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Solid Earth Discussions

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

Anonymous Referee #1

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In this manuscript, a new seismic model for the Central and Eastern Mediterranean is constructed based on full-waveform inversion (FWI). The authors not only inverted for vertically and horizontally shear and P wavespeeds but also density in their iterative model updates âĂŤ only the phase information used in the measurements, excluding amplitudes. The main focus is on the methodological part where the detailed model interpretation is reserved for a potential future paper.

It is generally a well-written manuscript, however, there are a few issues to be highlighted. The authors state that the focus is on the methodological part which is pretty similar to Fichtner et al. and Krisher et al. except for inverting for density. The method

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part may be shorten to avoid repetitions. The main difference compared to most of the previous FWI studies is the simultaneous inversion of density with wavespeeds. However, there are also other examples in literature from exploration to lithospheric studies where density is also inverted, but they are not cited. It is worthwhile to have a complete literature review, and it would also be more insightful to compare the results of density inversions to those previously published tomographic studies.

On the other hand, the message of the paper needs a bit of clarification. The authors very briefly discuss the potential reasons for the failure of retrieving density in conclusions. However, it is not clear if density can indeed be retrieved or not in FWI. If not, is it better not to invert it, or is there any advantage of inverting for it despite the trade-off between seismic parameters? It would be helpful tp extend these discussions considering the results of other studies.

Some further detailed comments are as given below: âĂĺ Page 2, line 40, last sentence: The authors state that not only P and S wavespeeds but also density can be constrained by full-waveform inversion. However, the results of the study seem not promising to support this statement. The authors discuss the potential reasons, but they do not discuss much what should be done to retrieve density or what the strategy in future studies should be. Will taking full anisotropy and attenuation into account in inversions help retrieve the density model correctly?Âă

Page 5, line 115: Parameterization is an important decision in FWI to capture the physics of the medium. It is not clear if the authors are inverting for only SV and SH wavespeeds for radial anisotropy or also the anisotropic parameter \eta? If the former, why not including \eta to more robustly consider the radial anisotropy?

Why was not the amplitude information used in the misfit, which may be more sensitive to density variations?

The manuscript nicely summarizes the strategies considered in the inversion algorithm. However, it is not described how the crust was considered in simulations and

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inversions, which can have a significant effect on the constructed model. Besides, focusing on a subdomain of the model of Fichtner et al. by performing ∼95 iterations, one would expect higher resolution in the model, which also deserves investigation of depths shallower than 60 km where some interesting crustal features may be observed.

Section 4.3: It would be good to mention the smoothing and/or pre-conditioning strategies if applied.Âă

Section 4.4: To my understanding, the starting model has multi-resolution (smooth global model, inverted European model, and higher-resolution Anatolian model), and a smoothed version of it is used. What is the resolution or the degree of smoothness of the chosen model to start iterations?

Figure 2: There are quite some small-scale variations (smaller than those in velocity models) in the starting model of density. How was density constructed in the starting model? How good is it to start with?Âă

Figure 6: Why is the histogram split into two parts? Why is there a gap around the zero phase shift?

Figure 10: Looks like there is a strong anti-correlation between P- and S-wavespeed models (i.e., the S-wavespeed model shows all slow wavespeed in continents, and fast wavespeed in oceans at shallower depths whereas the P-wavespeed model is predominantly fast). There is also a sharp boundary in the P-wave model on the Eastern part. How do the authors interpret these anomalies? Are these features also observed similarly in other tomographic models?Âă

Figure 15: Looks like the Gaussian anomalies used in the spike-tests of the P-wave model are larger than those of S-wavespeeds and density. Is it a plotting issue (or illusion?) or any specific reason for choosing it to be like that? It would be more insightful to show the spike-test results at other depths as well.

The authors have performed ∼95 iterations, plus five additional iterations for every

parameter during the spike tests. It is a large number of iterations. How expensive is each iteration? Or what is the overall computational cost?Âă

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