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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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I am a physicist with experience in modelling of the crustal structure using a variety of seismic methods, but mostly specialised in P-wave controlled source data, and the uncertainty analysis of the structure models.

General Comments

This is a very well written manuscript describing a new high-resolution 3D model of the shear wave velocities in the Bohemian Massif, based on ambient noise data. It contains an informative introduction, followed by short information about used data

(selected 24 258 station pairs for ambient noise processing) and detailed description of used methods (an iterative fast marching method resulting in shear velocity maps and a stochastic inversion of dispersion curves for a collection of multi-layered shear velocity models). Furter it describe the results with a large number of figures and useful supplements, resolution and sensitivity tests. The main content of the article is the discussion, that itself is a review of all important articles describing the structure of the Bohemian Massif and especially its lower crust.

The manuscript is long, but clearly describe data analysis, chosen assumptions and methods limitations. It is interesting as it presents the low shear wave velocity anomaly in the lower crust of the whole Bohemian Massif in contradiction to everything that was previously published, and explain that by anisotropy of seismic velocity. As an easy improvement, this manuscript could be shortened and even some figures (e.g. fig.5, 7, 8, 13) could be removed without harming the main message.

Specific comments

1. Reduction of shear wave velocities in the lower crust is a new and surprising result. No such effect was visible in previous P-wave velocity results from CSS experiments. It is, of course, possible that P and S waves have different characteristics, but would it be possible to verify S-wave velocities in the lower crust using existing CSS data? It would be less precise than P-waves, but still, such characteristics should be clearly visible.

The most surprising is the existence of those lower velocities in the whole area. As shown using P-wave CSS at profiles CEL09 (Hrubcova et al. 2005 JGR doi: 10.1029/2004JB003080), and S02, S03 (Majdanski et al. 2006 Tectonophysics doi: 10.1016/j.tecto.2005.10.042) anomalous P-wave velocities in form of layered lower crust were recognized in part of profiles, but not globally. Those areas were interpreter as remelting of the lower crust or magmatic underplating (Majdański et al., Tectonophysics, doi: 10.1016/j.tecto.2007.02.015). Is it really a global feature of Vs of BM?

2. Despite the possible difference in P and S-waves velocities in the lower crust that are possible the boundary of the lower crust, both upper and lower (Moho) should match the P-wave models, at least in range of the uncertainty of used methods. As shown they are quite different. The argument of 3D modelling advantage over 2D CSS is valid, but this effect is not that strong, because in the Bohemian Massif horizontal variations are not that significant. The question is, what level of S-wave anisotropy in the upper and middle crust would be needed to match P-wave models boundaries? As shown by Sroda 2006 GRL doi:10.1029/2006GL027701, for P-wave 10% anisotropy was observed.

3. I strongly disagree that the results of CSS should be neglected as less precise comparing to ambient noise methods. From personal experience, I am convinced that controlled source seismic and P-waves analysis is the most precise method of studying the crustal structure. The second one is CSS with S-waves (as they are less precise to recognize because arrive later, are converted and mixed). The next in precision are receiver function methods, and the least precise the Surface waves (dispersion analysis) methods. Authors refer to my paper (Majdański and Polkowski, 2014 PAGEOPH doi 10.1007/s00024-014-0840-9) as weakness of CSS. This paper shows the limitation of the layer-stripping approach. With previous paper (Majdański Geophysics doi: 10.1190/GEO2012-0280.1) it proves that the uncertainty of layer-stripping approach grows as dv*sqrt(n), where n is the number of stripping iterations. The same is valid for presented layer-stripping ANT in the discussed paper. So what is the final uncertainty of the presented model?

Small technical corrections:

The manuscript is partially written in the first person: "We picked", "We interpolate". Should be in third person.

28: remove space

166: remove space

405: remove . .

I hope that my comments will help to improve this manuscript.

With best regards,

Mariusz Majdański

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