Review Plomerova et al., SE-2021-56, first revised version

While several remarks of my first review were appropriately dealt with, there are still remaining issues regarding crustal correction, inversion procedure, resolution tests and interpretation.

Introduction:

There is one strange sentence in the introduction ("We show that thanks to data from the AlpArray-EASI ….") which should be reformulated. I also do not understand the relation between the HV-BM and the HV-anomaly found by Kästle et al. located 1 degree north of the PAL.

Data:

Line 94: A low-noise beam is formed by stacking cross-correlated time shifted traces. To do that some reference is needed relative to which the traces are correlated and shifted. So why is there no subjective choice of a reference trace?

Line 106: "aside" -> outside.

Line 108: Does this mean that the dataset with evenly distributed events was only constructed to show that the one with events in the 60 degree cones provides sharper images?

In Fig. S5, the velocity perturbations still reach up into the corrected crustal domain.

Fig. S3: The authors show Moho thickness as well as velocity and thickness of sediments but not velocities in the crust used for crustal corrections. And they only show these parameters for the station locations. The authors should show one vertical section along the EASI line with the assumed crustal velocities. They should also document in the manuscript (not only in the Reply) how the crustal corrections are computed. Is the 30 km gridding also used for calculating crustal corrections or is a more finely resolved model used. If the 30 km gridding is used, is the crustal model which certainly shows smaller-scale structures smoothed to the much coarser gridding? And finally, why not plot the a priori crustal perturbations relative to IASP91 into one or several of your vertical sections in addition to the Moho?

It is stated that the travel-time residuals are normalized to the array average. I understand from the reply that the average residual is subtracted. Normalisation would imply division which does not make much sense in this context.

Method:

Some explanations regarding the forward problem from the teleseismic source to the receivers would be helpful. How are travel times calculated? How is the region outside the inversion domain treated, how is the transition into the inversion domain managed?

It could be mentioned in the text that the inversion code does not offer an option for vertical smoothing.

I understand that the model domain is made of grid nodes at 30 km depth intervals and, horizontally, by the yellow and green nodes in Fig. 1. It is stated that the data are only inverted for velocity perturbations at the green nodes and not at the yellow nodes. My question is: what is then the purpose of the yellow nodes? Why are they needed at all? Is the velocity at the yellow nodes fixed to the values of the reference model? Moreover, would it not be favourable to allow perturbations at the yellow nodes to avoid mapping of heterogeneities into the central region (green nodes) due to rays which spend a significant path length in the domain of the yellow nodes, especially at greater depths? There are many of these as Fig S1b shows. I urgently recommend doing that because all ignored heterogeneities lying in the region of the yellow nodes will produce artificial perturbations at the green nodes. Are there contributing rays that propagate even outside the area covered with nodes?

Why do the authors still plot data variance instead of misfit which is normalized to the data uncertainties. In the caption of Fig. S4 it is stated that the data uncertainties are included in the calculation of the data variance. But how? If the residuals are divided by the uncertainty a dimensionless quantity results. Why do the authors not simply plot misfit over N defined as 1/N*sum((d-s)/sigma)**2 as is standard in tomographic work?

Why was the inversion stopped after only 2 iterations? What happens if more iterations are done? Please enter some results for further iterations into Fig. S4.

Instead of plotting data variance versus model variance, the authors should plot misfit over N versus model roughness as the latter is used to regularize the inversion and the trade-off occurs between misfit and model roughness (and not model variance).

Results:

What is the criterion for grey-shading in the model domain? Is it derived from the resolution matrix? You give the information in the reply but I do not find it in the manuscript. The caption of Fig. 3 just says that less-well resolved regions are shaded. This is not very informative.

In Fig. 3 the HV anomalies are surrounded by thick dashed lines. What was the criterion for placing these lines. They do not seem to follow a velocity contour. In Fig. 3 the dashed lines do not honour the small shallow area of less positive or negative perturbations which splits the HV-EA anomaly down to about 100 km. This area nicely coincides with the depth maximum of the Moho. In the interpretation later, the entire HV-EA is attributed to the Adriatic plate. Wouldn't it make more sense to attribute the left part of HV-EA to Europe and the right part to Adria given the shape of the Moho there?

Why did the authors saturate the colour scale? I prefer unsaturated scales.

Resolution tests:

In Fig. S6a-d, the vertical sections through the model obtained from real data shows (shaded) perturbations in the crust. I thought that the first layer of nodes at 30 km was not involved in the inversion.

The evaluation of the checkerboard resolution test is a bit optimistic. The vertical section shows oblique smearing owing to the ray geometry which could nicely mimic dipping slabs. Given this result and the massive artefacts in the test shown in Fig. S8, I would not be that confident in the difference of the dip directions as stated in line 225.

Imaging the high-velocity perturbations in different tomography model:

The paper by Paffrath et al. (2021) is erroneously referenced two times as Paffrath et al. (2020).

The HV-BM anomaly is described as trending SW-NE. In Fig.2 I rather see a NW-SE trend of the anomaly at 120 km and 150 km depth that seems to rotate to SW-NE at 180 km depth.

I am seriously worried by the resolution tests shown in Fig. S8. For each one of the detached slab test models there is massive leakage reaching up to the surface which mimics an either northward or southward dipping continuous slab. Apparently, it is hard to distinguish between a slab reaching up to 45 km and a detached one with top as deep as 150 km! Inversion for test models all deliver a shallow north-dipping HV anomaly. In view of Figure S8, I am not at all convinced of the author's arguments for northward dip and for attachment of the slab. I would be interested in explanations for this massive leakage.

This leakage also affects the HV-BM anomaly as shown in Fig. 7. Moreover, the test in Fig. 7 cannot reproduce the shallow splitting of the HV-EA. What would be the result of a resolution test which takes the configuration in Fig. 14c of Paffrath et al. 2021 as test model (a fast shallow Adriatic lithosphere and a detached EU slab)?