Response to reviewers' comments

'Bayesian Reassessment of Seismic Moment Tensors and Their Uncertainties in the Adriatic Sea Region'

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Professor Wenbin Xu, Editor of Seismica

12 August 2025

Dear Editor,

We appreciate your handling the manuscript and the two reviewers' constructive comments and suggestions to improve it. In response to Reviewer B's main concerns, we restructured the manuscript. Specifically, we shortened the Introduction section by moving the first nine paragraphs about the tectonic background and local seismicity to a new section '2 Study Area'. We also deleted all repeated event-specific descriptions. We moved all interpretations and comparisons of results from the Results section to a new subsection of the Discussion named '6.2 Comparison with solutions from available routine catalogs.' One paragraph in the Results section was also moved to another new subsection of the Discussion as '6.3 Challenge for small events.' In addition, we rewrote the parts of the Abstract and Conclusion. One new table (Table 1) and one new figure (Fig. 10) were added from the Supplementary Material to the main text. Based on the comment from Reviewer D, a new paragraph was added in the main text to discuss the inter-parameter dependencies, and another was added in the Supplementary Material to provide details about the station-selection method. Fig. 3 was also revised as suggested by reviewer D.

In the file with tracked changes, we recorded all deletions and newly inserted texts with comments referring to related review comments. Below are the detailed responses to each review comment, where the original reviews are reproduced in italicised font, and our responses are in blue font. All line numbers in our responses below refer to those in the version with track changes unless indicated otherwise.

We hope the revision has now met the requirements for publication in Seismica.

Best Regards,

The Authors

Reviewer B:

This manuscript presents the application of a previously published Bayesian method for moment tensor (MT) inversion to seven moderate-magnitude earthquakes in the Adriatic region, an area that is tectonically complex yet underrepresented in detailed MT studies. While the technical foundation is solid and the topic is timely and relevant to the readership of Seismica, the manuscript is weakened by several structural and conceptual shortcomings. Some portions of the text are redundant or include background information that is not directly tied to the scientific objectives, obscuring the central message. The paper would benefit from a substantial reorganization to eliminate repetition, streamline the background, and clearly highlight the key findings. The use of objective comparison metrics, such as Kagan rotation angles, should be incorporated to strengthen the interpretation. Furthermore, the title and abstract should be revised to accurately reflect the nature of the study as an application of an existing method. Although the method itself is not novel, its application to new data and the potential improvements over catalog solutions - reassessing moment tensor solutions with uncertainties - represent a meaningful contribution, provided that the added value is more explicitly framed and discussed.

Response: We are grateful for your thoughtful assessment and comments and have revised the manuscript after carefully considering them. For your convenience, we summarized the key responses to your general feedback below and referred to them in the subsequent individual responses.

To streamline the structure, we shortened the Introduction section by moving the content of the tectonic background and local seismicity (the first nine paragraphs) to a new section '2 Study Area'. In this way, the Introduction is more focused on the main idea of this study, the Bayesian reassessment of MT parameters for seven moderate events in the study area. We then deleted all repeated event-specific descriptions. Third, we revised the Result section by only keeping the inversion results and moved all their interpretations and comparisons to a new subsection in the Discussion section named '6.2 Comparison with solutions from available routine catalogs' to compare and interpret our results with other studies. One paragraph in the Results section about challenges for smaller events was also moved to a new subsection in the Discussion as '6.3 Challenge for small events'. In addition, we reworded the Abstract and Conclusion by deleting unnecessary background information. We deleted all repeated event-related descriptions to make them focus more substantially on the main finding of this study.

Apart from that, we changed the title to 'Bayesian Reassessment of Seismic Moment Tensors and Their Uncertainties in the Adriatic Sea Region' as suggested by the reviewers. One new table (Table 2) and one new figure (Fig. 10) were moved from the Supplementary Material to the main text.

We address each of your specific comments in detail below. Please note that all line numbers mentioned in the response refer to the revised manuscript with tracked changes.

Below are my detailed comments, which I hope will be useful to the authors when revising the manuscript.

1) The current title is too long and does not clearly reflect the central findings of the study. A more concise and focused option, such as "Bayesian Reassessment of Moment Tensors and Uncertainties in the Adriatic Region" would better capture the main contribution of the paper.

Response: We agree with the title change to better reflect this paper's main point. Thanks for the proposed title. We changed it to "Bayesian Reassessment of Seismic Moment Tensors and Their Uncertainties in the Adriatic Sea Region".

2) The abstract is too general and lacks a clear summary of the main results.

Response: We revised the abstract in lines 35–56 to address comment#15 and make its structure more effective. Specifically, we deleted the first two sentences (lines 35-38) for the study area. We added one sentence in lines 48–50 to introduce our analysis and modified two other sentences in lines 50–56 to include the central finding. Other related modifications are also highlighted in lines 38–48. Thanks for your suggestions to improve our abstract.

3) The sections on the tectonic background and historical seismicity are disproportionately long, and much of the information are not used later in the manuscript. These sections should be significantly shortened unless they are directly related to the inversion results and their interpretation.

Response: After also considering the specific comment#16, we shortened the Introduction section in this revision by moving and shortening the tectonic background and seismicity (content before line 154) to a separate section titled '2. Study Area' as lines #213–293. We also modified lines 191–197, and lines 199–203 to make the context smooth. In the new section '2. Study Area', we retained the description of the tectonic background and local seismicity, including notable historical earthquakes, as we believe this information will benefit readers who may not be familiar with the region. We believe the modifications enhance the manuscript's structure, clarifying the main idea of this study.

4) Given the importance given to the non-double-couple (non-DC) components, their physical significance should be briefly explained in the introduction or discussion, including appropriate references. It would also be useful to discuss their sensitivity to inversion artefacts and the implications for their interpretation.

Response: Thanks; we added the suggested explanation with three appropriate references in the Introduction, as lines 161–168, to briefly explain the physical implications of the MT decomposition and their physical significance. The possible origins of non-DC are also discussed in this new content, including the inversion artifacts.

5) The descriptions of the seven events under investigation are repeated over several sections. Please streamline these sections by removing redundant event-related summaries.

Response: Thank you for pointing out these redundant descriptions of selected events. We have now only kept the event information in the last paragraph of Introduction (lines 199–206) and deleted all similar parts in other sections as highlighted in lines 385–392 for all seven events, lines 425–430 for Event 1, lines 476–480 for Event 2, lines 537–539 for Events 4 and 5, lines 604–607 for Events 6 and 7. However, we kept the description of Event 3 in lines 504–511 to justify the choice of this event.

6) The inversion parameters are listed both in the tables and throughout the manuscript. It is sufficient to list them only in the tables and to refer to tables.

Response: To reduce the repetition as suggested by the reviewers, we deleted the nodal planes parameters from the manuscript and only refer to Table 2 where it applies. The modifications are in lines 436–437, lines 485–486, lines 516–517, lines 548–549, lines 590–591, lines 616–617, and lines 636–637. We also deleted the parts of nodal planes in the Conclusion section (lines 808–840) and refer to Tables 1 and 2.

7) Table S1 should be moved to the main manuscript. Either Table 1 is expanded or included as a new Table 2 containing all results of the study.

Response: We moved Table S1 to the main manuscript as a new Table 2 in lines 409–412. Thanks for this suggestion.

8) Results and interpretation are currently mixed in section 4. Please, restructure the paper so that the Results section presents the inversion results clearly and concisely, while the Discussion interprets the differences with catalogue solutions, addresses uncertainties, and evaluates the implications for seismic hazard, regional tectonics, or catalog development.

Response: To address this comment, we revised the Results section to separate the presentation of results from their interpretation. The updated Results section now focuses exclusively on presenting the inversion outcomes for the seven events, including focal mechanism parameters, source depths, and waveform fits. All interpretations—such as differences from catalogue solutions, uncertainty considerations, etc.—have been moved to the Discussion section.

Specifically, we moved the interpretations of results in lines 440–446, lines 486–494, lines 520–528, lines 549–554, lines 591–596, lines 611–613, lines 618–622, lines 631–632, lines 634–635, and lines 638–640 to a new subsection in the Discussion section as Section 6.2 in lines 685–755 to compare and interpret our results with other studies. We believe this restructuring improves the clarity and logical flow of the manuscript, in accordance with your suggestion.

9) The manuscript contains comparisons of waveform fits, expressed as variance reduction (VR), between the authors' Bayesian solutions and catalogue solutions (e.g. INGV). However, VR is not an absolute metric and is very sensitive to filtering, structural models, time windows and pre-processing. Therefore, VR values from different inversions are not directly comparable unless the same input data and modelling assumptions are used (e.g. identical Green's functions, station distribution, etc.).

Response: Thank you for pointing it out. We missed the clarification for calculating VR in Figs. 2 and 4–9 when comparing our solutions with the INGV solutions. To ensure the VRs are comparable, we used the Green's functions (GF) from the same Earth model, at the same stations, and filtered in the same frequency band. The only difference was the source depth, which was taken from our solution and the INGV catalog, respectively. Then the predicted waveforms for the INGV solution were re-aligned to the observations by maximizing the cross-correlation coefficient. At the same time, those for our solutions were re-aligned using the inverted time shifts.

To clarify this process, we have added one explanatory sentence in lines 464–466 in the caption of Figure 2 and lines 688–689 when comparing VRs between both solutions.

10) The same applies to the CLVD values, which are compared with other solutions without clear physical or methodological justification.

Response: Our solutions for Events 3, 6 and 7 indicate the presence of the CLVD component. In particular, Event 3 included a substantial CLVD component of up to 50.9%. However, we have to acknowledge that it is challenging to investigate the origin of the CLVD component.

For Event 3 (the largest foreshock of the 2022 Mw 6.4 Petrinja earthquake), we excluded any volcanic and geothermal contributions given the regional tectonic setting. Instead, its large CLVD component may be a result of the complex source processes likely involving two or more nearby faults, considering the complex aftershock sequence of the mainshock (Herak and Herak, 2023). For Events 6 and 7, we do not have a plausible explanation yet.

In response to this comment, we added three sentences in lines 658–664 for Event 3 and two more sentences in lines 681–684 to clarify.

11) An objective metric for comparing different solutions of the moment tensor is the Kagan rotation angle. Consider including this measure.

Response: Thank you for this suggestion. We revised the manuscript by introducing the Kagan angle when comparing the solutions in the Discussion section, as in lines 689–690, line 697, line 699, line 711, line 718, line 725, and line 733.

12) Several events have significant CLVD components (for some of those report the full MT inversion results plot in the main text). While jackknife tests are used to demonstrate *robustness, the physical origin of these components is not discussed. If their origin cannot be clarified, at least plausible hypotheses should be offered or modelling limitations (e.g. unmodelled heterogeneity, localization uncertainties or 1D velocity assumptions) should be acknowledged.*

Response: In this study, we reported the presence of the CLVD component in three events, i.e., Events 3, 6 and 7. In particular, Event 3 included a substantial CLVD component of up to 50.9%. However, we did not acknowledge the challenges in investigating the origin of such a significant CLVD component. When compared with the INGV solutions, the presence of a CLVD component is consistent, though the level of significance varies. Notably, our solution for Event 3, which includes a higher CLVD component, provides a better fit to the observed data.

For Event 3 (the largest foreshock of the 2022 Mw 6.4 Petrinja earthquake), we can exclude any volcanic and geothermal contributions given the regional tectonic setting. Instead, its large CLVD component may reflect the complex source processes (likely involving two or more nearby faults), given the complex aftershock sequence of the mainshock (Herak and Herak, 2023). For Events 6 and 7, we do not have a plausible explanation yet.

Thus, in response to this comment, we added three sentences in lines 658–664 for Event 3 and two more sentences in lines 681–684 to clarify.

13) In the " Conclusions" section, the event-specific results are repeated. Instead, summarise the overall insights from the study: What does this application demonstrate? How did your approach improve the catalogue moment tensors? What are the implications for regional catalogs or hazard models? What is the perspective for applying this method in this area?

Response: Thanks for this suggestion. We revised the Conclusion section to focus on the main findings of this study and deleted all repeated event-specific results in lines 806–840. Accordingly, four sentences are added in lines 799–806 to summarize the main results from the inversion. The other five sentences are added in lines 843–854 to highlight the comparisons with the existing solutions, demonstrating its feasibility and potential improvement.

14) Many waveform-fit plots could be moved to the supplementary material if they are not covered in the main text. Conversely, the outcome diagrams for the two events with the most significant CLVD components should be included in the main text.

Response: We acknowledge this comment, and we have made the following revisions: we have retained the waveform-fit plots in the main text, as they are essential for demonstrating how well our solutions explain the observations compared to existing ones, and they are cited in the new subsection 6.2. This is particularly important given that the origin of the non-DC components remains unresolved; the quality of the waveform fit provides additional justification for the robustness of our results.

However, we deleted the waveform-fit subpanel (c) in Fig. S8. We moved it to the main text as Fig. 10 (lines 665–670) because the inverted solution for Event 3 contains the most significant CLVD, and the large component of CLVD is robust in the Jackknife test. Accordingly, the numbers of previous Figs. S9–S14 changed to Figs. S8–S13 as in lines 112, 118, 122, 128 and 132 in the Supplementary Material and lines 651, 657, 672, 673, 774, and 780 in the main text.

Specific details:

15) Abstract: A more effective structure could look something like this:

We apply a Bayesian moment tensor inversion to seven earthquakes of moderate magnitude (Mw 4.2–5.4) in the Adriatic region, which includes parts of Croatia and central Italy. Our analysis includes full posterior distributions to quantify the uncertainties in the source parameters, including the focal mechanism, depth and non-double-coupled components.

The results are compared with published solutions from regional catalogs. .... We find .... This study shows that ...

Response: Thank you for this specific suggestion to improve the abstract. To make the abstract's structure more effective, we deleted the first two sentences for the study area in lines 35–38. We added one sentence in lines 48–50 to introduce our analysis and modified two sentences in lines 50–56 to include the central finding. Other related modifications are also highlighted in lines 38–46.

*16) **Introduction:** Could start around line 145, summarising the earlier content in a "study area" section. Introduce the relevance of non-DC components if they become a focus later.*

Response: To shorten the Introduction, we moved the content located before line 154 into a new section titled '2. Study Area' (lines 213–293). We retained the description of the tectonic background and local seismicity, including notable historical earthquakes, as we believe this information will benefit readers who may not be familiar with the region. Additionally, we have included a brief explanation of the physical implications of the non-DC component in the Introduction (lines 161–168). We appreciate this helpful suggestion.

*17) **Lines 158:**"Theory error" is unclear terminology; suggest rephrasing*

Response: Thanks for this comment. We use the terminology from other papers such as Dettmer et al., 2007 and Vasyura-Bathke et al., 2021. To make it clearer, we modified it as 'the error due to imperfect theory' in lines 175–176 and revised that sentence in line 176.

*18) **Lines 188–196:** Repetitive content; remove or relocate to methods.*

Response: This part (lines 297-303) was moved to Section *4 Data preparation* in lines 372–375 as suggested in comment#20.

*19) **Line 261**- The term "2D structural model" is used incorrectly. Using two separate 1D velocity profiles does not constitute a 2D model.'*

Response: Thanks for pointing it out. We modified the term as 'a composite structural model' as in line 371.

*20) **Lines 262–270:** Discuss velocity models (lines 191-196) here.*

Response: Thanks. We moved lines 297–303 here and modified them to fit the context, as seen in lines 372–375.

*21) **Lines 272–279:** Repetitive, remove. Pls, check for repetition all over the manuscript*

Response: This part was deleted in this revision as highlighted in lines 385–392.

*22) **Lines 308–313:** Event descriptions should be consolidated in only one section. Also, many details are not relevant to the inversion goals.*

Response: Thanks for this suggestion. This part was deleted (lines 425–430), also based on comment#5. In this revision, the event descriptions with basic information are only retained in the last paragraph of the Introduction, as lines 199–206.

23) Line 333: Clarify what the observed feature implies (e.g., for source complexity or model bias).

Response: This is a fair comment. However, the reason for inter-parameter linear dependency remains unclear in this study. While such dependencies appear weak for Event 1, they are more pronounced in Event 7, as shown in Fig. S7. To address this, we added a paragraph in the Discussion section (lines 749–755) acknowledging the unresolved origin of these trade-offs and suggesting that they may be related to model complexity and depth-dependent resolution limitations in the inversion process.

*24) **Lines 443–456:** Move this content to the discussion. Check also the other paragraphs that need to be moved to the discussion.*

Response: This paragraph was moved (as highlighted in lines 568–581) to the Discussion section as a new subsection 6.3 (lines 756–770). Thank you for the suggestion to streamline the Results and Discussion sections.

*25) **Line 574:** Pls, pay attention to the coherence between the number of decimal places in the reported magnitude and its uncertainty. However, consider whether reporting orders of magnitude to three decimal places is justified given the typical precision of such estimates.*

Response: Thanks. We adjusted the number of decimal places to three in the reported magnitude and its uncertainty as shown in Table 2 in lines 409–411. In this study, parameter uncertainties are derived from the ensemble of solutions during the convergence stage, where the posterior probability peaks and fluctuates around its maximum. The uncertainty in the moment magnitude is found to be very small with three decimal places. Therefore, we kept three decimal places for both the value and its uncertainty.

Reviewer D:

Comments to paper by Hu et al. "Toward Improved Seismic Source Parameter Estimation with Integrated Handling of Data and Model Uncertainties: A Case Study of the Adriatic Sea and Surrounding Regions in Croatia and Italy"

The paper is interesting and valued for me from two reasons at least: (i) it represents an illustrative application of advanced inversion methodology within the hierarchical Bayesian framework developed by Hrvoje Tkalčič and his coworkers, and (ii) it uses a useful and inventive approach to select the set of 'prospective' stations to be inverted from the number of all available. Then, advantageously it avoids a corruption of the results due to presence of a low-quality data and, at the same time, optimizes the distribution of the observations around the source. The approach is briefly described in the 'Data preparation" chapter together with the relevant references, as a reader of the article I would however welcome a more detailed treatment applicable as a direct receipt how to do it in an own case study.

Technically, it is an excellent job – the mechanisms are retrieved by the comprehensive method reliably with a good confidence. The fault plane solutions are compared with the set of seismogenic structures listed in the EDSF database. Here the treatment ends. The Authors themselves declare that the aim is to demonstrate that a novel and high-quality catalogue of source parameters of earthquakes in the area under study can be established. It was done convincingly. A more interpretation would however enhance the impact of the research and would make it more appealing for the reader. First of all, the case of a large (and reliably determined) non-shear component within the focus of the foreshock of the 2020 Petrinja earthquake: how could it happen within a tectonic event? Nevertheless, I agree with the Authors that the issue is beyond the scope of the paper. Let's be looking forward to its follow-up.

Response: We sincerely thank you for your positive assessment and constructive comments on this study. We have revised the manuscript carefully considering your suggestions, and we believe that the updated version satisfactorily addresses your concerns. Please note that all line numbers mentioned in this response refer to the revised manuscript with tracked changes.

In response to your comment regarding the station selection method described in the 'Data Preparation' section, we added a detailed explanation in the Supplementary Material (lines 19–35), which is now cited in the main text (line 360).

We also appreciate your understanding of the challenges in investigating the origin of the non-double-couple components in Events 3, 6, and 7. For Event 3 (the largest foreshock of the 2022 Mw 6.4 Petrinja earthquake), we can exclude any volcanic and geothermal contributions given the regional tectonic setting. Instead, its large CLVD component may reflect the complex source processes (likely involving two or more nearby faults), given the complex aftershock sequence of the mainshock (Herak and Herak, 2023). For Events 6 and 7, we do not have a plausible explanation yet.

Thus, to clarify our interpretation, we added three sentences in lines 658–664 for Event 3. We also have added some additional explanation in the Discussion section (lines 681–684).

Below, we provide detailed responses to each of your specific comments.

In the following, there are several points (of different weights) which the Authors might consider in revising the paper.

1) p.5, l.142 „...the relative movement between the blocks of rock (i.e., the rake angle)...“This is inexact: rake angle is one of three angles which together describe the movement.

Response: We moved the tectonic background and regional seismicity to a new section '2. Study Area' (lines 213–293) based on the comments from another reviewer. In this new section, we deleted this paragraph to shorten the tectonic background because we do not provide their implications from our results.

2) p.6, l.149-152 A comprehensive overview of MT decompositions ever used and the explanation why just that one into ISO, DC and CLVD has been accepted universally is in Julian et al, Rev. Geophys., 36, 1998, 525-549.

Response: Thanks for reminding us of this paper. We added one sentence in lines 168–169.

3) Fig.2 caption "...and the eight stations (triangles)." There are 12 stations in the plot.

Response: This error is fixed as in line 454; thanks.

4) Fig.3 Even with the highest zoom available, the cross-distributions are nearly un-legible: are there isolines or shades of grey or both? And the scale of the posterior probability (are the maxima deep or shallow)? The PP on the main diagonal should be markedly higher than for the cross-terms. Wouldn't it be helpful to add a zoom of, e.g., 3 rows of the upper vertex of the triangle and add the scales there? There is a plenty of space next to the triangle in Fig.3! At the same time, also the mentioned linear dependency in the M_{xx} vs. M_{xy} case would be better to grasp. By the way, I see more of cases like this in the triangle, e.g., k_9 vs. M_{xx}, possibly also some others in the k_9 row. But the ellipses are not slim enough and surely the phenomenon is not worth of further exploration.

Response: Thanks for your valuable suggestion to improve the figure. To clarify, we added two sentences in the caption of Fig. 3 as highlighted in lines 471–474. As you suggested, we added a magnified plot for only five MT parameters in the upper right corner of Fig. 3 to help the readers understand this figure. For the linear dependencies between parameters, their underlying causes remain uncertain. To acknowledge this, we added a paragraph to the Discussion section (lines 749–755). We proposed that the strong linear dependencies might be attributed to inversion resolution and model complexity with the increased number of parameters, particularly with the presence of non-DC components in Event 7 (Fig. S7).

5) p.15 l.323, p.18 l.365 For the value of the fit (variance reduction) the choice of the set of stations used is critical. In the INGV evaluation, the set was probably different, partly at least.

Response: We missed the clarification for calculating VR in Figs. 2 and 4–9 when comparing our solutions with the INGV solutions. To ensure the VRs are comparable, we used the Green's functions (GF) from the same Earth model, at the same stations, and filtered in the same frequency band. The only difference was the source depth, which was taken from our solution and the INGV catalog, respectively. Then the predicted waveforms for INGV solution were re-aligned to the observations by maximizing the cross-correlation coefficient. At the same time, those for our solutions were re-aligned using the inverted time shifts. To clarify this process, we have added one sentence in the caption of Figure 2 in lines 464–466 and explanatory sentences in lines 688–689.

6) Fig.S4 The large positive time shift at the stations CR.DBRK and MN.BLY is strange. The Authors hypothesize about a mislocation effect, a mislocation however should cause negative time delays at the stations on the opposite side across the epicenter (IV.TRTR, IV.SGG, IV.MRVN), which is not seen.

Response: Thanks for this comment. We use the station-specific time shifts to account for the imperfect Earth model. Therefore, station-specific time shifts should exhibit a pattern to reflect the structural error, even though the location is correctly solved. For Event 6 and Event 7, where the same Earth model is used as Event 4, most station-specific time shifts are positive. Therefore, the positive time shifts are common in this region. When the mislocation occurred in the case of Event 4, stations CR.DBRK and MN.BLY required more time shifts, while stations on the opposite side required negative time shifts. However, when combined with time shifts from structural error, their overall time shifts may also be small positive, as seen in station IV.TRTR and IV.MRVN. Lines 560–563 were revised to clarify.

7) p.28 l.513-514 The waveform fit is so high that it is puzzling. The sentence "Such a good waveform fit could be caused by the shallower source depth based on the probability variation as the depth in Fig. 9(b)." however does not make sense to me.

Response: In response to another reviewer's comment, we have moved the interpretation of results to the *Discussion* section and kept only the concise inversion results in the *Results* section. Regarding your specific comment, we rephrased the confusing statement. The log-probability shown in Fig. 9(b) serves as a proxy for waveform fit, weighted by the data noise covariance matrix. As illustrated, the highest log-probability corresponds to a source depth of 3 km. However, the depth yielding the highest log-probability does not always align with the highest waveform fit as measured by VR in this study. Therefore, we revised the statement to clarify that the high waveform fit is explained using the actual VR values for the corresponding depths, as highlighted in lines 745–748.

8) p.28 l.516 The issue of the linear dependency seen in the posterior distribution of some of the inversion parameters is interesting. Several cases are seen in the columns related to M_{xz} and M_{yz} in Fig.S7 (for the M_{yz}, and for t_1, t_3 and t_4 parameters, possibly also for some others), can they be related to a decreased resolution for the shallow event? There are however some more ellipses seen (the k_3 row) which are obscure to me; have the Authors a hint to explain them?

Response: Thank you for this comment. While we do not have a definitive explanation for the observed linear dependencies between parameters, the decreased resolution associated with shallow events— as you suggest— is a plausible contributing factor, particularly since such dependencies are not observed in deeper events (e.g., Events 1, 3, 5, and 6). Additionally, the presence of significant non-double-couple components in this MT solution may further increase the model complexity, making it different from the other shallow events at similar depths, such as Events 2 and 4.

To address this point and comment#23 from another reviewer, we have added some clarification in lines 749–755 as a separate paragraph.

9) p.29 l.532-533 Concerning the event 6, the CLVD presence does not seem to be completely robust: in Fig.S11 it fluctuates from nearly zero to about 50%. The trials yielding very small values are fairly numerous – are they prevailingly related to cases of discarding many stations from the set of 8 for this event? Similar scatter is seen for event 7 in Fig.S13. How many stations were discarded at the most for events 6 and 7, where the total set consists of 8 stations?

Jan Šílený, Inst. Geophysics, Czech Acad. Sci., Prague

Response: We agree that the presence of a significant CLVD component for Event 6, as shown in Fig. S10, may not be as robust as initially expected. A likely explanation is the reduced azimuthal coverage caused by randomly discarding 1 to 3 stations from the original set of 8 stations used in Fig. 8(a). The effective number of stations (ENS) for the whole dataset is 6.57, indicating a non-uniform station distribution (an ideal ENS would be 8 for uniform coverage). For the sensitivity test, we performed a series of inversions by randomly removing one station (8 trials), then repeated the process for two and three stations, resulting in a total of 24 inversions. During the test, 3 stations were discarded at the most and 1 station was dropped at the least. Therefore, the coverage could become very poor, particularly for dropping 3 stations. Consequently, the inversion results fluctuate as shown in Fig. S10. Despite this, the outcomes are still dominated by solutions yielding a relatively large CLVD component. A similar pattern is observed for Event 7 in Fig. S12.

To clarify, we added one sentence in lines 674–676 and modified the sentence in lines 677–679.

References:

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Vasyura-Bathke, H., Dettmer, J., Dutta, R., Mai, P. M., & Jónsson, S. (2021). Accounting for theory errors with empirical Bayesian noise models in nonlinear centroid moment tensor estimation. Geophysical Journal International, 225(2), 1412–1431. https://doi.org/10.1093/gji/ggab034

Herak, M., & Herak, D. (2023). Properties of the Petrinja (Croatia) earthquake sequence of 2020–2021 – Results of seismological research for the first six months of activity. Tectonophysics, 858, 229885. https://doi.org/10.1016/j.tecto.2023.229885