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Interactive comment on "Seismic waveform tomography of the Central and Eastern Mediterranean upper mantle" *by* Nienke Blom et al.

Anonymous Referee #2

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This paper presents a new seismic waveform tomographic model for the upper mantle beneath the Central and Eastern Mediterranean down to the mantle transition zone. They utilize information from body and multimode surface waves, source effects, anisotropy and attenuation. This manuscript mainly describes technical parts of this work, and does not provide detailed interpretation about their solutions. I do have a range of major concerns about the innovation and improvements of this work in comparison to previous work done by the same group, as well as other groups working on full waveform inversion and its applications for imaging the crust and upper mantle structures underneath Europe.

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Discussion paper

Major concerns:

(1) The authors claim that they also include radial anisotropy in their inversion, however, I cannot find any results for radial anisotropy in this manuscript. I strongly recommend the authors to present these solutions in order to make the statement consistent.

(2) It seems to me that the ETH group has published a number of papers on the same region (Europe) using the same approach (FWI). Why publishing another one or performing another inversion for the same target? What is the long term goal of this group for imaging the European upper mantle using FWI? Just including more data and publishing another paper once a while is not a good scientific exploration as far as I consider. For instance, I can list some papers which are similar to this manuscript. Can the authors provide some statements about significant improvements of this work in comparison to their previous work? Such as (1) Fichtner and Villasenor, 2015, EPSL; (2) Rickers et al, 2013, EPSL; (3) Fichtner et al, 2013, EPSL.

(3) In fact, From Figures 7 and 14, I am surprised to see that predicted seismograms from their starting model (Fichtner et al, 2013) have such large mismatches in comparison to observed waveforms, even for this long-period frequency band. If their previous model is already good enough, we should not see such significant differences.

(4) The authors just present their own tomographic model. In fact, the Mediterranean region is well studied by using either travel time tomography or even full waveform inversion by other groups. The authors ignored the same endeavor towards FWI imaging for the European crust and upper mantle by other groups. I wonder how their current model compares with other previous tomographic or FWI models? As far as I can tell from their Figure 12, the resolution for subducting slabs underneath the Mediterranean is quite low in comparison to previous travel time tomography (Wortel and Spakman, 2000, Piromallo and Morelli, 2003) or FWI models (Zhu et al, a series of studies). I strongly suggest the authors to make some comparisons with these previous studies, in order to convince the readers about the advantages or disadvantages of their current

inversion.

(5) The authors claim that their model has good resolution down to the mantle transition zone. However, in their horizontal cross sections (Figure 10), I only see results down to 300 km. I suggest they to put more slices, say from 400 to 700 km.

(6) For vertical cross sections (Figures 12 to 13). I don't see any reasons to put slices with absolute velocity here. Since they will be dominated by 1D structures, in particular for the mantle transition zone, which involves velocity jumps across 410 and 660. I suggest only show relative velocity perturbations in their right panel.

(7) In fact, for the Hellenic subduction (Figure 13), numerous previous studies have demonstrated that the Hellenic slab can penetrate the 660 km discontinuity down to the lower mantle (from either travel time tomography or FWI), however, the authors' result seems different from previous conclusions. Do we need to re-evaluate this consensus, or the authors' model does not have enough resolution to resolve it?

(8) For waveform comparisons, such as Figures 7 and 14, the authors only show comparisons between observed and predicted seismograms for a couple of station-source pairs. However, as pointed out by the authors, there are numerous seismograms. I am not surprised to see some improvements for some selected pairs of earthquakes and stations. How about other pairs? Since there are many stations in the authors' dataset, why not plot all three components (vertical, radial and transverse) for most data and predictions, just like common-shot gathers as did by exploration seismologists. I think that will be a better way to convince the readers that their model can really explain data, instead of just intensionally or randomly selecting some paris with good match. In addition, I suggest the authors to change color for predicted seismograms from the starting model, such as blue or other colors different from the current waveforms.

(9) Figure 3, why changing iteration numbers for different frequency bands? In order to demonstrate that their model can fit data for different frequency ranges, I suggest just compare the starting one with their current model (100th iteration).

(10) Figure 4, it seems to me that the color of panel d is highly saturated, then they can see a strong banana kernel, how about using some reasonable colors. In addition, do they need to add additional weighting coefficients in order to use both body and surface waves for this inversion?

(11) Figure 11, I suggest to plot iso-surface for relative velocity perturbation instead of absolute values, which is a typical way to examine subducting slabs.

(12) In section 6.2, in order to evaluate their model, the authors chose additional 6 earthquakes as an independent dataset. As far as I can tell, this is a very small dataset to convince the readers that their model can explain waveforms that are not used in the inversion. I strongly suggest them to increase the size of this independent dataset.

Some minor modifications:

(1) Section 1.1, the authors mentioned that the deeper parts of the model are mainly constrained by long-period data, I suggest also add body wave here. Since they provide more constraints for deep structures, in comparison to surface waves.

(2) Page 2, line 49, “manuscript”-> “paper”

(3) Page 3, line 62, what are these plates and micro-plates?

(4) Page 3, line 68, “trench roll back”, I think it should be either trench retreat or slab roll back.

(5) Page 3, line 76, I don’t understand the sentence starts with “West of this, . . .” I suggest to rewrite this sentence.

(6) Page 3, line 80, what is “STEP”?

(7) In section 3.2, I suggest the authors to add the numbers of stations as well as the numbers of measurements they have to constrain the current model.

(8) Page 5, line 105, “download” -> “observed”

(9) Section 4.3, line 143, “some smoothing”, this is not clear to me, what is the smoothing and how large of it?

(10) Table 1, I think either use Period or Frequency, there is no need to use both of them.

My suggestion is major revision.

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