Rebuttal Letter for manuscript:

"Public Response to Two Rare Earthquakes: Crowdsourcing Constraints on Crustal Structure in South Bohemian Massif"

by *Pavla Hrubcová, Matěj Machek, Jiří Vackář, Anna Pohořalá, Hana Kampfová Exnerová, Petr Špaček, and Jan Zedník*

Dear Associate Editor, dr. Simone Pilla,

Thank you for your consideration of our work for publication in Solid Earth journal. This is the second review of our paper where we discuss modifications required by Reviewer 1. Detail explanations are below in red and the changes are highlighted in yellow in the enclosed manuscript. We are submitting the corrected paper for your kind revision.

Changes not requested by Reviewers:

To enhance the clarity of our goals, we numbered the individual sections.

We also added a short paragraph on Mirotice earthquakes preceded by a small event in 2012 in the same location with magnitude 1.7.

Answers to Reviewer #1:

The authors have submitted a second version of the manuscript, introducing several revisions that have improved various aspects of the paper.

Thank you.

However, despite these changes, some fundamental issues remain unresolved and the manuscript still does not meet the standards required for publication in Solid Earth. In particular, the statement conveyed by the title, "Crowdsourcing Constraints on Crustal Structure in South Bohemian Massif", is not supported by any quantitative analysis. Instead, it relies only on a visual comparison between the locations of volunteered observations and some tectonic lineaments.

We are aware of the limitations of macroseismic data, particularly for small events in sparsely populated areas, as in the case of the Mirotice earthquakes. We agree that results in general must be supported by robust analysis. However, we follow the approach adopted in our study, in which we examine the spatial distribution of crowdsourced macroseismic observations and compare it with population density. Our aim is not to focus solely on macroseismic intensities, but to use the macroseismic observations as part of a broader structural interpretation, as they show a clear correlation with geological features. In this way, the macroseismic data provide constraints on deep-seated structures that are otherwise difficult to detect in a region characterized by very low seismicity and sparse seismic instrumentation.

We feel that the title: "*Public Response to Two Rare Earthquakes: Crowdsourcing Constraints on Crustal Structure in South Bohemian Massif*" addresses this point.

The study demonstrates the potential of crowdsourced macroseismic observations for investigating crustal structure and addresses the three main points emphasized in the revised manuscript:

(1) Despite occurring in a region generally considered seismically non-active and characterized by relatively low population density, both earthquakes attracted significant public attention, resulting in a high number of completed questionnaires.
(2) The uneven spatial pattern of macroseismic observations, which was not controlled by population density, proved highly informative and showed a clear correlation with major geological structures.
(3) Both earthquakes occurred at unusually large depths, in the middle to lower crust below 24 km. They also showed very similar focal mechanisms and originated from nearly the same location, together with the small event in 2012. Their magnitudes, particularly ML 3.5 for the 2024 event, are unexpected for a region generally considered seismically inactive.

Such an approach is insufficient to support the main conclusion reported in lines 315–316: "The results demonstrate that major crustal-scale tectonic boundaries strongly influence seismic wave propagation and attenuation."

In structural studies, it is commonly observed that some geological structures attenuate seismic signals more strongly than others. Our interpretation is based on the spatial distribution of macroseismic observations and on population-density data provided by governmental sources. The analysis shows that the main asymmetry in the macroseismic observations cannot be explained by population density alone. In particular, the western sector towards the Pilsen region is more densely populated than the rural Mirotice region, but contains few or no macroseismic reports. Normalization by population density would even enhance the observed contrast.

We have added a paragraph to emphasize this aspect more clearly.

In lines 156–166, the authors state that "Intensities were initially assigned to individual reports and subsequently evaluated for localities". However, Figure 3 shows individual intensities and an interpolated field, while intensity data aggregated by locality are not shown. Furthermore, the manuscript does not explain how the interpolated field was obtained, nor whether it was derived from individual reports or from locality-based intensity values.

We reformulated the sentence and now clearly state that the intensities were initially assigned to individual reports and subsequently interpolated from individual values.

In line 175, the authors add, without any supporting references, that "the not-felt reports were associated with the perception of sounds resembling transport heavy vehicles". However, this procedure is problematic, since this is one of the typical noises historically described in association with earthquake shaking, already by Davison (1938). The procedure may be reasonable only if no shaking-

related effects, other than the acoustic effect, were reported. If this is the case, it should be explicitly stated. Moreover, the rationale for assigning intensity 3 to these claimed not-felt reports remains unclear.

We have reformulated the sentence and now explicitly state that the “not felt” reports refer to cases in which no shaking-related effects, object motion, or damage were reported, and only acoustic effects were perceived. We also clarified how these reports were treated in the intensity assignment and how the final locality intensities were derived from individual report values.

Reports classified as “not felt” correspond to cases in which respondents reported no perceived shaking, no object motion, and no damage, but only acoustic effects, most commonly sounds resembling heavy transport vehicles. Because acoustic effects alone are not diagnostic for the EMS-98 intensity assignment, these reports were treated separately as “heard-only” observations and were not used in determining locality intensity. When multiple shaking-related effects were reported, intensities were assigned conservatively, based primarily on object- or damage-related indicators. The final locality intensity was taken as the modal value of all retained responses.

As in the first submission, the results of the manuscript remain based exclusively on the spatial distribution of macroseismic observations, which is described in lines 236–237 as “unevenly distributed around the epicentre, forming a trapezoidal pattern, bounded by the boundary ...”. However, the density of observations has not been analysed, although this was specifically requested. Indeed, Figures 3 and 4 show only the observation locations, many of which overlap. In addition, contrary to the authors’ statement in line 162, the population distribution does not appear to be uniform in the governmental geoportal.

We agree that the spatial distribution of macroseismic observations should not be interpreted solely from the locations of individual reports, particularly where observations overlap. We have therefore revised the analysis and now explicitly evaluate the density of macroseismic observations. In the revised manuscript, we compare the observation density with the population distribution using the population-density data shown in Fig. 4. We also agree that the population distribution is not strictly uniform. We have therefore corrected this statement and now describe the area as sparsely populated and only relatively even. However, and quite importantly, the analysis shows that the main asymmetry in the macroseismic observations cannot be explained by population density alone. In particular, the western sector towards the Pilsen region is more densely populated, but contains few or no macroseismic reports. Normalization by population density would therefore enhance the observed contrast.

Accordingly, we have revised the text, modified Fig. 4 to better display observation density and population density, and softened the formulation of interpretation. The trapezoidal pattern is now discussed as a feature supported by the density distribution of observations relative to population density, rather than by observation locations alone.

The Discussion and Conclusions sections, as in the first submission, only comment on the spatial distribution of the observations — for example, “The spatial distribution of macroseismic observations proved highly informative and showed a clear correlation with major geological structures” — but do

not analyse the intensity values. As already noted in the previous review, such an analysis could be useful to support the authors' conclusions.

We agree that an analysis of intensity values could, in principle, provide additional support for interpretations of seismic-wave attenuation and propagation. However, in the present case we consider such an analysis to be of limited robustness. Both earthquakes were small, the assigned intensities cover only a narrow range, and the number of localities with reliable intensity estimates is limited. Therefore, a quantitative interpretation of intensity decay or intensity anomalies would be strongly affected by uncertainty and could lead to overinterpretation.

For this reason, we do not use the intensity values as the primary basis for the structural interpretation. Instead, the manuscript focuses on the spatial distribution and density of macroseismic observations, compared with population density and geological structures. We have clarified this in the Discussion and Conclusions and have softened the wording accordingly. The intensity values are now presented mainly as descriptive information on the strength of the felt effects, whereas the structural interpretation is based on the spatial pattern of reported observations.

Technical corrections

In Figure 4, the authors have added **an inset showing a small map of population density**. However, the legend is not provided, and the map may be misleading because it is superimposed **on topographic shading**. As a result, it is not evident, for example, that the area immediately west of the TBU has zero population according to the source from which the authors obtained the data, namely https://geoportal.gov.cz/. **The figure should therefore be redrawn in a clearer and more informative way.**

We felt that the previous version, which also included the faults, may have been more informative. However, based on recommendation of the reviewer, we have revised the figure and removed the faults to ensure consistency between the main panel and the inset. The same scale is used for both panels, as was also the case in the previous version.

Answers to Reviewer #2:

Consider to move the citations of agencies providing seismic data from the reference list to a more appropriate list (in Data & Resources, or an Appendix Table, according to the journal style

We keep the references for networks in reference list under section References Networks as required by Solid Earth Journal standard.