nature portfolio

Peer Review File

Seismological evidence for fluid accumulation beneath the dormant East Eifel volcanic region, Germany

Corresponding Author: Dr Hao Zhang

This manuscript has been previously reviewed at another journal. This document only contains information relating to versions considered at Communications Earth & Environment.

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The major claim of this paper are that secondary shear wave phases within the Ochtendung Seismic Zone are generated by reflections at a depth of 12-14 km, from a low-velocity zone which represents fluid accumulation, preferably magmatic fluids.

The nature of the seismicity in this region has been debated. It is important that analysis of seismic waves are supported waveform and radiation patterns. If the SxS phase is an reflection from a low-velocity zone, the S-SxS travel time difference should vary along the Ochtendung seismic Zone.

Figure 1e shows one of these events on five stations. I presume that it is one of the horizontal components. The P-phase seemingly arrives simultaneously on stations TB099 and TB144 from this C2 event, however the S-SxS traveltime between them is markedly different.

In order to verify the reflective moveout of the SxS phase the authors should provide composite record sections at different stations from the array. I would line up events with good signal-to-noise ratio on each station showing all three components (Z, R, T). Such a record section can also eliminate the possibility of the S being a SP phase and the SxS the direct S phase.

• Are the claims novel? Yes, to my knowledge this reflective phase has not been described previously.
• If not, please identify the major papers that compromise novelty.
• Will the paper be of interest to others in the field? yes
• Will the paper influence thinking in the field? yes
• Are the claims convincing? I am not totally convinced and thus suggest that the authors provide composite record sections of the SxS phase at different station in order to verify its origin. The authors also need to identify the components shown on their seismic figures. Vertical, radial or transverse.
• Are there other experiments that would strengthen the paper further? How much would they improve it, and how difficult are they likely to be? Providing three-component plots of the data would definitely strengthen their main claim of the paper. Some of these might be added to the supplementary and should not be very time consuming.
• Are the claims appropriately discussed in the context of previous literature? Yes, to my knowledge they are.
• If the manuscript is unacceptable in its present form, does the study seem sufficiently promising that the authors should be encouraged to consider a resubmission in the future? I recommend moderate revisions.
• Is the manuscript clearly written? If not, how could it be made more accessible? Yes, but many aspects should be better clarified, see my specific line-numbered comments.
• Could the manuscript be shortened to aid communication of the most important findings? Chapter 2 is rather diffuse to read and conclusions are missing. The authors need to separate the general discussion from their specific results and add a few concrete conclusions from this particular study.
• Have the authors done themselves justice without overselling their claims? yes
• Have they been fair in their treatment of previous literature? yes
• Have they provided sufficient methodological detail that the experiments could be reproduced? Not applicable.

• Is the statistical analysis of the data sound? Not applicable.
• Should the authors be asked to provide further data or methodological information to help others replicate their work? (Such data might include source code for modelling studies, detailed protocols or mathematical derivations). Not applicable.
• Are there any special ethical concerns arising from the use of animals or human subjects? No

Comments related to specific parts of the paper.

Enlarge Figure 1a and place other figures beneath it. Focal mechanisms are a bit small. I suggest you enlarge them and place outside of the seismic zone with a line indicator. Some events in C2 seemingly have a weak second phase. What components are shown in Figure 1cd, radial or transverse? You should provide a record section in 1e of these components.

Comments below marked by linenumbers.

49-50 Specify the type of strike-slip focal mechanisms along the OsZ.

57-60 Add referenced related to debated theories on the origin of the OSZ seismicity.

62-64 “Thanks to the unprecedented..” rewrite! The dense seismic network greatly improved accuracy of earthquake locations within the OSZ.
Specify what you mean by “novel insights” add a sentence on the upper crustal structure based on the tomography results.

66-67 “reliably located” what are the location errors!

90 explain/reference “time-field migration technique”

104-109 How many focal mechanisms do you have in C1? only one is shown on Figure 1, You should mention that the strike-slip events are left-lateral along the OSZ and the reverse FM is likely related to the Siegen thrust.
What components are shown on Figure 1bc? The P-S time is seemingly same for C1 and C2 in spite of station TB099 being markedly closer to C1. Explain!

128-130 Based on the station distribution and shear wave polarization you should try to evaluate radial anisotropy within your array. Do you see a difference in shear-wave attenuation between C1 and C2?

139-147 As vertical P and PxP phases are not prominent on horizontal components, you should include a vertical record section, i.e. with Figure S3. Indicate what the components are on your figures.

160-162 ..”limited similarity to the direct S-wave” frequency? scattering? specify!

165-166 be more specific, how would the geometry of the reflecting zone change the waveforms? Just as radial anisotropy serves as an important parameter to understand asthenospheric flow, it may help constrain the nature of the reflected shear wave.

195 What reflector imaging results are you referring to? Should your Methods, chapter 3 not be ahead of chapter 2?

207 explain what is meant by secondary boiling, what is being boiled and where?

231 specify the station (TB112)

263 I would use the word suggest instead of speculate.

Your conclusions are missing!!!

Reviewer #2

(Remarks to the Author)
The manuscript presents direct seismological imaging of localized mid-crustal reflectors at ~12-14 km depth beneath the Ochtendung Seismic Zone (OSZ) in the East Eifel volcanic field, interpreted as zones of magma-derived fluid accumulation within crustal weakness structures. The imaging is based on secondary S-wave phases (SxS) extracted from local microearthquake waveforms and migrated using an eikonal-based, reverse-time-migration-like approach. The authors link the imaged fluid bodies to the persistent burst-like microseismicity observed ~1-2 km above them, proposing that episodic deep fluid influx drives slip reactivation on pre-existing structures, which they frame as an example of natural hydraulic fracturing. The imaging result is convincing and the conceptual model is consistent with other available observations in the region.

The work is sound and should be of interest to a wide audience spanning volcano seismology, crustal fluid mechanics, and intraplate seismotectonics. I see no flaws prohibiting publication. However, several aspects of the methodology, figure presentation, and interpretive framing would benefit from minor revision, as detailed below.

- Stations used for imaging: It is unclear which stations were used for the imaging. Figure S4 shows a dense station distribution with only 2 stations labelled, and the caption suggests these were the only 2 stations used for seismic analysis, but the example in Figure 3c suggests 18 stations were used. Please clarify in Figure S4 or Figure 1a which stations were used for the imaging. In Figure 1a, the station symbols are too faint and very hard to see. It would also be useful to plot in Figure 3e a projection along the profile of the stations used for the reverse-time migration.

- Template matching detection: The authors mention template-matching was used to find additional events within a seismic burst (Figure 2g). Please clarify:
Was template-matching applied only to this particular sequence, or to the full dataset?
Are the 158 earthquakes used for imaging drawn exclusively from the relocated tomography catalogue, or do they include template-matching detections?
If template-matching events are included, how were they relocated relative to master events? Were time-delay measurements used?
How many additional events did the template-matching algorithm detect across the full dataset?

- Uncertainties on earthquake hypocentres(Figure 3e): What are the uncertainties on the earthquake hypocentres? Are they large enough to be visible at this scale? Also please indicate the vertical exaggeration in the figure caption.

- Events below the reflectors: In Figure 3e, several events at distances > 7 km along the profile appear located below or within the fluid accumulation zones. Please comment on whether the depth of the reflectors or events in this SW portion of the profile is reliable (i.e. their relative position is larger than associated location uncertainties) and if yes, how does that change their interpretation of the SxS phase paths? Are these events associated with the residuals outliers in Figure S6?

- Time delay uncertainties: Please comment on the time-delay uncertainties given that the stacked spectral-energy curves have a resolution dependent on the bandwidth used. For stations were a high similarity between the direct S and SxS phases is observed (station like TB112), a waveform-based time delay calculation could have given potentially increased subsample precision. It would be useful to discuss the undertainties of these time delays and how they translate into uncertainties on their reflector depths.

- The introduction notes that mantle-He observations in CO_2-rich wells support a deep mantle-derived fluid source. Figure 1a appears to show higher mantle-He concentrations northwest of the Siegen Thrust surface trace, rather than directly above the imaged fluid accumulation along the OSZ. Please comment in the discussion on how this spatial offset is consistent (or not) with the conceptual model. If the fluid pathway is structurally controlled by the Siegen Thrust (as suggested in Figure 4), some lateral offset between the surface degassing signature and the mid-crustal accumulation is plausible, but the argument could be made more explicit in the text and figure 4.

- Figure 4 comment: Red arrows appear to show fluid migration pathways along the Siegen Thrust, while blue wiggly arrows also indicate fluid pathways. Please clarify whether the different colours are intended to represent different fluid compositions, phases or pathway types. As drawn, this is ambiguous.

- Framing of the "natural hydraulic fracturing" analogy: The way section 2.3 is written seems to position industrial injection-induced seismicity (Oklahoma wastewater injection, Preese Hall shale-gas hydraulic fracturing) as the reference case and the OSZ activity as a natural analogue, which appears to me somewhat awkward. The characteristics of the C2 seismicity (small magnitude, no mainshock, closely-spaced events, episodic reactivation) as evidence of fluid-driven earthquake swarms are well recognised in the volcano-seismology literature. Slip reactivation due to increased pore pressure along pre-existing fractures is a well-known mechanism. These natural observations of fluid-driven earthquake swarms could be better referenced in section 2.3. For example, several of these papers could be relevant to discussing natural hydraulic fracturing:

Hill, D. P. (1977), A model for earthquake swarms, J. Geophys. Res., 82(8), 1347–1352, doi:10.1029/JB082i008p01347.

Foulger, G. R., Julian, B. R., Hill, D. P., Pitt, A. M., Malin, P. E., & Shalev, E. (2004). Non-double-couple microearthquakes at Long Valley caldera, California, provide evidence for hydraulic fracturing. Journal of Volcanology and Geothermal Research, 132(1), 45–71. DOI: 10.1016/S0377-0273(03)00420-7

Alicia J. Hotovec-Ellis et al., Deep fluid pathways beneath Mammoth Mountain, California, illuminated by migrating earthquake swarms.Sci. Adv.4,eaat5258(2018).DOI:10.1126/sciadv.aat5258

Parotidis, M., S. A.Shapiro, and E.Rothert (2005), Evidence for triggering of the Vogtland swarms 2000 by pore pressure diffusion, J. Geophys. Res., 110, B05S10, doi:10.1029/2004JB003267.

In conclusion, the imaging result is compelling and the interpretation is broadly consistent with independent constraints. I believe the points above can be addressed without major additional analysis, and doing so would strengthen what is already an interesting manuscript.

Reviewer #3

(Remarks to the Author)

Zhang et al. used S-wave phases observed in microseismicity near the dormant East Eifel volcanic region in Germany to map a local fluid-saturated zone in the upper crust. My expertise is in detecting, locating, analysing and interpreting microseismicity.

Overall, I believe the manuscript could be greatly improved through text revision and restructuring of the figures. Each figure has an enormous amount of data and detail, but they are all challenging to decipher as presented. Due to this and some lack of detail and consistency in the language within the text (e.g., long sentences or sentences that are vague), I find that the manuscript is difficult to follow at times. In particular, I found it very hard to follow almost all of Section 2.3. I provide feedback below in order to improve the presentation and the readers' understanding of the setting, data, and results.

Major points
Each Figure in the main text has too many panels that makes it hard to understand. I would recommend that the figures be restructured, and the panels that do not necessarily need to be in the main manuscript can become supporting figures. See comments below for more on this.

How does the fluid-rich zone (~ 10 km2) compare to the tomographic results of reference [19]? How does the Vp/Vs ratio distribution compare to the fluid-rich zones determined by the SxS phases? The Vp/Vs anomaly, unless I missed something, is only compared in Figure 3 briefly. It would be interesting to make a more qualitative comparison between the two. Would it be possible to include the Vp/Vs in a cross-section, for example?

Given that the depth estimates of these fluid-rich zones are dependent on the hypocentres of the seismicity, it would be great to provide more information on the quality of these locations (i.e., how robust they are). See comment below for line 67.

L 128-130: I am not sure I follow what the reason is suggested by the authors here on why the reflected SxS phase has higher amplitudes than the S phase. I think this is a very crucial point that needs to be carefully addressed. Would the type focal mechanism of the events (i.e., radiation pattern) analysed (e.g., geometry between waves and the location of the receiver) be a potential reason for this?

L 203-211: Are there any references to support this section of the text and the formation of these fluid-rich zones? Any other examples of similar structures around the world? If I may attempt a question far out of my expertise in relation to this point, please correct me if I am mistaken. Is the K-horizon feature, mostly observed with active seismic data, in the Tuscan magmatic field in Italy, a similar structure to what you observe?

Minor points

L 14: "only 13 ka ago"... (perhaps the authors mean in terms of geological time here; it would be good to clarify the text).

L 18: "active subsurface processes." It would be more helpful if the specific kind of subsurface processes were mentioned here, as at this form the sentence is a bit vague.

L 18-19: What time period do these 500 events cover? I think it might be a good idea to include this info in the abstract.

L 27: "... as burst-like sequences reflecting rapid, successive ruptures." This sentence is slightly repetitive, I think. Perhaps omit the rapid successive part or burst-like?

L 32: hosts a major Cenozoic or hosts one of the largest Cenozoic

L 45: "Since the last century," This can be a bit more specific.

L 57: "no pronounced surface deformation has been detected." Using what kind of information?

L 58: "limited the observations." What type of observations?

L 59: "remains debated". This implies that there are studies that propose different interpretations about the volcanic activity. Perhaps citing some of these studies and stating briefly how they differ might help with setting the background.

L 62: "495 station locations was deployed..." Perhaps 495 seismometers were deployed?

L 63-65: This sentence is very generic. I would suggest that it becomes more specific about what types of insights were achieved with the denser network of seismometers, or most of this can be omitted and merged with sentences either before or after.

L 67: Please specify what reliable located means. E.G., provide some additional information like what the hypocentral uncertainties are, distances to closest stations, number of picks, etc., or whatever the authors think is relevant here. But I think it would be great to provide more information here as the accuracy of the hypocentral depths would greatly affect the fluid-rich zone depth estimates.

L 84: omit time

L 94-95: "would be caused". Is likely caused?

L 95-97: The phasing here is slightly awkward and would benefit from some re-writing, perhaps. For example, it is not that clear how important the comparison is to human activities.

L 101: The first sentence can be omitted as it is a bit vague.

L 103: "two kinematically divergent clusters". Not very clear what that means until you read the following few sentences. I would suggest simplifying to "two distinct clusters"

L 109: Figure 1 b does not exist. Please modify the text accordingly.

L 136: Figure S2. The caption of the figure is not complete.

L 162-164: I understood that the seismometers used in this study do not consist of the same instruments. Could the type of sensor also be one of these factors?

L 168-169: Might be worth including in the supporting information some of these examples near LSV with long codas.

L 176: Figure 3c is not mentioned in this section.

L 187-191: The Vp/Vs show the opposite trend, getting deeper moving from NW to SE. What does that mean regarding the SxS phase imaged fluid saturated zone?

L 215: What are some of the proposed mechanisms controlling seismicity at OSZ according to these studies? It would be nice if these were introduced at least.

L 223: "presumed maximal horizontal stress"

L 227: Omit Collectively

L 227-228: "a localized and persistent forcing mechanism." Not very clear what the authors mean here. This has to be more specific.

L 233: Any citation for the template matching approach?

L 242-253: I found this part of the text kind of hard to follow. It feels long and repetitive. In particular, lines 242 to 248 could be either omitted or significantly shortened. Following this, the part mentioning that elevated pore pressures are capable of driving slip not just in industrial settings is confusing. There are numerous studies that have connected natural seismicity with changes of pore fluid pressures in the upper crust. Apologies if I misunderstood something here.

L 260: Is the maximum horizontal compressive stress orientation SHmax introduced in the text before?

L 283: What is the exact orientation of SHmax in the region and how is it calculated? Please cite the paper that calculated this.

L 254-289: Similar to the previous part, I found this part of the text hard to follow and at times repetitive. I recommend that this part be modified significantly for clarity.

Suggestions on figures

Figure 1: Figure 1 b does not exist. Current panels e and f could be moved to the supporting information.

Figure 2: Panel g can be moved to the supporting information.

Figure 3: Panel c is not mentioned in the main text. My opinion is that the most important panels here are d and e so I would recommend that only these two remain here and the rest of the panels are moved to the supporting information.

The authors have generally made good choices with their color scales throughout the figures. One exception is Figure 3, panel d, where the jet colormap is used. This colormap is perceptually non-uniform and can introduce visual artefacts that misrepresent the data, and it is also not colorblind-friendly. I would encourage the authors to consider replacing it with a perceptually uniform, colorblind-accessible alternative (e.g., viridis, batlow, or oslo). Several excellent resources on this topic are listed below for reference:

Please refer to the following links for more details on color scales:
- https://peterkovesi.com/projects/colourmaps/

- https://blogs.egu.eu/divisions/gd/2017/08/23/the-rainbow-colour-map/
- https://www.climate-lab-book.ac.uk/2016/why-rainbow-colour-scales-can-be-misleading
- https://www.fabiocrameri.ch/visualisation/

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

I have read the revised manuscript and ackowledge the authors response to my previous concerns.
The text has been clarified and reads well.
Below I list my comments/corrections line-referenced to the revised manuscript.

line 84 delete or explain what is meant by “after eruption”
lines 92-94 delete "In this study, we provide the first"
To minimize "we this" and "we that" I suggest
Seismological evidence for localized fluid-rich bodies in the mid-crust beneath the East Eifel, HAS BEEN inferred from….. or
We provide seismological evidence for localized fluid-rich bodies in the mid-crust beneath the East Eifel, inferred from…..

My comments below mostly refer to incorrect Figure references, which need to be checked throughout the paper.
97 change these volumes to these bright spots
142 change Fig. 2d,f to 2d,e
148 change Fig. 2d to Fig. 2f
change (d,f) in the figure text to (d,e)
169 change As shown in Fig. 1e,f (see more in Fig. S6), to As shown in Figs. 1e,f and S6,

For easier comparison I suggest you add a simple length scale to Figures 3c and 3d

210 Figure 3f does not exist
322 I presume you are referring to Fig. 2g here
336 presume you are referring to Fig. 1f instead of 1e here
342 no need to repeat reference to Fig 2c here.

Reviewer #2

(Remarks to the Author)
I thank the authors for carefully and thoughtfully addressing all reviewer comments, including mine. My concerns have been addressed, and I have no further comment. I recommend publication of this revised manuscript as is.

Reviewer #3

(Remarks to the Author)
Zhang et al. have been very meticulous in addressing the detailed point-by-point reviews. I’ve read through their responses and the annotated text. The clarity of the paper has improved and I’m happy with how the comments are addressed.

Response Letter (COMMSENV-26-1425-T)

Potsdam, 21 May 2026

Dear Editors and Reviewers,

We thank you for your careful evaluation of our manuscript and for your constructive comments. We sincerely appreciate the time and effort that all the editors and reviewers spent on our draft. Here we submit a revised manuscript together with a marked version in which all revisions are highlighted in color. We look forward to your further consideration.

Sincerely yours,

Hao Zhang

on behalf of the authors

The following are our responses (in blue) to the reviewers' comments (in black).

Reviewer #1

The major claim of this paper are that secondary shear wave phases within the Ochtendung Seismic Zone are generated by reflections at a depth of 12-14 km, from a low-velocity zone which represents fluid accumulation, preferably magmatic fluids.
The nature of the seismicity in this region has been debated. It is important that analysis of seismic waves are supported waveform and radiation patterns. If the SxS phase is an reflection from a low-velocity zone, the S-SxS travel time difference should vary along the Ochtendung seismic Zone.
Figure 1e shows one of these events on five stations. I presume that it is one of the horizontal components. The P-phase seemingly arrives simultaneously on stations TB099 and TB144 from this C2 event, however the S-SxS traveltime between them is markedly different.
In order to verify the reflective moveout of the SxS phase the authors should provide composite record sections at different stations from the array. I would line up events with good signal-to-noise ratio on each station showing all three components (Z, R, T). Such a record section can also eliminate the possibility of the S being a SP phase and the SxS the direct S phase..

We thank the reviewer for the constructive comments. We have read through all comments carefully, and our point-by-point responses are listed below.

First, to address the reviewer's concern about the arrival-time difference between TB099 and TB144 in Figure 1e, we have now explicitly marked the P and S arrivals to avoid misleading. P-waves at station TB144 arrived earlier than at TB099 and can be easily distinguished on the vertical components. Here, we plotted the horizontal component waveforms to emphasize the S waves, as noted by the reviewer. Hence, the P phases are less visible due to the lower SNR, particularly at station TB144. To address this point, we have added three component waveforms to the Supplementary (see Fig. S2).
For constructing moveout profiles as commonly used in reflection experiments, we have attempted to plot and analyze the section with different alignment strategies, but found them not suitable or challenging in our case. One of the fundamental difficulties is that the sources of earthquakes are not confirmed or controlled with respect to their spatial distribution, timing, or radiation pattern. Moreover, the network was not designed to produce profiles with regular trace spacing, like those from 2D seismic reflection survey lines. Consequently, given the absence of a predefined acquisition geometry and predetermined source mechanisms, it is not practical to generate normal-moveout (NMO) gathers or to further establish common

reflection point (CRP) gathers from natural earthquake waveforms.
In response to the reviewer's suggestion, we provide two types of profile examples in the supplementary material (see Fig. S3). One section presents different events aligned at a single station, and the other shows the same event aligned across multiple stations. Although these sections show minor variations in S–SxS travel-time differences, the reflected phases cannot be confidently associated with common reflectors for imaging, and we thus estimate their locations individually using the presented approach. We agree that a section like Fig. S3b can confirm that the first phase is not the SP phase in terms of both velocity and polarization. Additionally, polarization plots presented in Fig. 2c and 2e also show that the S and SxS phases are shear waves, and exclude the possibility of SP-converted P-wave. In Fig. S3b, the second phase cannot be observed at greater distances, depending on the geometry of the source and station. This suggests the reflectors are more likely composed of localized accumulations rather than a simple large layer, as discussed in Section 2.1.

• Are the claims novel? Yes, to my knowledge this reflective phase has not been described previously.
• Will the paper be of interest to others in the field? yes
• Will the paper influence thinking in the field? yes
• Are the claims convincing? I am not totally convinced and thus suggest that the authors provide composite record sections of the SxS phase at different station in order to verify its origin. The authors also need to identify the components shown on their seismic figures. Vertical, radial or transverse.
We thank the reviewer for the suggestion. As discussed above, we have added additional figures to the supplementary material to address this point.
• Are there other experiments that would strengthen the paper further? How much would they improve it, and how difficult are they likely to be? Providing three-component plots of the data would definitely strengthen their main claim of the paper. Some of these might be added to the supplementary and should not be very time consuming.
Three-component waveforms have now been included in the supplementary material (see Fig. S2), as discussed above.
• Are the claims appropriately discussed in the context of previous literature? Yes, to my knowledge they are.
• If the manuscript is unacceptable in its present form, does the study seem sufficiently promising that the authors should be encouraged to consider a resubmission in the future? I recommend moderate revisions.
• Is the manuscript clearly written? If not, how could it be made more accessible? Yes, but many aspects should be better clarified, see my specific line-numbered comments.
We studied the comments carefully and see our responses below.
• Could the manuscript be shortened to aid communication of the most important findings? Chapter 2 is rather diffuse to read and conclusions are missing. The authors need to separate the general discussion from their specific results and add a few concrete conclusions from this particular study.
We have revised Section 2 to improve its readability and clarity, and we now emphasize the key conclusions more explicitly. According to the journal guidelines, a separate conclusions section is not required. Instead, the main findings are summarized in the Abstract, placed in the Introduction, and reiterated within the revised section 2. We believe this structure provides a clear and consistent presentation of our results.
• Have the authors done themselves justice without overselling their claims? yes
• Have they been fair in their treatment of previous literature? yes
We thank the evaluation.

###Comments related to specific parts of the paper.
Enlarge Figure 1a and place other figures beneath it. Focal mechanisms are a bit small. I suggest you enlarge them and place outside of the seismic zone with a line indicator. Some events in C2 seemingly have a weak second phase. What components are shown in Figure 1cd, radial or transverse? You should provide a record section in 1e of these components.
We thank the reviewer for these suggestions regarding the figures. Figure 1 has been revised to improve readability. All events in C2 shown in Fig. 1 exhibit distinct second phases with high similarity. There may be minor differences in focal location and mechanism, SNR, or magnitude that lead to variability among

the waveforms, but the relative amplitude of the secondary phase remains pronounced in C2 events. The waveforms shown in Figures 1c-d correspond to the raw north components. To further clarify this point and address the reviewer's suggestion, we have added the corresponding record sections, and waveforms from additional components are also provided in the supplementary material (e.g., Figs S2 and S3).

###Comments below marked by linenumbers:
49-50 Specify the type of strike-slip focal mechanisms along the OsZ. Done.

57-60 Add referenced related to debated theories on the origin of the OSZ seismicity. Added.

62-64 "Thanks to the unprecedented.." rewrite! The dense seismic network greatly improved accuracy of earthquake locations within the OSZ. Rephrased.

Specify what you mean by "novel insights" add a sentence on the upper crustal structure based on the tomography results. Added.

66-67 "reliably located" what are the location errors!
The average location uncertainty is on the order of a few hundred meters (see also supplementary material in Isken et al., 2025a and Laumann et al., 2025). This information has been added to the manuscript.

90 explain/reference "time-field migration technique"
Explanation added (Lines 98-99).

104-109 How many focal mechanisms do you have in C1? only one is shown on Figure 1, You should mention that the strike-slip events are left-lateral along the OSZ and the reverse FM is likely related to the Siegen thrust. What components are shown on Figure 1bc? The P-S time is seemingly same for C1 and C2 in spite of station TB099 being markedly closer to C1. Explain!
A total of 10 focal mechanism solutions are available for cluster C1 (Laumann et al., 2025), and all of them are now shown in the revised Figure 1. We have also clarified that the strike-slip events are predominantly left-lateral along the OSZ. The thrust-type focal mechanisms are not further interpreted, as there is no clear evidence supporting reactivation of the Siegen thrust. The component name has now been specified in the figure. Regarding the reviewer's concern about the similar S-P times in Figures 1c and 1d, cluster C1 is approximately 2 km closer to station TB099 than C2, but is also about 2 km deeper. As a result, the overall travel distances are comparable, leading to similar P-arrival times and S-P time differences. This information has been added (Line 110).

128-130 Based on the station distribution and shear wave polarization you should try to evaluate radial anisotropy within your array. Do you see a difference in shear-wave attenuation between C1 and C2?
For the upper/middle crustal events analyzed here, and given the relatively short source-receiver distances, anisotropic effects are expected to be limited and are unlikely to significantly influence the observed waveforms. This aspect is briefly discussed in the manuscript, including attenuation and scattering near LSV. However, a detailed investigation of anisotropy is beyond the scope of this study. Based on our data, we do not observe clear evidence for radial anisotropy within the array, nor systematic differences in shear-wave attenuation between clusters C1 and C2. Instead, the observed waveform differences between the two clusters are more likely related to variations in source radiation patterns and focal depths, consistent with their differing focal mechanisms. A detailed analysis of anisotropy is therefore beyond the scope of this study.

139-147 As vertical P and PxP phases are not prominent on horizontal components, you should include a vertical record section, i.e. with Figure S3. Indicate what the components are on your figures.
Waveforms of the vertical components are now shown in Figures S2 and S3a. The P-wave energy remains weak on these components, consistent with the expected radiation pattern. Question marks indicate uncertain phase identifications where arrivals are not clearly defined. The component name has also been added in the caption.

160-162 .."limited similarity to the direct S-wave" frequency? scattering? specify! Rephrased.

165-166 be more specific, how would the geometry of the reflecting zone change the waveforms? Just as radial anisotropy serves as an important parameter to understand asthenospheric flow, it may help constrain

the nature of the reflected shear wave.
The geometry of the reflecting zone can influence the observed waveforms through its effects on reflection coefficients, amplitude, polarity, and waveform complexity. Irregular or rough interface geometries can lead to waveform distortion, reduced coherence, and scattered energy. In addition, dipping interfaces or localized structures may alter the incidence angles and radiation patterns, further modifying the observed amplitudes and polarities across the array. We cannot determine which factors dominate the waveform characteristics, and therefore, they cannot be used as constraints. However, these factors do not significantly affect the propagation velocity. It is therefore justified to use the travel time to locate the reflectors.

195 What reflector imaging results are you referring to? Should your Methods, chapter 3 not be ahead of chapter 2?
The reflector imaging results referred to here correspond to those obtained using the method described in Section 3. The placement of the Methods section after the Results section follows the journal's formatting guidelines.

207 explain what is meant by secondary boiling, what is being boiled and where?
The term has been replaced by a clarified explanation in the manuscript. It refers to the exsolution of volatiles (e.g., CO_2, H_2O) from a fluid or melt phase as pressure and/or temperature conditions evolve during ascent, migration, or crystallization. In our case, this process likely occurs at depth within crustal fluid reservoirs (e.g., stalled crystallizing melt) or along weakness zones where pressure decreases during melt ascent, potentially leading to gas accumulation.

231 specify the station (TB112) Done.

263 I would use the word suggest instead of speculate. Done.

Your conclusions are missing!!!
According to the journal guidelines, a separate Conclusions section is not required. Instead, the main findings are summarized and placed in context in the Abstract and Introduction, and are reiterated in the revised Section 2. In addition, we intentionally designed our manuscript to be concise and tightly structured: Section 2.1 presents observations of late-arriving waves, Section 2.2 describes the estimation of reflector locations that indicate fluid-rich zones, and Section 2.3 proposes a conceptual model to explain the ongoing activity beneath the region based on the combined observations. We believe this structure provides a clear and concise presentation of our key results.

Reviewer #2

The manuscript presents direct seismological imaging of localized mid-crustal reflectors at ∼12-14 km depth beneath the Ochtendung Seismic Zone (OSZ) in the East Eifel volcanic field, interpreted as zones of magma-derived fluid accumulation within crustal weakness structures. The imaging is based on secondary S-wave phases (SxS) extracted from local microearthquake waveforms and migrated using an eikonal-based, reverse-time-migration-like approach. The authors link the imaged fluid bodies to the persistent burst-like microseismicity observed ∼1-2 km above them, proposing that episodic deep fluid influx drives slip reactivation on pre-existing structures, which they frame as an example of natural hydraulic fracturing. The imaging result is convincing and the conceptual model is consistent with other available observations in the region.
The work is sound and should be of interest to a wide audience spanning volcano seismology, crustal fluid mechanics, and intraplate seismotectonics. I see no flaws prohibiting publication. However, several aspects of the methodology, figure presentation, and interpretive framing would benefit from minor revision, as detailed below.
We thank the reviewer for the positive feedback and insightful comments. Our point-by-point responses are provided below.

- Stations used for imaging: It is unclear which stations were used for the imaging. Figure S4 shows a dense station distribution with only 2 stations labelled, and the caption suggests these were the only 2 stations used for seismic analysis, but the example in Figure 3c suggests 18 stations were used. Please clarify in Figure S4 or Figure 1a which stations were used for the imaging. In Figure 1a, the station symbols are too faint and very hard to see. It would also be useful to plot in Figure 3e a projection along the profile of the stations used for the reverse-time migration.
We appreciate the reviewer's suggestions regarding the figures. The station symbols have been added and revised in Fig. 1a. For the projection in Fig. 3e, we instead show the stations directly on the map in Fig. 3d, as the projection causes some stations to fall outside the plotting area.

- Template matching detection: The authors mention template-matching was used to find additional events within a seismic burst (Figure 2g). Please clarify:
Was template-matching applied only to this particular sequence, or to the full dataset?
Are the 158 earthquakes used for imaging drawn exclusively from the relocated tomography catalogue, or do they include template-matching detections?
If template-matching events are included, how were they relocated relative to master events? Were time-delay measurements used?
How many additional events did the template-matching algorithm detect across the full dataset?
In our case, template matching can only effectively detect additional small events at stations with low ambient noise levels (Fig. S8) and a clear double-phase pattern (e.g., TB112, TB099, OCHT) and was therefore not applied to the entire dataset. Station TB112 yielded the most successful detections, recording more than 93 events from cluster C2 between 15.09.2022 and 28.02.2023. Unfortunately, it was not deployed for the remaining months. The number of additional detections varies across stations; overall, 98 or probably more events were not included in the raw catalog. These additional events were not used for imaging as their locations cannot be reliably constrained (most lack clear P phases), although they are likely to occur in similar areas. This information has been incorporated into the revised manuscript (Lines 249 and 324-325).

- Uncertainties on earthquake hypocentres(Figure 3e): What are the uncertainties on the earthquake hypocentres? Are they large enough to be visible at this scale? Also please indicate the vertical exaggeration in the figure caption.
The location uncertainty is difficult to quantify in a robust manner in our opinion, as it depends on multiple factors, including the velocity heterogeneity, the network geometry, and the picking quality, etc. Qseek provides uncertainty estimates for the primary catalog based on semblance, which are on the order of a few hundred meters in our case (see also supplementary material in Isken et al., 2025a); however, these values should be considered indicative rather than absolute. The hypocenters are further refined during subsequent processing, including tomographic inversion or source-specific station term corrections. The resulting location differences between catalogs are also on the order of a few hundred meters (see also supplementary material in Laumann et al., 2025). Overall, we infer that the average location uncertainty in our study is sub-km, and we think it can be visible in this figure, given the 8 km depth scale. The uncertainty information and references have been added to the manuscript. The figure has an aspect ratio of 1:2 (width: height), and the caption has been revised accordingly.

- Events below the reflectors: In Figure 3e, several events at distances >7 km along the profile appear located below or within the fluid accumulation zones. Please comment on whether the depth of the reflectors or events in this SW portion of the profile is reliable (i.e. their relative position is larger than associated location uncertainties) and if yes, how does that change their interpretation of the SxS phase paths? Are these events associated with the residuals outliers in Figure S6?
To account for the complexity of the reflector geometry as well as hypocenter uncertainties, we stacked all possible reflector locations to identify the most likely reflector region. The scatterers (reflector segments) may be inclined and discontinuous, and our method is designed to accommodate such complexities. However, because the imaging is based on intensity stacking, some outlier events may still occur, as the method estimates only the most probable reflector region. The SW part is considered more reliable because it corresponds to the scatterers associated with the majority of earthquakes. As suggested by the reviewer, location uncertainties may explain these outliers, although additional unresolved scatterers that are suppressed dur-

ing stacking could also contribute. We now reordered the earthquakes in Fig. S6a according to the profile in Fig. 3e, and the events with relatively higher residuals are concentrated at a distance of approximately 6 km. Additional discussion has been added to the Methods section (Lines 384–389).

- Time delay uncertainties: Please comment on the time-delay uncertainties given that the stacked spectral-energy curves have a resolution dependent on the bandwidth used. For stations were a high similarity between the direct S and SxS phases is observed (station like TB112), a waveform-based time delay calculation could have given potentially increased subsample precision. It would be useful to discuss the uncertainties of these time delays and how they translate into uncertainties on their reflector depths.
The frequency bandwidth used for spectral stacking is chosen to be consistent with the frequency content of the recorded waveforms. In our study, the frequency bandwidth of earthquakes is about 2-30 Hz, with a central frequency of approximately 15 Hz. As we mentioned in Method 3.2, for short-window signals, temporal resolution is more critical than frequency bandwidth, which is why we used the wavelet transform to compute time-frequency spectra. The time delay is determined by the spectral peaks and should also be affected by peak widths, which reflect the duration of the phases. The precision of 0.01s is sufficient for this study. As for the uncertainty, we could take the peak width at half prominence for reference, which is generally about 0.1-0.2 s.
The waveform-based method mentioned above relies on cross-correlation (if we understand correctly), similar to template matching, which is sensitive to waveform distortions and may introduce bias when the signals are not strictly similar. In our case, not all phases are ideally similar. In contrast, our spectral-energy-based method is less sensitive to such waveform variability and provides more stable estimates of the time delay. The uncertainty of the time-delay translates directly into spatial uncertainty in the inferred reflector positions in our approach, which can be interpreted as a finite-width sensitivity kernel in 3D space. Therefore, this uncertainty not only affects the depth of the reflector but also its lateral position. For a time-delay uncertainty of 0.1 s, the corresponding spatial uncertainty is on the order of a few hundred meters, depending on the source-receiver location and velocity. This relationship between time-delay uncertainty and spatial resolution has been clarified in the revised manuscript (Lines 368-369).

- The introduction notes that mantle-He observations in CO_2-rich wells support a deep mantle-derived fluid source. Figure 1a appears to show higher mantle-He concentrations northwest of the Siegen Thrust surface trace, rather than directly above the imaged fluid accumulation along the OSZ. Please comment in the discussion on how this spatial offset is consistent (or not) with the conceptual model. If the fluid pathway is structurally controlled by the Siegen Thrust (as suggested in Figure 4), some lateral offset between the surface degassing signature and the mid-crustal accumulation is plausible, but the argument could be made more explicit in the text and figure 4.
The observation of mantle-derived helium is consistent with our studies and conceptual model. The helium measurements were obtained at the surface, but subsurface gas and fluid migration processes are not necessarily vertical. The elevated mantle-derived helium near the Laacher See Volcano (LSV) may reflect inherited structures that preferentially facilitate gas migration, and the tomography results also image a structure dipping toward the southwest into the deeper Neuwied Basin at depths of ∼2-10 km, which may act as a pathway for such transport.
Furthermore, fluid accumulations are inferred in the mid-crust at depths of ∼12-14 km, and deep low-frequency events are also observed beneath the basin, suggesting that the deeper sources may not be located directly beneath the LSV. The Siegen Thrust is a much older fault system that formed prior to the Eifel volcanism. It likely plays an important role in controlling fluid pathways and possibly magma transport before the LSV eruption. Taken together, these observations support our proposed conceptual model. We have revised the text and Fig. 4 accordingly (Lines 279-300).

- Figure 4 comment: Red arrows appear to show fluid migration pathways along the Siegen Thrust, while blue wiggly arrows also indicate fluid pathways. Please clarify whether the different colours are intended to represent different fluid compositions, phases or pathway types. As drawn, this is ambiguous.
We thank the reviewer for this helpful comment. We have revised the figure and caption to explicitly clarify the meaning of the different arrow styles and colors. In Fig.4, the different arrow colors are intended to distinguish between deeper magmatic-related processes and shallower fluid migration. The red arrows

represent deeper pathways that are more likely associated with magma or magmatic fluids, whereas the blue arrows indicate shallower CO_2-rich or supercritical fluid migration within the crust.

- Framing of the "natural hydraulic fracturing" analogy: The way section 2.3 is written seems to position industrial injection-induced seismicity (Oklahoma wastewater injection, Preese Hall shale-gas hydraulic fracturing) as the reference case and the OSZ activity as a natural analogue, which appears to me somewhat awkward. The characteristics of the C2 seismicity (small magnitude, no mainshock, closely-spaced events, episodic reactivation) as evidence of fluid-driven earthquake swarms are well recognised in the volcano-seismology literature. Slip reactivation due to increased pore pressure along pre-existing fractures is a well-known mechanism. These natural observations of fluid-driven earthquake swarms could be better referenced in section 2.3. For example, several of these papers could be relevant to discussing natural hydraulic fracturing:

Hill, D. P. (1977), A model for earthquake swarms, J. Geophys. Res., 82(8), 1347-1352, doi:10.1029/JB082i008p01347.

Foulger, G. R., Julian, B. R., Hill, D. P., Pitt, A. M., Malin, P. E., & Shalev, E. (2004). Non-double-couple microearthquakes at Long Valley caldera, California, provide evidence for hydraulic fracturing. Journal of Volcanology and Geothermal Research, 132(1), 45-71. DOI: 10.1016/S0377-0273(03)00420-7

Alicia J. Hotovec-Ellis et al., Deep fluid pathways beneath Mammoth Mountain, California, illuminated by migrating earthquake swarms.Sci. Adv.4,eaat5258(2018).DOI:10.1126/sciadv.aat5258

Parotidis, M., S. A.Shapiro, and E.Rothert (2005), Evidence for triggering of the Vogtland swarms 2000 by pore pressure diffusion, J. Geophys. Res., 110, B05S10, doi:10.1029/2004JB003267.

We agree that this part was somewhat logically uncomfortable and hard to read, and we have reorganized and revised it. In this section, our intention in referring to industrial examples was to provide direct and close dynamical analogues for the fluid involvement, which is our key question, instead of just classifying the seismicity. Following this reasoning, fluid injection is directly controlled and independently verified in industrial cases without relying on indirect evidence or indication. Therefore, we refer to examples of hydraulic fracturing operations, but these sentences have been shortened and rephrased.

We thank the reviewer for highlighting relevant studies on natural fluid-related seismicity. We agree that fluid-related earthquake swarms have been documented in different tectonic and volcanic settings, yet their triggering conditions and focal mechanisms may differ. Among the four studies, the model in Hill (1977) explains the swarms in parallel magma-filled dikes and conjugate faults, which is not directly comparable to our case; swarms in Mammoth Mountain show episodic reactivation, but hypocenters show clear temporal migration with ongoing magma/fluid intrusion in the deep crust; the Vogtland swarm sequences indeed show similar hydro-shearing behaviour (e.g., Büyükakpınar et al., 2025), and it is included. A new large-N seismic network (ELISE) has recently been deployed in the region to further investigate the detailed characteristics of the swarm seismicity. The Long Valley caldera may also be a comparable case, where a low-velocity layer has been reported as an indication for the magmatic system roof (e.g., Nakata and Shelly, 2018), and this study is already included in our manuscript.

In conclusion, the imaging result is compelling and the interpretation is broadly consistent with independent constraints. I believe the points above can be addressed without major additional analysis, and doing so would strengthen what is already an interesting manuscript.

We thank the reviewer again for the recognition of our work. We also appreciate the constructive suggestions provided, which have helped improve the manuscript. We hope that the revisions and our responses have adequately addressed the concerns and meet the reviewer's expectations.

Reviewer #3

Zhang et al. used S-wave phases observed in microseismicity near the dormant East Eifel volcanic region in Germany to map a local fluid-saturated zone in the upper crust. My expertise is in detecting, locating, analysing and interpreting microseismicity. Overall, I believe the manuscript could be greatly improved through text revision and restructuring of the figures. Each figure has an enormous amount of data and

detail, but they are all challenging to decipher as presented. Due to this and some lack of detail and consistency in the language within the text (e.g., long sentences or sentences that are vague), I find that the manuscript is difficult to follow at times. In particular, I found it very hard to follow almost all of Section 2.3. I provide feedback below in order to improve the presentation and the readers' understanding of the setting, data, and results.
We thank the reviewer for the careful evaluation and constructive suggestions. We have revised the manuscript to improve clarity, structure, and consistency in both the text and figures. Detailed, point-by-point responses are provided below.

Major points

Each Figure in the main text has too many panels that makes it hard to understand. I would recommend that the figures be restructured, and the panels that do not necessarily need to be in the main manuscript can become supporting figures. See comments below for more on this.
We appreciate the reviewer's helpful suggestion for figures. We have revised and improved the figures as much as possible following the recommendations, and additional aspects are explained in detail in the responses below.

How does the fluid-rich zone ($\sim$ 10 km2) compare to the tomographic results of reference [19]? How does the Vp/Vs ratio distribution compare to the fluid-rich zones determined by the SxS phases? The Vp/Vs anomaly, unless I missed something, is only compared in Figure 3 briefly. It would be interesting to make a more qualitative comparison between the two. Would it be possible to include the Vp/Vs in a cross-section, for example?
In this reference, we applied local earthquake tomography (LET) based on direct P- and S-wave arrivals. As a result, the resolved structures are primarily constrained above the earthquake hypocenters. The high Vp/Vs anomaly identified indicates an upper-crustal, inherited magmatic structure beneath the Laacher See Volcano (LSV) at depths of $\sim$2-10 km (shown in Figure 4), but the structure below the seismicity is not well-constrained. In this work, we use late-arriving S-wave phases (SxS), which provide complementary constraints on deeper structures beneath the earthquake sources. Therefore, the fluid-rich zones inferred from SxS phases extend the structural interpretation to larger depths and cannot be directly compared one-to-one with the Vp/Vs anomalies derived from LET. For this reason, we show the Vp/Vs anomaly as contours in Fig. 3d rather than as a direct overlay.

Given that the depth estimates of these fluid-rich zones are dependent on the hypocentres of the seismicity, it would be great to provide more information on the quality of these locations (i.e., how robust they are). See comment below for line 67.
The uncertainty information and references have been clarified in the revised manuscript. Please also see our detailed response below.

L 128-130: I am not sure I follow what the reason is suggested by the authors here on why the reflected SxS phase has higher amplitudes than the S phase. I think this is a very crucial point that needs to be carefully addressed. Would the type focal mechanism of the events (i.e., radiation pattern) analysed (e.g., geometry between waves and the location of the receiver) be a potential reason for this?
This part has been revised and rephrased to improve readability. It is indeed counterintuitive that the late-arriving waves have higher amplitudes than the direct waves. The most plausible, or probably the only reasonable, explanation is the focal-mechanism radiation pattern, as also suggested by the reviewer. In this case, the SxS phase likely leaves the source at a different take-off angle compared to the direct S phase, resulting in a more favorable radiation direction toward the receiver. We included an example of a synthetic seismogram simulation in a layered crust in the supplementary material (Fig. S5). However, a complete quantitative assessment or simulation of this effect is challenging due to uncertainties in the source parameters, the complexity of the corresponding Green's functions, particularly for inclined or geometrically complex reflecting structures.

L 203-211: Are there any references to support this section of the text and the formation of these fluid-rich zones? Any other examples of similar structures around the world? If I may attempt a question far out of

my expertise in relation to this point, please correct me if I am mistaken. Is the K-horizon feature, mostly observed with active seismic data, in the Tuscan magmatic field in Italy, a similar structure to what you observe?
To our knowledge, studies identifying fluid-related structures beneath dormant volcanic systems using natural seismicity observations, as performed in this study, remain relatively limited. We thank the reviewer for pointing out this interesting case in Italy. The K-horizon described in the geothermal areas of southern Tuscany indeed shares similar characteristics with the structure identified in our study, as both indicate heterogeneous, fluid-rich, and physically perturbed reflective zones and are associated with nearby swarm-like microseismicity, often characterized by strike-slip faulting, although differences remain in their formation mechanisms, depths, and geological settings. We believe this is good support for our interpretation of the reflection structures and have therefore included this example in the revised manuscript. The formation of this structure is discussed later in this section. We speculate that these structures may be related to inherited magmatic systems formed in the mid-crust, as suggested by simulation and laboratory studies. Surprisingly, a recent geochemical study based on high-pressure crystallization experiments and magma differentiation modeling suggests that an additional magma or fluid storage region may exist at depths of ∼10–12 km (e.g., Förster et al., 2026), which is comparable to the depth range inferred in our study, and we included this study in the revised manuscript.

Minor points
L 14: "only 13 ka ago"... (perhaps the authors mean in terms of geological time here; it would be good to clarify the text). Clarified.
L 18: "active subsurface processes." It would be more helpful if the specific kind of subsurface processes were mentioned here, as at this form the sentence is a bit vague. Done.
L 18-19: What time period do these 500 events cover? I think it might be a good idea to include this info in the abstract. Done.
L 27: "... as burst-like sequences reflecting rapid, successive ruptures." This sentence is slightly repetitive, I think. Perhaps omit the rapid successive part or burst-like?
Redundant expression was removed.
L 32: hosts a major Cenozoic or hosts one of the largest Cenozoic Revised.
L 45: "Since the last century," This can be a bit more specific. Done.
L 57: "no pronounced surface deformation has been detected." Using what kind of information? Clarified.
L 58: "limited the observations." What type of observations? Revised.
L 59: "remains debated". This implies that there are studies that propose different interpretations about the volcanic activity. Perhaps citing some of these studies and stating briefly how they differ might help with setting the background. This part has been revised accordingly.
L 62: "495 station locations was deployed..." Perhaps 495 seismometers were deployed?
No, parts of the network were redistributed during the experiment.
L 63-65: This sentence is very generic. I would suggest that it becomes more specific about what types of insights were achieved with the denser network of seismometers, or most of this can be omitted and merged with sentences either before or after. The insight statement has been omitted, and this sentence has been revised accordingly.
L 67: Please specify what reliable located means. E.G., provide some additional information like what the hypocentral uncertainties are, distances to closest stations, number of picks, etc., or whatever the authors think is relevant here. But I think it would be great to provide more information here as the accuracy of the hypocentral depths would greatly affect the fluid-rich zone depth estimates.
Overall, we infer that the general location uncertainty is on the order of several hundred meters. The location uncertainty is difficult to quantify robustly, as it depends on factors such as velocity heterogeneity, network geometry, and picking quality. Qseek provides indicative uncertainty estimates for the primary catalog based on semblance, which are on the order of a few hundred meters in our case (see supplementary material in Isken et al., 2025a). Subsequent processing, including tomographic inversion and source-specific station term corrections, results in location differences of a similar magnitude (see supplementary material in Laumann et al., 2025). We added this information in the sentence.
L 84: omit time Done.

L 94-95: "would be caused". Is likely caused? Changed.

L 95-97: The phasing here is slightly awkward and would benefit from some re-writing, perhaps. For example, it is not that clear how important the comparison is to human activities.
The sentence has been rewritten, and the human activities part has been removed.

L 101: The first sentence can be omitted as it is a bit vague. Done.

L 103: "two kinematically divergent clusters". Not very clear what that means until you read the following few sentences. I would suggest simplifying to "two distinct clusters" Changed.

L 109: Figure 1 b does not exist. Please modify the text accordingly.
We thank the reviewer for pointing this out. The correct references are Figures 1c and 1d. Figure 1b corresponds to the inset map in the bottom right of 1a. The text has been revised accordingly.

L 136: Figure S2. The caption of the figure is not complete.
Revised.

L 162-164: I understood that the seismometers used in this study do not consist of the same instruments. Could the type of sensor also be one of these factors?
Yes, the sensor's response function could also affect the waveforms, and we clarified this.

L 168-169: Might be worth including in the supporting information some of these examples near LSV with long codas. The waveform examples have been added in the supplementary materials (Fig. S7) as the reviewer suggested.

L 176: Figure 3c is not mentioned in this section.
It has now been referred to in the revised manuscript.

L 187-191: The Vp/Vs show the opposite trend, getting deeper moving from NW to SE. What does that mean regarding the SxS phase imaged fluid saturated zone? The Vp/Vs anomaly imaged by tomography is primarily located in the upper crust ($\sim$2–10 km), while the fluid-saturated zone imaged by the SxS phases appears in the middle crust at greater depths ($\sim$12-14 km). Both features are likely related to the magmatic system associated with the Laacher See eruption at different depths.

L 215: What are some of the proposed mechanisms controlling seismicity at OSZ according to these studies? It would be nice if these were introduced at least. We rephrased this part.

L 223: "presumed maximal horizontal stress" Rewritten.

L 227: Omit Collectively Done.

L 227-228: "a localized and persistent forcing mechanism." Not very clear what the authors mean here. This has to be more specific.
Here, "localized" refers to the spatial confinement of rupture within a limited zone, whereas "persistent" reflects sustained fluid involvement and overpressure-driven fracturing processes, rather than large tectonic ruptures. We have clarified this part.

L 233: Any citation for the template matching approach?
A standard reference has been added.

L 242-253: I found this part of the text kind of hard to follow. It feels long and repetitive. In particular, lines 242 to 248 could be either omitted or significantly shortened. Following this, the part mentioning that elevated pore pressures are capable of driving slip not just in industrial settings is confusing. There are numerous studies that have connected natural seismicity with changes of pore fluid pressures in the upper crust. Apologies if I misunderstood something here.
We appreciate the reviewer's feedback. We agree that this section was somewhat difficult to read and have reorganized and revised it.

L 260: Is the maximum horizontal compressive stress orientation SHmax introduced in the text before?
We thank the reviewer for pointing this out. The relevant information has now been added to the Introduction.

L 283: What is the exact orientation of SHmax in the region and how is it calculated? Please cite the paper that calculated this. The exact orientation and citation have been added.

L 254-289: Similar to the previous part, I found this part of the text hard to follow and at times repetitive. I recommend that this part be modified significantly for clarity.
We apologize for the confusion. This part has been rewritten to improve readability.

Suggestions on figures

Figure 1: Figure 1 b does not exist. Current panels e and f could be moved to the supporting information. We modified and reorganized this figure according to all reviewers' comments. Panel b is the inset map in the bottom-right corner. Panels e and f are necessary to show that the second phase is not only observed by one station.

Figure 2: Panel g can be moved to the supporting information. It is discussed in Section 2.3, so we chose to keep it, but we adjusted the size of Panel g.

Figure 3: Panel c is not mentioned in the main text. My opinion is that the most important panels here are d and e so I would recommend that only these two remain here and the rest of the panels are moved to the supporting information.
We now explicitly refer to all panels. Figure 3 summarizes the workflow and key results of the reflector localization approach central to this study, so we believe that keeping the complete figure could improve the clarity.

The authors have generally made good choices with their color scales throughout the figures. One exception is Figure 3, panel d, where the jet colormap is used. This colormap is perceptually non-uniform and can introduce visual artefacts that misrepresent the data, and it is also not colorblind-friendly. I would encourage the authors to consider replacing it with a perceptually uniform, colorblind-accessible alternative (e.g., viridis, batlow, or oslo). Several excellent resources on this topic are listed below for reference:
Please refer to the following links for more details on color scales:

- *https://peterkovesi.com/projects/colourmaps/*
- *https://blogs.egu.eu/divisions/gd/2017/08/23/the-rainbow-colour-map/*
- *https://www.climate-lab-book.ac.uk/2016/why-rainbow-colour-scales-can-be-misleading*
- *https://www.fabiocrameri.ch/visualisation/*

We thank the reviewer for the helpful comment and for sharing this information regarding the colormap choice. We replaced the previous jet colormap with the colorblind-friendly batlow.

Response Letter (COMMSENV-26-1425A)

Potsdam, 16 June 2026

Dear Editors and Reviewers,

We are very pleased that the reviewers were satisfied with our responses and that the manuscript has been strengthened through the revisions. We sincerely thank the editors and reviewers for their time, effort, and constructive comments.

We have carefully revised the manuscript throughout, addressing both the reviewers' remaining concerns and the editors' instructions, and now resubmit it for your consideration.

Thank you again for your valuable feedback.

Sincerely yours,

Hao Zhang

on behalf of the authors

Our point-by-point responses (in blue) to the reviewers' comments (in black) are presented below.

Reviewer #1

I have read the revised manuscript and ackowledge the authors response to my previous concerns. The text has been clarified and reads well. Below I list my comments/corrections line-referenced to the revised manuscript.
We thank the reviewer for the careful follow-up review. All line-referenced comments and corrections have been addressed.
line 84 delete or explain what is meant by "after eruption" Deleted.
lines 92-94 delete "In this study, we provide the first" Deleted.
To minimize "we this" and "we that" I suggest Seismological evidence for localized fluid-rich bodies in the mid-crust beneath the East Eifel, HAS BEEN inferred from..... or We provide seismological evidence for localized fluid-rich bodies in the mid-crust beneath the East Eifel, inferred from.....
This part has been revised as suggested to reduce the use of "we".

My comments below mostly refer to incorrect Figure references, which need to be checked throughout the paper.
We deeply appreciate the careful check of the figure references. We have carefully reviewed all figure references throughout the manuscript to avoid any further mistakes.
97 change these volumes to these bright spots Done.
142 change Fig. 2d,f to 2d,e Done.
148 change Fig. 2d to Fig. 2f Done.
change (d,f) in the figure text to (d,e) Done.
169 change As shown in Fig. 1e,f (see more in Fig. S6), to As shown in Figs. 1e,f and S6, Done.
For easier comparison I suggest you add a simple length scale to Figures 3c and 3d Added.
210 Figure 3f does not exist Corrected to Figure. 1f
322 I presume you are referring to Fig. 2g here Corrected.
336 presume you are referring to Fig. 1f instead of 1e here Corrected.
342 no need to repeat reference to Fig 2c here. Deleted.

Reviewer #2

I thank the authors for carefully and thoughtfully addressing all reviewer comments, including mine. My concerns have been addressed, and I have no further comment. I recommend publication of this revised manuscript as is.
We thank the reviewer for the positive feedback and are delighted that the revised manuscript has satisfactorily addressed all concerns.

Reviewer #3

Zhang et al. have been very meticulous in addressing the detailed point-by-point reviews. I've read through their responses and the annotated text. The clarity of the paper has improved and I'm happy with how the comments are addressed.
We thank the reviewer for the positive assessment and are pleased that the revisions have improved the clarity of the manuscript and satisfactorily addressed the comments.