

Solid Earth Discuss., referee comment RC2
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Peer-review of ms. SE-2021-33 by Kind et al.

Anonymous Referee #2

Referee comment on "Moho and uppermost mantle structure in the Alpine area from S-to-P converted waves" by Rainer Kind et al., Solid Earth Discuss.,
<https://doi.org/10.5194/se-2021-33-RC2>, 2021

Peer-review of ms. SE-2021-33 "Moho and uppermost mantle structure in the greater Alpine area from S-to-P converted waves" by Kind et al.

PAPER AND REVIEW SUMMARY

The manuscript presents a large amount of teleseismic data of P phases preceding direct S arrivals, to deduce the crustal thickness and sub-crustal velocity structure. While this method is typically used with deconvolution and for targeting the lithosphere-asthenosphere boundary, the Authors argue for a direct use of raw waveforms with almost no additional processing. The methodology is of interest but further information on the details are needed, as currently the method is not reproducible. Moreover, due to some simplifications, uncertainties weigh on the results, therefore these need to be quantified. The results bear on the Moho depth, with comparison to earlier studies, as well as negative velocity gradients (NVG) below the crust. Some of the interpretation is sound but some others seem to be subjective, hence more caution would be wise. For example, among the three points presented as main conclusions: (1) the offset across the Mid-Hungarian Zone is not supported by the data and contradicts all previous results using more appropriate methodology, therefore it does not appear well-resolved and credible; (2) the Moho trough in the Bohemian Massif connecting with the Western Carpathians is a nice and somewhat new result; (3) the interpretation of NVGs is very subjective and other Readers may find more or less of the interpreted results, I would use more caution, and possibly focus on amplitude variations.

The text itself is well written for the language, and figures are technically clearly presented.

Overall, the study is worth being published in Solid Earth, however moderate additions to methodology and major revision of the interpretation is appropriate. Further details are provided below with line numbers referring to manuscript lines and figures as in the preprint.

POINTS OF MORE IMPORTANT CONCERN PRIOR TO INTERPRETATION

1. The method presented by the authors is suitable for imaging smooth, broad spatial-scale structure of the lithosphere, and not really for particular local details or sharp variations. Both inherently due to S waves, and because of the selected profile projections and widths. This should be declared clearly in the ms. This is especially important as the comparison with earlier results using inherently higher-resolution data or/and methods needs to reflect the ability and limitations of the current imaging approach. In other words, the "discrepancies" found in this manuscript are primarily due to the resolution ability difference with those methods, which is fine and should be simply said it is so.

2. The Introduction section is too succinct. While the paper title suggests results are on the greater Alpine area, the introduction (and the discussion) is almost only on the Eastern Alps. Moreover, references to earlier results from S-to-P converted wave studies are completely missing, including those co-authored by the first author of this manuscript (e.g. Geissler et al. 2010). On line 36-37, the sentence "Most controlled source... of the European plate." is debated, as you also know; moreover, it depends on longitude which direction of subduction is predominant in the results. Line 44 should also list the EASI experiment.

3. The Data and Method section is way too succinct and does not provide all details that would allow reproducing these results or to apply them elsewhere. The number of used stations should be stated, together with the time frame of data collection. Same goes for

the teleseismic earthquake catalogue, and whether a maximum magnitude was also used for event selection? Line 59 should give the exact time window for signal and for noise, as well as what is meant under "corrected for the sign of onset": is it absolute amplitude, or all polarities the same? Line 62 should mention over which time window was the P component amplitude measured, and how (maximum?). Why is this measured from -50 to -10 s when most of the signal is closer to the S arrival than -10 s? The subsequent sentence (L63-64) requires a reference. Line 70 should specify for which phase and how the moveout correction is applied.

4. The authors aim at using as little as possible processing steps, including no filtering. This is very interesting, and calls for an explanation how different seismic sensors are treated? These are known to have different lower corner frequencies (120s, 60s, 30s, and also other values) -- can they all be used? If not, what sensors or frequencies qualify for this method to be applied on their data? Alternatively, is it in the nature of teleseismic S waves that one can stack these signals seemingly so easily? If so, what is their frequency content and are they similar for events at different distance and different magnitude -- can you please illustrate that with suitable figures? Finally, the disadvantages of no filtering and no deconvolution should be also stated.

5. One source of possible artifact of waveform amplitudes is the rotation to the LQT coordinate system, which depends on the ray geometry and therefore the velocity at the surface. Applying the iasp91 model is therefore a simplification, as there are (locally deep) sedimentary basins throughout the study area. Please estimate the uncertainty due to the use of iasp91 instead of local velocity values in this processing step.

6. Onset time selection. Line 92 says that "the arrival times of the seismic signals must be picked ... at the beginning of the signal...". However, this onset time is frequency dependent, and stacking waveforms with different frequency content (either due to sensor characteristics or/and to earthquake spectra) is therefore error-prone. Here the authors chose not to apply any filtering, yet it remains to be shown that individual waveforms contributing to these stacks have similar dominant frequencies (see also comment 4 above), so that a "stack-onset" makes sense. See also point 8 below on noise levels.

7. The interpretation of depths can only be done very approximately, for two reasons: (a) the corresponding waveform stack is done at a constant depth of 50 km across the whole region for the Moho, and (b) the time-to-depth conversion uses a 1D global velocity model (iasp91). The results themselves showing Moho depths ranging from ca. 20 to ca. 65 km, one can easily see the deviations that apply horizontally (for a) and respectively vertically (for b). This limitation should be spelled out in the manuscript, possibly at the same place where the method's applicability is discussed (see comment 1 above).

Even more important would be to estimate the depth uncertainty of this method, primarily by the use of a 1D velocity model for time-to-depth conversion, and also due to the uncertainty in picking onsets on the waveform stacks + incoherency between neighbouring stacks simplified in the dotted lines added by hand over the general trend. Depth differences compared to previous results that are less than this uncertainty should not be reported as surprising or different.

8. Negative velocity gradients (NVGs) are mentioned on Line 97 as red signals, and these are present on most profiles below the Moho peak. What is not clear is the extent to which these red signals are interpretable: some of them are clear and high amplitude, some others are poorly defined or even opposite polarity. What is the threshold limit, the noise level, above which an NVG (or Moho) can be interpreted? Line 170-171 says amplitudes of ~10% and <4% are clear for Moho resp. NVG, where is the noise? At 1%? Can the onsets on the qualifying traces be highlighted with a dot to better support the interpretation of NVGs?

On many occasions the onsets are picked for a wave of tiny amplitude... Also, Line 97 says these NVGs are sharp or gradual velocity change -- could you please quantify? How sharp they can be with respect to the waveforms we see for the Moho? How gradual can they be considering the waves' frequency content?

INTERPRETATION CONCERNS WORTH RECONSIDERING OR REVISING

-Line 113-115: the Adriatic Moho "rise" on profiles 2 to 6 is overinterpreted and should be removed based on the following justification. On profile 2, not all onsets are followed by the dotted interpretation line. On profile 3, there are 0.6° without data, and the next stack (at 45.2°N) does not show the Moho onset where it is picked. Profile 4 has an even larger

gap and no data where the "rise" is drawn. Profile 5 also suffers from the data gap and the dotted line does not follow the onsets closely, the Adria Moho onsets reach as deep as the European one! Profile 6 has more than 1.0° data gap and therefore the interpretation of the rise is not supported.

-Line125: results here show 65 km depth, though Spada reaches clearly less! Please elaborate on the depth uncertainties of your method, as proposed under comment 7.

-Line129-133: these observations make sense, and they seem to concur with Bruckl et al.'s results on the Pannonian fragment starting already in the Eastern Alps; it is worth citing this here.

-Line139-145 on the MHZ: the "jump" and the "suddenness" are not justified by the data presented here and should be taken with a pinch of salt or two, see the following arguments. First, the presented profile is 2.0° wide, along which the MHZ changes its latitude significantly (~0.5°), and so quite a bit of lateral variations are projected (smeared) onto the profile. Second, 2 bins are lacking from the profile, making a gap of at least 0.4°, so no sharp change can be resolved in this area. Third, on Figure 11A, the MHZ is shown to separate the "Eu Moho" from the "Pa Moho" while it separates ALCAPA and Tisza-Dacia blocks. Fourth, your own comment on velocities (Lines 143-144). Fifth, all previous investigations, from the earlier active seismic results (oil industry and research profiles such as CELEBRATION and those compiled in Horvath et al.) to more recent local RF studies and ANT studies (e.g. Szanyi et al. 2021) show there is no significant jump of Moho depth across the MHZ. The approach presented here is resolving much coarser structures and therefore the interpretation of a large offset across a sharp fault does not stand.

-NVG interpretations. As alluded to above, the NVG onsets are less clear and the interpretations seem to be very subjective. (This is also reflected in the author's own statement in the Supplementary Material text, paragraph 2, last sentence -- why are these signals less correlated?)

For example, on profile 1, the northernmost 4 stacks do not have an onset where it is interpreted to have one with the gray lines; stacks number 7, 9 and 11 from north (left) have blue signals instead of red. So for profile 1, the NVG is not supported or at least not clear at all from the data. Profile 2 has numerous stacks where the gray lines are not over the red wave onset (around 47°N). Profile 3 also has a few stacks that are incoherent with the gray lines. On profile 4 the gray line interprets 10 stacks, out of which at least 4 have no clear red wave onset. Profile 6 also includes stacks not fitting the interpreted picture. Line 186 says there is no significant NVG in profile 8, but one could draw it, from -7s at 50°N to -13s at 48°N! Profile 11 shows an E-dipping NVG, but a nearly symmetrical one can be drawn, from -12s at 12°E to -7s at 18°E! Profile 12 has no NVG drawn, but one could also draw one there...

I hope these examples demonstrate the subjectivity of interpreting NVGs. Maybe it is more constructive to think of alternatives? For example, my visual impression is that sub-Moho negative amplitudes are higher on the European plate than on the Adriatic plate. Could this be checked and quantified? For example, above a set noise level (e.g. 1% amplitude?), one could show the max.amplitude of sub-Moho local minima on a map? Maybe even further details will show up.

-Under "Comparison with earlier results" the new results are compared with Heteyni et al 2018 (abbreviated here as H2018) and the comparison could be a bit more complete. Line 222 says that the new results do not show that the Adriatic Moho reaches 70 km depth. The H2018 results argue for a broad vertical gradient zone, from 50 to 70 km depth, and use proper migration with multiples, while the new results do not; this could be mentioned. The general agreement (Line 223) is actually encouraging. Line 226-227 says "Our data do not support the postulate of Hetényi et al. (2018b) that the Adriatic Moho in the Eastern Alps dips northward underneath the European Moho." The explanation is relatively easy, as H2018 used a narrower profile width, while the new results here cover 1.0° width, from 12.5 to 13.5°E; and since significant lateral variations are now known in this part of the Alps, it is therefore not surprising that you find a symmetric shape, as in Spada et al. results, simply because many rays sampling farther west are included.

-still under Comparison, the Paffrath et al. results are shown. First, these are still in review, and not yet final. Second, it is extremely hard to see their results on your Figure 17, the contour label texts of the tomography are very small and sometimes broken over two lines. Please improve this figure. Finally, the tomography results show a major gap of the high-velocity anomaly at 8.5°E. This should be mentioned and discussed in the interpretation, see e.g. Line 238. Line 247 refers to the Tauern Window, please report its location above the profile on Figure 17B.

-at the comparison with Bruckl et al. 2010 profile at 13°E, the original publication shows line drawings for plates, but also the location where original Moho depth data is taken from the map of Behm et al. 2007. It would be better to cite the original data, and show where that data shows Moho. The gap present in that dataset would allow a bit broader range of interpretations. In the same paragraph, comparison to Kummerow et al. is made, and qualified "fair". Locally >5 km depth difference is present with their results, although this is not surprising, as TRANSALP was located farther to the West than this profile, please mention this. In the same paragraph, comparison with Spada et al. 2013 is made (Figure 18B). The interpretation of this profile without Spada et al.'s results overlaid would actually be more difficult. Moreover, Spada et al.'s Moho depth is the same shape but clearly shallower than the onset of blue phases. Is it an indication of noise level, to be distinguished from onset times? Or some other effect?

-figure 20B, comparison with Grad et al. 2009a: here again, the Moho by Grad et al. is systematically shallower than the newly presented results. Why? (See detailed question above). And why is the eastward deepening more pronounced (L289)?

SOME MINOR COMMENTS

-Line 17: insert "depth" after "Moho"

-L19: "mantle trench" does not really make sense, if anything it is a crustal trench but even that reads odd

-L19-20: "where the Eurasian lithosphere is subducted below the Adriatic lithosphere": please specify the region where this is meant -- it seems like this is in the Western to Central Alps? (In the Eastern Alps it is contested, as you also say it, so without geographical specification this sentence is bizarre in the abstract)

-L21: "updoming" suggests there is dynamics, a movement of the Moho, while the structure you image is static. Can you use another word? See also Line 302 and 329.

-L22: "into" \square "to". By the way, this shallower Moho is already part of the Pannonian fragment according to Bruckl et al. See also Line 329.

-L23-24: "negative P-wave velocity gradient": indicate the source of this information

-L26: please replace commas with dashes in "Alpine, Carpathian, Pannonian"

-L121: Yuan et al. 1997 actually says "The Indian lower lithosphere is considered to underthrust the Asian crust to the Banggong suture [Ni and Barazangi, 1983; Beghoul et al., 1993; Owens and Zandt, 1997], which is beyond the present INDEPTH transect." so it may be more appropriate to cite one of these references? By the way, reading lower lithosphere here, and checking the Nabelek paper as well, "Indian crust" on line 121 would be better as "Indian lower crust and mantle" or "Indian lower lithosphere".

-L135 and section on the "new" Moho depression. It is the main result of the manuscript, and a comparison with earlier Moho maps, e.g. from Grad et al., or earlier S-to-P RF results, would be worth adding to the discussion.

-L148: please mark this longitude on top of the figure as an important boundary with Eu and Ad labels on either side.

-L149: does the eastern end of this profile reach the Pannonian? From the other (N-S) profiles it seems so; please mark that boundary as well.

-L151: less than 20 km depth seems extreme, no previous study reported such thin crust in the Pannonian. Maybe mention depth uncertainties here due to the applied velocity model with respect to reality.

-L160: please mark this longitude on top of the figure as an important boundary with Eu and Ad labels on either side.

-L176: "onset" instead of "arrival" seems to be a better choice following your earlier argument. Please append "in this time window" at the end of the sentence.

-L193: citing some of the MT results for thin lithosphere in the Pannonian Basin would strengthen this statement.

-L194: remove reference to 10 km step across the MHZ (see interpretation comment above).

-L199-200: because of the gray overlay one cannot see the waveforms to check the interpretation of the NVG... see also general reservation on NVG above.

-L203: see comment on possibility to draw symmetric NVG on profile 11.

-L204: see comment on possibility to draw NVGs on profile 12.

-L205: how steep is this "relatively steep dipping structure", considering that the profiles are ca. 9-fold exaggerated vertically? My estimate: about or less than 10° .

-L211: it may be worth adding that it is in the vicinity of the contact with Adria, but not crossing it!

-L231: replace "data, and those of" by "data, similarly to those of" (to clarify the sentence)

-L245: the word "rise" (two occurrences) seems to suggest there is dynamic movement, is there a better expression?

-L267: Hetenyi et al 2015 have no SW-NE profile, their three profiles are NW-SE. Please correct.

-L270 and 275: Handy et al reference year should be 2021?

-L283: "although there are some differences in details" is very gentle, as locally there are >10km differences (lon.9°E, jump at 14°E). What is the point showing this comparison of the differences are not discussed?

-L299-300: important along-strike changes in Moho topography were also highlighted in the EASI results (H2018), worth citing?

-L304: shallower Moho, continuing to the Pannonian, already proposed by Bruckl et al., worth citing?

-L304-305: this result is contested and should be removed, or reworked to one that is supported by data and data coverage.

-L306: "in front of" would be better as "north of"?

-L315: "rapid jump" seems to refer to a motion, but the image is static. See also L 334-335. "Sudden depth change"? Also, would it be worth comparing this result with one of the CELEBRATION lines?

-L323-324: "help to increase the uniqueness" -- in this study there is no demonstration of uniqueness... it could be done (by bootstrapping?)

-L332: Mroczek et al. seem to claim the same, worth citing?

-L336: mantle asthenosphere?

-Conclusions Line339-347: as written above: point 1 is not supported by the data and should be removed, point 2 is a good result, point 3 needs to be strengthened.

-L399: "Alparray" □ "AlpArray Seismic Network"

-Figure 7A profile 5: the "Li Moho" is interpreted at an unusually short wavelength over 4 traces, different from other interpreted lines. By simply connecting onsets a much flatter line should be drawn.

-Figure 9A profile 7: the black arrow is very hard to see. The "culmination" in the caption is already the Pannonian fragment discussed by Bruckl, also Mroczek. The "Moho gap" in Spada's work, H2018's work is not at this longitude and should be removed.

-Figure 10A profile 8: the black arrow is very hard to see. Some of this profile (the shallowest part) could be Pannonian Moho, if Bruckl et al's interpretation is followed.

-Figure 15: the overall depth uncertainty should be quantitatively mentioned in the caption.

-Figure 16 the cyan quadrant is missing from the map.

-Figure 19B: the reference below this panel is not Schmid et al. 2020 but Handy et al. 2021, most likely