Responses to the reviewer comments on the manuscript “New insights on the fault structure of a geothermal testbed and the associated seismicity based on active seismic tomography” by Schwarz et al.

We are most grateful for the thoughtful reviewer comments on our manuscript. They will undoubtedly help improving the quality of the manuscript. Below, you’ll find a point-by-point response to all reviewer comments. The reviewer comments are printed in blue, and our responses are shown in black.

Reviewer 1

1. The title involves four keywords: fault-structure, geothermal, seismicity, and tomography. Currently, the "Introduction" section provides no information on the geological setting of the geothermal testbed, the main fault structure (MFZ), and the seismicity in the area. In my opinion, this information should be introduced briefly, clearly stating the problem statement.

   More detailed information on these topics is provided in Section 2, but we agree with the reviewer that it would be helpful to briefly introduce this in the introduction. We have modified the text accordingly, and we have also clarified the problem statement.

2. A major work is required on the figures. For example, in Fig. 1a, it is very hard to read anything in the inset figure - the same goes for the legends used to describe the geology. Fig. 1b shows the fault zone, but is not described at all anywhere in the text, only referenced in section 3 dedicated to data description (Row - 103). A proper re-writing of the figure description is also required.

   Agreed. We have improved the quality of the figures.

3. Section 5 requires a major re-schuffling in my opinion. It was very odd to read the inversion setup, then the tomography comparison between thin- and fat-rays, and finally visualize the velocity model. In my opinion, section 5.3 should be first with information on inversion setup, then 5.2, and finally 5.1.

   Here, we disagree with the reviewer. Clearly, the inversion setup (Section 5.1) needs to be shown first. In Section 5.2, we demonstrate the superiority of the fat ray approach. In our view, this should be shown prior to the discussion of the velocity model (Section 5.3), because the comparison in Section 5.2 justifies why in the remainder of the manuscript, the discussion is restricted to the fat ray results. Unless the editor insists on the sequence suggested by Reviewer 1, we would like to keep the actual ordering of the subsections.

4. More technical details are required on the tomography. Currently, it is only descriptive without full details! I suggest also showcasing the picks against offset information or a similar approach, a resolution test for the obtained 3D velocity model (checkerboard?), ray-path coverage, etc., to provide the readers more confidence.

   In the revised manuscript, we have added the technical details requested. More specifically,

we provide statistics on the travel times, and we have performed checkerboard tests. The coverage plots are the “ray-coverage” equivalent for the fat rays, and they were already included in the original manuscript.

The checkerboard tests proved to be very useful. In brief, we could show that the spatial resolution is of the order of 10 m, and in particularly well-resolved areas, it can reach 5 m. At greater depths, the resolution is, as expected, quite poor. The results from our extensive checkerboard tests are provided in form of an appendix.

5. Section 5.7 should be rewritten. Currently, it lacks a proper flow of information. There are a lot of sudden jumps between sentences without proper explanation. I could not understand what the author(s) mainly want to convey here.

   We carefully reviewed Section 5.7, and we have tried to improve the flow of the text and to clarify matters. Hopefully, the explanations conclusions are now better understandable.

6. Several claims were made in the conclusion without any discussion or proper description in the article. For example, it is being said in Row-345 that "ray tomography is less dependent on the model parametrization of the forward and inversion grids (compared with thin rays)" without a discussion in the article.

   We have revised the text and the conclusions, such that all our claims are justified or at least properly explained. Indeed, the fact that fat ray tomography is less dependent on the model parameterization requires additional explanations, which we have included in the theory section.

7. At places, a few words had been very loosely used. A few examples are: 'a remarkable spatial correlation' (Row-9), 'a nice signal-to-noise ratio' (Fig. 2), 'we show in this paper' (Row-184), 'in the middle of the volume' (Fig. 9), etc. I suggest avoiding such a form of writing unless it is clearly defined what it means.

   We have removed all these rather qualitative statements, except at a few places, where they have been used intentionally.

8. Lastly, throughout the article, several other studies done in the same site have been referenced. At times, the main outcomes of those studies are either not stated or the main information is only described. In my opinion, those results should be shown in this article rather than just referencing the readers to them, especially when we are using that information to cross-check our results. For details, please see the .pdf file

   See our responses on the comments in the PDF file.

Responses to comments in the PDF file provided by Reviewer 1

Line 14: highlighted text was removed.

Line 17: Citation added

Line 22: Sentence was reworded

Line 23: See our response to point 1.

Line 24: Changed

Line 27: Clarified

Line 42: Text changed, such that the statement is better embedded.

Line 47: Modified according to suggestion.

Line 51: The references requested appear in the following sentences. We have modified the text to address the problem.

Line 55: Clarified.

Line 56: In our view, “hectometer scale” is concise, but we have explained it in more detail. The same applies to the comment on Line 70 on the “decameter scale”.

Line 58: The citation requested is actually this paper. We have clarified this in the text.

Line 61: We have added more details in Section 2 and modified the text accordingly.

Line 72-75: We have rephrased the text.

Line 77: 1500 m are now marked in Figure 1.

Lines 81-82: Corrected.

Caption Figure 1: Re-written.

Lines 93-94: We have added the information requested.

Lines 97-98: We have added more information on the MFZ.

Line 101: Citation and clarification added.

Line 105: Rephrased.

Line 110: Changed.

Line 113: Changed.

Line 120: Changed.

Caption Figure 2: Re-written.

Caption Figure 3: Modified as requested.

Line 124: We do not understand the question. We state that the we sum the spectra of all data, which should make clear that we do not use just a single trace.

Line 130: We have clarified our statement.

Line 131: Clarified.

Table 2: Modified according to suggestions.

Line 135: Rephrased.

Line 145: More details added.

Line 165: Removed.

Line 167: See response to your comment 3.

Caption Figure 4: Modified as requested.

Line 174: Clarified.

Line 179: More details on the regularization were added.

Line 183: We have rephrased the sentence.

Line 187: More details were added.

Line 191: This is already explained in the figure caption.

Line 193: The colorbar includes gray colors in the intermediate velocity range.

Line 194: We have made this part of the text more concise.

Line 197: Clarified.

Line 203: No, we mean indeed “coverage”, as defined in the beginning of Section 5.2. For fat rays, the concept of “(thin) ray coverage” is of limited use.

Line 205: Clarified.

Line 208: No.

Line 210: We have rephrased this sentence, such that it better explains the observations.

Figure 5: We disagree and prefer the figure to be kept.

Caption Figure 6: Re-worded.

Caption Figure 8: Open questions were addressed in the revised caption.

Line 231: ATV is now explained.

Line 233: When referencing another paper, it is in our view not necessary to summarize its content. Key results of relevance for this paper are already mentioned. In order to further improve the clarity of this part of the text, we have added more details.

Line 236: We disagree with the reviewer comment, but we have made an attempt to improve the text, such that our statement is clearer.

Line 237: Rephrased.

Line 239: We do not understand, why this statement is “random”.

Line 241-244: We have rephrased this part.

Caption Figure 9: Modified as requested.

Line 254: Clarified.

Line 266-269: We have clarified this part.

Line 281: See our response to your comment on Line 233.

Lines 283-286: We have added more explanations and justifications.

Line 294: The conclusions of Rosskopf et al. (2024) are not relevant in this context. Here, we simply state that the locations are broadly consistent. We have clarified this in the text.

Line 297: Changed.

Figure 11: Horizontal slices will be added

Line 301ff: We have re-written this part of the text.

Line 336: In our view, this is not a "sudden introduction to some other random field". The purpose of this paragraph is to outline that our observations are in line with other observations at different scales. We have modified the text to make our intention even more obvious.

Line 345: See response to your comment 6.

Reviewer 2

1. The authors have done a lot of literature review, but there is a lack of information on the geological setting of the geothermal study area, fault structure, distribution of ground monitoring stations, and regional seismicity, which needs to be provided and why high-resolution imaging is needed.

   In accordance with some of the comments of Reviewer 1, we have added more information on the geology. Since the BedrettoLab is a geothermal testbed and not an actual geothermal site (we have clarified this in the text), the regional seismicity and the ground monitoring stations near the BedrettoLab are not relevant for this paper. The magnitudes of the seismic events, discussed in this study, are predominantly smaller than Mw = -3 (this information is now also added to the text), only the local geological structures are of importance.

2. There are still some deficiencies in the verification process of the results. For example, whether the total inversion parameters of the entire model match the data. Although the authors use coverage to show this, it is still unclear about the scale that can be resolved. The checkerboard test is helpful in defining the resolution scale. The verification of well logging data can only show the resolution of certain points in depth, but it cannot guarantee that the same results are maintained at other locations in the study area

   This is a valid criticism, and we have expanded these aspects significantly in the revised manuscript. Particularly, the checkerboard tests proved to be useful (see also response to comment 4 of Reviewer 1).

3. The selection of damping and smoothing parameters does not seem to be clearly explained in the text. After all, this is an important parameter for inversion.

   We have added more information on the regularization parameters.

4. Line 111: The authors explain that they can compile a relatively large data set, including 42,843 manual P-picks, with an average picking uncertainty of about 0.15 ms. What method or software is used to estimate such a result? Please explain the details.

   We have used in-house software for determining the picks. All picks were determined manually, and the accuracy of the picks was estimated during this manual picking process.

5. Line 139: Regarding the argument that the discretization for the forward modeling and the ray segment lengths for the inverse problem can be different, please provide a reasonable explanation about this argument.

   We have clarified this point in the revised text.

6. Line 170: Why was the homogeneous model (5300 m/s) chosen for the initial model? Are there no previous 1D velocity model results for this study area? After all, the 3D velocity model obtained in this manuscript appears to be layered (Lines 200~207).

   The initial velocity was determined by averaging all ray velocities (source-receiver offset divided by travel time). In contrast to surface-based tomography projects, an 1D initial velocity model is in our view not useful. Although, the geological structures have some preferential orientation, we cannot see a pronounced layering that would justify such an approach. In the modified text, have provided further justifications for the choice of our initial model.

7. Line 236: The authors state that the comparison of the 3D seismic model with the borehole logs and core shows good consistency. Is there any literature or other supporting data to verify the borehole data?

   In addition to the material already provided in Section 5.4, we have added more information and references on this topic.

8. Line 242: This manuscript observed a general decrease in velocity along the MFZ, but also a large amount of heterogeneity. This may be partly due to the limited spatial resolution of the tomography images. So how to define the spatial resolution of this 3D model? After all, the authors have used a very fine grid distribution. Although part of the reason has been explained in Section 5.6, it is still unknown how large the anomalies can be observed for the overall model.

   See our comments on the checkerboard tests that we have added to the manuscript.

9. Line 285: What is the magnitude distribution of the selected earthquake events? What is the temporal distribution of the records? Please give a detailed explanation.

   We have added more information on these topics.

10. Since this manuscript images a main fault zone with a very complex structure and strong inhomogeneity, there are still many doubts about the above inversion or verification

process, so I am reserved about the interpretation of the results. The authors need to restructure the entire article because it is written in a jumpy way and sometimes it is really difficult to understand the meaning of the text. This manuscript needs a lot of revision work to make it have enough scientific volume.

Based on your comments and those of Reviewer 1, we have re-structured, clarified and expanded large parts of the text, and we hope that the revised paper does now better reflects the main findings of our research.