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Interactive comment on "POLENET/LAPNET teleseismic P-wave traveltime tomography model of the upper mantle beneath northern Fennoscandia" *by* H. Silvennoinen et al.

Anonymous Referee #2

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This paper presents the application of travel time tomography to elucidate heterogeneous structure of the P velocity structure within the upper mantle beneath northern Fennoscandia. Considering high quality of teleseismic observations with good azimuthal coverage the current work can be complementary with available studies on P velocity heterogeneities (e.g. Bruneton et al., 2004 Sandoval et al., 2004; Eken et al., 2007, 2008, 2010, 2012) as it extents the effort on imaging deep-mantle structure further north. Authors successfully perform the routine procedure including crustal correction, resolution assessment etc., which is followed through imaging using ACH-based tomography algorithms. In this respect I consider the concept of current work deserves to be published in the Solid Earth (SE) with its rich material and arguments.

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Interactive Discussion
Discussion Paper

I strongly suggest authors to consider following comments, suggestions and questions listed below before its publication in the SE.

General Remarks

I found the discussion is the weakest part of this manuscript. I strongly suggest authors: i) To establish a much improved link between the introduction and discussion ii) To perform a much detailed interpretation of results

To give examples, at some parts of the discussion I had difficulties to keep track of the links within the paragraphs and what authors exactly argue. For instance, between lines #8 and 25 of the pages 2539, authors describe very recent anisotropy results obtained from (Plomerova et al. 2011; Vinnik et al., 2014) but never discuss the relation with their P-velocity deviations. I also suggest authors to more focus on neighboring teleseismic tomography studies (i.e. Sandoval, et al., 2004; Bruneton et al., 2004; Eken et al., 2007; 2008; 2012) in a separate paragraph with a comparative manner. It could be even better to give examples from similar type of from other examples of regions of cratons to enhance the discussion part (e.g. African Craton, Indian Craton etc.).

- Recent studies on seismic anisotropy have shown that the study region accommodates a strong anisotropy due to the fossil anisotropic fabrics (Vecsey et al. 2007; Plomerova et al., 2008, 2011, Eken et al., 2010, 2012, Vinnik et al., 2014). Furthermore, numerical and observed data analyses in earlier works (e.g., Sobolev et al., 1999; Lloyd and van der Lee, 2008; O'Driscoll et al., 2011; Eken et al., 2012) seismic anisotropy could introduce bias into the travel time residuals. In particular, Eken et al. (2012) examined the effect of anisotropy on tomographic images beneath the Swedish part of Fennoscandian Shield and found out that anisotropy driven effect could produce significant artifact in tomographic images resolved from the isotropic inversion schemes although the real earth anisotropic. In this respect observed velocity anomalies should not only interpreted in terms of temperature effect and compositional changes but also the effect of seismic anisotropy must be clearly considered in the text.

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Other points

- I think the abstract must explicitly define the motivation for the use of teleseismic tomography method by referring to the geological problem desired to solve for.

- In the data section, authors must explicitly mention the total number of ray paths and threshold value for the signal-to-noise (SNR) likely used in data selection if exists.

- Authors must also explain how they unified travel time residuals extracted from both the LAPNET/POLENET network and the SVEKALAPKO network with two different operating time periods. I this combining data from various temporary networks is a good idea to expand the area of model resolution however needs some common stations to both networks. I think the strategy behind such data merges rarely in use but the process behind it strongly deserves a detailed description.

- The section 4 and its sub-sections are sufficient in general to illuminate reader concerning the details of the imaging method. However, two issues in this section are missing that could be critical during the error minimization. These are:

Eigen value selection: Since authors state that they have used Singular Value Decomposition (SVD) during when solving a weighted least-squares problem, then they must clearly mention how they decided on the optimum value that separates non-zero and very small (close to zero) eigen values. Truncation of the SVD is an important issue since in theory zero eigen values represent error space as non-zero values is only associated with the model space.

Depth resolution: According to the description in the text, authors prefer to represent the bottom of inverted model down to 360 km since below this range diagonal element of the resolution matrix implies poor resolution. There is no clear information about the bottom of the depth level used during the inversions but as far as understand from Fig. 12 the deepest fixed node for the inversion should be around 450 km. I wonder at that point how many different inversions in which the bottom fixed node set to another

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value different than 450 km. If this has already been done for different inversions where the bottom depth of fixed node extends varies from, for instance, 300 to 600 km then I would like to know that if 360 km and shallower parts still represent the best model resolution.

- Final P-velocity perturbations summarized in Fig. 11 indicates a very broad area with almost no perturbations especially for depth sections deeper than 120 km. I think author are cautious to discuss this pattern. I wonder whether it is true that those regions with zero perturbation because they can be very well represented by 1D radial earth model or the issue stems from low-resolution related problem. I strongly suggest authors to investigate and discuss this fact in the text.

Tiny Issues

- A sketch figure summarizing the location of the fixed nodes and floating nodes in horizontal and vertical directions will be very useful for reader.

- Better to inform the frequency content of the WWSSN short period simulation filter.

- They should also give the following reference when they mention about the Seismic-Handler software:

Stammler, K. (1993). SeismicHandler – programmable multichannel data handler for interactive and automatic processing of seismological analyses, Computers & Geo-sciences 19(2):135âĂŤ140.

- In Fig. 5: Annotation on bottom graphic: “Inversion cariances . . . must be changed with variances.

- In Fig. 14: Better to keep the same color code as given in Figs. 9,10,11, and 12.

Interactive comment on Solid Earth Discuss., 7, 2527, 2015.