Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-189-RC2, 2021

Solid Earth Discussions
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Interactive comment on "Teleseismic P-waves at the AlpArray seismic network: Wave fronts, absolute traveltimes and traveltime residuals" *by* Marcel Heinz Paffrath et al.

Anonymous Referee #2

Received and published: 28 January 2021

General Comments:

In their study, Paffrath and co-authors present a strategy for an automated, combined estimation of absolute and relative P-wave arrival times of teleseismic waveforms for the AlpArray seismic network. The authors apply their method to 370 teleseismic earthquakes recorded by the AlpArray network, which includes 1025 permanent and temporary broad-band stations (including OBS stations in the Ligurian sea). The method is applied in two different frequency bands: 1. HF (Band-pass: 0.03-0.5 Hz (2 s – 33 s)), 2. LF (Low-pass: 0.1 Hz (10 s)). The HF dataset contains about 170'000 arrivals, the LF set contains 214'000 arrivals. The authors analyse and interpret the correspond-

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ing travel-times and corresponding residuals in terms of quality of the arrival times as well as potential velocity anomalies in the mantle. From the stacked residuals as well as stacks of azimuthal bins, the authors infer the presence and orientation of slabs beneath the Alpine orogen. In addition, the authors analyse and interpret travel-time differences between the two frequency bands and conclude that the observed difference is related to "finite-frequency effects". Travel-times are planned to be used for a future tomographic inversion of the seismic velocity structure of the mantle.

The combined picking-procedure proposed by the authors is reasonable and the derived quality documented by the consistency of the derived travel-time fields and residuals seems very promising. The authors did a great job in attempting to quantify the errors of their arrival times (for the absolute picking as well as the cross-correlation differential time estimation) and their results (and their documented consistency) is quite impressive, hopefully leading to new high-quality images of the mantle structure beneath the Alps. Nevertheless, I have several comments listed below, which hopefully help to improve the quality of this study and presumably require at least moderate revision of the manuscript.

Specific comments:

1) My main concern relates to the interpretation of the presented travel-time residuals. The authors argue that the residuals are mainly related to mantle structure and in the discussion and interpretation residuals are mainly associated with the presence of lithospheric slabs and possible impact of crustal structures are largely ignored/faded out. According to the authors, the argument for this is that residuals calculated for the crustal model of Diehl et al. 2009 (shown in their Figure A2) look different and are of smaller amplitude. The comparison of absolute amplitudes, however, is difficult, since no colour-scale for the residuals is included in Figure A2. In addition, it is not clear to what reference 1D model the residuals shown in Figure A2 relate. Also, what is meant with "near-surface model" (caption in Figure A2)? Is it just the shallowest part of the crustal model of Diehl et al. 2009 or the entire crustal model? Most importantly,

the distribution of teleseismic residuals related to crustal structure (compared to a 1D crust) shown in Figure A2 deviates from other estimates e.g. the one of Waldhauser et al. 2002 (Figure 6 in that paper). Waldhauser et al's model predicts negative residuals <-0.5 s along the western Alpine Arc related to the Ivrea Body and strong positive residuals (>1.0 s) related to the Po-plane sediments. Both signals seem to completely missing in Figure A2 (although at least the Ivrea anomaly is completely included in the crustal model of Diehl et al. 2009). Also, the Molasse sediments in the northern foreland are not visible in A2. It seems to me that the residuals shown in A2 mainly correlate with the crustal thickness (negative where relatively thinner crust, positive where relatively thicker (e.g. crustal root of the Alps/Apennines) but other effects (e.g. sedimentary basins, Ivrea body) are missing. Especially the Ivrea body could have quite a huge impact and I would assume that parts of the negative residuals in the western Alpine Arc (the region labelled "W" e.g. in Fig. 6) are affected by the Ivrea body rather than evidence for a slab. Ivrea is not mention in the entire manuscript although it is expected to be one of the strongest shallow anomalies in the Alps. Some parts in the discussion of the observed residuals also seem inconsistent. E.g. on page 19 line 400 the authors say the "W" anomaly should be shallow (which would be consistent with the Ivrea body), then on page 25 (line 531) they associate it with the subducted European lithosphere (which I would not describe as a shallow anomaly).

Also, I do not fully understand the meaning of the ("demeaned") residuals shown in the different figures and if (and how) they are comparable in absolute terms. The authors write on page 15 (around line 335) that "the stacked residuals are relative to an unknown 1D earth model [...] and not to a standard earth model". Does the comparison of absolute residuals with the ones of figure A2 then makes sense? How different is the "unknown 1D model" from the 1D model assumed for figure A2? If comparable, it would then make more sense to subtract the predicted crustal part (A2) from residuals shown in other figures (e.g. Figure 6 etc). Then the corrected residuals would reflect pure mantle signals (assuming the crustal corrections are correct and complete). Why are the colour bars in most Figures (e.g. 6, 7) labelled as "average

time difference" and not "Travel-time residuals" (as used in the text)?

The authors spent a lot of thoughts and work into the error estimation of their automatic picks. But they do not seem to use this information for the residual analysis and their interpretation. In my opinion it would be more consistent to have section 5 (the description of error distribution) in front of section 4 (the section with the results) and use the observational weights for the stacking of the residuals to get quality-weighted stacks. In the current version, as far as I understand, no weighting is used for the stacked residuals, right?

In summary, the discussion of the observed travel-time residuals should be improved and extended and crustal anomalies possibly affecting the observed residuals (including conclusions of others like Waldhauser et al 2002 or Zhao et al. 2016) should be properly discussed. Also, the authors should address specifically the open questions of Alpine mantle structure and what their preliminary results could potentially contribute to solve them. E.g. Western Alps: Continuous vs detached slab (Lippitsch, Zhao), Eastern Alps: slab reversal (Lippitsch, Mitterbauer). E.g. what is the interpretation of the positive residuals in SE-France? Does this correlate with any feature of previously published models?

On the other hand, since the authors obviously also work on a tomographic inversion of this data set, the whole "preliminary" interpretation of the "raw" residuals could also be drastically shortened and residuals and travel-time fields can be discussed primarily with the goal to document the consistency of their derived travel-time data.

2) The description of the picking procedure is a bit difficult to follow in some places. Maybe the description could be supported by a flow-diagram summarizing the key steps of the proposed method and/or a sketch figure with a generic seismogram illustrating some of the key parameters such as tmpp, tepp, tlpp, etc. Also, I miss some key references in section 3, which indicate that the proposed method is not entirely new but has used/proposed already by others (in slightly different forms). Only in the discussion

the authors mention two papers (Rawlinson & Kennett, Mitterbauer) using a similar approach. But there are other groups using the same idea of CC, stacking, absolute pick on stack and then correcting absolute picks with the CC-information. One of them was proposed by Rowe and co-workers (e.g. Rowe et al 2002, BSSA (An Automatic, Adaptive Algorithm for Refining Phase Picks in Large Seismic Data Sets). Also "classic" references to AIC-pickers, Higher-Order statistics and beamforming should be added when introduced the first time in the manuscript. In the discussion, the authors should also compare the performance of their processing in a more quantitative matter with other studies. E.g. how absolute and relative uncertainties compare with other teleseismic data sets in the Alps (Lippitsch, Mitterbauer, Zhao) or elsewhere. Additionally, useful information to be discussed: How computationally expensive is their method? How long does it take to process this dataset? Is the code to parallelized? What are the computational requirements? Is the code published and usable for others?

3) I understand that the difference in travel-time residuals between HF and LF data is interpreted as "finite-frequency" effect (e.g. page 26, l. 565) by the authors. In the abstract, however, the authors write, "caused by velocity dispersion". Somewhere else (section 4.4) the authors explain "velocity dispersion due to attenuation" and then mention "finite frequency effect (finite wavelength effect)" as alternative explanation, which seems to relate to the different physical resolution due difference in frequency content. Later in that section the authors mention the effect of "wavelength smoothing". None of these mentioned "effects" comes with a reference and I find it confusing what "effect" the authors finally prefer to explain the observed difference in travel times. I interpret "finite-frequency effect" as the "wavefront-healing effect" e.g. described by Wielandt 1987 or Hung et al 2001. Is this what the authors mean? I would suggest to use a consistent and homogeneous terminology in all parts of the manuscript and to improve the description of potential effects (including references describing these effects).

4) The English should be further improved in my opinion. I spotted several issues in

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grammar, punctuation, and style, some examples are listed below. In general, I found superfluous repetitions as well as vague and rather qualitative statements (e.g. “highly important”, “highly accurate”, “drastically higher signal-to-noise ratio”) in many places of the manuscript, which should be improved in another round of proofreading. Try to replace these qualitative statements with quantitative ones if possible (e.g. “results in improvement of signal-to-noise by a factor of X”)

Detail comments:

- l. 5: At this point it is not clear what the frequencies relate to (high-pass or low-pass, lower or upper corner?). - l. 10: “reproducible” -> “stable”?

- l. 17: “. . . way lower” -> “X times smaller” or “ a factor of X smaller”

- l. 19: “location dependant” what does that mean? Site-specific noise at station or depending on the source (earthquake) location (e.g. shallow versus deep)?

- l. 25: “At its core” -> “The backbone”?

- l. 27: -> “river Main in Germany”

- l. 35: -> add references for tele-tomo methods

- l. 36: -> explain why resolution is limited to 500 km depth (network aperture controls cross-firing at depth) or give reference

- l. 40: I would mainly argue that FWI needs a good (3-D) starting model and therefore requires knowledge from travel-time tomo

- l. 50: “oscillatory” is there a better word? E.g. “monochromatic”? “emergent”?

- l. 64: “earthquake location” -> I would say that these picking methods work well on local to regional scales and have been used for earthquake location and local earthquake tomography methods.

- l. 65: it sounds like tele-tomo requires higher precision than local and regional

methods... I would rather argue that CC can be used for tele-tomo because of the homogeneous simple waveforms from a close to planar wave-front incident from below. Therefore, CC is not usable for local, regional methods, unless you have similar earthquakes (e.g. used in relative relocation).

- l. 75: “Using AlpArray data, we demonstrate that...”

- l. 81: “... prior to any tomographic inversion ... in the upper mantle.”

- l. 85: “... network Z3 started in 2015.” Also: somewhere here I would expect the reference to a Figure showing this network, could be combine with existing Figure 1 (a=network map, b=event distribution)

- l. 92: “Moho jump” -> “Moho offset” Also: Add references for all these possible structures in Moho and slab geometry.

- l. 96: -> “peak in station...”

- l. 102: You used the gCMT catalogue, did you use the centroid-time as origin time? This time might be different to the origin-time derived from P-waves (e.g. used by NEIC or ISC), right? Could that explain some of the larger differences between predicted and observed P-onsets you report later? Why not use a body-wave based OT as reported by NEIC or ISC catalogues?

- l. 105: What order did you use for your filters? Did you make sure that phase-shifts are avoided? Zero-phase filter? Can you exclude any impact of filters to the difference in the two data sets?

- l. 118: what do you mean with these “statistical anomalies”? Anomalies in terms of what?

- l. 140: “low-noise beam trace” add a reference in which beamforming is described

- l. 160: “higher-order ...and the Akaike” add references where this has been described and used for picking.

- l. 165: somewhere it has to be clearly mentioned that this HOS/AIC picker is used for the individual traces (to get a time reference for the CC) as well as for the absolute pick derived from the stacked seismogram. At least this is how I understand it... What is the advantage of applying AIC to the HOS-CF rather than directly on the seismogram? Shouldn't the AIC work as well on the original seismogram? Using the HOS-CF might introduce systematic shifts due to the finite window length used for the moving-window approach in the HOS-CF calculation. Can you exclude delays caused by window length? Is the window-length fixed or dynamic (considering the frequency content of the signal)?

- l. 198: You mentioned "manually evaluated". Did you compare picks and uncertainties to a subset of manually determined onsets at some point to assess the quality and reliability of this approach?

- l. 199: "The reason for this is ..."

- l. 205 and elsewhere: "anchor point" -> "time reference" or "reference time"

- l. 210: Again, is it possible that the large time offset between predicted and actual onset is due to the fact that you use the gCMT centroid OT? Would that be the same if you use NEIC or ISC times?

- l. 222: "foots on" -> "is based on the assumption"

- l. 234: "as representer" -> "to be representative of"

- l 244: not clear, did you pick the onset on the beam by hand or automatically (or both?) in your study? If automatic, it's the algorithm described in 3.2, correct?

- Figure caption 3: When reading the caption, it is not yet clear that there are uncertainties for the absolute as well as relative onsets calculated (the description of the relative only comes later). Maybe say "Onset uncertainties for absolute and relative onsets (as described in the text) are displayed by ..."

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- l 247: It's the first time the traces are correlated with the beam, not the second time, right? "The traveltime of. . ." -> "the traveltime at. . ."

- l. 252: delete "by construction"

- l. 253 and elswhere: "jitter" -> "scatter"

- l. 271: "any signs" -> "any evidence"

- l 285: Does your error assessment for CC-delays also identifies possible problems with cycle-skipping? This is particular problematic for emergent onsets. A potential sign for this is neighbouring local peaks in the CC-CF. Is your algorithms able to identify this?

- l. 286: not sure "significant coda" is the right term here. It is more additional complexity in the signal, e.g. caused by converted phases. It's not really a long lasting "coda". . .

- Figure 3: Should be bigger (portrait arrangement), in caption say that this is one event (and which one). "heavy outlier" -> "severe"? how is this outlier defined? Here and elsewhere in text and other captions: "temporal distance of 1 s": This sound quite wrong to me. You should rephrase this, e.g. "isochrone contour intervals of 1 s" or something. . .

- l. 293: "superiority" -> please rephrase that e.g. "demonstrates the improvement"

- l. 297: shouldn't it be NE to SW?

- Figure 5: Put azimuth and distance as bold text in the figure itself, this makes it easier to compare (otherwise one needs to find this in the caption).

- l. 310: isn't it a obvious fact that the larger the epicentral distance the steeper the incidence, the higher the apparent velocity?

- l. 314 and elsewhere: "widening" -> "broadening" ?

- l. 335: “find out” -> “identify”

- l. 335: “It is highly important. . .” Please rephrase that sentence, it’s also not clear to me what you mean here (see my general comment on comparison of residuals).

- l. 334: “mantle events” -> you mean “mantle phases”, right? Not earthquakes in the mantle. . .

- l. 346: “completely overwhelm” -> rephrase, maybe “dominate over data from poorly. . .” - Figure 6: Make the inset bigger (currently it has the same size as the original data, not clear what’s the benefit of it). What is “structures Vp values. . .”? Where is the tectonic map of M. Handy you mention in the caption?

- l. 378: “Features that start to move” please rephrase. . . the are “laterally shifted” or something.

- l. 382: “reinforced” -> ”enhanced”?

- l. 385 “OBS” -> “OBS stations”

- l. 395: “survive” -> rephrase, maybe “remain”?

- Figure 9: Too small, add color bar. . .

- l. 484: “positive impact . . . is salient” Not sure, I don’t see this in Figure 10, symbols are too small, make Figure 10 bigger. . .

- l. 496: “have tightened” -> please rephrase

- l. 497: “A way out of this dilema” -> please rephrase. . . “To overcome this problem. . .”

- l. 532: “We do not carry out here. . .” -> please rephrase

- Discussion: Please carefully revise the English in this part. . .

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