Response to Reviewers' comments for:

'Moho depths beneath the European Alps: a homogeneously processed map and receiver functions database'

By K. Michailos et al. submitted to ESSD

We wish to thank Anonymous Referees #1 and #2 for their constructive comments. We appreciate the time and effort dedicated to providing your valuable feedback on the manuscript. We provide a point-by-point response below to the comments raised.

The reviewer's comments are given in *blue italic text.*
Our responses are in black text.

<u>Anonymous Referee #1 comments</u>

Referee comment on "Moho depths beneath the European Alps: a homogeneously processed map and receiver functions database" by Konstantinos Michailos et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2022-397-RC1, 2023

Manuscript "Moho depths beneath the European Alps: a homogeneously processed map and receiver functions database" presents an exhaustive database of P-wave receiver functions computed for teleseismic earthquakes recorded by multiple networks of seismic stations across Europe, with a central focus on the AlpArray Seismic Network. The major highlights of the manuscript are: (a) homogeneous processing scheme to compute RFs, which includes multiple quality and signal-to-noise checks on the waveforms, (b) time-todepth migration in 3D, (c) Moho map created from the manually picking the discontinuity signal, (d) open avaliability of the RFs, codes for computation of RFs and the Moho map results. Overall, it is a well written manuscript and documents each step in fair amount of detail. I have a few observations regarding the data availability, quality checks applied and the results presented.

We thank Referee #1 for taking the time to provide this constructive review.

1. I downloaded the radial and tangential RFs from the repository, provided with the manuscript. However, the 3-component waveforms from which the RFs have been calculated are not provided. The RF waveforms have signal from 0 to 70 s, with the time marking starting ~30 s before the P-arrival time. Only 40 s of the P-to-S converted signal is provided. This is sufficient to observe crustal phases, but not upper mantle phases. Moreover, additional information about data processing e.g. Gaussian filter parameter (if any) applied to the waveforms during the RF computation is not provided. This restricts the scope of use of the data by other users. In my opinion if data is suppose to be made open and available to the community, it should be done so that (a) the RF computation can be re-done by an independent user, and (b) information/analysis, other than the one presented in this manuscript can be extractable/done. Providing the 3-component waveforms for 45 s before the P-wave arrival and 120 s after, will allow users to compute P-RFs using other algorithms, different frequency content, vary the number of iterations (other than 200) and also study structure beyond the crust.

We thank Referee #1 for downloading the data and going through them in detail. We agree with the referee's suggestion and we will make the 3-component (Z-N-E) waveforms also freely available in the same Zenodo repository where we share the RF traces. The 3-component (Z-N-E) waveforms provided contain the waveforms 120s before and after the P-wave arrival. We have updated the text in the availability section.

Please note that some of the data (i.e., PACASE data), as stated in the data availability section of the manuscript, are still under embargo and therefore we do not include the ZNE data from this network.

We confirm there has been no Gaussian filter applied, we have employed the iterative time-domain deconvolution method. Please refer to the methods section for details on the data processing.

2. An event list of all earthquakes used and the detailed information of all the stations (e.g. lat, long, elevation, instrument type and, if possible, response files) should be provided, so as to enable the user to perform time-to-depth migration for each trace using different velocity structures.

We have added two additional files in the Zenodo repository that include 1) the details of the teleseismic events and 2) the list of all the seismic stations with ZNE waveform data that we used (both files are in CSV format). The response files are easily accessible via the same website links that are provided for the continuous seismic data in the data availability section. We recommend that the metadata (response files) are downloaded directly from the data repositories in order to keep consistency and eventual future corrections.

We have also updated the text in the data availability section.

3. The CCP stacks uses only the P-to-S converted phases for ascertaining the Moho. These can have significant dependence on the velocity structure. Using the converted phases would reduce such dependence to a large extent.

We agree with the referee's comment here and assume that the comment refers to using multiples as additional converted phases. This has been developed and implemented for 2D profiles with their own RF-deduced velocity structures, however, in 3D it is more complex (cannot be easily extended over larger areas) and we consider that such additional work is outside the scope of this paper. We have stated that we only use Ps conversions in the discussion and acknowledge it as one of the limitations of this work. We added the following sentence in the discussion to suggest including more converted phases in future work. For full 3D Vs inversion of RFs, we refer to Colavitti et al. 2022.

"Extending the analysis by including the crustal multiples (e.g., PpSs, PpPs) is a potential future direction that would allow an estimate of Moho depth without assuming a fixed Vp/Vs ratio over local to regional scales. For fully 3D Vs structure inversion based on RFs, we refer to Colavitti et al. 2022."

4. The CCPs presented in this manuscript serve two purposes (as I see it): (a) provides confidence to the data quality and uniformity of analysis (b) re-confirms most of the Moho structure observed from previous studies. Aa this manuscript is more of a data mine article, I *believe that the discussion presented on the results is acceptable. Given the quality of the analysis and the results, I would have been tempted to discuss the results (variation in the structure) in greater detail and also correlate it to the geology/other geophysical observations.*

We have extended the discussion of the results and added more details along with a new map (see relevant comments from referee 2) that shows the differences between the Moho depths estimated in our study and previous ones.

5. A few minor points:
(i) I did not entirely follow the filtering scheme of the RFs. If the data is filtered between 0.05 and 1 Hz (L130), why perform a high pass filter at 1 Hz (L125)?

We thank you for bringing this to our attention. The given high pass filter is only applied during the quality control step that uses the STA/LTA algorithm to a copy of the trace that is later used for the receiver function calculations. In other words, the STA/LTA quality control is a completely separate step in our processing workflow that does not affect the traces that are used for the receiver function calculations.
To ensure and double-check, however, that this choice does not affect our results, we have re-calculated all our results using a low-pass filter (<1Hz) this time for the STA/LTA quality control step. As consequence, we have updated all the figures and maps based on the new receiver functions. All the images and patterns have remained practically unchanged. We choose to maintain the results with the low-pass filter in our resubmitted version of the manuscript.

(ii) L129 - What is meant by "the effect of the signal"?

We have removed this confusing phrase, which is actually not needed.

(iii) Why is the time referencing of the RFs from -30 s of the P-wave arrival time and not at the point of the largest amplitude arrival?

We use -30 s from the P-wave arrival time to (1) allow for a sufficiently large time window for the taper in order to avoid signal attenuation and (2) for the quality control steps we use that section of the traces for both STA/LTA and peak/background rms ratio quality control steps. We acknowledge that including 30 s before the P wave arrival can potentially introduce additional noise compared to only including a few seconds before the P wave arrival. However, if SNR is high this is not expected to make a significant difference to the results, In our case given that we apply very strict quality criteria we ensure that we only maintain the high-quality RFs.

(iv) L100 – Iterative deconvolution used for the RF calculation does not "deconvolve the vertical component seismogram". It follows a convolution of the updated spike train with the vertical to match the radial component.

We have updated the text and removed the given sentence.

(v) the CCP is done as a 3D migration, but the 3D models are not presented. This would reveal the influence of the 3D model in the final Moho maps obtained.

As we use the EPCrust model, a well-known and digitally available model of the European crust introduced by Molinari & Morelli (2011), we feel it is not necessary to replicate this model here. Interested readers can check the model in the original publication and on the related website (http://eurorem.bo.ingv.it/EPcrust_solar/index.html).

(vi) L300 – why are uncertainties “difficult to assess”?

We have rephrased the text to elaborate and make clearer why the uncertainties are difficult to assess.

Anonymous Referee #2 comments

The present manuscript (and dataset) provides a consistently processed database of receiver functions as well as a crustal thickness map of the Alpine region that is a product of the AlpArray initiative. This large dataset is the result of considerable effort in data acquisition and processing, and will be a very useful and widely utilized resource for the community. Overall, the manuscript is reasonably well written, and as it is mainly intended as a data description article it makes sense that the authors refrained from going into interpreting the results. However, some parts of how the presented Moho map was obtained deserve a clearer explanation and description, and some choices on what data are made available should also maybe be reconsidered, so that moderate revisions will be necessary. I will outline my main points below, followed by more specific comments by line number.

We thank Referee #2 for taking the time to provide this constructive review.

General Comments:

1. The main product provided here is the crustal thickness map for the Alpine region, which was manually picked on a series of CCP stacked receiver function profiles. While the receiver function processing and quality checking procedure is nicely and comprehensively described, there is no detail at all on how the manual picks on the CCP profiles were retrieved. This part of the analysis is a complete blackbox at the moment. I recommend to add:

- *A description on how manual picking was performed. What guided finding the right anomaly in the profiles? Was the pick set in the center of the anomaly or onto the maximum amplitude? What was the procedure in case of a double anomaly? Was any interpolation performed for sections where the Moho was not really visible, etc.*

We have expanded the description of how the manual picking of the Moho depths is performed. See the updated version of the first paragraph of section 4.3.

- *These descriptions could be accompanied by one or two examples where the set picks are shown on top of the CCP profile...at the very least, they could be added to the current Figures 7 or 9 (at the moment, as far as I can see, the manual picks made in this study are not shown anywhere in the article)*

We agree with the referee here and we have added manual Moho depth picks for the cross-section in Figure 7.

We have not added manual picks in Figure 9 because we think this would make the plot too busy and we think that the comparison between the results obtained using iasp91 and EPcrust is better represented as it is. We also note that all the manual picks are available in the electronic supplement (please refer to the following **link** for all our manual picks).

• *It is also mentioned, without further explanation, that these manual picks were labeled as either certain or uncertain. Based on what was this labeling performed, again it would be interesting to see examples*

All the manual picks are shown in the supplement (**link**) of the preprint as is also stated in our previous version of the manuscript. We made sure to state this clearer in the main text. We chose to add these profiles (along with their picks) in the supplement, and not in the appendix, because of the size of the images.

2. The other reviewer had a number of comments about the provided datasets, with which I wholeheartedly agree. It would make a lot of sense to also provide the raw, cut three-component waveforms, so that other researchers can apply different rotation (e.g. 3D, into LQT system) and/or deconvolution approaches.

We agree with the referee's suggestion and we will make the 3-component (Z-N-E) waveforms also freely available in the Zenodo repository. Further details are provided in our response to Referee #1.

3. The comparison to previously existing compilations of crustal thickness in the Alpine area should be extended, at the moment there is only a quite brief section on this, and the comparisons in Figures 7 and 9 are only along selected profiles, do not show the picks of the present study, and make it difficult to appreciate the differences due to the large scale of the cross sections. I would recommend to compile a map view figure that shows absolute differences between the new crustal thickness map and one or several pre-AlpArray ones.

We have extended the comparison to previous studies by adding our Moho depth estimates in Figure 7. We have not added manual picks in Figure 9 (refer to our response above for more details).
We have added two plots in the supporting material (A6 and A7) that compare our Moho depth estimates with those of Grad and Tiira 2009 and those of Spada et al 2013. We have also added the following paragraph in the beginning of section 5.1 of the manuscript:

“In general, our Moho estimates are relatively shallower in the European plate and relatively deeper in the Adria plate when compared to the two previous studies (Figure A6; Grad and Tiira, 2009; Spada et al., 2013). The main differences are located along the tectonic plate boundaries, especially along the Ivrea-Verbano Zone where estimates around the protruding Ivrea-Geophysical body cause large differences. This is mostly due to differences in the coverage of seismic stations and the respective amount of interpolation. Denser arrays can provide a higher local resolution for the Moho (e.g., 5 km spacing of IvreaArray; Scarponi et al. 2021). When all quantified Moho depth differences are considered (Fig. A7), our Moho depth estimates are on average 0.6 km deeper than those of Spada et al. (2013), and 0.4 km shallower than those of Grad and Tiira (2009). The standard deviation of the difference distribution - which includes the high values mentioned above - is respectively 7.6 and 6.4 km. Overall, there is a good general similarity to previous studies in terms of Moho depth, with some areas mapped at better spatial ray coverage thanks to the more densely spaced seismic array."

Specific comments:
l.1: Unnecessary first sentence; this may fit into the Introduction but not into an abstract

We have removed this sentence from the abstract.

l.14 (and elsewhere throughout the manuscript): why say crustal structure when you mean crustal thickness?

We have modified the given parts of the manuscript accordingly.

ll.19-32: this very basic introduction to the Moho is not necessary and makes the Intro chapter rather unstructured. Better leave out

The given paragraph presents some brief supporting background information that we think is useful to be established in the introduction of this manuscript. However, we can see where the referee’s comment is coming from and so we have rephrased parts of this paragraph to make it more compact and easier to follow.

ll.62-67: that is comparing apples and oranges. Tomography studies yield crustal velocity structure, whereas RFs give the crustal thickness (and NOT crustal structure, see above)...this means that the two methods are largely complementary

We have modified the text accordingly to implement this comment and made sure no comparison is made between tomography and RFs studies.

ll.79-88: quite repetitive, maybe better to present this basic network information in a small Table?

We have modified the text to make it more concise and less repetitive.

l.94: as well as having: sounds clumsy, please reformulate

We have reformulated the sentence. The new sentence looks like this now:

“We exclude earthquakes with large magnitudes M > 8.5 because their waveforms have long and complex source-time functions that can also be contaminated with signals from large aftershocks. In particular, we only removed the M=8.6 earthquake that took place off the west coast of northern Sumatra in April 2012 and was recorded by the CIFALPS seismic network.”

l.108: maybe mention how the orientation was determined for the ocean-bottom instruments?

The orientation corrections calculations are based on the information in the station metadata as stated in Line 107.
We have added more details to the given sentence to provide more information on the orientation corrections.

“...based on the alignment stated in the station metadata (i.e., azimuth and dip values of each channel stored in the inventory file).

l.130: this means that the last quality criterion (STA/LTA) was determined in a frequency range (f>1 Hz) that has basically no overlap with the one that is finally used for deriving the RFs (0.01-1 Hz). This seems like a rather strange choice to me.

Thank you for bringing this to our attention. Please refer to comment 5(i) of referee # 1 for the response to this.

l.146: how is this amplitude range chosen?

The amplitude range we use (0.05 - 0.8) was chosen based on previous experience on several large datasets, <0.05 would be too small, not adding to the signal (roughly in the noise), while >0.8 seems very large and usually does not happen when everything works well with the deconvolution. We have added the reference in the text that we base this choice on the amplitude range.

l.155: Unnecessary to go back to the very first RF studies here

We have removed that sentence.

l.158: I thought that the first uses of CCP stacking in RF analysis was by others...not sure who was first, but studies like Yuan et al. (1997) or Kosarev et al. (1999) already showed CCP stacked RFs

We have added the mentioned studies, thanks for pointing them out.

ll.170-175: circular text flow

We have modified the given part of the text. Here is the updated text:

"We opt to use EPcrust instead of a global 1D (iasp91 velocity model; Kennett1991) in order to include the local structure variations within the study area. In regions with thick sedimentary layers, such as the Pannonian Basin or Po Plain, the absence of a sedimentary layer in iasp91 model provides phase arrival times with significant time shifts. At the same time, higher-resolution both Vp and Vs models are only locally or sub-regionally available in the Alpine region but none of these models cover the entire study area. Therefore, we have chosen to use EPcrust instead of compiling an ad-hoc composite velocity model from various locally available models to ensure internally consistent results. "

l.174: grid spacing [...] consists of three layers: no, the model does! Please reformulate

We have changed this. Here is the modified sentence:

"EPcrust has a grid spacing of 0.5° in latitude and longitude and consists of three layers in depth (i.e., sedimentary, upper crust, and lower crust)."

l.175: Was some kind of half space added for the region below the EPcrust model (representing the mantle)?

For the depths below EPcrust (representing the mantle) and up to 120 km depth, we use the velocity values of the lower crust from EPcrust. We do this because imposing a Moho in the velocity model can make an artifact on the data: if the Moho in the data is deeper, the associated P-to-S conversion would be migrated with mantle velocities. Therefore we have used the lowermost crustal v-value as a half-space. This is fine as long as we focus on the Moho P-to-S conversion only.
We added some explanation to section 3.2 on this procedure

l.178 (and later): ray trace paths → ray paths

We have changed this throughout the text to keep it consistent.

l.197: look (-s)

Thanks for noticing this. We have changed it.

ll. 198/199: explain in more detail or provide a reference!

We have added a reference.

l.203: I would not call the ray coverage shown in Figure 5 great. There is a gap of 90-100 degrees in southern directions, and at least one station also has very few RFs from westerly directions

We have modified the text to accommodate this.

l.204: maybe use amplitude instead of strength?

We have updated this.

l.213: this was done using the EPcrust velocity model mentioned before?

Yes, this was done using the EPcrust velocity model mentioned before. We have added a sentence to highlight this.

l.216: maybe supply what the range of horizontal offsets from the stations is...this can then be compared with station spacing

We have modified the given paragraph:

"To assess the coverage, we compare the station spacing with the horizontal offset of the piercing points. The lateral offset between the station location and the piercing points at the Moho is roughly half the Moho depth. Hence for a Moho ranging from ca. 22-25 km depth (beneath the Pannonian Basin) to ca. 55-60 km (beneath the Alps) the corresponding range of horizontal offsets of the piercing points is about 11-30 km. Since the nominal station spacing is 50 km, over most of the network the piercing points of neighbouring stations are not significantly overlapping. We calculate the piercing points plotted in Figure 6 at a depth of 35 km, a compromise between areas of expected shallower and deeper Moho depths. The piercing points are well distributed and do not leave major gaps in coverage - with the exception of some seismic stations in the Po basin, in the Pannonian basin and the OBS stations in the Ligurian Sea where the coverage is less dense due to either short operational times or the strong quality criteria applied."

l.224: I fail to understand what direction exactly East-East-Northeast is supposed to stand for

Thanks for spotting this. It was meant to be East-northeast. We have updated the text.

l.226: distinguish from what?

Thanks, we have changed "distinguish" to "interpret".

l.228: Hard to compare in the profiles because this study's picks are not supplied, and scale is rather large. Maybe better to plot residuals somehow? (see General Comment #3)

We have added our manual Moho depth picks in Figure 7.

l.241: Needs more detail on how picking was performed, and what the criteria for certain/uncertain picks are (see General Comment #1)

We have added more details in this part of the text and also highlighted that all the manual picks can be found in the supplement of the manuscript.

l.256: just semantics, but aren't routines always systematic?

We have modified this. Here is the new sentence:

"Starting from continuous data, we apply a series of systematic processing steps (see Figure 3) and calculate an unprecedented number of high-quality RF…"

ll.260-262: This should be analyzed in much more detail, and I would appreciate some kind of map view residual plot compared to at least one previous study (see General Comment #3)

We have added two plots in the supporting material (A6 and A7) that compare our Moho depth estimates with those of Grad and Tiira 2009 and those of Spada et al 2013. See General comment #3 for more details.

l.271: how was the presence of a double signal (overlapping Mohos) treated in the present study? Was only the shallower signal picked, were both picked, or what? The manual picking procedure needs some more explanation!

Thanks for bringing this up. In cases of double signals, we choose to pick the shallower signal only. We have added a sentence to describe this in Line 241 of the preprint.

"For examples where the signal is double (overlapping Moho discontinuities), we chose to pick the shallower signal. Double signals might be associated with underthrusted lower crustal slivers or subduction (e.g. Spada et al. 2012; Mroczek et al. 2023, Hetenyi et al. 2018), therefore picking the shallower Moho ensures the bottom of a continuous crustal stack is picked."

l.275: 10 km is quite substantial

We have removed the word "less" from the given sentence.

l.300/301: quality and consistency of the manual Moho picks...these are a complete blackbox as is, no explanation of the procedure is given and no examples are shown

We have addressed this in previous comments raised by the referee. Also, we have rephrased this part of the text given a comment from referee 1.

l.307: Would a map of mists between dierent models not be a more straightforward way of identifying critical regions? Also, as your Moho picks have labels for certain/uncertain, can the spatial distribution of these labels be shown?

It is not entirely clear what the referee's comment refers to in line 307 of the preprint. The end of the discussion has been updated given other comments from the referees. Also, the spatial distribution of the certain/uncertain picks is shown in Figure 8a (see legend).

ll.309-311: Clumsily formulated, please change

We have re-formulated this.

"We start from continuous seismic waveform data, that cover the broader European Alps region, calculate RFs and perform 3D time-to-depth migration calculations in a spherical coordinate system."

l.312: These meetings are not relevant in the Conclusions of an article

We have removed the sentence.

Appendix:

In the text says that three figures are contained, then the text describes four, whereas the actual content is five

Thanks for noticing this we have updated the text.

Figures:

Most figures: I do not understand why the authors use color scales with (often very few) constant colors for rather large ranges of values. Using a continuous color scale would, in many cases, give more detailed information using the same plot

We have updated figures 1, 2, and 4 using continuous color scales.

Figure 2: Why plot a line for the close cut-off distance (30 degrees) but not for the far one? And why choose a color scale for depth that assigns constant color for 70 km intervals, instead of taking a continuous scale (see above)?

We have updated Figure 2.

Figures 4/5: is there any logic according to which the sequence of the highlighted stations (a through d) was chosen? Naming according to position (e.g. start with a in the W and move E, or something similar) would seem more straightforward

We have updated the order of the Receiver function stacks plots.

Figure 7: It would also be interesting to see where the picks performed in this study are situated

We have added manual Moho depth picks in Figure 7.

Figure A2: Here you show many many dots that are all on top of each other, thus it is really hard to see much...could you maybe plot point density instead?

We have updated the Figure.

Figure A3b: The absolute number of discarded RFs per station is not that interesting, could you maybe display the proportion of discarded RFs per station (i.e. what percentage of all RFs for that station was discarded)? This would be more of an indicator of data quality and less of data amount

We have updated the map in the appendix to include the percentage of discarded RFs over all the RFs calculated in each seismic site.