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Interactive comment on “Domains of Archean mantle lithosphere deciphered by seismic anisotropy – initial results from the LAPNET array in northern Fennoscandia” by J. Plomerová et al.

Anonymous Referee #2

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Review of “Domains of Archean mantle lithosphere deciphered by seismic anisotropy – initial results from the LAPNET array in northern Fennoscandia” by J. Plomerová, L. Vecsey, V. Babuska, and LAPNET working group.

General comments

This paper appears as a combination of a progress report and a research study and it presents initial results from the LAPNET array with respect to seismic anisotropy of the lithosphere. Personally, I am not a friend of presenting preliminary results and it is not clear why there is a need for an early publication before a more in-depth analysis of the complete data set can be presented.

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From a scientific perspective I would like to see more background information on the tectonic history of the region and some basic details / short description on the inversion methods used. The authors should also discuss the possible role of anisotropy in the crust and provide some information on crustal thickness and the “crustal corrections” used.

A further separation of section 4 may improve readability (see below).

Specific comments

Section 1:

The first paragraph may need some rephrasing. There are some (possibly) repetitive statements (interdisciplinary / multidisciplinary research; northern parts of Finland and . . . / northern Fennoscandia) also just mentioned in the abstract.

Some details on the geology /tectonics may be appropriate here.

Section 2:

This starts again with a reference to the territory of northern Finland . . .

Fig. 1 (unnecessarily) lists all participating institutions. These are again listed at the end of the paper, which I think is sufficient.

Comments on the size of the data base (740 GB) and the person in charge should be transferred to the acknowledgements, if necessary.

L. Vecsey is coauthor of the paper, therefore, I do not see the need to specifically mention his contribution in the software development. If possible, a reference to the software may be more helpful.

The authors should give some more details on the types of “crustal corrections” they apply. Do they also account for possible crustal anisotropy or effects of inhomogeneity?

The sentence starting with “We search for similarities . . .” is hard to understand and

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may need to be rephrased.

Section 3:

It may be helpful to the reader to describe the pattern of relative P-wave residuals that can be expected (for example) in case of an anisotropic layer with a horizontal (non-dipping) olivine a-axis.

Section 4:

This section should be separated into modelling (section 4) and discussion (new section 5).

Apart from some dashed lines in Figure 3, I miss some more information on the Archean domains that the authors can identify and that are visible at the surface.

Some information on the geological/tectonic development of the region may be useful for the discussion of the results.

The authors should avoid using arrows to symbolize shear-wave splitting effects in Figs. 3 and 4. This implies a non-physical directionality.

Interactive comment on Solid Earth Discuss., 3, 655, 2011.

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