

We would like to thank Javier Fernández-Fraile for his valuable suggestions and for appreciating our work. We took most of the comments into consideration.

The following is a point-by-point response.

### **A thorough review of the (southern Italy): constraints from macroseismology and insights from hydrology**

Andrea Antonucci, Corrado Castellano, Luigi Cucci, Andrea Tertulliani

#### **General comments**

The authors present an exhaustive review of all the macroseismic information concerning May 5, 1990 earthquake in southern Italy, analyzing and comparing previous studies with their results. They have reevaluated original information sources which are crucial to obtain accurate results, of the seismic intensity, the hypocenter and hydrological effects.

Sometimes macroseismic studies are not valued enough, but having a homogeneous seismic catalog is fundamental to begin any other seismic studies, such as seismic hazard studies or study the seismicity of any area.

Therefore, these results improve the seismic knowledge of this area of Italy.

In my opinion, in general terms, this manuscript (MS) is very well done, very rigorous and with a lot of details, that has allowed me to appreciate the amount of work and effort required to develop this study. Figures and tables are very clarifying and well selected.

The structure of the MS is very well.

The language of the MS isn't evaluated because it's not the native language of the reviewer.

All the recommendations that I have included here are mere suggestions that I think that can improve the explanations and comprehension of the paper, but it is up to the author to accept them or not.

#### **Particular comments of the MS:**

**Line 38-39:** You should mention that this problem has also been faced in other parts of the world. For example, in Spain, the recent study Fernandez-Fraile et al., (2025) makes a revision of the first part of the 20th century earthquakes, reevaluating all the contemporary sources.

Fernández-Fraile, J., Mattesini, M. & Bufo, E. Re-Evaluation of the Earthquake Catalog for Spain Using the EMS-98 Scale for the Period 1900–1962. *Pure Appl. Geophys.* 182, 1237–1261 (2025). <https://doi.org/10.1007/s00024-024-03461-9>

We agree with this comment. We neglected to mention this interesting work in the manuscript. We apologise!

**Line 45:** the “consistent criteria” are explained in the definition of EMS-98 itself. The criteria of Tertulliani et al., 2025 are particular for that work.

We are in full agreement. As stated in Line 46 of the track changes manuscript, this sentence focuses on Italian earthquakes that present incoherent or inconsistent data. Tertulliani et al. (2025) are an example of such events.

**Line 50:** Can you briefly indicate here the weaknesses of previous studies?

We have added a brief example of the weaknesses of previous studies (line 52 of the track changes manuscript) according to Section 3.

**Line 60:** “is highlighted in many studies” Indicate here some of them.

We have cited at line 63-64 of the track changes manuscript the articles by Azzara et al., 1993; Di Luccio et al., 2005; Frepoli et al., 2005; Boncio et al., 2007 and, Maggi et al., 2009.

**Line 99:** The intensity distribution may follow a pattern like the one you propose in line 106, but it will also be necessary to check whether the presence of high-intensity MDPs far from the epicenter is due to site effects or local amplifications. This cannot be ruled out without prior analysis. I have noticed that you mention it later (line 153), but I think you should mention it here.

We fully agree. This comparison of documented and synthetic intensities allowed us to identify potential biases in the intensity data, which could be due to errors in the intensity estimation or possible site effects. We have modified a sentence at Line 107-108 of the track changes manuscript.

**Line 111:** A complementary way to compare the MDPs with the model (the IPE proposed by Gomez Capera et al., 2024) is to study the residuals (differences between the model and the MDPs) and represent it with histograms or something similar.

Yes, in our work, we simply used the synthetic intensity to establish a systematic checking process for the macroseismic questionnaires, starting from those with significant differences between observed and calculated intensities (lines 120-121). However, an in-depth analysis of residuals is beyond the scope of this research.

**Line 122:** I miss examples of these exaggerations. I would include information literally taken from the questionnaires to support your conclusions.

As stated in line 140 and Figure 4, we provided an example in the text relating to the Apollosa site, which is located more than 90 km from the epicentre of the earthquake under study. Upon reading the questionnaires and comparing the information provided with other sources, we found exaggeration in the intensity estimate even at the Capodrise and Pannarano sites, which are located around 100 km from the epicentre. We have added a sentence to lines 143-147 indicating these two examples. The location of these sites is also shown in Figure 1a

**Line 193 and figure 9:** It could be interesting to represent the epicenters of 1980 and 1984 in the same figure, to study if the distribution of MDP with higher differences (between this re-evaluation and BMING dataset) are related to the previous earthquakes.

We highlighted potential overlap or cumulative effects from previous seismic events, such as the 1980 Irpinia earthquake and the 1984 Sannio earthquake, as the primary cause of the high intensity of the BMING dataset. Based on this, we would prefer to show the epicentres of these two earthquakes in Figure 5 rather than in Figure 9.

**Line 208:** Maybe a map with the information from Table 1 could be interesting.

We agree with the suggestion. To reduce the number of figures, we have decided to represent the epicentre of the final dataset in Figure 6, since this is strictly correlated with the two macroseismic distributions.

**Line 220:** Furthermore, the intensity distribution supports the conclusion that it is not a shallow focus. If it were very shallow, the earthquake would have affected the epicentral area and attenuated much more rapidly than an earthquake with a hypocenter at 30-40 km.

We fully agree with this comment. We have added a sentence at lines 235-267 of the track changes manuscript.

**Table 1:** You can include favorite depths in this table.

Sbarra et al. clearly state that uncertainty increases with depth, resulting in wider error ranges for depths greater than 35 km. For this reason, we prefer to present the range of computed depths in the text (Line 234) rather than including a single value in the Table.