting parameters suggests a deep source, although the actual depth cannot be determined confidently. The authors should discuss this possibility by comparing the observed fast directions and splitting times with various APM models and with results of geodynamic modeling of mantle flow (e.g., Conrad and Behn, 2009).

Finally, the current discussion section is not structured and hard to read. I would like to suggest that the authors reorganize the materials into a few sections with subtitles.

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