

Letter to Editor

Dear Editor,

we are pleased to submit the revised manuscript titled "*First Tomographic Imaging of Mid-Crustal Doubling at the Abruzzi Outer Thrust Front, Central-Southern Italy*", for your consideration. We have thoroughly reviewed the comments and suggestions provided by the reviewers and have focused our efforts on addressing all the raised issues. We would like to express our gratitude to the reviewers for their valuable and constructive feedback. Their comments have been important to improve the clarity and comprehensibility.

In response to the reviewers' comments, we have substantially revised the manuscript in accordance with all their suggestions. We refined several sections by incorporating the additional information requested by Reviewer #1 and Reviewer #2, and we introduced a new section entitled "*4. Parametrization and Model Resolution.*"

We also improved the readability of Figures 1, 2, 3, and 6, and added Figure 4 in line with the reviewer's request and the newly introduced section.

Furthermore, we have enriched the Supplementary Material to enhance the clarity and robustness of our results.

We annotated all the modifications within the manuscript using track. We also submitted the incorporated changes version, which is more readable. The line indications refer to the version of the manuscript without track changes.

Below, we provide a point-by-point response to the reviewers' comments, where the revised parts and improvements in the text and figures are explicitly indicated.

We sincerely hope that the revised manuscript adequately addresses all the constructive points raised by the reviewers and qualifies for publication in SE journal.

Thank you for your time and consideration.

Best regards,

Rita de Nardis on behalf of the co-authors.

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Reply to Reviewer #1

We thank the Reviewer, his/her comments were precious to make clearer the paper and the results. We followed all his/her suggestions. We provided two versions of the manuscript (tracked and integrated), and each modification within the manuscript is made with track changes. Line indications are referred to the version of the manuscript without track changes.

REVIEWER#1 (R#1)

GENERAL COMMENTS

The manuscript “First Tomographic Imaging of Mid-Crustal Doubling at the Abruzzi Outer Thrust Front, Central-Southern Italy” by de Nardis et al. presents a local earthquake tomography study of the Outer Abruzzi Thrust Front in Central Italy. By integrating Vp and Vs models with geological, geophysical, and seismological information, the Authors propose a three-dimensional conceptual model of the crustal structure, including a geometric reconstruction of the basal thrust of the Abruzzi Arc. The study addresses a geologically significant region and has the potential to provide valuable insights into the geodynamics and seismic hazard of the central Apennines. While the topic is appropriate for Solid Earth and the manuscript is generally well written, several substantive issues remain, mainly concerning the tomographic inversion and the resolution of the resulting models. On this basis, I recommend publication after major revision.

Response (Res)

We appreciate the Reviewer’s positive overall assessment of the manuscript and recognition of the geological importance of the study area, as well as the potential implications of our findings for the geodynamics and seismic hazard in the central Apennines. We also thank the Reviewer for noting that the manuscript is generally well written.

We fully concur that the main aspects requiring improvement concern the tomographic inversion procedure and the assessment of model resolution. In the revised manuscript, we have carefully addressed these points by 1) expanding and clarifying the methodological approach (chapter 3.2, lines 165-187) and 2) adding a new section titled “Parametrization and Model Resolution” (chapter 4, lines 248-295) providing a more detailed description of the parametrization in accordance with the Reviewer’s specific suggestions.

In addition, we have strengthened the evaluation of model reliability by adding and reorganizing figures in the Supplementary Material, including resolution and sensitivity analyses (Figs. S9-S12), to provide a more transparent assessment of data quality and the robustness of the resulting Vp and Vs models. We believe that these revisions significantly improve the clarity, reproducibility, and reliability of the study.

SPECIFIC COMMENTS

R#1

For the tomographic inversion the Authors used the Fast-Marching Tomography algorithm (FMTOMO). Although the Authors properly cite the related literature, the manuscript lacks a concise explanation of how the algorithm works. A brief methodological description would substantially strengthen the paper by allowing readers to better understand the inversion setting and assess the reliability of the results. For example, the manuscript mentions the use of two different grids (the

propagation grid and the velocity grid) but it remains unclear why they differ and how each is employed during ray tracing and velocity inversion.

Res

Preparing a multidisciplinary manuscript requires a careful balance among the different disciplines involved. In our effort to harmonize the text, we inadvertently devoted insufficient space to a detailed description of the FMTOMO algorithm. We thank the Reviewer for this constructive comment.

In the revised manuscript, we have added a concise yet comprehensive methodological description of the FMTOMO algorithm to clarify how travel times, ray paths, and velocity model updates are computed (chapter 3.2, lines 165-187). In particular, we now explicitly describe the forward and inversion methods and the role of the two grids used by FMTOMO: (i) the propagation grid, on which the fast-marching method solves the eikonal equation and computes first-arrival travel times, and (ii) the velocity grid, which defines the parameterization of the inversion model. Travel times are calculated on the finer propagation grid to ensure numerical stability and accuracy, while velocity perturbations are inverted on the coarser velocity grid to reduce the number of free parameters and stabilize the inversion. Ray paths are reconstructed by back-tracking through the normal direction with respect to the wavefront. This description has been incorporated into the Materials and Methods section to allow readers to better assess the inversion framework and the reliability of the resulting velocity models (lines 167-183).

R#1

The P- and S-wave velocity models are displayed using effective velocity values, but the chosen color scales do not adequately highlight the anomalies discussed in the text. Adopting color scales with stronger contrasts, ideally diverging around appropriate background reference values, would improve the readability and interpretation of the results.

Res

We thank the Reviewer for this comment. We chose the absolute velocities color scales by trying to avoid strong contrasts according to the literature on the use of color scales (Gołębiowska and Çöltekin, 2022; Stoelzle and Stein, 2021; Zhou and Hansen, 2016). Nevertheless, we have improved the visualization of both P- and S-wave velocity models by adopting color scales with enhanced contrast. Specifically, we now show both absolute velocity models and relative velocity perturbations with respect to the reference model in the Supplementary Material using diverging color scales centered on zero (Figs. S17-S28). This choice allows the reader to better identify and compare velocity anomalies discussed in the text and to clearly distinguish between background structure and significant deviations.

R#1

Figure 5a presents multiple cross-sections with different orientations, yet only four are shown clearly. All sections should be displayed, at least in the Supplementary Material, to provide a more complete visualization of the V_p model.

Res

We thank the Reviewer for this suggestion. We have revised Figure 5a (now Figure 6a) to include only the cross-sections discussed in the main text, improving readability. For completeness, we have added new figures in the Supplementary Material showing all

tomographic cross-sections used in this study, for both absolute and relative velocity models (Figs. S17-S28).

R#1

The treatment of seismicity also requires improvement. Relocated earthquakes are shown only in Figure 5a, and the varying orientations of the sections hinder a coherent assessment of the relationship between seismicity and velocity structure.

A map displaying the whole relocated dataset, along with seismicity plotted on each tomographic section, would greatly clarify this aspect.

Res

We thank the Reviewer for highlighting this important point. In the revised manuscript, we have clarified the treatment of seismicity and its role in the interpretation. A new map showing the entire relocated earthquake dataset has been added in the Supplementary Material (Fig. S7), and seismicity is now consistently projected onto each tomographic section to facilitate a coherent comparison with the velocity structure (Figs. S17-S28).

R#1

Additional ambiguity arises from the statements in lines 164–166 and 203–204, where the Authors indicate that seismicity was used to interpret the 3D velocity models and to construct the final 3D fault model. It is not stated, however, which seismicity dataset was actually used. Without this information, it is impossible to properly evaluate the modelling strategy. The authors should clearly specify whether the interpretation relies solely on relocated events, only on catalogue data, or on both, and they should provide a justification for their approach.

Res

As suggested, we have better clarified which seismicity datasets were used. The seismic tomography and the construction of the final 3D fault model rely on the relocated earthquake catalogue from ISIDE database (ISIDE Working Group, 2007), while the other catalogues of seismicity were used during the interpretation and validation phases. This distinction is now explicitly stated in the manuscript, together with a justification of the adopted approach (lines 145-146 and lines 161-164).

R#1

The assessment of model resolution also requires a more comprehensive analysis and evaluation. The Authors performed three checkerboard (CB) tests with different cell sizes, but they show in the Supplementary material results only at 10 km depth. A complete evaluation of the tomographic models cannot be made without access to the full CB test results. Maps at multiple depths for all three tests should be provided, at least in the Supplementary Material.

Res

Following the Reviewer's comments, we have included the resolution analysis of all checkerboard tests at multiple depth levels. These results are now shown in the new section "Parametrization and Model Resolution" (chapter 4, lines 248-295, Fig. 4) and provides more complete evaluation of the spatial resolution of the tomographic models.

R#1

Furthermore, in the manuscript is not reported any comparison between the sizes of CB cells with those of the anomalies interpreted in the real velocity models. Without such a comparison, it is difficult to distinguish between robust anomalies and those that may be artefacts.

In this framework, some more layers are actually displayed in Figure 4, but for these maps the cell dimension is not specified. At a pure visual inspection, it seems that the CB model shown is the one with 15 km horizontal and 12 km vertical cell size. Is it correct?

Res

We agree that a comparison between the size of checkerboard cells and the dimensions of interpreted anomalies is essential. In the “Parametrization and Model Resolution” section, we explicitly discuss this comparison, clarifying which velocity anomalies are larger than the resolving length indicated by the checkerboard tests and can therefore be considered robust (lines 273-281). We include also the three CBs analysed for V_p and V_s at different depth (Figs. S10-S11)

R#1

The recovered model exhibits unexpectedly reduced resolution at 8 km depth compared to the layers above and below. The authors should explain how this behaviour was evaluated and why it occurs.

Res

The reduced resolution observed at approximately 8 km depth has now been explicitly addressed in the manuscript (lines 269-273). It is mainly due to the position of the horizontal slice that was the same of the abrupt change of the synthetic velocity model (i.e. the boundary of cells with positive and negative velocity variations).

R#1

Finally, the authors state in lines 242–243 that the velocity models are well resolved down to 20 km and locally to approximately 24 km. However, the CB test of Fig. 4 indicates poor recovery at 20 km depth. Since one of the main features discussed in the manuscript lies within roughly 14–24 km, the resolution of this depth interval needs to be carefully revised and reassessed to avoid potentially misleading interpretations.

Res

We acknowledge the Reviewer’s concern regarding the resolution of the 14–24 km depth interval, which hosts one of the main features discussed in the manuscript. In the revised manuscript we show the coarse, medium and fine checkerboards at different depths (Figs. S10-S11) and discuss the model resolution accordingly (Parametrization and Model Resolution section, lines 273-281). As we stated we locally resolve the velocity model up to 22 km considering the coarse synthetic test.

TECHNICAL CORRECTIONS

R#1

In Figure 1a the FM solutions are reported with red or light-red background. Explain in the caption the difference.

Res

We thank the reviewer for the observation. The requested clarification has been added to the caption of Figure 1a.

R#1

The temporal range of the historical earthquakes shown in Figure 2a is not specified. Add this information in the caption.

Res

We thank the reviewer for this comment. We have now specified in the caption of Figure 2a the temporal range of the CPTI15 v4.0 catalogue (Rovida et al., 2020, 2022) used to represent the earthquakes.

R#1

Caption of Figure 3: the sentence “Red circles and green-blue contour lines as in legend panel d.” should be moved in the description of panel b.

Res

The caption has been revised accordingly.

R#1

Line 215: Fig. S5 or Fig. 5?

Res

The correct reference is “Figure 5”, and the text has been amended.

R#1

Line 496: “(see yellow dots in Fig.7)”. It should be “squares”.

Res

This correction has been implemented.

Reply to Reviewer #2

We thank the reviewer for the constructive comments, we appreciated his/her suggestions. We improved the text and the readability of the figures by following all the hints in the revision. Each of the related modifications/annotations within the manuscript is made with track changes. Line indications are referred to the version of the manuscript without track changes.

REVIEWER#2 (R#2)

MAIN COMMENTS

Review of the manuscript “First Tomographic Imaging of Mid-Crustal Doubling at the Abruzzi Outer Thrust Front, Central-Southern Italy” by Rita de Nardis, Donato Talone, Luca De Siena, Maria Adelaide Romano, Francesco Brozzetti, and Giusy Lavecchia submitted for publication to Solid Earth.

This work presents a newest addition to the research of the complex crustal structure of the Abruzzi Outer Thrust Front in Central-Southern Italy. The investigation mostly relies on results from local body wave tomography with addition of other geological, geophysical, and seismological information. The main finding of this study is the geometric reconstruction of the basal thrust of the Abruzzi Arc along

with new the V_p and V_s 3D models of the region. The topic of investigation is interesting, and the methods used are sound and my main critique of the manuscript is mostly about the usage of the tomography method and presentation of the results. The language used is appropriate and reads easily but the overall impression is that the manuscript overly complex with some parts hard to follow due to branching at too many findings and discussion of previous result (tomography, seismicity, FMS, geology, macroseismic, etc.). In my opinion not all these results are necessary in the process of getting the main result - mapping of the basal thrust – and manuscript would benefit from focusing on this and then discussing previous results in the context of the new findings.

1- Response (Res)

We thank the Reviewer for the careful and constructive evaluation of the manuscript, and for recognizing both the relevance of the topic and the soundness of the methodological approach.

We fully acknowledge the Reviewer's main concern regarding the overall complexity of the manuscript and the potential risk of dispersing the main message due to the parallel discussion of multiple datasets (tomography, seismicity, focal mechanisms, geological, and macroseismic information). While this complexity reflects the intrinsically multidisciplinary nature of the seismotectonic problem addressed, we have made a substantial effort to simplify the original version. In the revised manuscript, we have therefore reorganized the structure and simplified the figures accordingly.

- We have strengthened the presentation of the seismic tomography method and its results through two main actions:
 1. Improving the Methods section by providing a more detailed and clearer description of the seismic tomography approach adopted.
 2. Adding a new section explicitly discussing the limitations of the dataset and, consequently, the reliability of the tomographic results. We deleted the appendix and summarized the content in the new section and the supplementary.
- We have tried to simplify the figures and added the new one requested by Reviewer#2.

Fig. 1 – We improve the readability of the figure by editing the image, with particular emphasis on the geodetic information. Shortening rates within compressional domains differ significantly from those observed in other regions worldwide. Including this information allows the reader to focus on this key aspect within a general figure that introduces the main subject of the paper, namely the Abruzzi Outer Thrust System.

Fig. 2 – We remove all details related to the macroseismic field, leaving only two panels: (a) historical earthquakes and (b) instrumental seismicity used for seismic tomography. The text has been modified accordingly.

Fig. 3 – We remove all unnecessary details. The revised figure now displays geological information on the left and seismological information on the right. We delete the probability density function derived from the CLASS catalogue (Italian seismic catalogue), as well as the kernel density representation. In the section view, we now show only the relocated seismicity from the detailed studies of Romano et al. (2013b) and Chiarabba et al. (2005b).

Fig. 4 – We add a new figure to clearly show the P- and S-wave checkerboard tests at different depths, in accordance with the section requested by the Reviewer.

Fig. 6 – We modify the location map to show only the cross-sections presented in the figure. All additional information has been moved to the supplementary material, which now includes all tomographic cross-sections shown both in absolute and relative Vp and Vs values.

We hope that this reorganization significantly improves the readability and flow of the manuscript, addresses the Reviewer's concerns, and strengthens the overall clarity and impact of the manuscript.

2- R#2

All the seismological methods used are relatively poorly journaled i.e. the details of each processing step should be better explained and discussed. Although all the methods used are well known and used a brief description of each method would be beneficial along with discussion on potential problems when applying them. This is especially relevant for the main result, tomographic image of the central Italy. The Authors perform tomography using the Fast-Marching method as implemented in FMTOMO package, but the relevant details of the method are not discussed in the context of this research.Also, some discussed details are not clear. For example, what is propagation grid and what is inversion grid. Why is the propagation grid 4.5 km above sea level? And other such details...

2- Res

We thank the Reviewer for this constructive comment. We agree that the description of the seismological methods, and in particular of the tomographic approach, required a more detailed and coherent presentation. In the revised manuscript, we have expanded the Methods section to provide a clearer, step-by-step description of all processing stages (chapter 3, lines 143-245). We have added a concise but explicit description of the Fast-Marching Tomography approach as implemented in the FMTOMO package. We now explain how travel times, ray paths, and velocity updates are computed. We have also clarified the distinction between the propagation grid and the inversion (velocity) grid, which was insufficiently explained in the original version (lines 165-187). The propagation grid is used to solve the eikonal equation and to compute first-arrival travel times using the fast-marching method, whereas the inversion grid defines the parameterization of the velocity model. The finer spacing of the propagation grid ensures numerical accuracy and stability in the travel-time computation, while the coarser inversion grid reduces the number of free parameters and stabilizes the inversion (lines 171-173). The vertical position of the propagation grid relative to sea level is now explicitly explained and justified in the revised manuscript. These clarifications have been incorporated into the Methods section to allow readers to better follow the inversion framework and the reliability of the resulting tomographic models (lines 179-183). The grid boundaries were defined by considering the topography and the altitude of the seismic stations (above-sea limit), as well as the depth distribution of earthquakes. Earthquakes are filtered at depth ≤ 30 km. Then, we fixed the lower boundary surface to 31 km by considering a buffer of 1 km. Respecting the same 1 km buffer, we fixed the upper boundary surface to 4 km since the maximum altitude of stations is about 1.8 km, and the highest mountain peak in the area is about 2.9 km. Both these boundaries, which define the velocity layer to be inverted, must lie within the model grid. Consequently, we set the vertical grid extension from 4.5 km above sea level to a depth of 31.5 km.

3- R#2

Furthermore, the Authors discuss setting of the tomographic inversion process along with synthetic tests in three separate sections (in Method, Appendix and Supplement) therefore not allowing the reader to properly follow the whole process. I would suggest this be done in the main part of the manuscript.

Synthetic tests done are good but showing of these results is poor. Why not show the input checkerboard at each depth along with resulting inversion? Why not show the results of the checkerboard inversion along some of the more relevant cross-sections shown in Fig 5. (sections 1- 4) so readers can see how well they perform at depth.

3- Res

We fully concur that the main aspects requiring improvement concern the presentation of the tomographic inversion procedure and the assessment of model resolution. Following the Reviewer's suggestion, we have addressed these issues in the revised manuscript by reorganizing the description of the inversion setup and the synthetic resolution tests.

Specifically, we have added a new section titled "Parametrization and Model Resolution" in the main body of the manuscript, where the tomographic inversion strategy and the resolution assessment are now described in a coherent and unified manner (chapter 4, lines 248-295). This reorganization allows the reader to follow the entire inversion workflow, from model parametrization to resolution evaluation. Only detailed or complementary results are now presented in the Supplementary Material.

Supporting the new section, we have added a new figure (Fig. 4) for the synthetic tests. We now show the input checkerboard models together with the corresponding recovered models at multiple depths, allowing a direct visual comparison between input and inversion results. In the main text, we present the checkerboard with a size comparable to the discussed anomalies. In the supplementary material, we report all the synthetic tests, including the small-size checkerboard and the spike test of the main anomaly (Figs. S10-S12).

4- R#2

Authors state that there is good resolution at depths of 20 km and even at 24 km but from Fig 4. This is not obvious. Also, what about poor resolution in upper crust (Fig. 4 – 8 km depth)? The spike test is excellent in addition to the synthetic test but discussion about resolution for negative over positive anomaly (section 1 and 3 in Fig S10) is not discussed. Is there any resolution in this case and does it resolve this anomaly? How do the Authors comment on this.

As the tomography is the most important result of this work on which the main conclusion of the manuscript stands it needs to be discussed and journaled in detail.... Please emphasize more how the lower part of the basal thrust was mapped. It was extrapolated from tomography results but it is not clear exactly how....

4- Res

We thank the Reviewer for these detailed and constructive comments, which allow us to clarify several key aspects of the tomographic results, in particular, 1) the model resolution at different depths, 2) the recovery of velocity anomalies, and 3) the procedure used to construct the conceptual model of the basal thrust geometry.

- Regarding model resolution at depth, the checkerboard and spike tests indicate that the main velocity anomaly observed along sections 3 and 4 (now Fig. 6 and Supplementary Material S17-S28) is reliably resolved between approximately 10 and 22 km depth. Below this range, resolution progressively decreases, as evidenced by the spike test, and becomes poor below ~24-25 km depth. This limitation is now explicitly stated and discussed in the revised manuscript in section 4 "Parametrization and Model Resolution" (lines 273-288).
- Concerning the apparent poor resolution in the upper crust (above ~8 km depth, Fig. 4), it was mainly due to the position of the horizontal slice that was the same of the abrupt change of the synthetic velocity model (i.e. the boundary of cells with positive and negative velocity variations). The effect is amplified due to the reduced ray coverage and unfavourable ray

geometry in the shallow portion of the model. Consequently, shallow structures are not interpreted in detail, and our geological interpretation focuses on the better-resolved mid- to lower-crustal levels. We discussed this aspect in section 4 “Parametrization and Model Resolution” (lines 269-275).

- The Reviewer also raises an important point regarding the resolution of negative versus positive velocity anomalies (e.g., sections 1 and 3 in, Fig. S12 of the revised version). We explicitly discuss this issue in the new section “*Parametrization and Model Resolution*” (lines 284-286). Specifically, we tested both original- and reverse-polarity configurations to verify the capability of the inversion to image velocity variations and to exclude artifacts related to ray geometry (Rawlinson and Spakman, 2016). The spike tests show that positive over negative anomalies are recovered more efficiently than the opposite ones within the investigated volume. This is expected due to the seismic ray configuration. In the second case, rays tend to avoid the above low-velocity zone because of a defocusing effect induced by the lower high-velocity zone. The partial and good recovery of both types of spikes demonstrates that rays still traverse the anomaly volumes in any case, reinforcing the conclusion that the model is well resolved at those depths.
- Finally, in response to the request for clarification on how the lower part of the basal thrust was mapped, we refined Section 3.5 (“Model building”, lines 226-245). To make the model-building procedure clearer, we also added to the supplementary material all sections spaced 18 km apart and oriented N10°E, N40°E, and N60°E across the tomographic model. The sections used for the conceptual model reconstruction are highlighted with red rectangles (Figs. S17-S28).

5- R#2

Please comment on RMS and covariance of tomography. Is RMS good enough?

5- Res

We have added a comment of the RMS data misfit reduction achieved during the inversion by comparing it with the RMS distribution of the relocated catalog (centred around 0.5 s) (lines 300-302). The final values are about 0.2 s and 0.3 s for the P and S models, respectively. These values are better than the previous results and consistent with expected data uncertainties.

6- R#2

Similarly, a lot of discussion in this work is based on relocated seismicity but details of the relocation process are few especially in the context of used 1D model. How do the new locations differ from old ones? Is there significant difference? Please add more information and discussion to this part. It is only very briefly described without any details, and it is done only for one peculiar set of data. Where there more datasets where this could be done?

6- Res

Moreover, we clarify the role of the seismic datasets cited (lines 145-146, lines 154-157, and lines 161-164). The seismic tomography rely on the earthquake catalogue from ISIDe database (ISIDe Working Group, 2007), that we relocated with a uniform 1D model (Trionfera et al., 2019) before the tomography inversion. The other catalogues of seismicity were used during the interpretation and validation phases. The CLASS database (Latorre et al., 2023) is referenced to debate the seismic activity of the whole area and the thickness of the seismogenic layer. The unpublished seismicity relocated by Romano et al. (2013), and the catalogue from Chiarabba et al. (2005), are referenced for local seismotectonic details.

FIGURES

7- R#2

A lot of figures suffer from too many details crammed in one image. For example, why show subfigure with GPS velocity (Fig 1c.) from other investigations without explaining it properly or use it in current work? Also, details in that image are not visible. Is this horizontal velocity or vertical velocity.

7- Res

We thank the Reviewer for this comment. Figure 1 is intended as an introductory overview that places the investigated slow-deforming area within its broader geodynamic framework. Given the scope of the study, this context is important to illustrate the regional-scale compressional regime affecting central Italy, particularly for readers who may not be familiar with the characteristics of compressional domains in this region.

In the revised manuscript, Fig. 1 is explicitly referenced in the Introduction in relation to crustal shortening in Italy and the constraints provided by focal mechanisms and geodetic data. To address the Reviewer's concerns, we have (i) enlarged Fig. 1 to improve readability, (ii) revised the layout to reduce visual clutter, and (iii) explicitly stated in both the caption and the text that the displayed GPS velocities represent horizontal motions.

These modifications clarify the purpose of the figure and ensure that the geodetic information is both readable and properly integrated into the narrative of the manuscript.

8- R#2

Figure 2. please change marking from a, b', b'' and c to a, b, c, d.

8- Res

Thank you for this suggestion. As explained in the first response (Res-1) we have simplified the figure that now is only panel a and panel b.

9- R#2

Figure 3. Is especially hard to get through with too many details show and some not properly explained or used in the main text. For example, what is shown in subfigure 3d? What is kernel density? Where is this used in the text. How was this calculated? I can't see any green circles in Fig 3. as mentioned in figure caption.

9- Res

Following the comment of the Reviewers we simplified the figure and now it is more essential.

10- R#2

Figures S13 and S14. In which sense does the Authors apply extrapolated? Why was this done? The grid spacing in depth was 2 km was the extrapolation necessary? Please explain.

10- Res

We changed the term "extrapolated" to avoid potential misunderstandings. The meaning is that we are displaying the horizontal slices that correspond to the velocity model at specific depths. The revised caption now reads: "[...] represents the velocity model at intervals of 2 km [...]".

11- R#2

Figure 5. Please reduce number of profiles shown in uppermost image as currently nothing can be seen with these many profiles in same image. How was the red line marking basal thrust shown in this image inferred here?

11- Res

Thanks to the reviewer for the correction. We removed the profiles, and now we only represent the section traces of the images shown. The red dashed line is the basal thrust trace along each section inferred from the tomographic images. We clarify this point in the caption.

TECHNICAL CORRECTIONS

12- R#2

- Line 18 “...marked *V_p* inversion...” *Inversion in which sense? Decrease in velocity with depth or ...?*

12- Res

Decrease in velocity with depth. We wrote it explicitly.

13- R#2

- Line 215 Fig. S5 -> Fig. 5

13- Res

The correct reference is “Figure 5”, and the text has been amended.

14- R#2

- Line 319 (green dots... -> (green and blue dots...

14- Res

The sentence has been revised accordingly.

References

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