

Peer Review File

A lower crust shear zone facilitates delamination and continental subduction under the Apennines.



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Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

11th Oct 23

Dear Ms Menichelli,

Your manuscript titled "A lower crust shear zone favors delamination and continental subduction in the Apennines" has now been seen by 3 reviewers, and we include their comments at the end of this message.

The three reviewers agree that this contribution is of importance to the understanding of the Apennines and of continental subduction more broadly. They do however raise a number of compelling points that need to be addressed in relation to the need for more methodological details and quantitative justification of some aspects of the interpretation. They also request that the significance of the work be better explained in a larger context, with additional referencing. In terms of technical issues, the reviewers are requesting resolution tests, data/code availability details, and some forward modelling of receiver functions from the derived Vs model for comparison with their own computed receiver functions. I agree with these suggestions.

We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

J. Kim Welford, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-5919-5303

Joe Aslin
Senior Editor
Communications Earth & Environment

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and also in our style and formatting guide [Communications Earth & Environment formatting guide](https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

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- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

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If a community resource is unavailable, data can be submitted to generalist repositories such as [figshare](https://figshare.com/) or [Dryad Digital Repository](http://datadryad.org/). Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at <http://www.nature.com/authors/policies/availability.html>.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This study focuses on understanding the physical properties and structure of the lithosphere in the central Apennines, with a specific emphasis on a controversial aspect of continental subduction - the delamination process of the crust from the subducting plate. The authors present detailed station-wise 1-D shear wave velocity profiles resulting from the inversion of Receiver Function waveforms and emphasized that a delamination of the continental lithosphere is facilitated by the emergence of a low V_s shear wave velocity zone within the middle-lower crust. This weak zone appears to play a critical role in the process of crustal delamination. Furthermore, the authors identify the presence of a double Moho beneath the Apennines forearc and concluded that the edge of the crustal delamination process is situated also in the forearc region, further to the east than previously anticipated.

This study makes a timely and important contribution to the discussions of the continental subduction beneath the Apennines. The major claims of the paper and its conclusions are novel and will be of interest to others in the field and beyond but however these findings need to be worked out in more solid, clear and quantitative manner. A first concern is the lack of detail in discussion and interpretations of the results. They lack the proper referencing to recent work of others, specifically in terms of placing the results from this study in the context of other works. Other concerns regarding the applied methodology are raised in the following comments. Overall, I found the paper too short to be understood by a broad readership. Given the policy relevance of this work and its important and novel contribution, I think this should be rectified. The paper would benefit from expanding the text, to explain more clearly the work conducted and situate the results in the wider literature.

General issues:

- The deconvolution approach used in the analysis and the type of filters applied should be explained in a couple of sentences and with a cross check for potential processing artefacts on both sides of the Ps arrivals. I am afraid that assigning deconvolution or filtering to the raw waveforms might impose processing artefacts. It is rather preferred to apply a plain waveform stacking of converted waves without deconvolution.
- The isotropic RF waveform at the station CRE shows a very weak positive pulse compared to those of the OSSC and CSNT whereas the corresponding 1D models show a similar Moho depth values at the three locations. Stronger RF pulses would result in stronger Moho sharpness on the 1-D models. It is not clear how these converted phases are weighted during the 1D inversions.
- The authors states "At 74 km a gentle jump below the CDCA may be related to the deeper Adriatic Moho". The CDCA station is located at the central Apennines where a slab window has been clearly imaged in this area by several tomographies in the depth range between 70 km and 250 km (Amato et al., 1993; Piromallo & Morelli, 2003; Rosenbaum et al., 2008; Spakman & Wortel, 2004; Zhao et al., 2016; El-Sharkawy et al., 2020). The isotropic RFs signal at this station does show two strong positive pulses at 5 s and 7.5 s, respectively. On the other hand, the distribution of the Vs below this station shows a mostly constant increase with depth however with no indications for sharp interfaces/transitions except at 74 km. For me this is more reflecting the thickness of the lithosphere beneath the Central Apennines rather than a deeper Moho.
- The 1D Vs models beneath NARO and MPAG stations both show two interfaces at about 25-26 km and 52-54 km depths with a low velocity zone in between. On the hand, the RFs waveforms show only one sharp positive pulse at about 6 s beneath MPAG station and a wide weak positive pulse on the NARO waveform. Also the APEC station shows a sharp RF positive pulse at about 5 s, that is not reflected in the corresponding 1D Vs model. This needs to be clarified?
- Discussing vertical changes in the obtained Vs models required high resolution imaging. What is the vertical resolution of the obtained 1D models? Can you please show a resolution test? Is the low velocity zone in the lower crust well-resolved?
- The Moho topography, yes can be resolved with higher accuracy by receiver function techniques, however the inversion of RF waveforms only is non-unique and thus it cannot conclusively resolve or image variations in absolute shear wave velocities (Vs) with depth. It is suggested that a joint inversion of receiver function and surface wave dispersion measurements to be ideally suited to constrain the Moho topographies and to determine absolute velocities with depth. Is the RjMcMC algorithm allows for a joint inversion? Is it possible to implement the joint inversion (even using previously existing surface wave datasets) with receiver function measurements to constrain the inversion results for the obtained 1D Vs models? How different are they from the current ones?
- Huge number of tomographic studies have imaged the slab window beneath the "Central" Apennines not only the work by reference no. 27. The word "Central" should be added at this line in the text. Please also cite the necessary works – I gave examples in previous comments.
- Low shear weak zone: do you mean low shear wave velocity zone? Please clarify that throughout the text.

- What is the Adriatic LID stand for?
- Fig. 4 shows no green and yellow dots. Do you mean the green and yellow crosses?
- How and on which bases do the authors draw the dashed lines connecting the red and yellow crosses on vertical cross sections of Fig. 4D that represents the Tyrrhenian and Adriatic Moho?
- On top of Fig. 4D, there is an undulating black line – is this representing the topography along the profile? What is the difference to Fig. 4A?
- In the caption of Fig. 4, yellow and green lines should be changed to yellow and green “dashed” lines. Also please change Rfs to RFs.
- Fig. 4D does not show the cross sections of the V_p/V_s ratio, rather the absolute and the relative V_p values. Please correct the caption. Also, it is not clear how the relative velocities have been calculated?
- A Moho map showing the variations in the Moho depth beneath the stations in this area would be good to show.

Reviewer #2 (Remarks to the Author):

The authors presented teleseismic receiver function results for the Apennines. Teleseismic receiver functions were computed for 56 seismic stations using earthquake signals between 2012 and 2020. Harmonic analysis was used to decompose receiver function signals. The decomposed receiver function signals were inverted with a Reversible-jump Markov chain Monte Carlo method for the 1D shear wave velocity model as well as V_p/V_s ratio. Based on the results, the authors suggest a double Moho beneath the forearc. The geological evolution of the region requires additional investigation besides the published literature. However, the manuscript requires significant improvements and should not be accepted in its current form. The following are a couple of major points that the authors need to address.

1. The manuscript is missing data availability and code availability statements, author contributions, and competing interests statements. All of these are required based on the submission guide. It would be difficult to reproduce the results that the authors presented without the data and code availability statements.
2. The uncertainty of the inverted V_s is very high (Figure 4B) and V_p/V_s ratios are poorly constrained (Supplementary Material). V_p values would have large uncertainties as well. Are the lateral velocity changes in Figure 4D larger than the uncertainties?
3. The authors used the averaged S-wave velocities for interpretation. You can compute predicted receiver functions with these S-wave velocity profiles. Do the predicted receiver functions match the observed receiver functions?
4. Yellow triangles in Figure 1 need to be labeled. Otherwise, the readers cannot easily find out where the stations used in Supplementary Material are located. Please specify the x- and y-axis labels for the Geom. coverage panel in Figure 3. What does the green shaded area mean in Figure 4B? The black dots in Figure 4D are earthquakes, right?

Reviewer #3 (Remarks to the Author):

The authors compute S-wave velocity model by inversion of receiver function waveforms to provide additional information of the complex lithospheric structure of the central Apennines where convergence of the Tyrrhenian and Adrian microplates occurs. The paper provides new information about the Vs structure of the lithosphere across the belt which the authors compare with the recent tomographic Vp and Vp/Vs results of Menichelli et al. 2023. Their work allows to support the presence of a weak lower crust at the origin of the crustal lithosphere delamination in the forearc. The paper is interesting, the claims are novel and of interest in the community.

There are some issues: The data do not strongly support all their findings, there are some missing elements to document the data resolving power and inversion method, incompleteness of Figures and some minor ones (see below). But in overall I consider that their interpretation and proposed model is supported by the data. The paper may influence the understanding of the Apennines and possibly of other Mediterranean retreating subduction systems sharing similarities with the Apennines orogeny.

Detailed comments

1. The data analysis and inversion are carried out in line with good practice. The authors use the transdimensional method of Agostinetti and Malinverno 2010 to overcome the non-unicity problem of the inversion of receiver function waveforms for Vs structure. This provides posterior probability distribution of the number of layers, their thickness and S-wave velocities with depth. They also separate the isotropic component from the higher-order terms due to anisotropy and dipping geometry by applying harmonic decomposition of RF. The authors invert only for the resulting isotropic component of Vs.

The authors claim that they determine the Tyrrhenian Moho beneath the western (OSSC, CSNT, CRE) and eastern (NARO, MPAG) stations. This is the case for the former but very doubtful for the later ones. There is no signal above the noise level for MPAG (as also shown on the data Fig.4C) and the authors put the green line at the start of a negative jump. This is also the case for the NARO. For the later station a distinct velocity change (Vs slightly larger than 4km/sec) appears at depth closer to 20km instead (as far as I can see) but this is not seen on the data. Comparison with an independent methodology that provides the Moho depth such as the Vp tomographic model (Fig.4D) helps picking from the probabilistic model. But I do not see how this “justifies” the picking of a negative jump. Still the interpreted Tyrrhenian Moho located at 26-27 km depth in the Vs model (green line) it is placed deeper on the dVp and Vp section (green crosses). Please correct. Same remark for the green crosses of the western stations.

The Adriatic Moho picking is relatively convincing for the eastern stations but not at all for the CDCA. I have again the impression that it is not correctly transferred on section 4D. It is interpreted at 74 km depth on the Vs model but placed at 70 km on the cross section of Fig.4D.

2. The authors show the stacked RF traces for three representative stations (Fig.2) and the Vs model for 7 stations (Fig. 4B) including the three former ones.

(i) I would like to see the stacked RF traces for the other 4 stations (in case there is a space pb these could be incorporated in the supplementary material)

- (ii) There is a gradation of grey colour within the standard deviation interval of the Vs models which I assume that represents the probability density values. Can you add a grey scale?
- (iii) I would like to see the interface distribution histograms for the 7 stations of the main text as you show these for the rest of the stations in the supplementary material.

3. You refer to the Vp/Vs cross-section from the model of Menichelli et al. 2023 in the caption of Fig.4D as well as in the main text but this is not included.

Minor issues

1. In the abstract you write “....From the analysis of complete and dense teleseismic Receiver Function (RF) data set”. What do you mean by “complete”?
2. You should mark the Adria boundary on the panel of Figure 1.
3. Fig.4D displays crosses for the interpreted Mohos but in the manuscript you refer to dots (Page 5). Please correct
4. Fig.4A: Change the colour of station CRE from pink to brown.
5. Suppl Figures: Fig.1 third panel (from left to right). Correct “Vs (0-100km)” to “Vs(0-10km)”. Please explain what represents the panel on the bottom.
6. General comment for the stations of the suppl. material. Please provide a map with the names of the stations for which you show the Vs models

Rheology controlled continental delamination and thermal unbalance beneath the Apennines

Response to reviewer's comments

Menichelli I., Bianchi I., and Chiarabba C.

November 13, 2023

We thank the Editor and the Reviewers for their comments and suggestions, which we have applied and considered in the revised version of the manuscript and in the new supplementary materials.

Comments by Reviewer #1

This study focuses on understanding the physical properties and structure of the lithosphere in the central Apennines, with a specific emphasis on a controversial aspect of continental subduction - the delamination process of the crust from the subducting plate. The authors present detailed station-wise 1-D shear wave velocity profiles resulting from the inversion of Receiver Function waveforms and emphasized that a delamination of the continental lithosphere is facilitated by the emergence of a low V_s shear wave velocity zone within the middle-lower crust. This weak zone appears to play a critical role in the process of crustal delamination. Furthermore, the authors identify the presence of a double Moho beneath the Apennines forearc and concluded that the edge of the crustal delamination process is situated also in the forearc region, further to the east than previously anticipated.

This study makes a timely and important contribution to the discussions of the continental subduction beneath the Apennines. The major claims of the paper and its conclusions are novel and will be of interest to others in the field and beyond but however these findings need to be worked out in more solid, clear and quantitative manner. A first concern is the lack of detail in discussion and interpretations of the results. They lack the proper referencing to recent work of others, specifically in terms of placing the results from this study in the context of other works. Other concerns regarding the applied methodology are raised in the following comments. Overall, I found the paper too short to be understood by a broad readership. Given the policy relevance of this work and its important and novel contribution, I think this should be rectified. The paper would benefit from expanding the text, to explain more clearly the work conducted and situate the results in the wider literature.

Thank you for all the suggestions. We changed the text to fit your requests trying to improve the quality and the clarity of the manuscript. We added new references to previous work to make the article more solid, as suggested, and new detailed text was added in the discussion part, in accordance with the word limit defined by the journal. We believe that now the document is more solid and can be understood by a wider audience.

General issues:

The deconvolution approach used in the analysis and the type of filters applied should be explained in a couple of sentences and with a cross check for potential processing artefacts on both sides of the Ps arrivals. I am afraid that assigning deconvolution or filtering to the raw waveforms might impose processing artefacts. It is rather preferred to apply a plain waveform stacking of converted waves without deconvolution.

We do not fully understand the comment. RFs are time series that contain converted phases from P to S computed by deconvolution of the vertical component from the horizontal component. The deconvolution process allows removing the effects of the source and rays propagation in the velocity structure, so the RFs ideally contain only direct conversions and multiples at discontinuities under seismic recording stations. We have added a few sentences in the text to be clearer and not confusing.

The isotropic RF waveform at the station CRE shows a very weak positive pulse compared to those of the OSSC and CSNT whereas the corresponding 1D models show a similar Moho depth values at the three locations. Stronger RF pulses would result in stronger Moho sharpness on the 1-D models. It is not clear how these converted phases are weighted during the 1D inversions.

Yes, we agree with these observations. The pulse is smaller at CRE and this is consistent with the relative smaller Vs jump (see Fig. 4). The Vs increase at the Moho depth is of ~ 1 km/s at OSSC and CSNT seismic stations and ~ 0.6 km/s at CRE. This is consistent and not related to different weights in the inversion.

The authors states "At 74 km a gentle jump below the CDCA may be related to the deeper Adriatic Moho". The CDCA station is located at the central Apennines where a slab window has been clearly imaged in this area by several tomographies in the depth range between 70 km and 250 km (Amato et al., 1993; Piromallo & Morelli, 2003; Rosenbaum et al., 2008; Spakman & Wortel, 2004; Zhao et al., 2016; El-Sharkawy et al., 2020). The isotropic RFs signal at this station does show two strong positive pulses at 5 s and 7.5 s, respectively. On the other hand, the distribution of the Vs below this station shows a mostly constant increase with depth however with no indications for sharp interfaces/transitions except at 74 km. For me this is more reflecting the thickness of the lithosphere beneath the Central Apennines rather than a deeper Moho.

The location of the CDCA station is neither above the supposed slab window nor in the central Apennines. CDCA is located in northern Apennines, where all the past tomographic studies showed the presence of the

Adriatic slab (i.e., a deep w-dipping high velocity anomaly) below the Apennines wedge (Giacomuzzi et al., 2011, 2012, plus all the other references indicated by the reviewer). The interruption in the slab structure related to the presence of a window is supposed southward with respect to CDCA location, at around 41° of latitude (see also Di Stefano et al., 2009). Hence, the RF signal related to the interface at about 70 km depth can be related to deeper Adriatic Moho as reported in our text. This is also completely consistent with the tomographic model shown in Figure 4D (see the orange crosses). To avoid further misunderstanding we have modified part of the introduction for a better description of the area and the slab window.

The 1D Vs models beneath NARO and MPAG stations both show two interfaces at about 25–26 km and 52–54 km depths with a low velocity zone in between. On the hand, the RFs waveforms show only one sharp positive pulse at about 6 s beneath MPAG station and a wide weak positive pulse on the NARO waveform. Also the APEC station shows a sharp RF positive pulse at about 5 s, that is not reflected in the corresponding 1D Vs model. This needs to be clarified?

Right, we agree. The 6 s pulse is evident at MPAG station, but it is also present at NARO (although with a smaller amplitude). This is reflected in the Vs jump in the 1D Vs model (Figure 4B). The discontinuity at 25–26 km depth is not a positive jump in the model, but a negative jump consistent with the red (i.e., negative) pulse at 3–4 s (Figures 4C). In our interpretation, this signal is related to the incipient Moho that is not mature enough to define a positive reversal. We clarified this point in the text, in the Results and Discussion sections. In addition, the positive pulse at APEC corresponds to an increase in Vs of about 1 km/s between ~ 45–50 km depth. We have added all this information in the text.

Discussing vertical changes in the obtained Vs models required high resolution imaging. What is the vertical resolution of the obtained 1D models? Can you please show a resolution test? Is the low velocity zone in the lower crust well-resolved?

The vertical resolution of RFS depends on different aspects including the frequency content and the number of teleseismic events analyzed. In a test performed by Agostinetti and Malinverno, 2018 (<https://doi.org/10.1190/geo2017-0350.1>), it was demonstrated that the average gross resolution of 1D Vs models from RF is on the order of about 10% of the depth of the discontinuity. Thus, for a shallow Vs at a depth of 25 km we can estimate a resolution of about 2–3 km.

The Moho topography, yes can be resolved with higher accuracy by receiver function techniques, however the inversion of RF waveforms only is non-unique and thus it cannot conclusively resolve or image variations in absolute shear wave velocities (Vs) with depth. It is suggested that a joint inversion of receiver function and surface wave dispersion measurements to be ideally suited to constrain the Moho topographies and to determine absolute velocities with depth. Is the RjMCMC algorithm allows for a joint inversion? Is it possible to implement the joint inversion (even using previously existing surface wave datasets) with receiver function measurements to constrain the inversion results for the obtained 1D Vs models? How different are they from the current ones?

There are examples of joint inversions for RFs and surface waves (Bao et al., 2015; Diaferia et al., 2019; Li et al., 2017; Shen et al., 2013 and others) and are more significant when the set of one of the two kinds of data

is insufficient. Indeed, the main weakness of surface wave inversion is the lack of sensitivity to the depth of seismic interfaces; Rfs data are used to add constraints in this regard. In our study, the RFs data are abundant and we do not think that the joint inversion of surface waves will add significant constraints, also for the different resolving power of the two data. In any case, this is obviously another kind of study. Our goal is to show how different techniques, i.e., LET tomography and Rfs analysis, and datasets show the same results, adding consistency and reliability to the proposed geodynamic evolution model for the northern Apennines.

Huge number of tomographic studies have imaged the slab window beneath the \Central" Apennines not only the work by reference no. 27. The word \Central" should be added at this line in the text. Please also cite the necessary works { I gave examples in previous comments.

Thank you for the comments. The references were subdivided based the techniques and the ref 27 was specifically addressed to RF and regional tomography studies. Anyway, we added the new references indicated in the text.

Low shear weak zone: do you mean low shear wave velocity zone? Please clarify that throughout the text.

Thank you for the suggestion. In that case, the term "low shear weak zone" refers to the low velocity zone found in the velocity cross-section of Figure 4b and we speculate that this weak zone is imputable also to a weak rheology zone, as demonstrated in past numerical models, (Duretz and Gerya, 2013; Faccenda et al., 2008) promoting the process of continental delamination.

What is the Adriatic LID stand for?

Thank you for the question. We added the definition of LID in the text. LID is the equivalent of lithosphere.

Fig. 4 shows no green and yellow dots. Do you mean the green and yellow crosses?

Thank you, we changed the term "dots" with "crosses" in the caption.

How and on which bases do the authors draw the dashed lines connecting the red and yellow crosses on vertical cross sections of Fig. 4D that represents the Tyrrhenian and Adriatic Moho?

Thank you for the question. The dashed lines indicating the Tyrrhenian Moho, to the west, and the Adriatic Moho, in the eastern part of the cross section, were drawn on the basis of two different and complementary data: (i) the strong jump in S-wave velocity that is easily observed along the cross section (for both the absolute Vp model and the Vp perturbation model (%)),

(ii) the comparison of these results with other past studies on the Moho depth beneath Italy such as Di Stefano et al., 2011; Piana Agostinetti and Amato, 2009; Spada et al., 2013. The value of the Tyrrhenian Moho seems quite similar, some differences are related to the Adriatic Moho, but these validate the hypothesis proposed in the text on the new model of Apennine evolution.

On top of Fig. 4D, there is an undulating black line { is this representing the topography along the profile? What is the difference to Fig. 4A?

Yes, the undulating black line indicates topography; the difference lies in the scale (in Figure 4a we exaggerated the scale along the y to make the variation in topography more visible). We decided to eliminate the line in Figure 4d and keep only the line in Figure 4a to make the figure clearer and not confuse the reader.

In the caption of Fig. 4, yellow and green lines should be changed to yellow and green \dashed" lines. Also please change Rfs to RFs.

We applied the suggested changes in the caption.

Fig. 4D does not show the cross sections of the V_p/V_s ratio, rather the absolute and the relative V_p values. Please correct the caption. Also, it is not clear how the relative velocities have been calculated?

Yes, thank you. We corrected the caption removing " V_p/V_s ". The relative velocities have been calculated in the work of Menichelli et al., 2023 where a tomographic model (V_p and V_p/V_s) of the central Mediterranean area is presented.

A Moho map showing the variations in the Moho depth beneath the stations in this area would be good to show.

Yes, thank you for the suggestion. We added this new map in the SI.

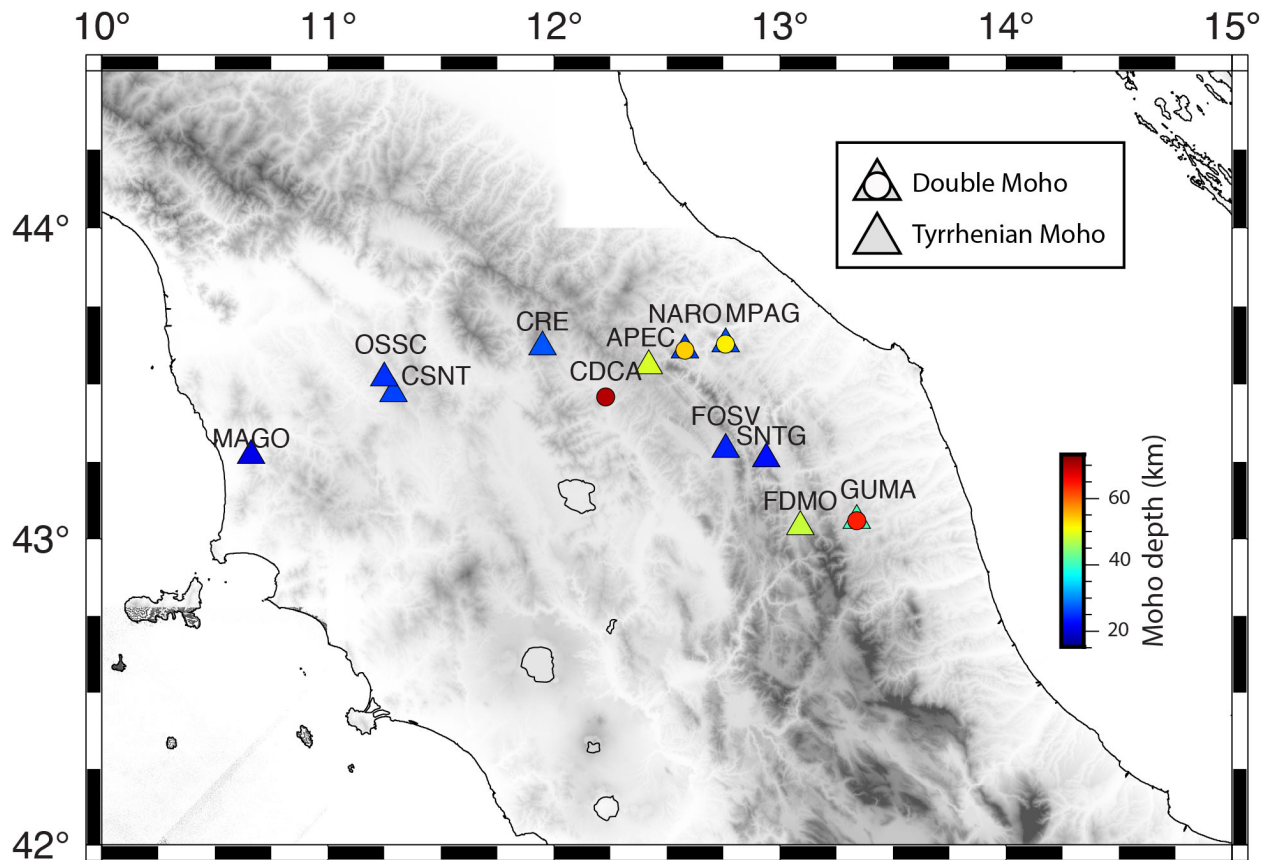


Figure 1: Moho map of the seismic stations analyzed, where the Moho depth has been extracted from the Vs profiles (MAGO, OSSC, CSNT, CRE, CDCA, APEC, NARO, MPAG, FOSV, SNTG, FDMO, GUMA). The triangle indicates the Tyrrhenian Moho whereas triangles with circles indicates the presence of a double Moho (i.e., Tyrrhenian and Adriatic ones) depicted below the station.

Comments by Reviewer #2

The authors presented teleseismic receiver function results for the Apennines. Teleseismic receiver functions were computed for 56 seismic stations using earthquake signals between 2012 and 2020. Harmonic analysis was used to decompose receiver function signals. The decomposed receiver function signals were inverted with a Reversible-jump Markov chain Monte Carlo method for the 1D shear wave velocity model as well as Vp/Vs ratio. Based on the results, the authors suggest a double Moho beneath the forearc. The geological evolution of the region requires additional investigation besides the published literature. However, the manuscript requires significant improvements and should not be accepted in its current form. The following are a couple of major points that the authors need to address.

1. The manuscript is missing data availability and code availability statements, author contributions, and competing interests statements. All of these are required based on the submission guide. It would be difficult to reproduce the results that the authors presented without the data and code availability statements.

Data can be retrieved in the European Integrated Data Archive (EIDA) database (<http://eida.rm.ingv.it>). The codes are open source developed on unix framework and available upon request because they need training for mindful use. Anyway, the specific description of the code is in Agostinetti and Malinverno, 2010.

The uncertainty of the inverted Vs is very high (Figure 4B) and Vp/Vs ratios are poorly constrained (Supplementary Material). Vp values would have large uncertainties as well. Are the lateral velocity changes in Figure 4D larger than the uncertainties?

We do not fully understand the comment. Why compare the standard deviation of the Vs 1D model (Figure 4B) with the amount of lateral velocity changes in the tomographic model (Figure 4D) or its uncertainty? In the work, we used and interpreted the mean 1D Vs model and the depth of conversions with the Vp tomographic model. Moreover, the uncertainty of the 1D model of Vs is not as high as commented by the reviewer. It is about 0.3 km/s, so much smaller than the range of models sampled that was probably taken as a reference for the comment. To avoid possible confusion we have included the probability scale in the figure; we apologize if the lack of it made interpretation difficult.

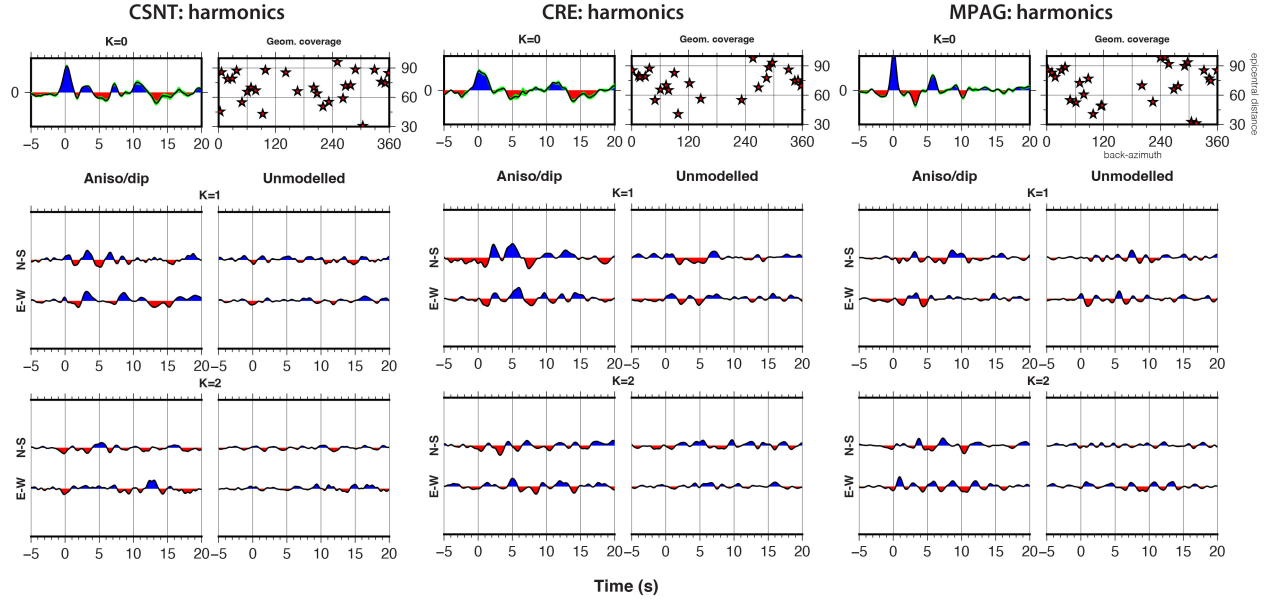
The authors used the averaged S-wave velocities for interpretation. You can compute predicted receiver functions with these S-wave velocity profiles. Do the predicted receiver functions match the observed receiver functions?

Yes, they match. We have introduced a specific figure in the SOM for the stations shown in Figure 4B where the observed isotropic RF component is plotted with the corresponding mean synthetic radial component. A similar figure was present in the SOM for all other stations, but we believe that the addition of this new figure in the SOM creates more clarity (see also REV3).

Yellow triangles in Figure 1 need to be labeled. Otherwise, the readers cannot easily find out where the stations used in Supplementary Material are located. Please specify the x- and y-axis labels for the Geom. coverage panel in Figure 3. What does the green shaded area mean in Figure 4B? The black dots in Figure 4D are earthquakes, right?

Yes, we added labels to the yellow triangles in a new map inserted in the SI. The x-axis is the back-azimuth and the y-axis the epicentral distance; we added these labels in the new version of Figure 3. The green shaded area in Figure 4b is only the graphic background whereas the black dots are the relocated earthquakes (2005-2020) in the 1D velocity model of the Central Mediterranean (Menichelli et al., 2022). We added this information in the caption.

Here, the revised Figure 3:



Comments by Reviewer #3

The authors compute S-wave velocity model by inversion of receiver function waveforms to provide additional information of the complex lithospheric structure of the central Apennines where convergence of the Tyrrhenian and Adriatic microplates occurs. The paper provides new information about the Vs structure of the lithosphere across the belt which the authors compare with the recent tomographic Vp and Vp/Vs results of Menichelli et al. 2023. Their work allows to support the presence of a weak lower crust at the origin of the crustal lithosphere delamination in the forearc. The paper is interesting, the claims are novel and of interest in the community.

There are some issues: The data do not strongly support all their findings, there are some missing elements to document the data resolving power and inversion method, incompleteness of Figures and some minor ones (see below). But in overall I consider that their interpretation and proposed model is supported by the data. The paper may influence the understanding of the Apennines and possibly of other Mediterranean retreating subduction systems sharing similarities with the Apennines orogeny.

We thank the reviewer for comments and suggestions. We have revised the text by adding more details about the results and missing elements to the data. We believe that the manuscript now appears more robust and solid.

Detailed comments

The data analysis and inversion are carried out in line with good practice. The authors use the transdimensional method of Agostinetti and Malinverno 2010 to overcome the non-unicity problem of the inversion of receiver function waveforms for Vs structure. This provides posterior probability distribution of the number of layers, their thickness and S-wave velocities with depth. They also separate the isotropic component from the higher-order terms due to anisotropy and dipping geometry by applying harmonic decomposition of RF. The authors invert only for the resulting isotropic component of Vs.

The authors claim that they determine the Tyrrhenian Moho beneath the western (OSSC, CSNT, CRE) and eastern (NARO, MPAG) stations. This is the case for the former but very doubtful for the later ones. There is no signal above the noise level for MPAG (as also shown on the data Fig.4C) and the authors put the green line at the start of a negative jump. This is also the case for the NARO. For the later station a distinct velocity change (Vs slightly larger than 4km/sec) appears at depth closer to 20km instead (as far as I can see) but this is not seen on the data. Comparison with an independent methodology that provides the Moho depth such as the Vp tomographic model (Fig.4D) helps picking from the probabilistic model. But I do not see how this "justifies" the picking of a negative jump. Still the interpreted Tyrrhenian Moho located at 26-27 km depth in the Vs model (green line) it is placed deeper on the dVp and Vp section (green crosses). Please correct. Same remark for the green crosses of the western stations.

The point of the reviewer is correct. The jump at 25-26 km in the eastern part of the model is a negative pulse and not positive. We interpreted this to be due to the transition from the high velocity Adria lower crust to a lower Vs weak material of the shear zone along which delamination occurs. This is according to our interpretation the signature of the incipient Tyrrhenian Moho, that will become a positive jump (like for the OSSC and CSNT stations) only after the entire evolution of the delamination (that we speculate will last for about 7-10 Myr). We thank the reviewer for the comment that helped us to better describe the point.

The Adriatic Moho picking is relatively convincing for the eastern stations but not at all for the CDCA. I have again the impression that it is not correctly transferred on section 4D. It is interpreted at 74 km depth on the Vs model but placed at 70 km on the cross section of Fig.4D.

Yes, you are right, the location of the Moho below CDCA station was wrong in the cross section of the Vp model. We moved the location in the right position at about 72 km depth (we extracted the depth more precisely from the Vs model), although the resolution at this depth is about 6-7 km and there is a general fit with the tomographic model. RFs at CDCA do not cover all the back-azimuth values (see Figure SOM), but K0 is stable and gives a first-order indication for the two samplings at 4-5 and 7 s (Figure 4BC), the latter being consistent with Moho at about 70 km depth.

2. The authors show the stacked RF traces for three representative stations (Fig.2) and the Vs model for 7 stations (Fig. 4B) including the three former ones. (i) I would like to see the stacked RF traces for the other 4 stations (in case there is a space pb these could be incorporated in the supplementary material)

Yes, we added the stacked RF traces in the SI.

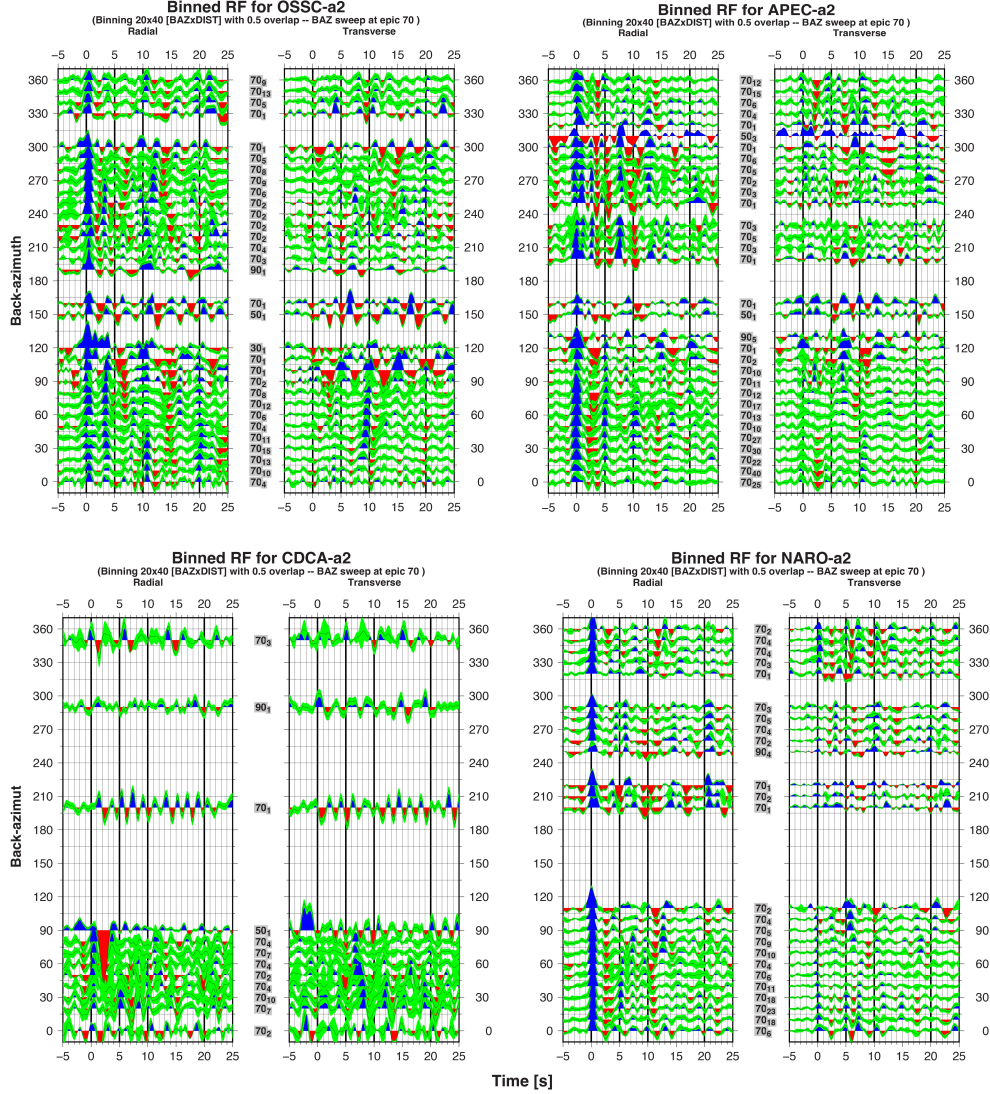
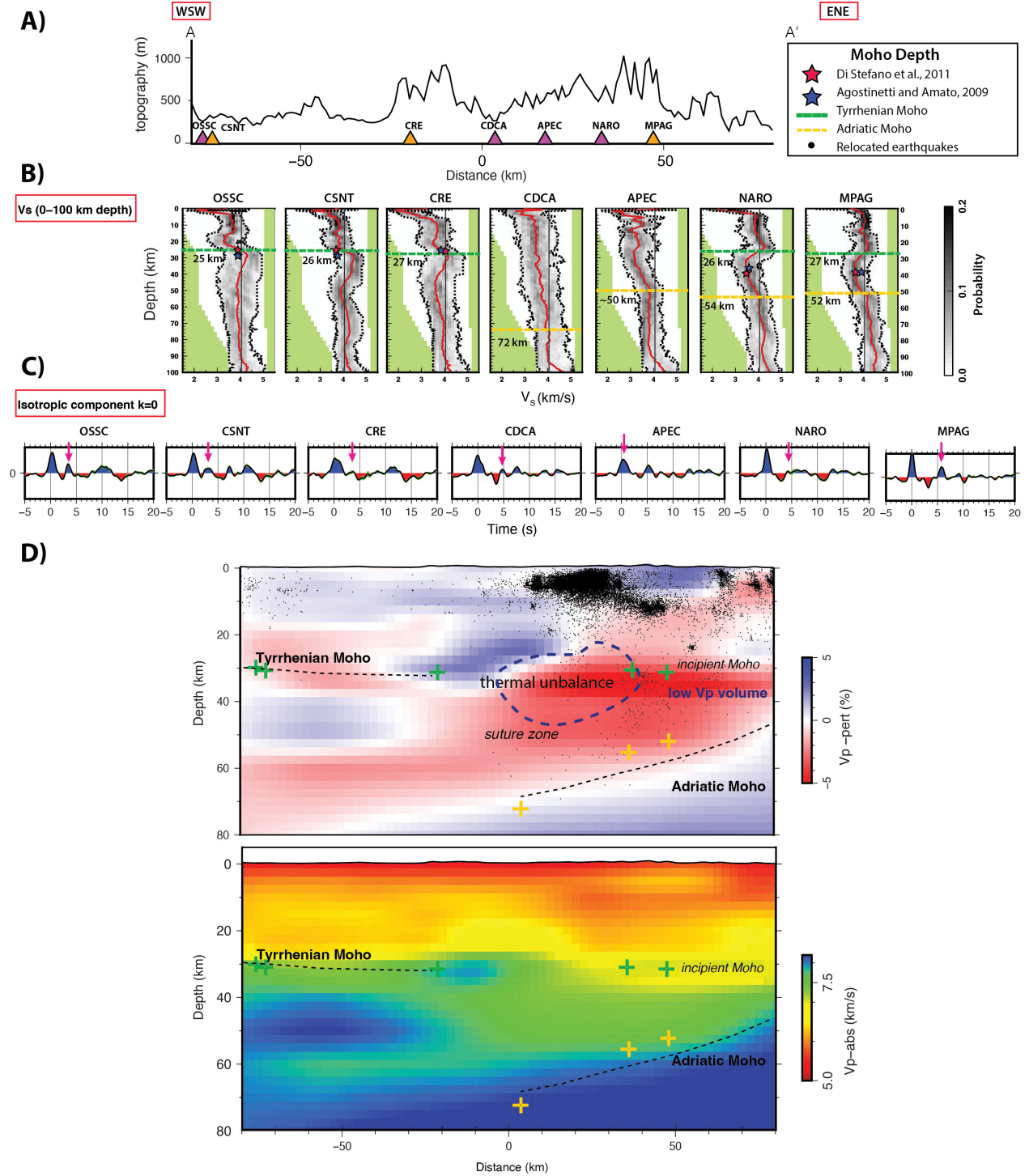


Figure 2: Stacked RF traces for OSSC, APEC, CDCA and NARO seismic stations.

(ii) There is a gradation of grey colour within the standard deviation interval of the Vs models which I assume that represents the probability density values. Can you add a grey scale?

Yes, we added the gray scale as you have suggested.



(iii) I would like to see the interface distribution histograms for the 7 stations of the main text as you show these for the rest of the stations in the supplementary material.

Ok, we added the histograms in the SI.
Here, the new figure:

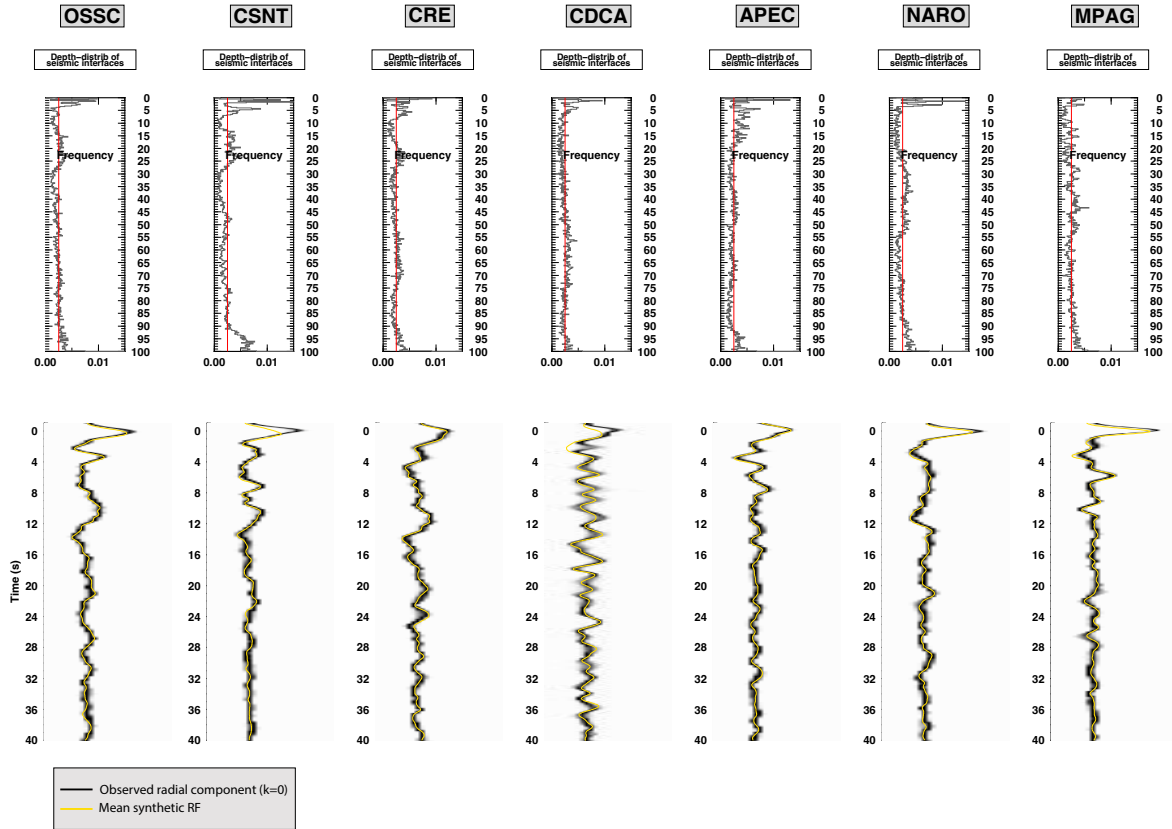


Figure 3: Interface distribution histograms and corresponding mean synthetic (yellow) and observed (black) isotropic RF component ($k=0$) for OSSC, CSNT, CRE, CDCA, APEC, NARO and MPAG.

3. You refer to the V_p/V_s cross-section from the model of Menichelli et al. 2023 in the caption of Fig.4D as well as in the main text but this is not included.

Yes, it was our fault, we changed the information in the caption.

Minor issues 1. In the abstract you write \....From the analysis of complete and dense teleseismic Receiver Funcon (RF) data set". What do you mean by \complete"?

We use the term complete to mark that the data set used in this work has a more good geometric coverage compared to previous work.

2. You should mark the Adria boundary on the panel of Figure 1.

Which Adria boundary are you referring to? Anyway, I don't think this is the correct map where to place it, this map is to show the location of the analyzed stations.

3. Fig.4D displays crosses for the interpreted Mohos but in the manuscript you refer to dots (Page 5). Please correct.

Yes, we changed it in the text.

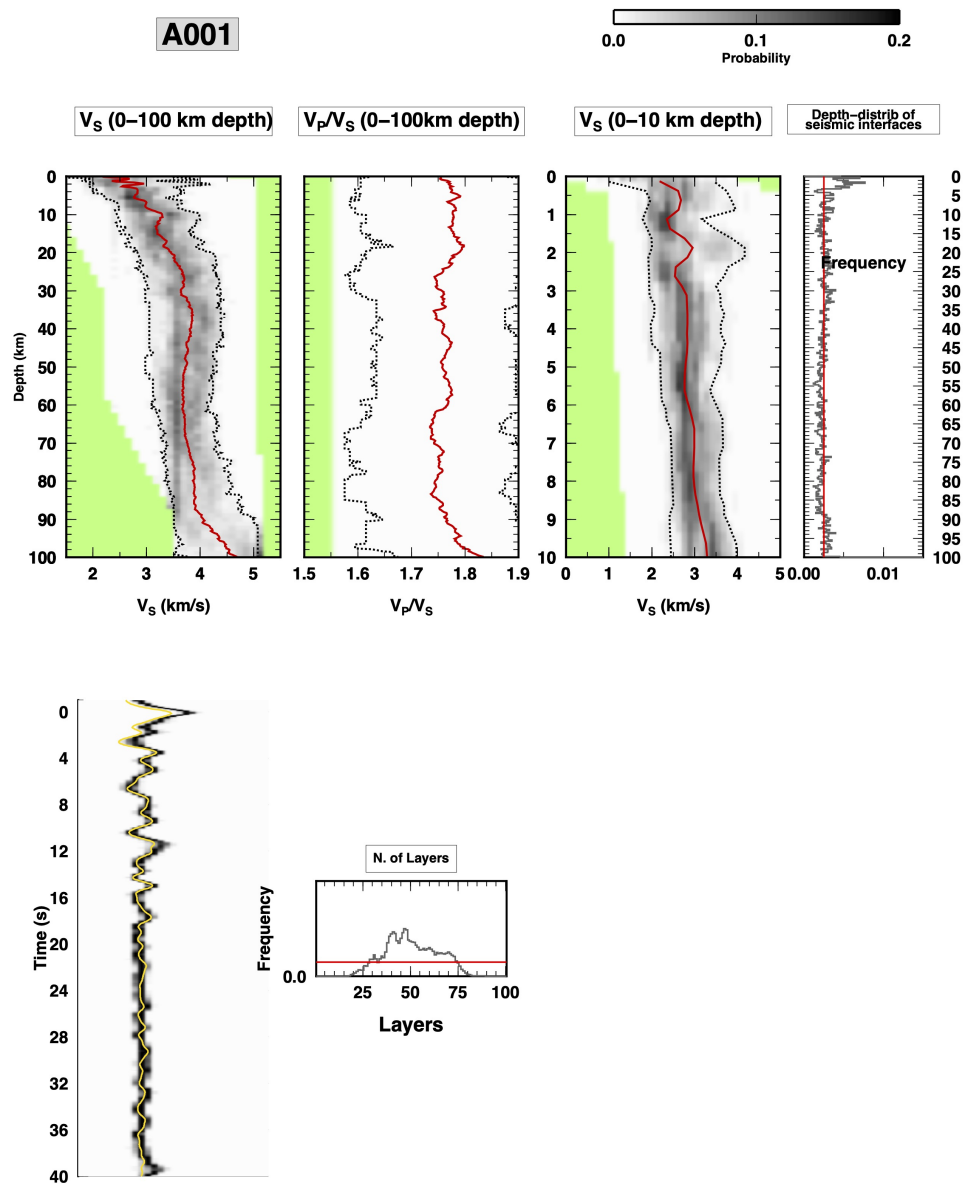
4. Fig.4A: Change the colour of station CRE from pink to brown.

Ok, thank you for the correction.

5. Suppl Figures: Fig.1 third panel (from left to right). Correct \Vs (0-100km) " to \Vs(0-10km)". Please explain what represents the panel on the bottom.

Ok, we corrected it and we added the explanation of the bottom panel meaning. The black line is the observed stacked radial ($k=0$) component meanwhile the yellow line is the mean synthetic RF. On the right, the diagram shows how often the number of layers from 0 to 100 can be found.

Here, the revised figure:



6. General comment for the stations of the suppl. material. Please provide a map with the names of the stations for which you show the Vs models.

Yes, we added a new map with labeled stations reported in the SI.

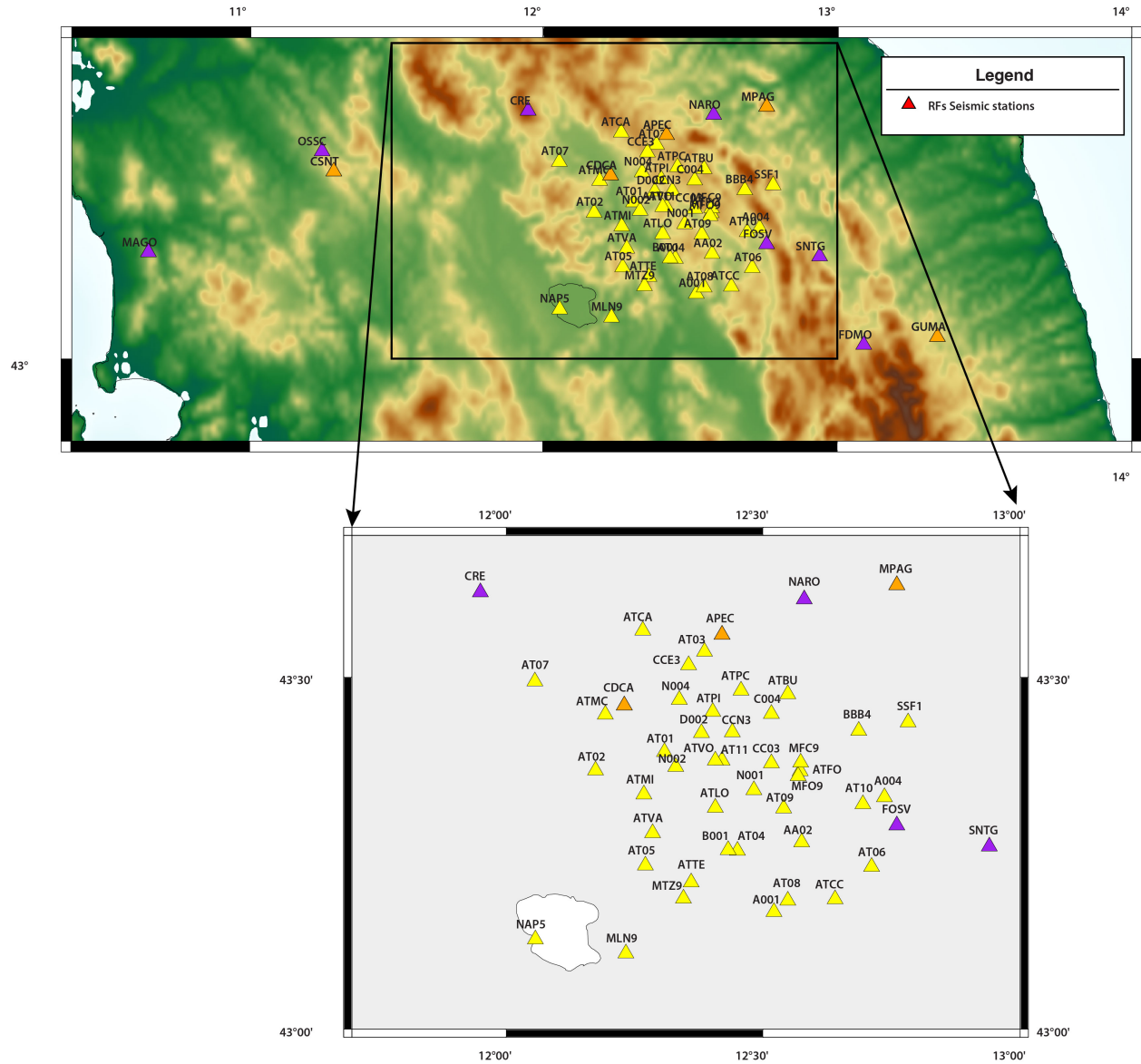


Figure 4: Map of seismic stations analyzed and whose results are reported in the SI.

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7th Dec 23

Dear Ms Menichelli,

Your manuscript titled "A lower crust shear zone favors delamination and continental subduction in the Apennines" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Communications Earth & Environment

Joe Aslin
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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Thank you for the point-by-point response. The manuscript is currently in a better shape. The only thing that is still vague and not easy to be accepted is the very large crustal thickness (72 km) beneath CDCA and the corresponding 1D Vs model - especially the Ps phase from the Moho at this station is picked at about 5 s. Have any previous studies indicated such a deep Moho? How thick the neighboring station's crustal thickness, i.e., ATMC?

Seems that the pink arrow is not in the right position at the APEC station in Fig. 4. Is this really the converted phase from the Moho? What about the pulse at 5 s?

The caption of Fig. 4D needs to be revised?

The Moho map is expected to show the lateral variability of the Moho depth from all analyzed stations not only from selected few ones.

Reviewer #2 (Remarks to the Author):

The authors answered the questions I raised. The manuscript is easier to understand with the

changes that the authors made. The additional evidence that the authors added makes the manuscript more convincing. The manuscript may be published with the following points addressed:

1. Since the authors said that the codes used in the manuscript are available upon request, this statement should be included in the manuscript. I am not sure whether the authors used source code from Agostinetti and Malinverno, 2010 or developed their own version. If the first case is true, the authors should add a statement like “the original source code was obtained from Agostinetti and Malinverno (2010)”.
2. The added color scale and updated caption of Figure 4 helped a lot. The authors may also explain what the green and yellow crosses are in Figure 4D. I know these were mentioned in the main text. It will be much easier to read the Figure if all the symbols are clearly defined.
3. The comparisons of the observed and synthetic receiver functions show a good agreement in general. The updated captions for Figure 1-43 in the Supplementary Material are much better than the original version.
4. The authors should mention in the caption of Figure 4 that the green color in Figure 4B is the background color.

Reviewer #3 (Remarks to the Author):

See attached PDF

Dear Editor

I'm pleased to see the revised manuscript. The authors appropriately addressed the comments from the three reviewers. I believe that the manuscript is suitable for publication.

Maria Sachpazi

A lower crust shear zone facilitates delamination and continental subduction under the Apennines

Response to reviewer's comments

Menichelli I., Bianchi I., and Chiarabba C.

December 22, 2023

Comments by Reviewer #1

Thank you for the point-by-point response. The manuscript is currently in a better shape.

Thank you too for the comments and suggestions that helped to improve the shape of the manuscript.

The only thing that is still vague and not easy to be accepted is the very large crustal thickness (72 km) beneath CDCA and the corresponding 1D Vs model - especially the Ps phase from the Moho at this station is picked at about 5 s. Have any previous studies indicated such a deep Moho? How thick the neighboring station's crustal thickness, i.e., ATMC?

Thank you for the comment. We revised the velocity model below CDCA. The presence of the Ps phase at 5 seconds effectively indicates the Tyrrhenian Moho's location at approximately 25 km. This aligns consistently with data from nearby stations (OSSC, CRE, CSNT, and ATMC) and corresponds to the velocity anomalies identified in the tomographic model (refer to cross-section Figure 4d). Hence, there is not a 72 km thickness of the crust and the velocity values between 30-70 km are coherent with the presence of the mantle weak zone as also detected by the tomographic section (Figure 4D). Nearby seismic stations, such as ATMC, show a velocity distribution similar to that of CDCA: at ~ 4.1 km/s jump is detected at a depth of about 27 km, while below 35 km the velocity is at ~ 3.8 -4.0 km/s consistent with the presence of the weakened mantle wedge.

Seems that the pink arrow is not in the right position at the APEC station in Fig. 4. Is this really the converted phase from the Moho? What about the pulse at 5 s?

Yes, that is true. The right position of the pink arrow is at 5s, it was my fault. I have corrected it.

The caption of Fig. 4D needs to be revised?

Thank you, we revised it.

The Moho map is expected to show the lateral variability of the Moho depth from all analyzed stations not only from selected few ones.

Ok, we have updated the map by including the most constrained seismic stations for the scale of analysis of this study.

Comments by Reviewer #2

The authors answered the questions I raised. The manuscript is easier to understand with the changes that the authors made. The additional evidence that the authors added makes the manuscript more convincing.

Thank you.

The manuscript may be published with the following points addressed:

1. Since the authors said that the codes used in the manuscript are available upon request, this statement should be included in the manuscript. I am not sure whether the authors used source code from Agostinetti and Malinverno, 2010 or developed their own version. If the first case is true, the authors should add a statement like "the original source code was obtained from Agostinetti and Malinverno (2010)".

Thank you. We have added the statement in the manuscript. The code used is the original of Agostinetti and Malinverno (2010).

2. The added color scale and updated caption of Figure 4 helped a lot. The authors may also explain what the green and yellow crosses are in Figure 4D. I know these were mentioned in the main text. It will be much easier to read the Figure if all the symbols are clearly defined.

Ok, thank you. We added the explanation in the caption of Figure 4D.

3. The comparisons of the observed and synthetic receiver functions show a good agreement in general. The updated captions for Figure 1-43 in the Supplementary Material are much better than the original version.

Perfect, thank you for the suggestion.

4. The authors should mention in the caption of Figure 4 that the green color in Figure 4B is the background color.

Ok, we added it in the caption

Comments by Reviewer #3

See attached PDF

Thank you.

Here, we have attached the revised Figure 4 and the related updated caption and Figure S5:

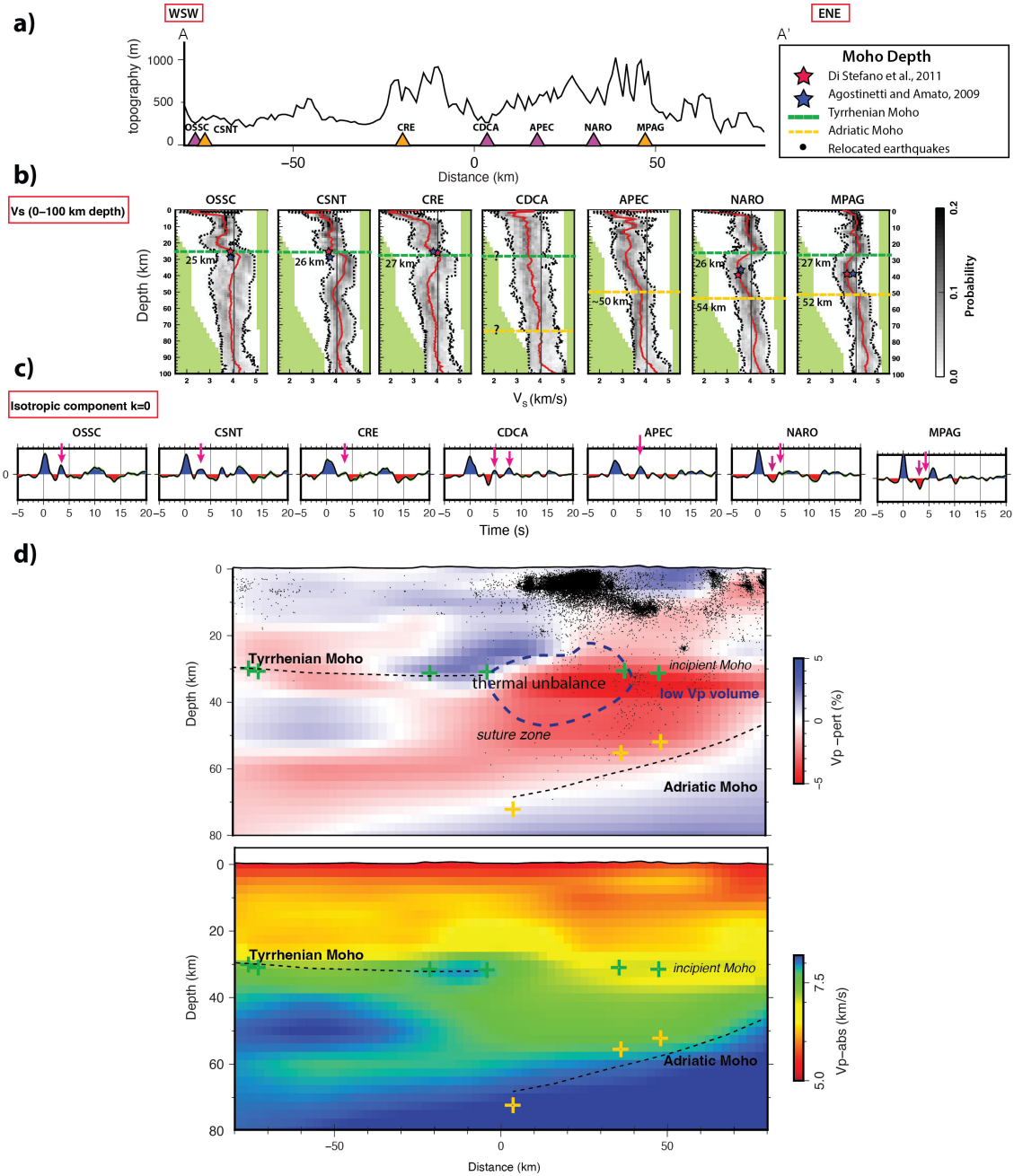


Figure 1: a) Topography of the profile A-A' in the map (see Figure 1) with the location of the seismic stations reported below. b) 1D Vs profile (0-100 km depth) for the seismic stations crossed by the trace. The red line indicates the average S-wave velocity, while the standard deviation interval falls within the black lines. Yellow and green dashed lines define the Adriatic and Tyrrhenian Moho depth, respectively. The red and blue stars are the Moho depth extracted from Di Stefano et al., 2011 and Piana Agostinetti and Amato, 2009. The green color is the background color. c) Isotropic composition ($k=0$) along time (s) extracted by the Rfs analysis at the different seismic stations. Blue and red lobes indicate positive and negative pulses and pink arrow the pulses related to the Moho. d) The corresponding 3D Vp cross-sections from the model of Menichelli et al., 2023. The black dots are relocated earthquakes (2005-2020) in the 1D velocity models of the Central Mediterranean computed in the work of Menichelli et al., 2022. The green and yellow crosses represent the Tyrrhenian and Adriatic Moho depths extracted from the Vs 1D models for each seismic station, respectively.

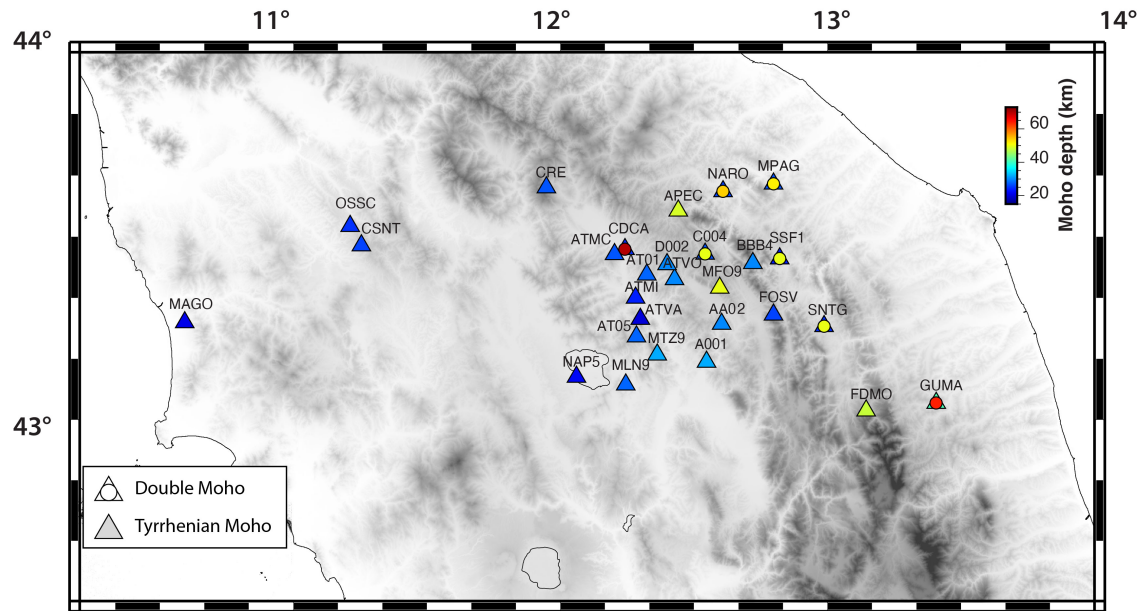


Figure 2: Moho map of the most constrained seismic stations analyzed, where the Moho depth has been extracted from the Vs profiles. The triangle indicates the Tyrrhenian Moho whereas circles plus triangles indicate the presence of a double Moho (i.e., Tyrrhenian and Adriatic ones) depicted below the station.

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