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Discussions

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Comment on nhess-2022-30

Anonymous Referee #2

Referee comment on "Modern earthquakes as a key to understanding those of the past: the intensity attenuation curve speaks about earthquake depth and magnitude" by Paola Sbarra et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-30-RC2, 2022

Anonymous Referee #2 comment on the manuscript Sbarra, P., Burrato, P., De Rubeis, V., Tosi, P., Valensise, G., Vallone, R., and Vannoli, P.: Modern earthquakes as a key to understanding those of the past: the intensity attenuation curve speaks about earthquake depth and magnitude, Nat. Hazards Earth Syst. Sci. Discuss. [preprint], https://doi.org/10.5194/nhess-2022-30, in review, 2022. Preprint dated/Discussion started: 17 February 2022 (in the following referred to as 'the paper'). Review posted 11 April 2022.

1. General comments

The paper presents an approach to derive the hypocentral depth of historical earthquakes from macroseismic intensity observations. The method is calibrated using the instrumentally determined depth of recent earthquakes. Subsequently, in a second step, the moment magnitude of the historical earthquakes is derived, again by calibration, using the instrumental magnitude of recent earthquakes. Whether this step-by-step inversion of depth and magnitude or a joint inversion is superior, in general or in individual cases, remains to be discussed further.

The paper can be seen as a continuation of an earlier publication by the authors (Sbarra et al., 2019a with application to Northern Italy), in as much as the method is now applied for the entire Italian peninsula.

Magnitude and depth of historical earthquakes are relevant to assess earthquake hazard in Italy and elsewhere. The paper contains a substantial contribution to the evaluation of historical earthquakes and in turn to the assessment of earthquake hazard in Italy. Upon revision, this work will be suitable for publication in the Natural Hazards and Earth System

Sciences (NHESS) journal.

My review is intended to contribute to an improvement of the paper. My review is mainly related to the readability, comprehensibility for a non-specialist, consistency of content, and relevance for publication in NHESS. I could not check all details, hence comments and suggestions in the review are not meant to be exhaustive. My review does not include verification, plausibility calculations or proofreading. I have not checked or validated the content of the equations, figures, tables, and references. -- In my review, I refer to the numbers of text line(s), figures, and tables of the manuscript. Double quotation marks ("...") denote text as quoted from the manuscript. Topics/issues referenced in a previous review by referee #1 (see RC1: 'Comment on nhess-2022-30', Anonymous Referee #1, 17 Mar 2022, https://doi.org/10.5194/nhess-2022-30-RC1) are generally not repeated or co-commented here.

On the whole, I recommend a quality check and major revision of the paper by the authors before publication in NHESS.

2. Specific comments

2.1. Title

In my personal opinion, the title of the paper is too general in the first part and too vague in the second. I would propose a title that reflects the content of the paper in a simple and specific way, similar to that of the pilot study Sbarra et al., 2019a, for example 'Inferring the depth and the magnitude of pre-instrumental earthquakes from macroseismic intensity data for Italy', or the like.

2.2. Pilot study 2019a

Apart from some minor updates in methodology, the paper is essentially an application of the Sbarra et al., 2019a method ("pilot work") to the whole territory of Italy. This should not be overlooked, and could be stated more explicitly in the abstract. However, this does not detract from the importance of the paper. The present work warrants publication in a unique form, not just as a follow-up. The paper should be self-explanatory (see also comment of referee #1), hence there should be no need to repeatedly refer the reader to the pilot study. It would be interesting to know a bit more about the differences in results compared to the pilot study and a statement as to which results are now regarded to be relevant for the N-Italy region.

2.3. Uncertainties

I would recommend some more discussion on uncertainties, for example: As the method is based on macroseismic intensity data, a statement about assessment and uncertainties of intensity in Italy would be helpful for the general reader. To what extent errors in intensity are transferred into those of inferred depth and magnitude of the analysed set? By contrast, what can be said about uncertainties of the instrumental data used in the learning set? In respect to error statistics, I suggest to explain somewhat more what has been calculated; further to use the standard terminology or, if in doubt, provide a formula (confidence interval, standard deviation or standard error, error bar, root mean square deviation, etc.).

2.4. Trade-offs

Possible trade-offs between depth and/or magnitude on one hand and seismic wave attenuation properties on the other hand are not clearly resolved in the paper, to my opinion.

2.5. Large magnitudes

The authors recognise that the "point source approach" is limited and propose a correction for "larger magnitude earthquakes" (Mw 6.75 and above, Section 3.7 of the paper). My suggestion would be to consider treating earthquakes larger than a certain threshold in a case-by-case fashion (arguments see below). In any case, it would be helpful to present the min-max limits of intensity, magnitude, distance, depth, etc. for which the method is applicable, preferably in a small table.

2.6. Cumulative effects

The authors are aware that for earthquakes occurring closely in time and space (multiple events, strong aftershocks, etc.; see e.g. Graziani et al., 2019 and several others) there is a severe problem to assess macroseismic intensities for separate events individually, particularly in cases of historical earthquakes, and particularly for the larger ones. Macroseismic data may then reflect accumulated effects. The method to infer depth and magnitude may fail completely in such cases. As for the results of the paper, I recommend that this problem be discussed in summary in a separate section rather than in individual passages.

2.7. Length of the paper

Notwithstanding the need to explain the method in general and some technical procedure in detail, the length of the paper could be shortened somewhat without loss of significance. Some repetition in the text could be avoided, and some less relevant or obvious details could be omitted, or moved to supplements (see my comments line by line below). A more stringent structuring of the manuscript would be helpful to make it easier to follow the 'red thread' (see also comment of referee #1) and to avoid addressing the same topic repeatedly in different parts of the paper (e.g. data selection criteria, compilation procedures, etc.). Furthermore, the consequences for the Italian earthquake catalogue, for seismic hazard and for seismotectonic implications in Italy resulting from this work are, in my opinion, beyond the scope of this paper. I recommend that these issues be discussed in a separate paper, since the target audience and objective of such discussions are different (and as promised in lines 84-86). -- On the other hand, some further explanations are needed in the paper (see details line by line below). Generally speaking, the figure and table captions could also be more detailed (as I think figures and tables should be understandable to some extent without reading the whole paper).

2.8. Wording, terminology, formal issues

Some more precision and uniqueness in wording would be helpful. Terms should generally be used in an invariable way (for example: 'hypocentral depth' (in short: 'depth'), 'moment magnitude' (in short: 'magnitude'), 'macroseismic intensity' (in short: 'intensity'), 'attenuation slope' (in short: 'slope'), etc.) to avoid possible confusions. Using variable terminology frequently for the same parameter, for what reason soever, may cause confusion (for example 'hypocentral depth' alternatively denoted as 'earthquake depth', 'focal depth', 'source depth', etc.). In particular, as steepness and slope denote the same parameter, either the term 'steepness' or the term 'slope' should be used, not both alternatively. I suggest to always use the same notation for the same parameter, if there is no reason not to. Also, the alternating way of denoting magnitude and depth results of the analysed set as being 'inferred', 'expected', 'estimated', etc. can occasionally cause confusion. Any possible confusion between analysed set versus learning set parameters as well as between macroseismic versus instrumental parameters should be avoided (see below).

On the contrary, whenever there is ambivalence a different designation is needed. The term 'attenuation', for example, is used in the paper mainly in the sense of decay of macroseismic intensity with epicentral distance but at some point also in the sense of seismic wave attenuation, an ambivalence that may cause confusion. In particular also, it would be helpful to always state whether parameters or values either have been derived from seismogram measurements ('instrumental') or from intensities ('macroseismic'); this concerns for example magnitude, depth, epicenter location, etc. A clear definition of the term 'pre-instrumental' and the term 'historical' is needed for understanding the paper.

For the general reader, all abbreviations, acronyms etc. should be explained, and referenced if necessary, the first time they appear in the text (for instance MCS, EMS; Mw, CE; as well as the data sources INGV, DBMI15, CPTI15, CFTI5Med, CSTI1.1, Italian CMT, ISIDe, DISS, IPSI, etc., a small table would be helpful for those); the same holds for terms that are not well known in general ('Rosetta stone', 'apparent magnitude', etc.). For figures, tables, and equations all physical quantities, parameters, and numerical values, as well as their respective errors, should be specified together with their symbols (if any) and physical units (if any), for example: 'depth D in km', 'slope S in km-1', etc., even though it has been done elsewhere already. For equations (formulas), the valid range of application should be specified.

3. Detailed comments

Line 9-12: How is this sentence to be understood? I presume it should be referred to the learning set ('… we observe for the learning set …'). Are all three observations, (1), (2), and (3), observed "rather unexpectedly"? For earthquakes beyond a certain size, observation (1) cannot be expected a priori, most obviously so for earthquakes that are both large and shallow (see the "larger magnitude earthquakes" in Section 3.7 and respective comments). On the other hand, I think that observation (3) could have been expected to some extent.

Line 15: ... 'by elastic and anelastic attenuation', I suppose.

Line 21-22: "... macroseismic intensity ... a rough proxy of a set of accelerometric records": I don't understand what this is trying to say (PGA?).

Line 25-26: This statement is vague, at least, and possibly misunderstood with regard to the term "damaging earthquakes". To take an example: What is the "length of the instrumental record" in Italy and what is "the average recurrence interval" of an intensity 6 MCS earthquake in the whole of Italy, or, for an MDP of intensity 6 in Rome, for example?

Line 34, 63, 82, 219, etc.: What is meant by "earthquake propagation", "propagation characteristics", "propagation properties", etc. ? Is it about seismic wave propagation?

Line 48 and 53: At these points it is the trade-off (singular!) between magnitude and depth.

Line 52: What is the meaning of "apparent magnitude"?

Line 61-66, 82-84: What can be said about mutual trade-offs (plural!) between magnitude, depth, and seismic wave propagation properties (in particular seismic wave attenuation)? What do the authors of this study and those of other studies think about the variability of relevant crustal properties in Italy and the possible influence on determining magnitude and depth of historical earthquakes? I suggest to clarify.

Line 87-133: Section 2 about seismotectonic complexity could be shortened (see my comment in 2.7. above). Figure 1 should be kept, however.

Figure 1: I suggest a figure caption text starting with: 'Location of the 42 earthquakes of the learning set ...', or the like. Mw is moment magnitude.

Line 138: Does the averaging include weighting, for example based on the number of responses per MDP (as might possibly be surmised from Figure 2b)?

Line 139: Actually not "curves" but data points are calculated and shown.

Line 139-142: What is the explanation of the observed change in slope ("abrupt drop") of the intensity attenuation? If it has something to do with the Moho reflections between 50 and 100 km epicentral distance (Gasperini, 2001) shouldn't it be observed almost everywhere?

Line 141 and also Line 243, 255, 278, 338, 453, etc. and caption of Figures 6 and 7: What is meant by "experimental" at these points? Isn't it rather 'empirical' or 'observational'?

Line 141-142: I suggest to formulate once in detail, for example: '… we calculate the slope of the line that best fits the intensity average data points from 0 to 50 km epicentral distance (in short: "attenuation slope" or "slope") ... ', or the like. For graphical explanation it could be referred to Fig. 2c and subsequent ones.

Line 149-153: I guess what is meant is '… each one is shifted …' and '... averaged MDP intensities ...'. I presume that the instrumental epicenter is used. I suggest to clarify.

Figure 2: I suggest clarifying which legend belongs to which panel(s) in the figure. Does the "Responses" legend in Fig. 2a also apply exactly to Fig. 2b? In what order are the MDP data plotted in Fig. 2a? There is a chance that the red and blue colors in Fig. 2b (circles for the rings) and Fig. 2c (dots for the averaged intensities) are confused with the red and blue colors used in the MCS intensity scale in Fig. 1a nearby. Moreover, the colors red and blue in adjacent Figures 1, 3, 4, and 5 again have a different meaning. Therefore, I suggest to consider changing colors for the circles in Fig. 2b and the dots in Fig. 2c, accordingly. I suggest adding: 'number of responses', 'intensity (MCS)', '… averaging the MDP intensities …', etc.

Line 166-181: It seems that quite a lot of criteria are necessary to form a successful learning set. Is the learning set, thus, a set of earthquakes that are particularly well suited to the method? How many earthquakes do not fit the 'linear fit of intensity attenuation up to 50 km' scheme?

Line 178: I suggest to specify the unit of the "attenuation slope" (km-1), and thus also that of its standard error (km-1).

Line 211 and following: I presume that 'slope' and 'steepness' are effectively the same parameter S (as in the paper the absolute value is taken for both, see Figure 5, Table 1, etc.). Hence there is no sense to distinguish between these two terms (see my comment in 2.8. above).

Line 213 and following: For Equation 1, I suggest to denote that S is given in units of km-1 and D is in units of km. This holds for Eq. 2, 3, and 6 as well. Bracketing the last two figures would be helpful for the appearance of the Eq. 1 to 4 (as it is done already in Eq. 5). How are confidences / errors calculated for Eq. 1 to 5?

Line 217: Is this referring to 'intensity attenuation' or to 'seismic wave attenuation'?

Line 225-226: I suggest to re-check numbers with Equation 3 and with green line in Figure 5.

Figure 4 and also Fig. 3: Comparing with Table 1 and Table S2, instrumental depth values (km) are expected to be given next to the event ID's in the insets (and should be mentioned in the figure caption accordingly). A spot check shows, however, that respective depth values in the tables are in a number of cases not the same as those in the figures (see also comment of referee #1). Apparently there is a need for a quality check and correction in the figures and/or in the tables, respectively. Intensity should be denoted as 'Intensity (MCS)'.

Line 229: same as in line 217

Line 229-231: Are there no similar "plateaus" in the intensity attenuation data in central and southern Italy? What difference do the authors expect in the effect of Moho reflected waves between northern and central/southern Italy?

Line 233: What is meant by "efficiency of the crust-upper mantle system"?

Line 238-239: For completeness of Equation 4, I suggest to add that Mw is moment magnitude, I is intensity (MCS), and to denote to which base 'log' is the logarithm (10?).

Line 237-240: How was Equation 4 derived? I would generally recommend that the derivation be documented when a "new intensity prediction equation IPE for Italy" is published here. A regression plot of the new IPE I(r, Mw) would be helpful as well (see comment of referee #1). Within what limits of r and Mw is the new IPE considered valid? Following the basic idea of the paper, shouldn't there essentially be two IPE's, one for the near and one for the far field? Finally, if I am not mistaken, the new IPE is not relevant for the results of the paper; nor is it used below, except for a casual comparison (in Section 3.5) with the IPE of Musson, 2005 (which applies to the UK, is not the most recent, and uses a different magnitude (ML) and intensity scale (EMS) anyway).

Figure 5: How are the "95%-confidence intervals" determined? How is "standard error bar" determined, is "bar" twice the standard error? I suggest to use the label 'Slope S (km-1)'.

Line 241: I suggest to re-word the section header to: 'Independence of inferred depth from magnitude', to avoid misunderstandings.

Figure 6: same as for line 241

Line 249-253: I suggest to maintain "invariance of attenuation slope with magnitude" to the earthquakes of the learning set in the first place. Whether this invariance holds for all earthquakes is an open question at this point (see also probable exceptions for the "larger magnitude earthquakes" discussed in Section 3.7). I also suggest to re-check and eventually re-word the statement "nearly all the methodologies developed in the past to calculate earthquake depth use magnitude as an essential input parameter", along with the references listed here in connection with this topic.

Line 254: The header of Section 3.4 is inconclusive, in my opinion. It could be re-phrased to 'Comparison with synthetic models', or the like.

Line 258-260: To my opinion, there generally is no "endemic lack of interest" in hypocentral depth (see also comment of referee #1), but there frequently is lack of data to determine depth.

Line 268-269: Which magnitude(s) have been used for the "hypothetical earthquake"? It should be stressed that Figure 7 is exemplifying the case of a "M 5.0 earthquake". Can the results of Section 3.4 be generalised for all magnitudes relevant in this paper?

Figure 7 and corresponding text in Section 3.4: I suggest to unify terms (see my comment above in 2.8.) What is the meaning of M (Mw, ML, or other, respectively)? I would suggest to refer the green line in Fig. 7b to Eq. 3, as it is done in Fig. 5. Data points in Fig. 7b do not necessarily need colors, as they are coded by symbols already (colors in Fig. 7b must not be mistaken with that of Fig. 7a, anyway).

Line 262 and Figure 7: The IPE of Musson (2005) uses ML whereas the IPE of Eq. 4 uses Mw. Why not take the updated IPE relation of Musson, R.M.W., 2013, Updated intensity attenuation for the UK. Nottingham, UK, British Geological Survey, 13pp. (OR/13/029), which is for Mw as well? Moreover, Musson (2005) and Musson (2013) are for EMS whereas IPE of Eq. 4 is for MCS, I presume. The agreement of the data from the IPE of Musson (2005) with that of the IPE of Eq. 4 is surprisingly good, though.

Line 276-278 and Figure 7: The differences in slope caused by use of the two different conversion equations seem to be larger than any other difference from prediction models in Fig. 7b; I suggest to clarify and re-phrase eventually. Is there any idea why the empirical (observational) results for Italy (Eq. 3 and green line in Fig. 7b and Fig. 5) generally 'over-predict' the hypocentral depth for the deeper earthquakes compared to the (synthetic) prediction models shown here, or the other way round. Why, in particular, the slope S from Eq. 3 (Fig. 7b, green line) deviates considerably from the slope derived from the IPE Eq. 4 data even though the underlying data set is the same? To my opinion, the "trend" of the "curves" of the prediction models (the four dotted ones in Fig. 7b) is quite similar, for the empirical one (green line) it is not.

Line 285-286: see my comment above (2.3.)

Line 290-301 and Figure 8: The "depletion test" procedure has not become clear enough, to my opinion (e.g. how many slope calculations in total?). I suggest to re-phrase and explain in more detail with reference to Figure 8.

Line 297-298: I suggest, to clarify what is meant by "For deriving Eq. 3 we use an even more conservative selection of learning set data". Hence, is Eq. 3 derived from a sub-set of the learning set that is "even more" conservatively selected? The conditions for derivation of Eq. 3 should be presented elsewhere (in Section 3.1 and 3.2, I guess).

Line 300-301: What does "only a few MDPs" mean at this point? -- Taking 30 MDP's in 10 distance rings ("windows"), just for example, must there be an average of 3 MDP's "homogeneously distributed for each distance window"?

Line 302-307: I recommend to clarify and re-structure the text concerning the various criteria applied to the learning set (42 events) and to the analysed set of earthquakes (206 events). It would be helpful to have the criteria for the analysed set clearly set out in listed form (similar to that of the learning set in Section 3.1); maybe in two versions, first: general selection criteria of the analysed set, second: stricter criteria (from averaging particular values of the analysed set) to distinguish a class of slope values that are significantly "more reliable" (see above). By the way, couldn't that be read from the standard error of the slope (Table S1) as well?

Line 308-315: The example given here is well suited to demonstrate that applying this method for earthquakes occurring closely in time and space poses a most severe problem to the method. See my comment above in 2.6. I suggest to add ID-numbers for reference with Table 1.

Line 311: There is a typo in 'http'.

Line 317: The statement "depth is independent of magnitude" needs a restrictive relation to the context. I suggest the wording: 'the hypocentral depth inferred for the analysed set is independent of magnitude up to a certain size', or the like.

Line 318: "affects the y-intercept" or 'is derived from the y-intercept', what is meant at this point?

Line 320: I suggest to complete: '... and decreases if depth increases for a constant magnitude'.

Line 323-327: I suggest to describe the regression analysis leading to Equation 5 in more detail (in comparison to the regression leading to the IPE, Eq. 4, see above). What is the valid range of application of Eq. 5? What is the difference (it seems to be large) between the relation Mw(r = D, I = IE) taken as a reversion of Eq. 4 at the epicenter on one hand and the relation Mw(D, IE) of Eq. 5 on the other? In other words, I suggest to explain for the reader why Eq. 5 has not just simply been derived from a reversion of Eq. 4 (in a way it apparently had been done in the pilot study Sbarra et al., 2019a).

Line 325-326: What is meant by "the contour lines of the function that accounts for the geometrical spreading from the hypocenter to the epicenter"? Why just "geometrical spreading"? I suggest to clarify.

Figure 9: I suggest to re-phrase the caption describing what is shown in the figure in more detail. I also suggest to use detailed wording for moment magnitude Mw inferred from y-intercept, hypocentral depth D, and expected epicentral intensity IE (see my comment

above 2.8.), and to add '... is shown with colored lines for Mw 4.0 (blue), Mw 5.0 (green), and Mw 6.0 (orange)', or the like.

Line 328-330: I recommend to publish the "attenuation curves" of the 206 earthquakes of the analysed set in the supplements.

Line 333-340: There may be pro's and contra's of a step-by-step and a joint-inversion method. Given the well-known trade-off between magnitude and depth, a joint inversion is appropriate from the outset, in my opinion. I see advantages of a joint inversion particularly in cases where magnitude and depth are poorly constrained, and possibly also in cases of "larger magnitude earthquakes" (see below).

Line 341-380 (Section 3.7): The procedure of "variable moving windows" dealing with "larger magnitude earthquakes" is hard to comprehend from the text and poorly justified, in my opinion. What does the slope of intensity attenuation measure in such cases? It would be helpful to also see the "attenuation curves" of these large earthquakes (e.g. in the supplements). An additional figure could help to explain the procedure, and in particular the geometries and parameters (Re, RJB, etc.). Several questions remain open, for example, why "every fault" (!) is assumed to have a dip angle of 45 degrees (line 348-349). Apparently Mw 6.75 is adopted as a threshold for the "point source approach" in the method. 21 earthquakes out of 206 of the analysed set seem to need a correction in this respect. Unfortunately there is no learning set for these larger earthquakes. Moreover, for the larger earthquake sizes epicenter location and hypocenter depth become less relevant. Would it not therefore be better to limit the use of this method to the smaller earthquakes and leave the larger ones to a case-by-case examination? See my comment above in 2.5.

Line 357-365: The 'cumulative effect' for intensity is a serious problem for the entire method. It is, however, not limited to "larger magnitude earthquakes" (this section). Due to its importance it is better to discuss it in a separate section, see my comment above in 2.6.

Line 366-374: The example of the 13 January 1915 Marsica earthquake apparently did not show significant differences of slope after correction. Does this finding hold for all (21) "larger magnitude earthquakes" of the analysed set?

Line 379: The formula for correcting IE does not appear to be a mathematical equation, but a computer program assignment. The meaning of S in this formula is not specified.

Line 383: Which parameters are meant by "their parameters"?

Line 390-392: For Equation 6, I suggest to recall that D is hypocentral depth in km and S is attenuation slope in km-1. For the valid range of application it could be referred to Section 3.2.

Figure 10: The depth scale is imprecise; I suggest to improve color gradations and subdivisions of depth scale, and to denote 'Depth D (km)'. Legend for magnitude scale Mw could be improved such that it does not use a color from the depth scale legend. What is the sequence in which the data are plotted on the map? Seismotectonic information (back arc extension, ... etc) is not mentioned in the caption, is it relevant for the earthquake data shown in the figure (see also Fig. 1)?

Line 394-395: I suggest to clarify, rephrase the sentence, and clearly distinguish the cases D < 5.0 and D = 5.0, as well as D = 73.0 and D > 73.0 km (also for in Table S1).

Line 397: "instrumental location of the learning set earthquakes", I suspect this relates to their depth in particular.

Line 404: "departure", wording?

Line 405, 411, etc: "mean squared" or 'root mean squared'?

Line 407-408: I suggest to add a reference for the "Boxer method".

Line 415-419: Which results (Table S1) "may appear unrealistic"? -- If some results seem unrealistic, I would propose that the authors label these results as 'apparently unrealistic', or, if there are serious doubts, even omit them, together with a justification for doing that.

Line 420-423: This statement (and example) is evident from Equation 5 and Figure 9, and hence fits better in Section 3.6, I suppose.

Line 412, 424-442: Comparing magnitude Mw estimates using the "Boxer method" with those using this method, which are considered more reliable and why?

Line 431-435: From my point of view, I would not call the two estimates "generally consistent", but rather 'slightly but significantly different'. Is there any idea how this difference can be explained?

Line 432-434: Why do the magnitudes of "pre-instrumental earthquakes" (i.e., no seismogram data available) depend on "differences in the response of pre-1960 seismographs"? I suggest to clarify.

Line 437-440: "It is important to be aware ...", does this statement refer to the Boxer-Mw or to both?

Figure 12: Are the data shown in the figure "all the events" of the analysed set (Table S1)? I suggest to clarify in the caption.

Line 441: "... on average our seismic moments are 2.3 times larger than those obtained with conventional methods", I suggest to give a reference for the M0(Mw) relation used. What is meant by "conventional methods" and which M0 values have been compared?

Line 443-470: I suggest starting (rather than ending, line 463) the conclusion section with the usefulness of this method for deriving depth and magnitude from macroseismic data of historical earthquakes in Italy. An a priori definition of "pre-instrumental" and "historical" in the context of this paper would be helpful. Without further investigations, all findings should be limited to Italy.

Line 447-450: I suggest mentioning that HSIT data were only used in the learning set due to availability. The statement "HSIT data were .... almost always the only available observations", is not clear at this point. Does "observations" mean "macroseismic observations" here? I suggest to clarify.

Line 454-455: '... independent of magnitude up to a certain threshold' ? (see above).

Line 467: 'The historical records in Italy ...', I suppose.

All tables: I suggest that the table headings and column headings be described and specified in more detail, even if the details have already been explained in the text. This is especially necessary for the supplements since the text is not in the same file. I recommend that the authors perform a quality control on the tables. I also suggest that the content of all tables be standardized in terms of format, resolution, terminology, layout, alignment, missing decimal places, etc. For unification of terminology see my comments in 2.8. above. Physical units are needed throughout.

Table 1: I suggest to complete the table caption and the column headings, for example:

moment magnitude Mw (my question: are the values in this column all instrumental Mw?), source of Mw (refer to the list of References), epicentral longitude (degree E), epicentral latitude (degree N), hypocentral depth (km), source of depth (Two questions: 1. Depth values in this table are supposedly instrumental, why is depth termed "estimate" at his point?; 2. Is this also the source for the epicenter?), number of MDP's within 55 km epicentral distance, total number of MDP's, data source for MDP's, attenuation slope S (km-1, this study), standard error of the attenuation slope S (km-1, this study) (Questions: How is it calculated? Why "bar", from Fig. 5 one could assume that "bar" denotes the doubled error, this is to be clarified, see comment in 2.3.), intensity intercept value IE (MCS, this study). -- I suggest aligning the numbers in columns appropriately. -- The results of the learning set of this work are shown partly in Table 1 (attenuation slope and intensity intercept), partly in Table S2 (inferred depth and inferred moment magnitude ("intercept Mw")). Table 1 and Table S2 are mostly identical (or should be). Hence, why not just merge Table S2 with Table 1?

Table 2: I suggest to complete the table caption: 'Comparison of macroseismic Mw estimates (this work versus Boxer-code results) with instrumental Mw for 15 earthquakes of the learning set.', or the like. I suggest also providing ID numbers of the events listed for reference with Table 1; "Time UTC" and "Source of instrumental Mw" columns can then be omitted from the table. A quick spot check revealed that ID 2 dated 2-May-1987 is listed here as 5-May-1987. In addition, some Mw values (y-intercept, this work) differ from those reported in Table S2. I suggest consistency of the data in the tables. I assume that further quality control is needed. -- Taking the instrumental Mw as the reference, the differences with regard to Mw (this work) and Mw (Boxer) can be compiled and evaluated. Is there an improvement in Mw (this work) over Mw (Boxer)?

Supplement Table S1: I suggest that the table be explained in more detail in the caption and in the column headings along the lines I have commented for Table 1 (above), particularly because Table S1 is part of the supplement and hence not contained in the paper itself. For this reason, references to the text, equations, figures, reference list of the paper are needed. I suggest to unify terminology, wording, data resolution and format, sequence of columns, etc. as much as possible with that of Table 1 (which could be combined with Table S2, as suggested) and with that of the text of the paper. Seismic moment M0 is derived from the corresponding Mw, I assume (a reference is missing, I suggest to use the Hanks & Kanamori, 1979 formula), and is thus not independent information in the table. -- In particular, I suggest to complete: Physical units (if any), explanation of the data source abbreviations and references to DBMI, CPTI15, epicentral longitude in degrees E, epicentral latitude in degrees N, moment magnitude Mw, attenuation slope S (km-1), inferred hypocentral depth D (km), number of mobile averages within the first 55 km epicentral distance (what does "mobile averages" mean?), standard error of attenuation slope S (this column could be expected next to the slope), number of azimuth slices (the meaning of "azimuth slices" is unclear), number of MDP's ..., intensity intercept value IE (MCS), moment magnitude Mw (from y-intercept, this work), seismic moment M0 (Nm, from ... of CPTI15), seismic moment M0 (Nm, from Mw of this work), or the like.

Supplement Table S2: The contents of Table 1 and Table S2 largely overlap. I suggest to merge Table S2 with Table 1 (see above).

-- end of review --

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