RC1: 'Comment on egusphere-2026-686', Anonymous Referee #1, 07 Apr 2026 reply
General Comments

We would like to thank the reviewer for his valuable comments and we appreciate his effort in improving our manuscript. We accepted all his comments; detailed responses are provided below in red, and all corresponding changes are highlighted in yellow in the enclosed manuscript.

The paper aims to investigate deep crustal structures in the Bohemian Massif by integrating seismological data with crowdsourced macroseismic observations. While the manuscript is well-written and provides a thorough geological background of the study area, there is a significant disconnect between the stated objectives and the actual data analysis presented.

In the text, we clearly emphasized what are the two main achievements of this study and that they extend beyond the identification of macroseismic intensities themselves. We highlighted namely the considerable public response to the earthquakes and the geological insights derived from the spatial distribution of the observations.

- Despite occurring in a region generally considered seismically non-active and characterized by relatively sparse population density, both earthquakes attracted significant public attention, resulting in a high number of completed questionnaires.
- The spatial distribution of macroseismic observations proved highly informative and showed a clear correlation with major geological structures.

We feel that this helps to justify our work. We also revised the title to better reflect the scope and focus of our work. The new title is: *Public Response to Two Rare Earthquakes: Crowdsourcing Constraints on Crustal Structure in South Bohemian Massif*

The authors claim that the study demonstrates how citizen-reported data can reveal geological controls on earthquake effects. However, the manuscript lacks the analytical support necessary to justify such conclusions. Furthermore, there is a notable absence of macroseismic literature or citations linking macroseismic intensity to geological structures, despite the extensive geological references. In its current form, the paper describes the dataset and the geological setting but falls short of providing the seismological and macroseismic analysis promised in the abstract. Major revisions are required to substantiate the interpretations with quantitative evidence.

We have clarified the objectives of the paper to better emphasize its main contribution. Specifically, we demonstrated that earthquakes occurring in south Bohemia, an area typically considered seismically non-active, generated significant public attention and engagement, despite its low population density. This is reflected in the high number of completed questionnaires. Furthermore, the spatial distribution of these responses showed a clear correlation with geological structures.

The geological context is essential for interpreting the upper-crust structural control on the macroseismic observations. As the study area exhibits sparse or absent seismicity and lacks a well-defined contemporary seismotectonic framework, this context is particularly important. However, we recognize that the geological section in the previous version was overly detailed; therefore, we have streamlined it to improve clarity. We have also revised Figure 1 to enhance readability and included country borders. The focal mechanism has been moved to the new Figure 2. For further details, please refer to the comments of Reviewer 2.

We need the geological context for explaining tectonic control on macroseismic observations. The area is with sparse or missing seismicty and there is no current seismotectonics. However, we realized that in its previous form the geological part was too extensive and to improve clarity, we simplified the geological section. We also revised Figure 1 to make it clearer and added country borders. We moved the focal mechanism to the new Figure 2. For further details, please refer to comments of Rev. 2.

Specific Comments

Although the abstract mentions the integration of seismological and macroseismic analyses, no actual seismological analysis is presented; the authors only provide a figure showing waveforms from some stations.

We agree with the reviewer, and as requested, we improved the seismological part of the investigation. We added the details and reference for the location method, accuracy of the solution, number of stations applied for calculation and the velocity model. We provide information on how data on the source location is obtained, along with the corresponding reference for source data.

We also included modified Figure 2 (we replaced the older Figure 2) and we included stations used for location and focal mechanism calculations. We reference different networks and document various data (stations from differtent networks) for seismic interpretation. In this figure, we also included the cross-correlated waveforms of the two earthquakes at station ZVC pointing to similar location and mechanism of the earthquakes.

We also revised Figure 1 to make it clearer and added country borders for reference. We moved the focal mechanism to the new Figure 2. Moreover, in Figure 2, we included seismicity in Czechia and seismotectonic lines for southern and central Bohemia to document the aseismic character of the Bohemian Massif (see also the answer to Rev. RC2).

The authors state that individual responses were converted to macroseismic intensities but do not explain the methodology. It is unclear how intensities were calculated, what weights were given to specific answers, or whether intensity was assigned to individual questionnaires or geographic localities.

Thank you for this comment, you are right. Our initial focus was on the correlation between the observations and the major geological structures, and as a result, we initially underestimated the importance of providing a more detailed explanation of this aspect. We have now clarified this part of the manuscript.

We expanded the description on macroseismic intensity assessment, included the information on the description of the engagement of population, included overall density population of the area in Figure 4, quality of responses, corrections applied. We also clarified how qualitative terms such as “few,” “many,” and “most” were treated by assigning three not-overlapping ranges (0–20%, 20–60%, and 60–100%), enabling a more quantitative assessment. We also referenced relevant literature. We stated that the intensities were assigned to individual questionnaires and then evaluated through geographic localities.

For low intensities (2–5) such as those discussed here, "not felt" reports are critical. The authors should clarify if they have access to these data and how they were utilized.

Again, you are right. Reports indicating that the earthquake was “not felt” are particularly valuable for estimating macroseismic intensity at lower intensity levels, where the assessment is commonly based on the proportion of people perceiving the shaking (e.g., Grünthal, 1998; Tosi et al., 2015). In our case, the “not felt” reports were associated with the perception of sounds resembling transport heavy vehicles (in 13% of all cases) and were assigned an intensity of III. We stated this in the text.

Figures 3a and 3c show the locations of observations but fail to display the actual intensity values. Additionally, while Figure 3d shows an area of intensity 2, there is no explanation of how this specific intensity was assigned.

Yes, we have now included the intensity values at the corresponding locations. About intensity II - originally, it was assigned on the basis of very sparse observations, typically from reports of only sole individuals mainly reporting sounds but being isolated. However, according to your suggestions and because these observations were sparse and unevenly distributed, we chose a conservative approach and did not assign the intensity II.

In the Discussion the authors interpret the low density of observations in specific sectors (e.g., NW of the epicenter and near the RBFS structure to the east) as evidence of geological control or seismic energy attenuation. However, a low density of data points may simply reflect low population density or a lack of participation, which provides no objective evidence regarding the actual intensity value. Moreover, in Figure 4 the lack of observations NW of the epicenter appears to coincide with low-density areas. To support these claims, the authors should show the density of observations relative to the population and provide attenuation plots of the intensity values for those specific sectors.

The entire southern Bohemia region is characterized by low population density. To illustrate this, we added an inset to Figure 4 showing the population density across the broader area. The density ranges between 20 and 50 inhabitants per km² and, as evident from Figure 4, is relatively uniform throughout the region. However, publicly available population data do not exist at a resolution comparable to that of the reports; therefore, we decided not to include population density attenuation figures. Moreover, only two to three intensity values are applicable, which could bias the results.

At line 143, it is claimed that a booming sound often preceded the shaking. However, the attached questionnaire does not include a question regarding the timing of the sound; the authors must explain the source of this information. It would also be beneficial to provide a map showing the spatial distribution of these acoustic reports.

You are right, this information was derived from additional details provided in the individual comments of questionnaires. However, as it was not consistently reported for all sound observations, we excluded this information from the text. Given that the sounds were reported very frequently (in over 90% of cases, as noted in the text), we consider an additional map showing their locations as unnecessary.

At line 247, the macroseismic fields are compared to a field with quadrupolar symmetry, but no bibliographic references are provided to support the existence of such a field.

This statement was meant as a general statement in seismology related to double-couple force with four alternating lobes of compression and dilation. However, as the reviewer pointed out, it can be considered misleading and we removed it. Instead, we included formulation considering the radiation pattern for S waves.

The manuscript does not specify how the population was invited to complete the questionnaires, which is a vital part of assessing crowdsourced data quality.

In both cases, respondents submitted their observations voluntarily. However, following stronger or unusual events in Czechia, we routinely use media interviews to encourage the public to complete the “Did You Feel It?” questionnaire and provide a link to our website.

This finding supports the quality of the dataset, as also reflected by the fact that the weaker 2025 event was reported even more frequently. We included this information to the text.

Technical Corrections

The citation "Cifelli et al., 1999" (line 54) is missing from the References section.

We included this reference.

The Appendix, which defines magnitude and macroseismic intensity, is unnecessary for a specialized scientific journal like Solid Earth and should be removed.

We removed the Appendix.

The authors should provide access to the dataset in a dedicated "Data Availability" section.

We added the data and their availability to society.