Dear Editor, dear Emanuel,

Many thanks to you and both Referees for their work on our manuscript. We have now carefully revised all points that have been raised, and made changes to the manuscript together with replying to the comments below in italics. We appreciate the second reviewer encouraged us to include our thoughts on a possible dual origin of the high-velocity heterogeneity beneath the Eastern Alps.

We truly hope that the revised version will be to your satisfaction, and if you find further points to fix, please let us know.

Best regards,

Jarka, Helena, György and Ludek

Reply to Referee 1 letter and commented reply to previous revision.

Dear Editor, dear Emanuel,

The authors have made a big effort to amend the text and figures. This has made their article much more accessible for the AlpArray and the earth science community, to which they have contributed so much.

Yet, there are first-order issues that were addressed in the first review that still need fixing and clarification. These are commented on in annotations to the authors´response to the editor.

The statement in lines 245-247 is still not correct. As a co-author of Paffrath et al. (2021) and its companion interpretational paper (Handy et al. 2021), I can say for sure that the slab anomaly in our images across the Central Alps dips to the SSE, not to the N (please see Fig. 14b in Paffrath et al. 2021, as well as many vertical sections in Handy et al. 2021, e.g., their Fig. 3c, sections B and 6 in their Appendix). In fact, the S-dipping positive anomaly is also clear in our TRANSALP section (their Fig. 4A). This latter section is across the westernmost part of the Eastern Alps, just east of the Giudicarie-Brenner Fault and certainly not in the Central Alps. At the risk of being forward, I have proposed a formulation in the annotation.

As recommended, we corrected the text according to Paffrath et al. (2021) and Handy et al. (2021) papers and deleted reference to Paffrath et al. (2020) presentation.

Lines 250-252: Perhaps it would be useful for the authors to state before (rather than after) lines 245-247 that they place the boundary between the Central Alps and Eastern Alps at the western end of the Tauern Window, i.e., at the northward prolongation of the Giudicarie Fault, as already proposed by Handy et al. (2015) and other more recent authors to separate different crustal (lower crustal wedges) and mantle (slab gap, slab dips) structures.

The paragraph has been reorganised and now reads as:

Though the upper mantle structure is less diverse in comparison with the crust, in general, ongoing studies of the Alpine upper mantle continue to reveal new and more detailed features in geometry of the lower lithosphere, dip direction of the Alpine slabs, tears or detachments of the slabs and interactions of the Alps with the Apennines and Dinarides. The current stage of knowledge from results of various disciplines – seismology, geology, petrology, tectonics, paleo-magnetism, geochemistry, GPS studies etc. - reflect differences in the segmented slab responses to the acting forces. The complex structure of the fragmented Alpine slab(s) and the broader Europe/Adria collision zone is now visualized in tomography snapshots. The boundary between the central and eastern slab segments of the Alps, is placed at the western end of the Tauern Window, i.e., at the northward prolongation of the Giudicare Fault, as already proposed by Handy et al., (2015) and other more recent authors to separate different crustal (lower crustal wedges) and mantle (slab gaps, slab dips) structures (e.g, Lippitsch et al., 2003; Schmid et al., 2004; Rosenberg et al., 2018). In recent studies, Paffrath et al. (2021) and Handy et al. (2021) document a change from a S-dipping slab to a N-dipping slab beneath the western part of the Eastern Alps (i.e., beneath the Tauern Window). This is in agreement with the previously proposed geometries of Babuska et al. 1990 and later by Lippitsch et al. 2003 and Zhao et al. 2016. Mock et al. (2020) note a discordance between the slab geometry at depth and the boundary between the Eastern and Central Alps observed at the surface.

Lines 373-375: Instead of "…as suggested by Handy et al. (2015)…", I would substitute the words "…, analogous to the scenario proposed by Handy et al. (2015) for delamination of European lithosphere further to the S beneath the Eastern Alps (Fig. 8a)." This would distinguish the models more clearly.

Text was modified as suggested:

The simplest explanation would be to consider it as a fragment of the delaminated part of the European plate subductions, analogous to the scenario proposed by Handy et al. (2015) for delamination of European lithosphere further to the South beneath the Eastern Alps (Fig.8a).

Lines 404-409: For the purposes of their text, I would recommend that the authors cite Royden & Burchfiel (1989, Tectonics, 8, 1, 51-61) and/or Royden (1993, Tectonics, 12, 629-638) instead of Kissling & Schlunegger (2015). Although both papers use the term "rollback subduction", they invoke quite different mechanisms. The Carpathians-Pannonian system is generally regarded as a prime example of oceanic and continental subduction in the absence of significant plate convergence, as discussed in numerous papers of Frank Horvath, Wiki Royden & Clark Burchfiel (ob cit). The consequence of this is that significant upper plate (Pannonian) extension and mantle flow are required to accommodate lateral retreat of the downgoing (European) plate in a mantle reference frame. I believe this is exactly (or close to) what Jarka Plomerova and her co-authors mean in their discussion of "complex flows of the mantle". Note that this is not identical to the mechanism proposed by Kissling & Schlunegger (2015) which involves minor slab-hinge retreat due to slab steepening at the very end of continent-continent collision. The amount of mantle flow in their model is minimal, if not negligible, at least compared to the model of Royden et al. It is rather unfortunate that Kissling and Schlunegger (and in their papers since) also termed this rollback subduction (I would have called it "late orogenic slab steepening").

Thank you for this good suggestion; the reference used (Kissling & Schlunegger, 2015) was substituted by the two new references:

Differences in the roll-back subductions of the Alps (e.g., Royden and Burchfiel, 1989; Royden 1993) and the Carpathians, northward push of Adria and European slab delamination beneath the EA could have formed complex flows in the asthenosphere (e.g., Vignaroli et al., 2008), which could "transport" a purely oceanic lithosphere or a mix of oceanic and continental lithosphere fragments through the open space between the E. Alpine and Carpathian slabs north-northeastward into the mantle beneath the BM.

Figures: The labelling is much improved, but still falls short of desirable, as commented in the annotations. This is the now the editor`s prerogative to decide how much is enough. Surely, plotting boundaries on a map to help locate anomalies with respect to the surface geology is much less arduous that processing seismic data to determine the anomalies themselves! The benefits for the community would be immense. The trace of the cross section in Fig. 8A should be plotted on a map, as recommended already in my first review (though please excuse me if I have overlooked them). It would also be a great help to have longitudinal coordinates above the cross section in this figure.

We continue keeping tectonic boundaries and do not plot political boundaries as they have no geophysical meaning in map views with sufficiently dense geographical coordinates. We modified caption of Fig. 8a (there are no exact coordinates in Handy et al., 2021). In the first revision we chose the proposed option and included the coordinates into caption of cross-sections through our tomography, not to overload the figures with labels.

The manuscript can be accepted after minor revisions along the lines recommended above.

Reply on "commented on in annotations to the authors´response"

We have incorporated the overwhelming majority of suggested changes in the manuscript (and do not report all of them here unless there is a thought to add); similarly, other very tiny changes are in the manuscript as well.

May I suggest the following minor change to make it even clearer:

"...imaged two segments of Alpine slab, one dipping S beneath the Western Alps and another dipping N beneath the Eastern Alps, with a gap between them." Note: "slab" instead of "slabs" -plural- which can be misinterpreted to mean that two slabs were each separated into segments.

Text modified as suggested.

The word "keel" is no more used to avoid any potential misunderstanding.

HV-BM   HV-EA

Ok, fine. The readers will follow you if you explain the terms like you do here.

The text where these abbreviations are introduced has been coplemented.

Dip change C./E. Alps

Reference to Paffrath et al. 2020 was deleted and text modified as recommended.

Recommended references for C/E Alps boundary added.

Ad (2) triple junction (Brückl)

This was a heading in our reply, with which you agreed, to your part 2 of the paragraph:

The suggested formulation "….speculating the anomaly beneath BM could be derived from subduction and lateral transport in the mantle of the oceanic embayment of Alpine Tethys." was incorporated.

Regarding Fig. 8A (cross sections), the authors appear to have ignored the recommendation to show the location of the cross sections on a map. This would be necessary to check the superposition of the anomalies that are compared (Paffrath, this work, model of Handy et al. 2015). One asks oneself if this science is reproducible...

Note that the anomaly labelled EU in the cross section needs an equivalent in the legend (is it labeled EU-BM in the caption?).

We complemented the figure, its legend and caption. We do not have the proper coordinates of the profiles from Handy et al (2015) as taking it from their figure 10 would be very difficult and imprecise; instead, we refer to their figure 10 for the location. The sizes of the high-velocity heterogeneities shown (contours) are modulated to fit the depth and lateral scales according to the NCA, TW, PL locations.

Reply to Referee 2 comments.

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.

This is now clarified in the reformulated the text:

Based on data from the AlpArray-EASI (Hetényi et al., 2018b) and AlpAray Seismic (AASN) networks (AlpArray Seismic Network, 2014; 2015; Hetényi et al., 2018a), our tomography shows a small-size high-velocity heterogeneity at ~100 - 200km depths beneath the south-eastern part of the BM (referred to as HV-BM throughout the paper), sub-parallel to and distinct from the E. Alps high-velocity heterogeneity (HV-EA). Kästle et al. (2018) identified in their surface-wave tomography approximately 1° to the north of the Periadriatic Fault a high-velocity heterogeneity similar, similar to HV-BM, but further to the west.

Data:

Line 94: **A low-noise beam i**s 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?

Answer: The reason for that is simply to avoid subjectivity in the selection of the reference trace.

Text in revision 1:

A low-noise beam trace created from stacked cross-correlated and shifted traces of an event serves as a reference in the second cross-correlation step and beam forming in the P-arrival time picking. This means, there is no subjective a priori selected single reference trace.

Modification for Revision 2:

Instead of a subjective selection of a reference trace, we cross-correlate all pairs of traces, in the first step. For each trace a time shift related to maxima of the cross-correlation function is determined. Traces at individual stations are then shifted by a weighted average of the time-shifts gained from cross-correlations with all other stations which recorded the event. The low-noise beam trace, created as a stack of the shifted traces at all stations, form the reference trace for performing new beam forming and arrival time picking of an event in the second cross-correlation step.

In the second step, we correlate traces of all stations with the reference low-noise beam trace. The waveforms are aligned according to the times related to the maxima of the cross-correlation functions. The new low-noise beam is computed as a median of all aligned traces and the P-wave onset on the beam is determined automatically (see Fig. S2). Then, arrival times at individual stations are derived from differences between times of corresponding extremes. Arrival times on the station signals are measured by three different methods. The final times of individual extremes (green P1 and P2 in Fig. S2a) and their error estimates are computed from the normal distribution, which approximates a mixture of normal distributions of partial picks (for details see Fig. S2a).

############

Line 106: “aside” -> outside.

Answer:

We understand “outside” as generally external to the region of interest, whereas “aside” here refers to the area that is adjacent and just out of the longer sides of the region, i.e., west and east of the regions (along Alpine strike structural variations). We therefore will keep aside in the text here.

#############

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?

Answer:

We have explained in the text the reasons for limiting data to 60 degree wide cones:

*“To eliminate mapping effects of heterogeneities **aside** the model (meaning adjacent and along the long sides of the array) into its internal part and to enhance the resolution in direction of the subductions, we selected additional rays coming from the northern and southern 60° wide azimuth bins.”*

The last sentence of the paragraph was modified as:

Figure S5 demonstrates that potential bias coming from heterogeneities west and east of the N-S oriented elongated array is weak and that the events from the 60deg. cones illuminate better the Bohemian Massif (BM) - Eastern Alps (EA) structures.

#########

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

We apologize to have forgotten to correct the plotting error in the supplementary Fig. S5. This is now corrected and this figure is compatible with the figures in the main text.

############

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?

Answer: We have rewritten the last paragraph of Data section to address all these queries:

Teleseismic data cannot resolve velocities in the crust itself due to their sub-vertical propagation at shallow depths. To avoid mapping effects from the crust into the velocity perturbations in the upper mantle (e.g., Karousová et al., 2012), one has to introduce crustal corrections. Unfortunately, up to now, there is no uniform, sufficiently detailed model of the crust for Europe, neither for the AlpArray region. For our body-wave studies in Europe we have collected accessible information on the crust for each station from different sources: from Karousová et al. (2012, and references therein) for the BM mostly based on results of control source seismics; from, e.g., Di Stefano et al. (2011), Hua et al., (2017), Tesauro et al.(2008) for areas south of the BM; and from Hetényi et al. (2018b) along the EASI transect. The crust is characterized beneath each station by the depth of Moho, the dip angle and dip direction if the Moho is not flat, the Moho jump if there is any, velocity in the crust, thickness of sediments and sediment velocity (Fig. S3). Sometimes there are significant differences between different models, in their overlapping parts, therefore, we do not attempt creating any kind of „fine“-gridded model of the crust. Instead, we tune the corrections individually beneath each station and correct the travel time residuals along each individually traced ray for the difference in the „real“ crust and the crust of the reference model. Carefully pre-processed P-wave travel-time residuals calculated relative to the IASPEI'91 velocity model (Kennett and Engdahl, 1991), corrected for the crust, normalized to the average residual per event and cleaned from outliers serve as input to the inversion in which we do not invert for the crust. With this approach we gathered a high-quality and uniform dataset of travel time residuals for a proper tomographic inversion to resolve structures in our target region and below the crust. Of course, all crustal models remain idealized models and aren't hundred percent correct. Therefore, small "uncorrected" perturbations remain in each teleseimic tomography model of the upper mantle, which one needs to consider when interpreting the results.

########

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.

Answer:

The term "normalization" is used since decades (even since pre-digital recording era) for computing relative travel-time residuals. The relative residuals can be calculated from absolute travel times relative to (1) an array travel time average, or to (2) the travel time of a reference station, or to (3) an average travel time of a subset of stations. Each of the procedures has its advantages and disadvantages, which are reflected in the accuracy of the relative residuals as well as the stability of the reference level (see e.g. Babuska and Plomerova, 1992). Regardless of *the choice of this reference level, it is always subtraction and never division that is applied in this type of normalization.*

######

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?

Answer:

It is unusual to describe basic information on traveltime calculation in each scientific paper. They can be found in any tomography text book, e.g. "Seismic Tomography: Theory and Practice", by Iyer and Hirahara. Regarding the region outside the inversion domain: Each teleseismic regional tomography has to deal with heterogeneities outside the studied volume. It is standardly done by the normalization as described above. Transition into the inversion domain: Bottom entrance to the volume is fixed according to the reference velocity model and then the ray is traced according to ray-bending technique by Steck and Prothero (1991). The forward task is solved only within the region studied.

################################

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

Answer:

Sentence incorporated: „There is no vertical smoothing in the code."

Comment: Because teleseismic body-wave tomography optimizes velocity perturbations in horizontal layers, and because the ray geometries may already cause sub-vertical leaking, vertical smoothing in teleseismic tomography is generally not useful and not applied.

#################

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?

Answer:

Basic principles of body-wave tomography say that only blocks/grid-node surroundings with crossing rays can be considered as resolved. Which is not the case of the yellow points, but those grid nodes are still used in ray tracing in the entire volume.

The question in the last sentence is not clear, unless the answer is a trivial "no". In teleseismic tomography with foci outside the region, only rays entering through the bottom of the volume contribute to computation, contributions from ray-paths "outside the area" are not considered.

#################

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?

Answer:

*The formula for evaluation of data variance in the code is similar to what you write in your comment. It is 1/(N-1)*sum((residual - average of residuals)*weight)**2, i.e., instead of dividing by sigma, the numerator is multiplied by a unitless weight. The weight of each travel time residual depends on the measurement uncertainty, determined during the arrival time picking (see the decription in the text).*

Caption of Fig.S4 is modified.

#########################

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.

Answer:

In our response to the previous revision we have written that we performed one more iteration, but overall imaged perturbations remained without significant changes, therefore we decided to stop calculations after the second iteration to save the time.

########################

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).

Answer:

We plot the trade-off curve in the same way as numerous authors of tomography models using the codes Telinv or AniTomo, e.g., Lippitsch et al. 2003; Sandoval et al. 2004; Shomali et al. 2006;

Eken et al. 2007; Karousová et al. 2012; 2013; Plomerová et al. 2016; Silvennoinen et al. 2016; Chyba et al. 2017; Munzarová et al., 2018b. This enables a closer comparison with their results.

#######################################

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.

Answer:

Yes, the shading of less-well resolved regions was set according to the resolution matrix, ray coverage grid cells and derivatve weight sums. The contour follows the value of 0.15 of diagonal elements of the resolution matrix.

Caption of Fig. 3 is complemented accordingly.

################################

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?

Answer:

We have deleted the contours in Fig.3, which followed smoothly the 0% perturbations, and have newly denoted the high-velocity heterogeneities as labels using Roman numerals I, II, III (the text of the 3rd para. of the Results section has been modified accordingly). We have also added a new paragraph at the end of the section pointing to the possible dual source of the high-velocity heterogeneity beneath the E. Alps. Corresponding tiny modifications are in the Abstract and in the Conclusions.

New text:

Looking at perturbations of the HV-EA heterogeneity at greater details, one can recognize its potential dual source. The positive heterogeneities I and II (Fig. 3) are separated just beneath the TW, where we modelled the European and Adriatic crust contact (Hetényi et al., 2018b). Dip directions of the heterogeneities slightly change – HV-EA-part II seems to immerse southward (Fig.3a,b), whereas HV-EA- part I dips to the north. This allows us to argue for a mixing of a detached EU slab fragment and the shallow Adria slab connected at depth, as already suggested in Babuška et al. (1990) using a much coarser model inferred from P residuals.

##

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

A saturated scale in mapping velocity perturbations is frequently used. The reason is not to mask variations in a real range of perturbations by possible outliers, mostly at the edges of the region. In our example here, we have inverted at 1625 nodes, out of which only 6 % exceed the (-4, 4%) dv range with the applied damping.

##

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.

Answer:

The same as for Fig.S5: the first layer of nodes at 30km was not involved in the inversion. We apologize but forgot to correct a plotting mistake in the supplementary figures, including FigS6a-d. Now these are all corrected.

##

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.

Answer:

Checkerboard tests are always requested, but inversions with targeted synthetic anomalies, as e.g., in our Figs. 4 and S6a-d, document the specific resolution and provide more plausible grounds for evaluating the results (E. Kissling, personal communication).

There is some leakage in the presented tomography, but it is far from producing massive artefacts.

###

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).

Answer:

Reference to Paffrath et al. (2020) has been erased thorough the text and in References.

####################################

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.

Answer:

We do not derive the trend of the HV-BM anomaly only from its shape retrieved in our tomography itself, as it would be speculative due to its size and the array width, let alone to speculate about a change with depth. We derive the trend from the comparison with two larger tomography results (Karousova et al., 2013; Paffrath et al., 2021). The text stays as:

Considering the NE continuations of the HV-BM as imaged in body-wave tomography of a larger extent (e.g., Karousová et al., 2013; Paffrath et al., 2021), the heterogeneity strikes with the SW-NE trend, in parallel with the boundary of the Moldanubian (MD) and Brunovistulian (BV) mantle lithosphere in the BM, and the westernmost part of the Carpathian front.

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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.

Answer:

Due to leakage, which exists in each teleseismic P-wave tomography, one has to be careful when interpreting and concentrate only on distinct features. Figure S8 demonstrates that the dark blue perturbations in the model with shallow heterogeneity reproduce the dark blue perturbations in the model from real data better than the model with heterogeneity below 150 km depth. This test aims at verifying whether our tomography is able to image and distinguish between an attached and a detached heterogeneity, and is not targeting the dip. Tests focusing on the respective dips of the heterogeneities are presented in Figs. 4 and S6a-d.

Text complemented as:

<u>Due to the leakage, which exists in each teleseismic P-wave tomography, one has to be careful and concentrate only on distinct features. Figure 8 demonstrates that the dark blue perturbations in the model with shallow heterogeneity reproduce the dark blue perturbations in the model from real data better than the model with heterogeneity below 150 km depth.</u>

###

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)?

Answer:

Fig. 7 shows that leakage of the model perturbations of the HV-BM towards the surface is weak and acts against the negative perturbations imaged from real data. There is no surprise that a simple prism model cannot reproduce details of the complex HV-EA heterogeneity.

We have performed a variety of resolution tests, and continuing could be an endless process. The region of the Eastern Alps is complex and tomography studies will definitely continue investigating this area. We will consider suggested tests, as well as special tests focused on effects of crustal corrections in future work.

Text complemented:

<u>The leakage of the model perturbations of the HV-BM towards the surface is weak and acts against the negative perturbations imaged from real data. There is no surprise that a simple prism model cannot reproduce tiny details of the complex HV-EA heterogeneity.</u>

##