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Interactive comment on "Transversely Isotropic Lower Crust of Variscan Central Europe imaged by Ambient Noise Tomography of the Bohemian Massif" *by* Jiří Kvapil et al.

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General Comments

The authors conducted an ambient noise tomographic study for imaging the crust of the Bohemian Massif, central Europe. A new high resolution 3-D shear-wave crustal structure, constrained by about 21,066 Rayleigh wave group velocity dispersion curves in the period range from 2 to 74 s, is presented. In addition, estimates of the Moho topography across the region are calculated from surface wave dispersion measurements as well as Receiver Functions. Waveform data in the time period from 2002 to 2016, recorded by permanent and temporarily deployed seismic stations in the area

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(e.g. Alp Array network, EASI network, etc.) are used in this study. The manuscript starts with a tectonic summary of Bohemian Massif and the previous studies. The authors then provide a brief description of the ambient noise cross correlation processing and the group velocity dispersion measurements that are followed by a two-step inversion approach for calculating the 3-D Vs model via period-dependent group velocity maps. The manuscript ends up with a detailed discussion of the outcomes that are furtherly compared with the previously available studies, mainly controlled source seismic profiling (CSS), in the area.

The manuscript is generally in a good shape but can largely be improved if a very careful revision is considered prior to publication. There are a couple of methodically-related points that should be made clear in advance. The referencing needs to done properly especially in the introduction and the method sections. The quality of some figures can to be further improved. I would also suggest trying extra efforts to argue for and verify some of the model features: especially the very interesting, newly-imaged low shear wave velocity zone in the lower crust across the entire Bohemian Massif that has not been imaged before. The text itself can be shortened and streamlined more. Extra spaces and minor typos are frequently occurring in the text but can be easily tracked and corrected, I gave examples in the section of the technical comments.

More specific comments

- Since the cross correlation functions are available, I am wondering why the phase velocities are not included in the tomographic inversion as well.

- In Fig 3a and b, what is the bandpass filter applied to the shown cross-correlation functions? Can you show the CCF at different filter bands? Also, in the same figure the authors show the directionality of the sources, but they did not discuss its impact on the group velocity dispersion measurements. Would be very good if they can show the seasonal variability of the dispersion curves estimated for one or two station pairs. In addition, I am wondering about how large the period-dependent signal to noise ratio

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during the FTAN that is more relevant to the quality of the dispersion measurement rather than how noisy the cross correlation function is. I would suggest adding a figure to show the average signal to noise ratio as function of period for all station pairs instead of Fig. 3c. The variability of the signal to the noise ratio as a function of the azimuth and inter-station distance can be investigated at specific period.

- Figure 4 might be better added to the supplement, right?

- L 134 - 136: the employed tomographic method, the fast marching algorithm, allows for smoothing and regularizing the inversion. It is not clear from the text how this is affecting results of the tomographic inversion. Taking this into account, how dependent is the final group velocity maps from the initial models? Can you show a measure of model roughness and smoothness? May be an L-Curve would be of help? How do you weigh the picking errors in travel times during the inversion? Do you estimate the uncertainties of the group velocities as function of period from the tomographic inversion?

- Using a standard synthetic checkerboard tests might not be of help to effectively test the lateral resolution of the inversion or even to simulate the effects of errors in the measurements due to travel time picking procedures of the FMM. I would suggest performing more tests with adding Gaussian noise to a synthetic input model or may be at randomly distributed spikes.

- Fig. 5b: is not clear if this is a group velocity map from the inversion or just representing the path coverage at this period? Can you please indicate that also in the figure caption and explain it a bit more?

- The grid size is set to 0.1° x 0.1° during the tomographic inversion. How does the different grid sizes affect the tomography results at different periods? Important that you show examples of the final group velocity maps at different periods (from short to long periods) sampling the period range in which the measurements have been obtained and the corresponding path coverage maps at same periods.

- Is there a reason for constructing the local dispersion curves at 0.2° x 0.3° grid, while the group velocity maps have a regular 0.1 x 0.1 ° grid?

- What is the period range used for the 1-D inversion? Did you apply any quality measures and/or outliers removal to the rough parts of the dispersion curve? A primary reason for the large misfit between the measured (dashed) and the synthetic (red) dispersion curves in Fig. 6b might be due to the erroneous group velocity measurements at very short periods that are affected by the interference with higher modes. At longer periods, the group velocity measurements might not be well constrained due to the low number of crossing paths. Rejecting those parts of the dispersion curves may help improving the misfit.

- L 168 – 169: This cannot be generalized just by looking at the results at one node. Can you do the same test at different locations? Tests might be done with and without the rejection of the rough parts of the dispersion curves.

- How do you estimate the Moho depth from the surface wave inversion? How different are these estimates from those of the initial models? It is suggested to show a map of the errors in the Moho estimates from group velocities inversion. The discrepancies between Moho estimates from Receiver functions and surface wave inversion are partly due to the different sensitivities of both data sets to the Moho interface. Furthermore, RFs suffer from the inherited non-uniqueness solution (velocity-depth trade-off). It is not clear from the text how you do deal with this issue. Meanwhile, the surface wave dispersion measurements are sensitive to velocity gradients across the interfaces. In the future, the authors may wish to perform a joint inversion of the two data sets. This might help minimizing the trade-offs and also to better constrain the crustal structure and Moho topography.

- In most parts of the BM, Moho depth estimates from the Receiver function measurements are in a good agreement with those from CSS profiling. However, they do not correlate with Moho depths from the inversion of surface wave dispersion data. Can

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you please comment on what could be the reason for that?

- L 376 – 380: Relying on the layer-stripping technique, the authors stated that 3-D Vs ambient noise-based measurements are much more realistic constraining the crustal structure and the sharp Moho interface than the CSS studies. However, Majdanski and Polkowski (2014) applied the same technique in the CSS data analysis and showed that the layer stripping method has its own limitations: i.e. large uncertainties are expected with increasing the number of iterations. Therefore, it is suggested that the authors show the effect of the increasing or decreasing number of layers as well as the frequency bands used in the inversion on the associated uncertainties in the resulted shear velocities as function of depth.

- Relying only on this argument, I would say that the sentences on preferring the ambient noise tomography than CSS-based results between line 376 and line 380 cannot be accepted like this and has to reformatted or even deleted as it is generally agreed, that CSS studies give the most detailed images of the crustal structures, intra-crustal interfaces and Moho depth.

- Fig. 8b and c: how do you define the thickness of the upper and the lower crustal layers? Do these velocities represent average Vs values in both layers, respectively?

- Fig. 10 shows the depth to the top of the imaged velocity-drop interface (VDI). Taking into account its undulating topography that apparently has no correlation with the Moho topography, how thick is the imaged low velocity layer? Is the thickness of this layer laterally variable? What is the vertical resolution of the Vs model? Is this feature well resolved? The trade-off between lower crust and its low velocity zone, the upper mantle velocities and the Moho depth should be checked.

- In Fig. 16. The authors argue that NW-SE shortening of the core of the BM produced or enhanced already existing horizontal foliation (fabric) within the lower crustal rocks. However, shortening would be expected to lead to vertical foliation instead. Can you please comment on that?

- The discussion presented in subsection “6.3.2 - Anisotropy of lower crust” is mainly based on the discrepancies between the high Vp velocities from CSS data available in the region and the low Vs velocities in the lower crust from the presented model. The presented evidences are not clearly supporting the presence of the anisotropic lower crust beneath the entire Bohemian Massif. Thus, I would suggest that the authors make extra efforts to reconsider this part of the discussion.

- The quality of the figures in general is also of another major concern: several figures are not decent, e.g. Fig. 5, 8, 9 and 13. Colors are misleading and barely distinguishable sometimes, for example the measured dispersion curves in Fig. 9 are hardly visible. In addition, the font-size of the labels and titles are too small.

Technical corrections

The manuscript is full of typos like the following: I suggest a carful revision and cross checking of similar issues. In the following only few examples are listed:

- In Fig. 2, the map shows two different map projections. Why? Correct it please?

- L 9: with increasing -> with the increasing

- L 14: $\sim$0.2 -> $\sim$ 0.2 space is missing

- L 28: central Europe . -> Excess space at the end of the sentence.

- L 33: References to Fig. 1 are missing.

- L 37 -38, this is very general sentence, please cite references properly. Who did what?

- L 47 - 48: similarly please cite references properly. You may indicate that, for example, Shapiro et al. (2005) inferred group velocity map at a local scale, and Yan et al. (2007) also deduced group velocity maps at a regional scale.

- L 49: is in its -> remove “in”

- L 54: remove excess spaces.

- L 59: What do you mean by Rayleigh wave extraction? Not clear!

- L 63: standard deviations, -> remove the comma

- L 69: the number of the stations is not consistent in the abstract and data section, is this on purpose?

- L 98 - 99: thanks to the . . . -> please remove or reformulate it?

- L 113: space is missing at the beginning of the sentence. . . and so on!

- vS -> should be Vs and also Vp!

- Caption of Fig. 10, circles is -> circles are — This happens frequently.

- L 206 and 207: what do the numbers (1, 7) in the subscripts refer to in the equation?

- L 370 – 371: Not clear enough, what do you mean?

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