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Comment on nhess-2022-30

Anonymous Referee #1

Referee comment on "Inferring the depth and magnitude of pre-instrumental earthquakes from intensity attenuation curves" by Paola Sbarra et al., Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-30-RC1>, 2022

Review of Sbarra et al.: **Modern earthquakes as a key to understanding those of the past: the intensity attenuation curve speaks about earthquake depth and magnitude**

The authors of this manuscript present a procedure based on seismic intensity to determine first the focal depth of earthquakes in Italy and on a second step to determine the magnitude once the depth is assessed. The procedure is an extension for the whole Italy of that developed in a former work of the authors which was applied to earthquakes in Northern Italy.

Main Comments

The manuscript seems hasty and the text is not precise and/or formal enough in many parts. Also, the manuscript has too many references to Sbarra et al (2019a) assuming the reader is familiar with it. Even if the present manuscript is an extension of the work of Sbarra et al (2019a), it should be self-explanatory by itself. Thus, the manuscript needs extending descriptions and/or giving enough details whenever necessary so that the reader is able to follow it.

The structure of the manuscript is confusing for the reader; it would need some reorganization. I would suggest e.g.:

- Introduction
- Seismotectonic complexity and depth variability of Italian earthquakes
- Methodology and data analysis (original section 3 adding a description of the method to

estimate depth from the steepness)

3.1 Data selection and analysis (original sections 3.1, 3.2, 3.3,3.4)

3.2 Reliability and validation of the depth estimation method

3.3 A two-step method for estimating magnitude based on intensity and depth (original sections 3.6 and 3.7)

3.4 Reliability of the magnitude estimation method (original section 3.9)

4. Application to the CPTI15/DBMI15 catalogues (original sections 3.8 and 3.10)

5. Conclusions

Regarding the data and procedures more specific details and discussion is needed on the following:

The text should incorporate a short description of the catalogues used and referred in the text. The 'analysed set' should be described in e.g., 3.1

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. Are the averaged intensities normally distributed? I recommend a check.

Both steepness and slope are used indistinguishably but formally the meaning is different.

Because some of the relations are not supported with figures showing the data I suggest that the authors include some figures at least in the supplementary materials (see detailed comments). The location of the epicentre in the analysed events is never detailed, it seems both instrumental and macroseismic epicentres from the catalogues are used, but whether or not it would affect the results differently or the estimations of depth and magnitude is not addressed.

In general, the formal statistical validation of the procedures and the formal estimation of uncertainties should be improved.

The text lacks discussions on critical and key issues such as:

- Possible distortion introduced by the instrumental epicentre/hypocentre in the computed distances and linear fits considering that the point where the fracture originates is not necessarily the point from which seismic wave energy radiates (Ground motion, macroseismic intensity).

- Choice of crossover distance of 50 km even for such seismotectonic complex region as specified in section 2 which would imply Moho depth variations and thus fluctuations on the distance at which reflected/refracted phases control the attenuation (see detailed comments). Some discussion and/or some estimation of the uncertainty introduced by the assumption (some trials to check the choice of 50 km crossover distance?)

- Learning set macroseismic data: How much does mixing HSIT and dedicated traditional studies affect the results of the learning set ? (see e.g., Hough, BSSA 103:2767-2781, 2013; Hough, BEE 12:135-155,2014)

- Uncertainties in location (epicentre/depth) in learning set. Depth uncertainty is critical in the analysis.

- Fitting of the slope-depth function. The curve is not constrained for depths above around 35 with only few data and apparently some outliers would need to be more extensively discussed. Also, while uncertainty in slopes is taken in account, depths are assumed not to be affected by uncertainty although the authors are well aware of it as mentioned in P13 L259-260 (see detailed comments P8 L170).

- Residual plots (Obs-Calc), not included in the manuscript, will greatly help to check for unbiased estimates of the empirical parameters.

The comments and the suggestions in below are meant to improve the quality and readability of the manuscript and figures.

Detailed comments

P3 L61-66: Very unclear, this paragraph needs rewording and adding corresponding references to support the statements in the text. In its actual wording it is difficult to read and follow. Please explain better and refer to the literature.

P3 L74: The derived empirical equations relate the decay of intensity with distance (slope) with depth. They are not "Intensity-depth equations" as it reads. Please correct

P3 L78: It reads "($M_w \geq 6.75$), because their causative fault cannot be assumed to be a point source....". The assumption of a point source is valid or not depending on the distance from the source not only on the magnitude. Please elaborate and explain better.

P4 L82-83: Where throughout the manuscript "the role of crustal propagation properties versus the variability of depth" is evaluated as stated?. Please clarify.

P6 L125-129: It seems that quite a number of events in the learning set with depths > 30 km (e.g., 31, 19, 32....) do not fall in any of the four independent depth classes described (most of them are not in the Calabrian Arc) This is confusing. Please review.

P6 L130-133: I suggest to reword "... making difficult their interpretation just with the usual parameter determinations in the case of historical earthquakes for which only epicentre locations are given."

P6 L137: "well-located" is weak phrasing, how well? criteria, uncertainty,?

P6 L138: "within 10 km-wide ring-shaped moving windows" Specify where the origin (0,0) of the rings is set.

P6 L140-141: I suggest to reword "... as described in Fah and Panza (1994) and Gasperini (2001) and empirically observed by Sbarra et al. (2019a) for earthquakes in Northern Italy".

P6 L146-147: "Notice that the size of our circular moving windows is now calculated from the earthquake epicenter rather than from the innermost MDP average, as proposed by Sbarra et al. (2019a)". Hasty?

Size? Should it be "radius", or do you mean origin (0,0)? Please clarify

"circular moving windows" or "circular rings moving windows"?

earthquake epicentre" I assume instrumental? It should read "instrumental earthquake epicentre"

"innermost" is innermost 10 km? specify Explain and clarify.

"MDP average" MDP average would be an intensity measure not the location of a point. Do you mean barycentre? Explain and/or clarify

P6-7 L147-148: "This minor improvement makes the algorithm more uniform across the full earthquake magnitude range" why? Explain.

P7 L148: "The new procedure" I would suggest new "approach". It is not really a new procedure

P8 L169: criterion #2 why? Explain

P8 L170: criterion #3 even if depth is instrumentally determined, it does not warranty the quality or small uncertainty in the calculated depth value. It seems a very weak criteria considering that it is one of the most critical parameters in the further analysis of the learning set and in the results. I would suggest to establish more strict criteria for the instrumental locations, especially in what regards depth determinations.

P8 L171-172: better ".....within a week time since the mainshock...."

P8 L177: "... six or more averaged points" I guess "in each of the rings"? specify.

P8 L179-180: the reason for making an exception and include these two events #6 and #17 does not seem strong enough

For section 3.1, it would be helpful an Appendix (e.g. as supplementary material)

including some figures illustrating the geographical distribution of MDP's for each of the 42 selected events within 50 km distance from instrumental epicentre.

P9 L194-195 Which latitude separates Northern Italy from the rest of Italy in the analysis?, and why this latitude?

P9 L196: "... lithospheric structure and wave propagation properties are rather homogeneous ..." "rather" is weak phrasing, meaning? Include references in the literature.

P9 L200: "... slightly different ..." This is weak phrasing. How much? Approximate range? Please quantify.

P9 L207: I suggest "... distance of about 50 km (see Figures 3 and 4)".

P10 L223: "... quite good ..." Weak phrasing. Explain

P10 L225: "... $0.058 \leq S \leq 0.012$..." formally should be $0.058 \leq S \leq 0.0097$ or rounded, $0.058 \leq S \leq 0.010$

P11 L229-230: "... In particular, the attenuation of earthquakes occurring in northern Italy, where the crust is ..." Quite confusing sentence when looking at the plots in Figure 3, there is not a regular or homogeneous behaviour (e.g. # 6, # 8, #19.....). Need to be more precise, rephrase maybe adding "typical", "frequent"

P11 L235: "... implying that **most likely** it does not show in our analyses ..." Weak phrasing, why do you assume this? To what small extend do you assume it? Surely negligible in the analysis? If different crust-mantle systems, some differences are to be expected even for crustal phases dominating closer distances and also on the transition distance from crustal phases to Moho reflected or refracted phases.

P11 L237-L238: Add a new Figure with a dispersion plot showing the points Intensity-hypocentral distance and magnitude isolines.

P11 L239-240: "... This equation rests on the assumption that the macroseismic fields used to build it contain fairly well-distributed data, both in the near-field and in the far-field ..." Weak phrasing. Does the assumption hold? "Fairly well" meaning? Are lower

intensity data at long distances equally represented in the learning set? Are data complete for lower intensities in HSIT? Below which distance? These details should be specified and discussed and given to establish the range of hypocentral distances for which the relation can be considered valid.

P13 L249-250: "The invariance of the attenuation slope with magnitude ..." I suggest "The invariance of the attenuation slope with magnitude for the events shown ...". Few examples as shown do not allow to extend this statement to all earthquakes (learning and analysed sets) and state "invariance" as it reads. Some more discussion (expected uncertainty at most introduced by this assumption) and more details are needed to support it which is key in the procedure following.

P13 L258-259: "The endemic lack of interest for this parameter....." I don't think this statement is supported. I don't find advisable at all this general judgement. Rephrase.

P14 L264: "... magnitude-distance mixed term ..." I suggest "... both magnitude and distance as one of the independent terms ..."

P14 266: "... to turn the PGA ..." I suggest "... to convert the PGA ..."

P14 L271: I suggest "... using the IPE proposed by Musson (2005) for a magnitude M5.0 ..."

P14 L276-277: "... It is worth noting that the differences caused by the use of the IPE in place of the GMPE are comparable to the differences caused by the use of two different conversion equations ..." Very unclear sentence, please rephrase

P14 L288: "... reliable estimate ..." Weak phrasing. How "reliable" is defined?

P14 L291: What's the criteria for reducing the number of MDP's? randomly?, azimuthally dependent?

P14 L292: "... The regression of the attenuation ..." should read better "... The regression linear fit of the attenuation ..."

P15 L294: 0.01 is considered as the threshold for the standard deviation or for the

standard error (in agreement with criterion #9 in P8)? Being in the order of magnitude of the steepness values, isn't it maybe too high for a threshold?

P15 L299: Please explain how "reliable" is defined, in relation to uncertainty

P17 L325-326: "... and the contour lines ..." I guess they are isolines of the function (magnitude isolines) not contour lines

P17 326: "... of the function that accounts for the geometrical spreading from the hypocenter to the epicentre ..." Explain better

P18-P19 L360-365: Eliminate: "... which make it difficult to separate the individual contribution of a specific shock to the cumulative damage (Grünthal, 1998; Grimaz and Malisan, 2017; Graziani et al., 2019); a circumstance that would ultimately affect the attenuation slope and hence contaminate the inferred earthquake depth. This is a recurring problem in historical earthquake catalogues; a condition that is hard to overcome even for modern earthquakes, and even if a very rapid damage survey is carried out, because the first large shock inevitably causes an increase in the vulnerability whose effects on later shocks are virtually impossible to identify ..." It does not directly relate and does not add to the explanation of the choice of the 7.1 magnitude earthquake.

P19 L366: The sentence should read: "... the R_{JB} distance or using the moving window or the variable moving window ..."

P19 L367: "... modest fluctuations ..." Weak phrasing. Please quantify or give a threshold

P19 L367-368: Which is the range of the errors arising from the uncertainties in the epicentral location?

P19 L374: The analysis of a single earthquake shouldn't support a general conclusion for the set of 21 earthquakes in the analysed set.

P19 L375: "... is not negligible ..." Why?, range? Specify

P19 L386-387: Eliminate the title of section 3.5. Not needed.

P19 L387-388: Add a figure in the supplementary materials including a histogram showing number of MDP's at distances less than 50 km for earthquakes in the analysed set (to complement table S1)

P19-21: In section 3.8, a discussion of the results on depth and magnitude estimation should be added.

P22-23: In section 3.10, a discussion of the results of the comparison should be added.

P22 L432-435: The trend of the estimated y-intercept M_w is mostly above the CPTI15 M_w . CPTI15 M_w includes both instrumentally determined magnitudes together with macroseismic determined magnitudes. If according to Vanucci (2021), instrumental M_w might be overestimated this would explain the differences for part of the data. How would the overestimation be explained in the case of macroseismic determined magnitudes in CPTI15?

P24 L445: Need to be more precise, e.g.: "... i.e. from the traditional macroseismic data in DBMI15 and from the new web based macroseismic data in HSIT dataset ..."

P24 L451-452: According to L444 this conclusion holds for Italian earthquakes not for "any given earthquakes". Review.

P24 L453: Better "... Based on our learning set empirical observations ..."

P24: The Conclusions should incorporate an important part devoted to the involved uncertainties if to be applied in seismic hazard studies.

Figures

Figure 2 caption: "... first 50 km from the epicentre ... "... areas centered in the epicentre ..." Specify "instrumental" epicenter

Figures 3, 4:

Y_labels: should read Intensity (MCS)

Caption: No curves are shown in the plots, it is the data points together with the linear fit for distances less than 50 km. Reword the captions.

Besides the ID and depth in the inset in each individual graph include also the M_w and the number of MDP within 50 km (at least!, if not also the at all distances). I assume depths in insets correspond to instrumental depths, specify in reworded caption.

Figure 4: Depths in inset of each plot do not correspond with instrumental depths (I assume it is instrumental depths as in Figure3) in tables 1 and S2. Clarify and/or correct accordingly.

Figure 5: For clarity, I suggest to use smaller sizes of symbols and smaller fonts for text for the data points. Thicker line for the complete dataset curve, thinner for the two other curves.

Figure 6: As in Figures 3 and 4, include in insets M_w , Depth and number of MDPs.

Figure 7a: Musson's IPE is developed for EMS intensities not MCS, review label in Y axis.

Figure 7b: Formally the green line should be Eq. 3 and not Eq. 6. Correct

Figure 9: The coloured lines should be labelled and/or explain it better in caption. "expected intensities" meaning?

Tables

In tables, include the meaning of acronyms in all table captions (e.g. ISIDE, HSIT, DBMI15, CPTI15.....etc) even if this is described in text.