| Rev. #1<br><br>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. | First of all, we would like to thank anonymous Reviewer #1 for his/her comments, which were helpful in improving the clarity and readability of the text. We have followed as much as we could his/her suggestions in the revised version of the manuscript. |
| **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 refered 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 | We thank the reviewer for the clarification. We modified the sentence to read, "Instead, other methodologies (Traversa et al, 2018; Provost and Scotti, 2020) are subject to a trade off between depth and magnitude, as both parameters are treated as unknown. Our approach is similar to that of (Kövesligethy, 1907; Sponheuer 1960; Musson, 1996) which is based on isoseismals drafting, but directly uses the fit of the attenuation curve computed on averages of the original MDP computed inside moving circular windows." |

| | |
|---|---|
| and Scotti, 2020, the magnitude and depth are not used as an input but as an unknowns. The authors should correctly use the bibliography. | |
| **Lines 305-307**<br>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. | We thank the reviewer for noticing this misspelled sentence. We meant to say quite the opposite, so we rephrased the sentence as follows: "Conversely, a functional form containing a term combining magnitude and distance would lead to a..." |
| **Title**<br>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 except a more general approach than that the one described in the paper. | We have changed the title following the reviewer's suggestion to:<br><br>"Inferring the depth and magnitude of pre-instrumental earthquakes from intensity attenuation curves." |
| **Introduction**<br>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. | We added this part of the text (lines 31-44) in response to a specific request from the previous Reviewer #1, whom we quote below.<br><br>"The manuscript needs a comment to qualitative nature of macroseismic intensity and the use of average intensities and rational intensity values instead of integer values".<br><br>Nevertheless, we moved it to the beginning of the "Methodology and data analysis" section, as requested by current Rev. #1 |
| 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. | We thank the reviewer for pointing this out. We streamlined and shortened the Introduction, which should now be easier to follow and more informative. |
| **2 Seismotectonic complexity and depth variability of Italian earthquake**<br>This section is quite confusing for the reader. It also introduces the notions for example of « new faults » and « inherited faults », which are | We do not see why this section appears "confusing" to the reviewer, and also overly long. We believe that the reason why this section is needed is clearly outlined in its final sentence: |

| | |
|---|---|
| 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. | *"The earthquakes generated by the new faults and by the inherited faults are often geographically overlapped, as seen in the Po Plain (Sbarra et al., 2019a), which makes their seismotectonic interpretation rather difficult if only the epicentral location is available. Conversely, assigning each pre-instrumental earthquake to a specific depth class helps assigning that event to its relevant domain, thus greatly supporting its seismotectonic interpretation and the calculation of accurate global earthquake."*<br>Clearly, in this section we describe a seismotectonic occurrence – the strong variability of earthquake depth in Italy – and delineate a fundamental, potential outcome of our work – inferring the depth of historical earthquakes to assign them to the relevant tectonic framework. Plotting historical earthquakes to show how close they fall to existing faults is a common thing to do, but if the depth of those earthquakes is larger than 20 or 30 km their potential association with the surface fault must definitely be reconsidered.<br><br>We necessarily had to postpone the interpretation of our results to a further paper. Nevertheless, we meant to propose a methodology that may be used by other workers in regions that exhibit a similar depth variability, such as Greece or southern Spain. |
| **3 Methodologies and data analysis**<br>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.<br><br>Line 159-160: the part of the sentence « we use only well-located earthquake[…]. » should be moved in the 3.1 Data selection criteria. | We added a brief description of magnitude calculation at the end of Section 3.<br><br>We moved the sentence at line 159-160, as suggested. |

| | |
|---|---|
| **3.1 Data selection criteria**<br>The authors should explain the two first criteria: why did the authors add criteria on magnitude to select their learning dataset? | As stated in the text, these two criteria are the same we used in our previous work (Sbarra et al., 2019). The magnitude selection criteria served mainly to avoid considering too many small intensity/magnitude events, which would result in an incorrect fit to the attenuation curve. A reliable fit requires a sufficient number of macroseismic data up to 55 km from the epicenter. |
| **3.2 Analysis of the learning set**<br>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 centra-lsouthern Italy datasets. | We rephrased the sentence, adding the average steepness values calculated beyond 50 km of epicentral distance for the northern and central-southern Italy learning-set earthquakes (shown in Figures 3 and 4). |
| **3.5 Reliability and validation of the depth estimation method**<br>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. | We did compare the instrumental depths with those estimated by our method for the learning set earthquakes: the results are shown in Table S2. A citation to the table has also been added to the end of "Reliability and validation of the depth estimation method" paragraph. We also added a new supplementary figure to the new Fig. S1, as requested. |
| **4 Reliability of the magnitude estimation method**<br>The authors should include their estimated depth in the comparison with CPTI15 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. | As suggested, we added the inferred hypocentral depth in Figure 12, differentiating earthquakes into three depth classes :<br>0-20 km;<br>21-30 km;<br>>30 km |
| **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. | We specified that we are talking about "damaging earthquakes", i.e. events whose magnitude is generally large enough to grant good quality data. We also removed "well" from "well-known", to avoid appearing too optimistic. |

| | |
|---|---|
| **Line 45:** « Italy affords a unique opportunity to explore this often overlooked problem » : which problem? The use of integers, half or decimal intensities ? | Here we were referring to the problem of determining what information can actually be gleaned from intensity models and how reliable it is. We rephrased the sentence to make this concept stand out clearly. |
| **Line 54:** earthquake instead of eqrthquake | Done. |
| **Line 316**: even instead of eve | Done. |
| **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. | Done. |