Dear Editor, please find enclosed a revised version of our paper, in which we address all the reviewer comments. We have attached a version of the paper with modifications highlighted in red. Most of the figures have been improved compared to the previous version. Additionally, we have corrected an error in Table 2 regarding the modified compositions of the upper crust with varying silica content.

Reviews Solid Earth

We would like to thank Prof. Tony Lowry and an anonymous reviewer for their constructive comments, which helped us to improve the paper. Below, we provide detailed answers to each point and indicate how the revised version of the paper has been modified accordingly.

First review (anonymous):

Review of the manuscript "On crustal composition of the Sardinia-Corsica continental block inferred from receiver functions" by Fabio Cammarano, Henrique Berger Roisenberg, Alessio Conclave, Islam Fadel, and Mark van der Meijde submitted for publications to Solid Earth.

This work presents a newest addition to the investigation of the complex crustal structure of Sardinia-Corsica continental block using the P receiver function method at 21 seismic stations. For this the authors use data from both permanent stations and temporary ones from the project LiSard.

The topic of investigation is interesting, and the methods used are sound and my main critique of the manuscript is mostly about the presentation of the results and the usage of the receiver function method. The language used is appropriate and reads easily but the overall impression is that of the manuscript lacks a bit more depth in the analysis of the results. The authors compare the receiver function modeling results with crustal composition obtained by thermodynamical modeling but a bit more discussion on various influences of complex structure on receiver functions would make the main conclusions more robust.

Bellow, please find the comments that I suggest authors address before the manuscript can be considered for publication.

Comments:

-Introductory section is well written with abundant information about tectonics and previous investigations in the area, but, in my opinion, accompanying Figure 1. is rather poor with part of the text in both images a) and b) not readable. Also, image showing seismic stations used should be plotted in more presentable way. Ttry some of the plotting packages like GMT,

Matplotlib basemap or variety of similar packages. Also, please put in the Figure caption what are red and what are black station markings in the station plot.

The authors should invest a bit of time to fix this as good introductory image will help reader connect conclusions with research aims.

We have modified the right panel of Fig. 1, which now shows a high-resolution topography and bathymetry map with locations of seismic stations, color-coded by network to highlight our in-house LiSard stations and publicly available stations used in this study. We used pygmt to produce this map (the python script is provided for reproducibility). We did not modify the geological map, which is based (and modified) on Malusà et al. 2016

Line 54: change "The average P-wave velocity…" to "The average crustal P-wave velocity…"

Done

- Data and methods section

Lines 99-100: In this sentence It is a bit unclear what is deconvolved from what. Please make it more concise.

There was indeed a typo, and the sentence was unclear. We have revised it.

Table 1: Why is the part of the caption above and part of it bellow the table? I suggest authors correct this and put everything above the table.

Done

Also, it is not clear which of the six groups is part of the 3 provinces mentioned in the caption?

We clarified in the caption which compositions belong to the Variscan batholith, Alpine Corsica and Cenozoic Volcanism of West Sardinia.

Line 178: Links to the software packages should go either in the Code and the data availability part after the main text or in the References section.

We add the links to the data availability section.

Table 3 should be moved to Supporting material as it will clutter the main body of the text without bringing any substantially relevant information that is not already shown in Figure 1.

We believe that Table 3 provides useful information, as it shows the total and selected number of waveforms after two quality-control criteria, giving readers insight into the relative "quality" of the inferred receiver functions. For this reason, we chose to keep it in the main text.

Line 209: "…as indicated in Table 1." The authors probably mean Table 3 not Table 1.

Yes, we thank the reviewer for spotting this error, that we have now corrected.

Line 212: Correct reference from Zhu and H (2000) to Zhu and Kanamori (2000) and do the same in the Reference section.

Done

Line 215: Please provide reference for the statement in this sentence.

We think that the reviewer refers to the statement: "... a variation of 0.1 in the latter causes about 4km change in the crustal thickness." This trade-off was estimated by us, so we have clarified this in the revised version of the paper.

Figure 2 and Figure 3 (and figures S1 – S17) These figures should be made more presentable as currently some of the important details are not visible. The amplitudes of the main Ps arrival and reverberation are not visible at all. I suggest that authors make figures bigger by removing unnecessary text on Y-axis (listing of event) and remove last image showing Back-azimuth for each event and sort both R and T receiver functions by Back-azimuth. Why is currently only T-component sorted by back-az and R not?

In this way figure will be bigger and details clearer.

In the previous version, we used the default SeisPy visualization, which we found generally clear. However, we agree that this format was not optimal for all stations. We have thus followed the reviewer's suggestions to improve the figures. We enlarged the fonts, we removed event names and we amplified the waveforms. The quality of the figures has also been improved, as in the previous version we realized that they were blurred.

P.S. Note that both the R and T components were already sorted by back azimuth. In the R plot, it was listed the event name and in the T plot the back-azimuth for that event.

- Results section

Lines 234 – 235: "...would be only made SV (S-wave in the vertical plane) energy." Wording here is a bit unclear. Please make it more concise and clearer.

We have rephrased this sentence

Figure 2-3 (and S1 – S17) Authors show T-component RFs but are not actively discussing them. TRFs are crucial in interpreting possibly 3D structural complexities (dip, anisotropy, etc.) especially important in H-Kappa stacking and interpretation of these results. Authors should spend some time to at least try to interpret some of the signal on T-component for stations that do not have clear maximum in H-k stacking (e.g. station IV.DGI or IV.AGLI).

We thank the reviewer for the suggestion. We recognize the importance of transverse RFs for understanding dipping structures and anisotropy, and we agree this analysis is highly

valuable. However, as we noted in the manuscript, this analysis is the subject of future (and ongoing) work. We have invested time in understanding why H-K stacking proved complex for some stations, often due to heterogeneity, dipping structures, and anisotropy. To disentangle the anisotropic component, we conducted a harmonic decomposition. However, this analysis indicated not-significant variations, particularly in the Vp/Vs ratio, consistent with expected results (see, for example, results from the H-k-c stacking by Li et al., 2019, JGR). Additionally, harmonic decomposition was only feasible when a statistically significant number of high-quality waveforms were available. While analyzing the transverse component is not in our goals, we have added a sentence on our observations from H-K stacking when attempting to isolate anisotropy.

Overall, discussion about possible influences on interpretation of RFs H-k stacking is thin and should be done more thoroughly as conclusions are based on these results that could be overinterpreted.

We agree that H-k stacking is prone to errors and that a thorough analysis could help. In any case, we are confident that the method is effective in this case. The VP/VS ratio obtained in Sardinia and Corsica align reasonably well with petrological expectations, and the Moho depth is consistent with finding from independent studies. Importantly, the observed Vp/Vs crustal dichotomy in Sardinia provides new insights into crustal composition within this continental microplate. In response to the reviewer's point, we have slightly extended the discussion regarding the reliability of our H-K stacks.

Lines 241 – 242 "…those show a noisy pattern." How is this estimated? How do the authors see that something is noisy and other stations are not? This needs to be more concise.

We have rephrased and explained what we meant by "noisy pattern". We agree with the reviewer that the term 'noisy' was an awkward choice in this context.

Figure 4-5-6-7:

In my opinion there are too many figures for showing H-k stacking. It would be better if the authors show only 4 relevant H-k stackings for stations and the rest can be moved to supporting information. Additionally, if there is a discussion about some the problems at particular stations with H-k that (or those) stations can be showed in separate Figure indicating possibly problems.

Also, text in the Figures and on both axes is small and hard to read.

We have partially implemented the reviewer's suggestion. The original figures have been moved to supporting information, and only the H-K stacked panels remain in the main text. We have also enlarged the font size and labeled each panel with the station name.

Line 260:

Authors are using station UT.011 in discussion and deeming that station as showing intricate features as on the other hand they dismissed that station on Line 241. as noisy? Why use it if it is noisy?

As stated earlier, we have remove the term “noisy” to explain complicated features in the receiver functions. We now state: “The only exception being UT.011 (Figure 2, bottom panel), which has a limited number of records satisfying the quality criteria adopted, and even those show some inconsistency. For instance, reverberations are not always at the same distance and amplitudes on the transverse RF are high.”

Line 281:

“…for station UT.009, the search had to be extended to k = 1.5.” Why it had to be extended to 1.5 when the maximum is at 1.7? There are several such inconsistencies through the manuscript connected with h-k that needs to be dealt with. Why hasn’t all been calculated in the same broad range?

As shown in Fig. 4, for UT.009, the 95% confidence interval extends to values below 1.55, necessitating an adjustment in the parameter search. We reviewed each choice in the H-K search and ensured all were justified and based on specific criteria. We’re unsure which specific inconsistencies the reviewer references, but we checked to ensure each choice is well-supported.

Lines 289-290: “…deeper Moho depth.” wording depth is redundant. Put “…deeper Moho.”

Done

Lines 290 – 291: remove “indicating a substantial Moho depth for both locations.” as it is already stated in the previous sentence.

Done

Lines 294 – 295:

“Additionally, it is worth noting that the CORF station exhibits an indistinct Ps phase, implying the presence of intricate wave propagation characteristics specific to the Central Corsica region.”

Or more likely complex structure.

If we understand well, the reviewer is referring to the fact that the complex structure can be related to observed receiver functions. If this is the case, we think that the reviewer’s point is

well taken. The complex wavefield is indeed likely due to structural complexity, which we discuss later in the manuscript.

Line 307: "...situated at a depth of 111 m..." borehole or underground cave station? Give a couple of words to describe it.

We explicitly say now that the station is located inside a dismissed mine gallery.

Lines 307 - 311: Authors indicate possible problems with this station that may be connected with orientation problems. Please check.

There are no current problems related to orientation with the MN.SENA. A problem with the instrument response was identified by us during the first year of installation (2019) when the station was named IV.SENA. We had a confirmation of this problem with personal communication with personnel of INGV (Dr. Marco Olivieri). The present study uses data from MN.SENA, which started recording in 2021 and does not have these issues.

Lines 312 - 317: In the case of station IV.DGI if only Ps phase was used for H-k stack then the authors should not use the resulting Vp/Vs values from that stacking as a viable results as the uncertainty is too great.

If multiples are not used, there is a complete trade-off between Moho depth and crustal VP/VS, as noted in the text and shown in Fig. 7 (DGI panel). As reported in Table 4, we estimated a broad confidence interval for this station, with VP/VS ranging from 1.38 to 1.78.

Line 341: "Additionally, we have computed, the P-T properties of the upper-, middle- and lower-crust from the global compilation of Rudnick and Gao (Table 2)."

The sentence is a bit misleading as one expects that the authors show P-T properties in Table 2. Correct this please.

We have rephrased it to indicate that Table 2 reports the global compilation of Rudnick and Gao

Line 376: "...average Vs in the eastern part." Figure 10 shows that western side has lower Vs values?

We thank the reviewer for catching this error. Yes, Vs is higher (and not lower).

Line 383: "...slightly from 3.75 to 3.80..." In Figure 11. one can see that these values are approx. 3.85 to 3.90.

Thank you for identifying this discrepancy. We have corrected the values to 3.82 to 3.87.