Anonymous Referee #2
Review of "Domains of Archean mantle lithosphere deciphered by seismic anisotropy
– initial results from the LAPNET array in northern Fennoscandia" by J. Plomerova, 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.

*Huge amount of data recorded during several year passive seismic experiments are expensive in a
sense of time consumed during their preparing, running the filed measurement, data archiving and
processing. Results from different methods are step by step published by different teams commonly
during more than one decade. Especially in first publications, the great potential of co-workers is
distinctively and justifiably acknowledged, and a kind of the array documentation is presented, which
might evoke a form of progress report. Gradual publishing of results from different methods is
important and the only way to keep all the teams deeply informed and to be able to progress to the
joint aim - to synthesize all the results into a joint model and its development. However, not all
authors use the wording as "initial", "first" or, "preliminary" in titles of their papers. We changed
the title.*
*We modified the last par. of Introduction in a sense that presented results from P-wave anisotropy,
though measured on a subset but large enough, would hardly change when all the set is processed. On
the other hand, the shear-wave splitting evaluation, whose modelling in 3D is more time consuming
than that evaluated only for azimuthal anisotropy, requires additional measurements.*

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.

*We extended the introduction by notes about tectonic history as requested by the referee (the new first
par. of Introduction), described shortly the crustal corrections (3rd par. of Section 2), complemented
the inversion method (last par. of Section 2), and mentioned effects of crustal anisotropy (4th par. of
Section 2 and new par. of Section 5). On the other hand, we follow recommendations of
respected journals such as, e.g., Gondwana Research with IF 5.5, not to republish methods
which have been already published and instead, indicate them in a reference.*

A further separation of section 4 may improve readability (see below).
We separate the section into two parts as recommended.

Specific comments

Section 1:
The first paragraph may need some rephrasing. There are some (possibly) repetitive
statements (interdisciplinary / multidisciplinary research; northern parts of Finnland
and : : : / northern Fennoscandia) also just mentioned in the abstract.
Some details on the geology /tectonics may be appropriate here.

*We modified the section as recommended, i.e., we added new paragraph on geology/tectonics and
rephrased the former first paragraph.*

Section 2:
This starts again with a reference to the territory of northern Finnland : : :

We eliminated repetition of the territory.

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

List of all participating institutions were deleted from the figure and kept only at the end of the paper.

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

We modified this part as recommended, but we prefer to keep the size of the database in the text.

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.

We modified the text, but unfortunately, the software has not been published yet, neither made available via www.

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 may need to be rephrased.

The large-scale characteristic of the residual pattern can be due to Moho depth variations, which might appear as heterogeneities. We complemented this part of the text with some additional details.

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.

Already in Babuška et al. (1993) we modelled the P spheres for different symmetries and axes of inclinations. We complemented this part as well (1st par of Section 3)

Section 4:
This section should be separated into modelling (section 4) and discussion (new section 5).
We split this section into two parts as recommended.

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.

We added new Fig 1b, new 1st par of Section 1 and extended 1st par of Section 5.

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

We keep arrows to emphasize the 2π periodicity instead of the π periodicity of azimuthal anisotropy presented by bars only and to show the dip direction. We use this convention since we develop the method in early 90th.