Response letter for manuscript "Exploring the Use of Adjoint Noise Tomography to Refine a Shear-Wave Velocity Model Beneath the Iranian Plateau"

In the following, we provide detailed description of the adjustments made in response to comments by Reviewer #1 and Reviewer #2, comments of the reviewer are shown in black and the responses are in blue.

We would like to thank both the Reviewers for the valuable and constructive feedback.

Reviewer #1

1) The referencing of previous studies is generally appropriate; however, the review appears to stop around 2022. Several important studies on the velocity structure of Iran have been published since then and should be incorporated to provide a more complete and up-to-date context. In particular, the authors should consider recent tomography studies listed below that have done within the same study region and provided high resolution Vs models:

Zarunizadeh, Z., Motaghi, K., Movaghari, R., Yang, Y. and Priestley, K. (2024). Seismological constraints on the lithosphere-asthenosphere system beneath the central and east Iranian Plateau. *Tectonophysics*, *873*, p.230215. https://doi.org/10.1016/j.tecto.2024.230215.

Zarunizadeh, Z., Aoudia, A., Motaghi, K., Pachhai, S., Manu-Marfo, D., Yang, Y., Abdulnaby, W., Ai, Y. and Chen, L. (2025). Imaging the northern Zagros collision zone by ambient noise and earthquake surface wave tomography. *Tectonophysics*, p.231041. https://doi.org/10.1016/j.tecto.2025.231041

Mehrdar, A., Motaghi, K., Ghods, A., Sobouti, F., Priestley, K., Pachhai, S., Shabanian, E., Zarunizadeh, Z., Zeynaddini-Meymand, R. and El-Hussain, I. (2025). Crustal and uppermost mantle structure of the Iranian Makran subduction zone from ambient noise and earthquake surface wave tomography. *Geophysical Journal International*, *241*(1), pp.70-85. https://doi.org/10.1093/gji/ggae419

Enayat, M. and Ghods, A. (2023). 3D Shear-Wave Velocity Model of Central Makran Using Ambient-Noise Adjoint Tomography. *Journal of Geophysical Research: Solid Earth*, *128*(11), p.e2023JB026928. https://doi.org/10.1029/2023JB026928

Amiri, S., Tatar, M., Maggi, A. and Zaroli, C. (2025). Shear wave velocity structure of Iranian plateau using SOLA Backus-Gilbert Rayleigh wave group velocity dispersion tomography. *Tectonophysics*, p.230931. https://doi.org/10.1016/j.tecto.2025.230931

Agreed. We will update the Introduction and regional-comparison parts of the Discussion to incorporate these studies. Please see lines 53-64 and 65-72 in the introduction, and lines 441-448 and 503-515 in the discussion.

2) When discussing low-velocity anomalies beneath the eastern Iranian Plateau, earlier foundational studies should also be cited and discussed. For example:

Motaghi, K., Tatar, M., Shomali, Z.H., Kaviani, A. and Priestley, K. (2012). High resolution image of uppermost mantle beneath NE Iran continental collision zone. *Physics of the Earth and Planetary Interiors*, *208*, pp.38-49. https://doi.org/10.1016/j.pepi.2012.07.006

Wu, Z., Chen, L., Talebian, M., Wang, X., Jiang, M., Ai, Y., Lan, H., Gao, Y., Khatib, M.M., Hou, G. and Chung, S.L. (2021). Lateral structural variation of the lithosphere-asthenosphere system in the northeastern to eastern Iranian plateau and its tectonic implications. *Journal of Geophysical Research: Solid Earth*, *126*(1), p.e2020JB020256. https://doi.org/10.1029/2020JB020256

We appreciate the suggestion to include foundational references for the eastern Iranian Plateau. These references are cited in the revised introduction part (lines 57 and 60). However, as shown in our resolution tests (Figure 8), the southeastern part of the Iranian Plateau, particularly in the eastern areas, falls outside the well-resolved regions of our model. Consequently, we deliberately avoided making interpretative claims or discussing structural features in this part of the model. Our focus was instead on the better-resolved regions of the central and western Iranian Plateau and Zagros collision zone.

3) I suggest that the authors include additional cross-sections to better illustrate the three-dimensional structure of the model. For example:

* Several parallel cross-sections across the Zagros collision zone (e.g., five profiles) to clearly show lateral variations and underthrusting geometry.

* A long profile in eastern Iran extending from the Makran subduction zone across the Lut Block to the Kopeh-Dagh Mountains.

* A profile crossing the South Caspian Basin (SCB) from east to west to better constrain its lithospheric structure.

* A section crossing both Sahand and Sabalan volcanoes.

* A profile along the Urumieh–Dokhtar Magmatic Arc.

We agreed and implemented this in the Supplement to keep the main manuscript focused and readable. In the revised version, we added a new Supplementary figure S9 with all the profiles existing in the Irandoust et al. (2022) for a better comparison to the reference model. Those profiles already have 5 cross sections that transects Zagros and also some other profiles at the north, northeastern and eastern part of the model. We also added 2 other profiles, M and N, which as the reviewer suggested, covers the Urumieh–Dokhtar Magmatic Arc and Sahand and Sabalan volcanoes, respectively. Please refer to lines 290-291, 480-485 and 503-515.

4) It is surprising that the manuscript includes little or no discussion of the Makran subduction zone and the Kopeh-Dagh Mountains. These are first-order tectonic domains within the study region and deserve specific attention. I recommend adding a discussion of the velocity structure and tectonic implications for both regions.

We thank the reviewer for this valuable suggestion. We have reviewed the recent studies by Enayat and Ghods (2023) and Mehrdar et al. (2025), which provide detailed and focused investigations of the Makran subduction zone using ambient noise adjoint tomography and joint surface-wave methods, respectively.

Although the data coverage in the southeastern region is limited overall, as demonstrated in Figure 1, we do include a cross-section (K) in Fig. S9, traversing the Makran–UDMA–East Basin corridor. Along this section, our model recovers broad first-order crustal structure, including relatively thickened crust beneath the Makran accretionary prism and upper plate, as well as a well-imaged low-velocity zone in the mid-crust beneath the UDMA. However, our model does not resolve the steep subduction geometry or slab-related features reported in the Makran trench by Enayat et al. (2023) or the vertical velocity gradients observed in Mehrdar et al. (2025). We interpret this as a limitation due to both station coverage and maximum period (50 s). Please refer to lines 512-515 and 480-485.

5) The deep structure beneath Sahand and Sabalan volcanoes remains poorly constrained. Since your model covers this region, it would be valuable to analyze and discuss the lithospheric structure beneath these volcanoes and compare your results with previous regional and teleseismic tomography studies of NW Iran (e.g., Bavali et al. 2016). Such comparison would strengthen the regional geological interpretation.

Bavali, K., Motaghi, K., Sobouti, F., Ghods, A., Abbasi, M., Priestley, K., Mortezanejad, G. and Rezaeian, M., 2016. Lithospheric structure beneath NW Iran using regional and teleseismic travel-time tomography. *Physics of the Earth and Planetary Interiors*, *253*, pp.97-107. https://doi.org/10.1016/j.pepi.2016.02.006

We agree with the reviewer that discussing the volcanic regions of NW Iran adds value to our geological interpretation. We have now included a comparative analysis of our results with the study of Bavali et al. (2016) in the revised manuscript (Lines 503-512). The newly added N profile (Figure S9) closely follows the location of the cross-section analyzed in Bavali et al. (2016), which integrates seismological and potential field data across the Sahand–Sabalan axis and the southern Alborz. Our Vs model reveals a mid-crustal low-velocity anomaly beneath the Sahand–Sabalan region, consistent with the potential presence of partial melt or crustal magmatism, as inferred from gravity and magnetic data by Bavali et al. (2016).

6) The manuscript reports interesting intra-crustal low-velocity anomalies beneath the Zagros collision zone, yet their origin is not discussed in detail. This is a significant observation. The authors should explore possible explanations, such as:

* Partial melt

* Fluid presence

* Crustal thickening and underplating

* Shear zones or distributed deformation

Thank you for this suggestion. In the revised Discussion we added a clearly framed paragraph describing plausible physical causes of intra-crustal lowVs anomalies, linked to regional context and the period-depth sensitivity of our data. Please refer to lines 407-419 in the revised version of the manuscript.

7) Providing a geodynamic interpretation would substantially improve the Discussion section.
Your results suggest variations in the degree of underthrusting and lithospheric structure along the Zagros collision zone. This likely reflects different convergence mechanisms along strike. I recommend expanding the discussion in this regard and comparing your findings with recent geodynamic interpretations of segmentation and along-strike variability in Zagros (e.g., Irandoust et al., 2022 and Zarunizadeh et al., 2025).

We agree that the Discussion will be stronger with a concise synthesis linking our alongstrike patterns to published segmentation interpretations. In the revision, we expanded the discussion part, in which we discuss the along-strike variations in the low-velocity anomalies underthrusting central Iran and compare the results with the previous studies mentioned by the reviewer. Please refer to lines 441-448 and 407-419.

Reviewer #2

1. The statement in line 30, “The use of a numerical spectral-element solver in adjoint tomography provides highly precise structural sensitivity kernels,” is not entirely accurate. The precision of the kernels primarily stems from the exploitation of full waveform information, rather than from the spectral-element solver itself. The solver improves the numerical accuracy of forward wavefield simulations, but it does not inherently determine the resolution of the kernels.
In addition, in the context of adjoint tomography, the term “sensitivity kernel” is somewhat ambiguous. Since the kernels are derived from the gradient of a specific misfit function, it would be more precise to refer to them as “adjoint kernels” or “misfit kernels.”

We appreciate this clarification. We agree that the ability to obtain precise imaging results comes from leveraging the full waveform content, while the role of spectral-element solver is to accurately simulate wave propagation. The wording in our Abstract has been corrected to reflect this distinction. We now state that using the spectral-element method ensures accurate forward wavefield simulations, and by exploiting full-waveform information we obtain precise adjoint misfit kernels, leading to more robust images. Please see the lines 29-32.

We will also ensure consistent terminology throughout the manuscript, using "misfit kernel" as appropriate instead of the ambiguous "sensitivity kernel."

2. Introduction - motivation and scientific context. The motivation in the introduction is not sufficiently clear. The statement: "Nevertheless, these images are subject to simplifying assumptions (such as high-frequency assumptions) with limited resolution, which leave the geometry of the crust and the underthrusting slab in the Zagros collision zone a topic of ongoing debate." is too general.
The manuscript would benefit from a more explicit description of the existing debates. For example, what specific geometric features of the crust and underthrusting slab remain contro- versial? Which competing models exist, and what are their key differences? Clarifying these points would better highlight the scientific gap that this study aims to address.

We agree that the Introduction should more explicitly outline the debated geological issues in the Zagros collision zone to motivate our study. In the revised Introduction, we have expanded the discussion of the open questions regarding the crust and slab geometry. However, we have also emphasized later in the introduction that our study is framed as a test of how applying ANT can help resolve some of these details. Please refer to lines 53-64 and 66-72.

3. Introduction - final paragraph In the last paragraph of the Introduction, it would be preferable to briefly summarize the main findings rather than using forward-looking expressions such as "aiming to" or "may." A concise statement of the principal results would make the introduction clearer and stronger.

We have revised the final paragraph of the Introduction to be more concise. Please refer to lines 101-104. The Introduction now concludes with an overview of what our study accomplished.

4. Workflow (Figure 3) The workflow could be improved by organizing it into two parallel branches: (1) A simulation branch to obtain synthetic Green's functions (SGFs). (2) An observation branch to obtain empirical Green's functions (EGFs). The comparison between SGFs and EGFs would then lead to the construction of adjoint sources, after

which the two branches converge into the inversion/update procedure. Such a structure would make the methodology clearer and more logically organized.

Agreed. To avoid any potential misunderstanding that the CCs were produced as part of this study, we edited the line 138 and have revised the flowchart to improve clarity as suggested by the reviewer. The workflow now begins with the forward modeling step, with a separate side block indicating the “selection of CCs” and “Computing the EGFs”. Please refer to Figure 2.

5. Line 145-146 - Earth’s curvature. Your model domain spans 2300 km×2400 km. At this
scale, Earth’s curvature cannot be neglected. So please clarify how Earth’s curvature is taken
into account in the modeling. It is not clear whether the curvature is explicitly considered in the numerical setup or approximated in some way. A brief explanation would improvemethodological transparency.

As stated in the manuscript lines 152-155 a global version of the SPECFEM3D, called SPECFEM3D_GLOBE is used in this study to account for the curvature of the Earth. The configuration used in the study is also depicted in Figure 3, showing the curvature of the Earth in the used model.

6. Inversion and waveform fitting. Did you perform the inversion using only a single period band (10–50 s) with 12 iterations? To my knowledge, most studies adopt a multiscale strategy, proceeding from low frequencies to higher frequencies, in order to reduce the risk of cycle skipping and overfitting. Please clarify whether a multistage inversion strategy was tested or considered.

We performed the adjoint inversion using the full 10–50 s period band simultaneously across 12 iterations, rather than a stepwise multi-band approach. We chose this single-stage strategy because our initial model was derived from previous tomography and already provided a reasonably good starting fit in this band, reducing the risk of cycle-skipping from the outset. We acknowledge that a multiscale inversion is a common best practice. However, given the relatively narrow bandwidth and our computational resource constraints, we opted to invert all available frequencies together. We also note that if we were to include much lower frequencies or a broader period range, a multistage approach would be advisable. Please see lines 163-168, in which we clarified this point.

In addition, regarding the waveform fits shown in Figure 7, the agreement between the final model and the observed data is clearly improved compared to the initial model. However, the observed waveforms appear to be systematically advanced relative to the synthetics in the final model. Does this indicate that the model has not been sufficiently updated? A brief discussion would help clarify this point.

We thank the reviewer for this careful observation. We agree that a residual phase offset remains visible in the waveform example shown in Figure 7. Further iterations could also lead to better improvement, but as we shown in Figure 6, we could not reach much lower misfits than iteration 10, by doing 2 more iterations and because of the computation costs, we stopped at iteration 12. Figure 7 also indicates a clear overall improvement in waveform agreement for the final model, particularly in the full 10–50 s band and in the 10–20 s band, where the cross-correlation coefficients increase from 0.38 to 0.63 and from 0.58 to 0.66, respectively. In contrast, the improvement is not uniform across all period bands; for the 20–50 s band, the cross-correlation remains nearly unchanged.

We therefore interpret the residual phase shift not as evidence that the inversion failed, but rather as an indication that the model update is period dependent for this particular source–receiver path. This interpretation is also consistent with our supplementary analysis (refer to Figure S10), which shows that the best performance of the final model is obtained mainly in the intermediate period range of about 12–35 s. We revised the manuscript to clarify this point in lines 320-323.

Finally, it would strengthen the manuscript if you could present waveform-fitting results for additional station pairs with different azimuthal directions and from different regions of the model. This would better demonstrate the robustness and spatial consistency of the inversion Results.

Agreed. In the revised submission, we included additional waveform fit examples from other station pairs covering different azimuths and distances across the model in Figure S12. Please also see line 323-324.

7. Underthrusting, line 384-390. Both your study and previous works observe low-velocity anomalies and interpret them as the underthrusting Arabian plate. However, underthrusting lithosphere is typically expected to be cold and mechanically strong, which would produce a high-velocity signature. I therefore wonder why the

underthrusting is imaged as a low-velocity anomaly in this case. Could you clarify the physical mechanism behind this interpretation?

We have revised the discussion regarding the low-velocity nature of the underthrusting zone (Lines 407-419 and 441-448). We have incorporated references to previous seismic studies (e.g., Irandoust et al. 2022, Zarunizadeh et al. 2024, Mahmoudabadi et al. 2019) that support the interpretation of low-velocity signatures as either deeply underthrust crustal material or the result of lithospheric delamination and thermal anomalies in the Zagros-Central Iran transition zone.

Minor comments

1. Line 29. ”is dropped” should be ”has dropped”.
Corrected.

2. Line 50. The statement “as well as by lithospheric-scale displacements along the transcurrent faults within the less deformed microplates in Central Iran (CI)” requires appropriate citations. Please provide supporting references for this interpretation.
We have added the missing citations to support this statement.

3. Line 51 and figure1. Since GPS measurements are mentioned in the text, they should be clearly represented in Figure 1. If the grey arrows denote GPS velocities, this needs to be explicitly stated in the figure caption. In addition, seismicity is shown using earthquakes of varying sizes; however, no legend is provided to explain the size scaling. Please include a clear label or magnitude scale in the figure legend.

We agree and have updated Figure 1 accordingly. We have plotted the GPS measurements for some stations from Zarifi et al., (2013). Now shown by red arrows on the map and also depicted in the legend and mentioned in the caption.
Regarding seismicity, we have included a legend for earthquake magnitudes: the figure caption now notes that earthquake epicenters are plotted with symbols scaled by magnitude.

4. Line 54-56. It would be clearer to classify t;he different methods explicitly, for example, body-wave travel-time tomography, surface-wave dispersion tomography.
Agreed and corrected. Please refer to lines 53-64 in the revised version.

5. Line 89. “Classical tomographic methods” would be more precise if replaced with “ray-theory-based tomographic methods.”
Done. We have changed the phrasing to “ray-theory-based tomographic methods” as suggested.

6. Line 108. “Selection” is more appropriate than “choice” in this context.
Corrected

7. Line 119. If variables are defined immediately after an equation, the sentence should not end at the equation. Please place a comma after the equation and continue the description in Lowercase.
Thank you for pointing this out. We have made the recommended formatting change in this line and elsewhere in the text.

8. Line 178. Same comment as above regarding equation punctuation and variable definitions.
As noted, we applied the same fix here.

9. Figure 4. Please consider reorganizing the layout so that slices at the same depth in the initial and final models are aligned in the same row or column, which would facilitate a clearer comparison.
We appreciate this suggestion and have reconfigured Figure 4 for easier comparison.

10. Line 349-350. Figure 5 does not appear to present sensitivity kernels. Please check whether this figure should instead be referenced as Figure S5.
Thank you for catching this referencing error. Indeed, the figure we intended to cite for the sensitivity kernel values is a Supplementary Figure (we have a Figure S5 in the Supplement that shows examples of our adjoint sensitivity kernels/misfit kernels). However, it is worth it to point out that those kernel values are also used for the shaded area in figure 5 to keep only the parts that shows meaningful changes in the kernel values (please also see the caption of Fig.5)

11. Inconsistent usage of terminology. Please ensure consistent spelling and formatting throughout the manuscript, e.g., ‘travel time” vs. ‘traveltime” (choose one style and use it consistently). Similarly, terms such as “adjoint noise tomography” should be used consistently throughout the manuscript, including consistent capitalization.
We have conducted a thorough proofreading of the manuscript to eliminate such inconsistencies. We standardized terms like “traveltime” (now using one word without a space, to be consistent with common seismic terminology, and ensuring all instances match this choice). We also unified the usage of “adjoint noise tomography (ANT)”.

Response letter for manuscript "Exploring the Use of Adjoint Noise Tomography to Refine a Shear-Wave Velocity Model Beneath the Iranian Plateau"

In the following, we provide detailed description of the adjustments made in response to comments by the editor, comments of the editor are shown in black and the responses are in blue.

We would like to thank the editor for the valuable and constructive feedback, which helped significantly to improve the manuscript.

- "utilizing records from broadband stations situated within the study area (Fig. 1)": Can more details about these stations be provided? For instance, are all the stations part of temporary networks, or are some permanent stations (e.g. from the Global Seismographic Network) included? Which institutions were involved in deploying these networks? Based on the legend, it looks like GSN stations (II) are included. What is the average path length between the station pairs used? Also, can citations (DOIs) for the networks be provided in the acknowledgements? In particular, the GSN should be cited.

Detailed information about the networks used to obtain the cross-correlations (CCs) in this study is provided in the data and code availability section, including the institutions that deployed the networks, whether they are permanent or temporary, and the DOI for the GSN. We have also included the average path length between the stations in the figure caption. In this figure caption the readers are also asked to refer to the data and code availability section for more information.

- In Figure S3, the caption states that the "mean velocity of each depth slice" is "written on the lower left part of the map". I do not see these velocities provided. Please correct this figure accordingly and include the mean velocities.

We apologize for this mistake. Since Figure S3 shows the percentage differences between the initial and final models, the mean velocity of each depth slice is not meaningful for this figure and has therefore been removed from the caption.

- Similarly, in Figure S6, the mean velocities of each depth slice are not included in the lower-left corner of the map. I do not see these velocities provided. Please correct this figure accordingly and include the mean velocities.

We have edited the caption of figure S6 to express that these mean velocities are only shown for the maps a-f.

- Line 294: "smaller" is misspelt as "samller"

Thank you, corrected.

- In line 121, when the starting model is first introduced, perhaps a few details about it can be provided. What is its resolution? What depth-range is it defined over? Does it constrain all of Vp, Vs, and Density, and if not, what assumptions are made about these parameters? Is the starting model isotropic or anisotropic? What is its approximate resolution?

We agree that additional explanation of the initial velocity model from Kaviani et al. (2020) would provide more context for the readers and help improve the flow of the paper. We have added a new paragraph in the Data and Methods section to explain the initial velocity model (lines 135-140).

- In Table S1, "Iteration Number" is mis-spelt as "Iteration Nummer"

Thank you, corrected.

- Also in Table S1, why is there a hyphen instead of Iteration Number 11?

Thank you, corrected.

- The authors have chosen to define the Moho via extracting the 4.2 km/s iso-surface. This choice is valid, but it should be defended by including an appropriate citation to other studies that have made a similar choice. This will help convince the readers that the choice of 4.2 km/s is common and reasonable. An example of such studies include the following:
Lynner, C., and R. W. Porritt (2017), Crustal structure across the eastern North American margin from ambient noise tomography, Geophys. Res. Lett., 44, 6651–6657, doi:10.1002/2017GL073500.

We thank the reviewer for this important suggestion. We have now added references to previous studies that used a similar velocity threshold as a proxy for Moho depth, including Lynner and Porritt (2017). Please refer to the lines 297-299.

- I think Qinya Liu might be mis-spelt as "Qynia Liu" in the acknowledgements. Please verify that the spelling is correct.

Thank you, corrected.

- Line 92: " we apply ANT to improve the pre-existing model " should be " we apply ANT to improve a pre-existing model "

Corrected in the revised manuscript.

- Line 150: "These points serve as the nodes over which the partial derivatives equations are solved": I think "partial derivatives equations" should be "partial differential equations".

Thank you, corrected.

- Line 172: "Both the EGFs and SGFs are tapered to include only Rayleigh waves.": What kind of taper is used?

Thank you for your observation. A cosine taper is used. We have revised the text so that the type of taper is now explicitly mentioned (lines 195-196).

- Line 225: "To prevent large values in the sensitivity kernels around the stations, we activated the "SAVE_SOURCE_MASK" flag in the software.":
You should clarify what this flag in the software actually does, i.e. how this prevents large values in the misfit kernels.

We agree that an explanation of this flag would help readers better understand the methodology. In the revised version, we have described the role of this flag and how it prevents large values in the sensitivity kernels around the stations (lines 249-251).

- Line 230: "High-velocity anomalies are primarily associated with the central Iranian microplates, where the crust is reported to be thinner and, therefore, warmer": Consider clarifying that the high-velocity anomalies you're referring are being interpreted to correspond to the mantle and not the crust, and that you are inferring a warmer crust because it is closer to the mantle adiabat. Otherwise, this statement is a little confusing because all other things being equal, higher velocities correspond to colder material, not warmer.

We thank the reviewer for this helpful clarification. Our intention was to indicate that the high-velocity anomalies observed beneath the central Iranian microplates likely correspond to uppermost mantle material imaged at depths where the crust is locally thinner. We have revised this sentence for more clarity (lines 254-256)

- Line 482: "mantles" should be "mantle"

Thank you, corrected.

- Line 481: "...Zagros is characterized by a thick lithosphere, whereas Central Iran has a relatively thin lithosphere and hot upper mantle." Can approximate depths be provided here in parentheses for the lithospheric thicknesses in these regions?

We have added approximate lithospheric thickness values for these regions based on the following study. Please also see lines XX.

Tunini, L., Jiménez-Munt, I., Fernandez, M., Vergés, J. and Villasenor, A. (2014). Lithospheric mantle heterogeneities beneath the Zagros Mountains and the Iranian Plateau: a petrological-geophysical study. *Geophysical Journal International*, *200*(1), pp.596-614. https://doi.org/10.1093/gji/ggu418

- Line 483: The phrase "mantles beneath the Zagros and Central Iran at shallow depths are in a comparable state." is unclear: what does "comparable state" mean in this context? Presumably, you are referring to temperature, but this should be clarified.

Thank you for pointing this out. In the revised version, we have changed the text to "comparable thermal state" for better clarity (line XX).

Editor Suggestions [2]: (Based on the current Version of Manuscript; therefore, line number references below now correspond to the current version):

- Line 23: "We perform an adjoint waveform tomography" -> "We apply adjoint waveform tomography"

Thank you, corrected.

- Line 32: "ray-theory tomography" should be "ray-theoretical tomography"

We prefer to retain the term "ray-theory tomography," as this is a standard and widely used expression in seismological literature.

- When the term "SGFs" is first used, please ensure the acronym is defined.

The acronym SGF is defined when it first appears in line XX.

- Line 103: "resolving along-strike variations in Arabian underthrusting beneath Central." This sentence feels like it is missing a word after "Central".

Thank you for pointing this out. "Iran" is missing and we have edited the text in the revised version to be "Central Iran".

- Line 163: "This choice reduces the computational burden by diminishing mesh dimensions" should this be "This choice reduces the computational burden by increasing the mesh dimensions"? My impression was that a smaller mesh would result in increased computational time, not decreased time.

We thank the reviewer for pointing this out. Our intended meaning was that restricting the inversion to longer periods allows the use of a coarser mesh discretization (i.e., larger element size and fewer elements), which reduces the computational cost. We have revised the sentence for clarity (lines XX)

- Line 208: "high wavenumber" should be hyphenated.

Thank you, corrected.

- Line 249: "non-homogeneous" can be replaced with "inhomogeneous"

Corrected as suggested.

- Please ensure that Figure 4. (Continued) is either combined with the current Figure 4 into one figure, or is renamed and referred to in the text as Figure 5.

We have combined these figures as Figure 4.

- Please ensure that Figure 5. (Continued) is either combined with the current Figure 5 into one figure, or is renamed and referred to in the text as Figure 6.

We have combined these figures as Figure 5.

- Line 385: "provides a better fit to the waveform data compared to the initial model" should be "provides a better fit to the waveform data than the initial model"

Corrected as suggested.

- Line 401: "and extends" should be "and extend"

Thank you, corrected.

- Figure S9: In the new Figure S9, the panels are too small, and the colorbar labels and axis ticks are illegible as a result. Please make each of the panels in Figure S9 much larger so that the axis labels can be read.

In the revised version, we have used higher-quality images and divided the full panel into five parts instead of two, allowing the panels to be displayed at a larger size so that the axis labels and colorbar can be clearly read.

- Line 498: "which can be associated with a magma addition at the.." Should be "which can be associated with the addition of magma to the.."

Revised accordingly.

- Line 505: "partial melts" should be "partial melt"

Thank you, corrected.

- Line 512: "In southeast" should be "In the southeast", and "Southeast" does not need to be capitalized

Revised accordingly.

- Line 524: "sub-Moho mantles" should be "the sub-Moho mantle"

Thank you, corrected.

- Line 526: "between the two lithospheres" should be "between the lithosphere", or "between the two regions of lithosphere" (lithospheres is not a valid plural here, since the lithosphere is a global layer)

Revised accordingly.

- Line 531: "while less velocity contrast" should be "while a lower velocity contrast"

Thank you, corrected.

- Line 538: "mantles" should be "mantle"

Thank you, corrected.

- Line 32: "Our study also demonstrates improvement in lateral resolution and depth sensitivity". Improvement relative to what? Maybe clarify in the same sentence that you mean relative to previous state-of-the art upper mantle tomographic studies.

In the revised version we have clarified that this improvement is relative to the initial model obtained from conventional ambient-noise tomography (lines …)

- Figure S12 Caption: "Examples of waveform fitting for a source-receiver pairs" should be changed to: "Examples of waveform fitting for source-receiver pairs" (remove the "a")

Thank you, corrected.

- Line 630: "SPECFEM3D GLOBE" should have an underscore ("_" between specfem and globe)

Thank you, corrected.