

Report of the review of « Modern earthquakes as a key to understanding those of the past: the intensity attenuation curve speaks about earthquake depth and magnitude »

### **General Comments**

The authors present a two-step method to estimate magnitude and depth from macroseismic intensity. This method uses only data within the first 50 km around the epicenter. First step estimates depth from the steepness of the decay of intensity and second step uses the depth estimated in the first step and epicentral intensity to compute magnitude. The method was calibrated and applied to Italian data.

In my opinion, the strong points in the paper are the accessibility of the data used and the bibliographic study to collect reliable instrumental depth (finding reliable instrumental depth is generally challenging itself).

However, even if the authors did a lot of changes since the last review, the paper is still difficult to read. I found also some misuses of the bibliography. For these reasons, I recommend a major revision of the paper.

### **Specific comments**

The authors cite Kövesligethy, 1907, Sponheuer, 1960 and Musson, 1996 (lines 291-294) and write that « nearly all the methodologies developed in the past to calculate depth use magnitude as an essential input parameter », which is wrong. Indeed, Kövesligethy, 1907 used a mathematical formulation to estimate depth from the decay of macroseismic intensity with epicentral distance which does not include magnitude. Moreover, Sponheuer did an inventory of existing methodologies to estimate depth from macroseismic intensity including methodologies that do not use magnitude, especially the Kövesligethy, 1907 methodology. Nowadays the Kövesligethy, 1907 methodology is referred to as Sponheuer 1960 methodology, as in Musson, 1996. Musson, 1996 modified the Sponheuer methodology to estimate depth, once again without including magnitude in his methodology. Ambraseys, 1985 and Levret et al, 1996 estimated also depth based on Sponheuer methodology, independently from magnitude. In the work of Traversa et al, 2018 and Provost and Scotti, 2020, the magnitude and depth are not used as an input but as unknowns. The authors should correctly use the bibliography.

Lines 305-307, the authors writes that « Conversely, a functional form containing both magnitude and distance as independent terms would lead to a change in the shape of the attenuation curve with distance and to a variation of the steepness for a variable magnitude ». I don't understand this sentence. What do you mean by independent terms? As is it done in the Musson, 2013 and the Tosi et al, 2015 IPE? If it is the case, this sentence is wrong: I did the check for different depths, using for each depth different magnitudes. For each depth, all curves (obtained with different magnitudes) present the same steepness.

*Title*

I agree with the comments of the previous reviewer 2 : the title is too general in the first part and too vague in the second. The title does not help the reader to understand the exact content of the paper. When I read the title, I expect a more general approach than that the one described in the paper.

### *Introduction*

Introduction is long and quite confusing.

Why did the authors add a part about the half-degrees and decimal intensities in the introduction ? This part should be either deleted or moved with the description of the distance binning method line 159. In this case, the authors should precise if they use integer intensities, half-degree intensities or decimal intensities as « raw » data before using the distance binning method.

The introduction after line 84 should be reorganized to reflect the plan of the paper. This will help readers to find their ways in the paper.

### *2 Seismotectonic complexity and depth variability of Italian earthquake*

This section is quite confusing for the reader. It also introduces the notions for example of « new faults » and « inherited faults », which are not used afterwards. I understand that it is important for the authors to stress out the large variability of the depths in Italy and thus the importance to take into account depth when estimating magnitude. However this could be done in two or three sentences and could be done in the introduction or in the introduction of section 3.

### *3 Methodologies and data analysis*

The introduction of the long section 3 should also include a short description of the second step of the proposed methodology, i.e. the magnitude estimate. Currently, it only describes the first step of the method.

Line 159-160 : the part of the sentence « we use only well-located earthquake[...] » should be moved in the 3.1 Data selection criteria.

#### *3.1 Data selection criteria*

The authors should explain the two first criteria : why did the authors add criteria on magnitude to select their learning dataset ?

#### *3.2 Analysis of the learning set*

Line 268-270 : I don't see on the figure the difference of attenuation between the northern and the central-southern Italy datasets after 50 km. The authors should add in the text additional information, as for example the mean steepness after 50 km between the northern and the central-southern Italy datasets.

### *3.5 Reliability and validation of the depth estimation method*

The authors should give more details about the last sentence of this part. When I read the title, I expect a comparison between instrumental depth and the depth estimated by the authors. This part is missing. A figure similar to figure S1 or figure 10 in the supplements would be welcome.

### *4 Reliability of the magnitude estimation method*

The authors should include their estimated depth in the comparison with CPT15 in Figure 12, for example as a color of each point. It would perhaps (i) help them to explain the differences observed between the two magnitudes, (ii) highlight the particularity of their two-step methodology.

### **Minor comments**

Line 19 : I would not write that depth and focal mechanism are generally well-known. A favorable network geometry around the epicenter is necessary to have reliable depth and focal mechanism, which is rarely the case.

Line 45 : « Itlay affords a unique opportunity to explore this often overlooked problem » : which problem ? The use of integers, half or decimal intensities ?

Line 54 : earthquake instead of eqrthquake

Line 316 : even instead of eve

Line 369 : I would moderate the simpler and more intuitive part of the sentence. From my point of view, it would take more time to use a two step method than using the joint inversion. In my opinion, the part « it may allow a geological verification of the depth before estimating magnitude » is enough to enhance the authors methodology. Indeed, it is important to check at least the depth estimates in the light of geological and known seismicity when computing historical parametric catalogues.