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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "When probabilistic seismic hazard climbs volcanoes: the Mt Etna case, Italy – Part 2: computational implementation and first results" *by* Laura Peruzza et al.

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Dear Anonymous Reviewer, Thank you very much for careful and helpful revision. We hope we have satisfactorily answered to all the questions you raised, and appreciate your comments, as the manuscript in our opinion is now better than before. Here in the followings the detailed list of your comments is given with our replies, a zip attached contains the revised manuscript, with tracked changes and some figures that have been modified; the quality of other pictures is fixed in the original ones.

On behalf of all the authors Laura Peruzza

R2. Abstract

Reference of Ordaz et al., 2013 is cited in order to justified the used of software CRISIS 2008. Nevertheless the software package has been upgraded since this publication with CRISIS 2012 and more recently CRISIS 2015. Why the last version of CRISIS has not been used? => https://sites.google.com/site/codecrisis2015/home

We modified the text to make clear that the most recent release available has been taken into account, with updates that we developed that are now available for users. The references are old, as not all the new versions of software packages have an accompanying paper.

It is well explained in the text of the paper why M>6 regional seismogenic sources have not been taken into account. But in the abstract it is not clear why these faults have not been considered, as it is standard in PSHA studies, even if calculation are for short return periods (5-30 years). This fact could be repeated because it is an important limit of the study to focus on short return periods.

We acknowledged the limit in the abstract, and briefly comment a simple disaggregation tests that we performed (Figure A1-4) for the benefit of the discussion only.

R2 Introduction I have no comment on this section.

R2. Seismic Source Model

Difference between level 1, level 2 and level 3 are not very clear in the text. Nevertheless the figure 2 is remarkable and explains well these differences. Figure 2 should be largely cited in order in order to make sure that this paragraph is more readable. It is not clear if maximal magnitudes used for Source Model level 1 are for the 4 zones Mmax 5.2 or they are the maximal magnitudes indicated in Table 1 as in Source Model Level 2. It should be specified. Even if it is written in line 30-31 of page 4 "that the Pernicana Fault is always modelled as GR, not having historical observations supporting the choice of a characteristic earthquake model", characteristics of historical and geological-kinematic models are indicated in Table 2 that is confusing.

We modified the text to eliminate duplications and hopefully to increase the readability of this chapter.

Page 4, line 19: XIX century => XIXth century. Done

Definition of Figure 1 must be improved The original image has higher resolution, some minor bugs fixed (coordinates were wrong, now corrected, surface Etna outline on the top)

R2. GMPE at Mt Etna

This section is very interesting but it is in my opinion unbalanced: it is too developed or, on the contrary, too short and too superficial. A distinct paper could maybe favorably describe the procedure and the results to develop a specific GMPE for this Etna case study. Developing a new GMPE is always touchy and must be carefully studied. So this part should be develop if it serves as reference for a new GMPE in a volcanic context or it should be simplified if the GMPE is just an element of the PSHA, as in this study. For instance lines 27-29 page 8, lines 1-3 page 9 lines 18-30 are not indispensable. In particular the lines about the standard deviations are not useful for the study because it is not used in the following of the paper. The conclusion “higher inter-station variability than the inter-event component, suggesting that the local site effects are the main source of ground motion variability for our data” is not used in the following, mainly in the dedicated part on site-specific response. Nevertheless, it could be interesting to compare the variability estimated here with the differences in the site-specific response part.

We tried to keep it short as many of the issues are already explained in a previous paper by Tusa and Langer, 2016. Nonetheless we keep the information regarding the statistical parameters, as the GMPE presented here uses hypocenter distances instead of epicenter distance, so reporting the statistics is mandatory. Moreover, we

removed the lines that you defined “not indispensable” from the original version of the manuscript.

Line 11, page 8 it is written that “about the same values” are predicted by ITA10 and this model near the source. On the Figure 19 we note that the difference for ‘soil A’ could be about a factor of 3 for distance less than 1 km (about 30 cm/s/s for this study and more than 100 cm/s/s for ITA10) and so these are not about the same values. Moreover there are no data to check this observation because on the figure 3 we note that there are no data with d<3 km, for 3.9<M<4.1. This paragraph could be formulated with other words even if the idea is well explained.

Only the number total of data (1’200) used for the calculation of this GMPE is indicated and not the repartition of these events. On the figure 3, only 3.9<M<4.1 events are represented. Moreover on the figure ESM1 no data are represented. As usually done in articles which deal with a new GMPE it could be useful to indicate the repartition of the data used in terms of magnitude, distance, focal mechanism, depth, soil classification. We rephrased the sentence in p8 / line 11, following your suggestion. Concerning the figure 3 on page 19 we should remark that ITA and our results converge $\pm$ for soil B and D at low distances, but strongly diverge at larger distance for all soil classes. The nationwide used ITA10 turns out to be conservative close to the epicenter, and overly conservative at larger distances. The black dots are shown in order to show some real data, certainly the cover is poor for M=3.9…4.1 as only few records are available for this magnitude class and soils A or D. We do not see a way to fix this, like many other papers on empirical GMPE, where certain parameter ranges are often poorly covered. Concerning a figure ESM1, we can refer to Tusa & Langer, 2016, where the magnitude-distance coverage is shown in all detail. In fact, in the original version of the manuscript, page 6 and lines 28-29, we wrote “For an extensive description of statistical characteristics of the data set, for example, in terms of number of data–magnitude, number of data-recording site class, and distance-magnitude distributions, we direct the readers to TL16”. Therefore, we wonder whether it is worthwhile to add figure regarding the partition of the events, which would equal to the ones in Tusa and Langer.

R2. Accounting for topography

This section is very interesting because it is a configuration that is not described and taken into account usually. It is quite well explain in the text and particularly on Figure 4. Lines 14 and 17, Page 9: functionalities and version of CRISIS are cited but no reference are given.

The new functionalities of DEM/topographic surfaces are available in the actual release of both software, but not documented by specific publications.

Definition of Figure 5 must be improved: screenshots are usually of too low quality for publication.

The original images have higher resolution, we slightly modified figure 5 as given in the attached zip file of figures

Legend of Figure 6 must be completed with indications for a), b), c), d): for instance, a) Etna DEM, b) results without DEM, c) results with DEM and d) difference between b) and c). Done

R2. Accounting for Site-Specific Response

This section is very interesting. It is developed and well-argued while that is not taken into account in lots of PSHA studies. Lines 7 and 12, page 10: no reference are given for HVSR method or Konno-Ohmachi filter

Following the reviewer's advice we added some references for HVSR, HVNR and Konno-Ohmachi filter

Line 23, Page 10: "the incoming waves are assumed to be travelling vertically": this strong hypothesis should be checked and at least justified. Seeing Figure 4 (that is nevertheless not at scale 1:1) it is not obvious that incoming waves could be assumed

as travelling vertically. Moreover this hypothesis, and the fact that horizontal and vertical motions at bedrock have no amplification, that are usually assumed in order to determined site-specific response, should be justified in this very particular case of volcanic context. In our opinion only shallow events may be considered in such ideal conditions.

Figure 4 is not used in the manuscript for the assumptions concerning the 1D site response modelling. The hypothesis that "the incoming waves are assumed to be travelling vertically" is stated by the author of the 1D modelling software as we now better specify in the manuscript adding also the relative reference. Moreover, we agree that volcanic areas are a complex geologic environment, however the adopted techniques showed appreciable results (see also answer to Reviewer 1).

Line 30, page 10: "The input parameters are the Vs, the density (), and Q": it is not useful because it is already indicated in lines 22.

The sentence was removed following reviewer's suggestions

Line 10, Page 11: "the good matching" of the comparison could be specified using criteria as following SESAME criteria.

Following the reviewer's suggestion we modified the sentence specifying that, as stated by SESAME, ambient vibration method is a reliable alternative approach to earthquake records especially in estimating the fundamental frequencies of a site.

Lines 18-20, Page 11 are not useful, it is already clear with the sentence that SESAME criteria are used

The sentence was removed following reviewer's suggestion

Line 28, Page 11: algoritm => algorithm Done

Resolution of Figure 7 is very low and it is not possible to read neither the name of the stations (7a) that amplification function plots (7b).

Figure 7 was improved in quality by increasing the label of the station names in (7a) and the amplification levels in (7b): see attached zip file of figures

R2. Results

Presented results are interesting and well explained. However no reference is done about uncertainties despite the fact that it is the principal interest of a PSHA study with reference to a classical DSHA. For instance in the text aleatory uncertainties are pointed on parameters of "historical" or "geological" models, on the GMPE (inter-event, intra-event, inter-station, intra-station). It is written that GR methodology is used, we can assumed that there are also uncertainties on α, β, or $\lambda(5)$ parameters, ... Moreover epistemic uncertainties could be associated to other parameters (Mmax, Mmin, depth, choice of source model, choice of GMPE, choice of the software CRISIS or Open-Quake, . . .). In our opinion it seems very important to underlined these uncertainties and indicate if results that are presented are close to the median, the mean, or other percentile of the set of simulations that could done using the same parameters.

The main aim of this paper is to account some computational implementations done for PSHA in volcanic areas. In particular, epistemic uncertainties due to different modelling choices are considered via the logic tree. We made clear that the results of the logic tree correspond to the mean. Aleatory uncertainties in source modelling are accounted for and described as much as we can, in the companion paper PART I.

Moreover a logic tree that summarizes all the epistemic uncertainties that are taken into account for the study seems to be indispensable, because of the lot of hypothesis that are taken into considerations. For instance in line 26, Page 12 it is indicated that "the four models in level 3". It may significate Historical-Poisson + Historical-Time dependant +Geological-Poisson + Geological-Time dependant hypothesis? The weights of each branch of the tree are often not very clearly explained in this study and this figure of the logic tree could permit to clarify that point.

We now state explicitly that the "the four models in level 3" correspond to Historical-

Poisson + Historical-Time dependant +Geological-Poisson + Geological-Time dependant hypothesis, and each are assigned equal weights of 0.25.

Finally a desagregation of the results could be assumed in order to justify some results. For instance Etna-specific PSHA desagragation could be compare with desagregation of the results of national hazard map in order to show that M>6 are not significant in the estimation of the hazard in this region.

This is a good suggestion, in order to assess the contribution of regional M6+ events to the hazard, as a justification for not including them in our model. We performed a simple disaggregation in terms of magnitude and distance considering only the area sources of Level 1 (time independent model) and 4 area sources from the MPS04 national hazard model (those closest to Mt Etna). The results confirm that the hazard at Mt Etna is dominated by the local volcano-tectonic events for exposure periods of 5 and 30 years, and not the larger (M6+) events in the region. For T=5 years, the contribution of regional events is almost negligible, while for T=50 years their contribution is larger, although still less than the local volcano-tectonic events. Some images (Figures A1-4) for the benefit of the discussion are added here, but not in the manuscript/electronic supplementary material, as we believe they are very preliminar, and this point is out of the scope of this paper.

It is noticed in Lines 4-8, Page 13 the main results of different tests, in terms of maximal accelerations. It could be specified the type of difference that are observed in terms of geographical representation, frequency content, velocity, displacement, . . .

We are not sure we understood the reviewer's comment, that part refers to results given in Spectral Acceleration at 2 periods (0.2 and 1.0s) instead of PGA (theoretically SA at 0.0s). They are motivated as they are relevant for site-specific studies and risk analyses.

The justification of using CRISIS and not Openquake for site-specific test is not given. The quantitative difference between the two software's is not given. What is the percentage of difference between the results respectively? It is rare to use in a PSHA study two software's even if we know that the choice of the software is a source of epistemic uncertainties and it is badly not exploited in the results of the study.

In this paper we made a conscious choice NOT to focus on the differences in the software results (notice that we did not show any of the same results using the two software). Rather, we intended the focus to be more about the new functionalities we developed for both the software (topography, customized GMPE and MSR) and to the impact that some model components may have on final results. This was for instance the reason to explore some other functionalities such as the site effects, even if they are still "immature" (they are mapped accordingly to how they were available at the time of the end of the project that promoted and funded this paper) for a complete analysis of the study area.

The observed intensities (Azzaro et al., 2008) are only cited in introduction and in the last sentence of the results. It could be interesting to indicate main results of this study of 2008 and compare to intensities that could be derived from PSHA accelerations in the same places.

In previous papers we demonstrated the impact of the local volcano-tectonic sources by means of macroseismic data. This paper roughly confirms the previous results for the inhabited areas (where macroseismic data can be assessed), but new sectors of high hazard appear at Mt Etna most deserted areas. We add a brief comment on that in the results, but we did not perform a PGA/SA – Intensity comparison, as in our opinion this is a very "delicate" and "uncertain" procedure.

Results on Figure 9 are presented for mean values? Median values? Mean plus/minus one standard deviation values?

They are mean values, more clearly stated in the caption now.

The figures on Figure 9 and 10 are shown horizontally (with legend at the top) while on

the figure 11 the figures are show vertically, with the legend in the right. Also it is far more difficult to make comparisons and it could lead to errors in the interpretation.

The pictures are complex but we hope they will be properly published without rotation. The quality of figures is increased in the original picture.

R2. Conclusions

The main conclusions and limits of the study are well summarized in the conclusions. The exploitation that could be done of these results is also presented. However it is unfortunate that the comparisons between CRISIS and OpenQuake are not properly exploited.

We will consider these suggestions for a future analysis, and invite the reviewer to post additional comments if interested.

Line 25, Page 13: "47-284" => "47-284 years" Line 20, Page 14: "DPC" => "Dipartimento della Protezione Civile". Done

Bibliography The same typo must be respected for all the references. For instance: Line 22, Page 15: => Azzaro R, D'Amico S., Tuvè T: Line 4 to line 15, Page 17: alphabetical order must be respected

Partially fixed, to be checked at the final submission with the Editorial Staff.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-121/nhess-2017-121-AC2-supplement.zip

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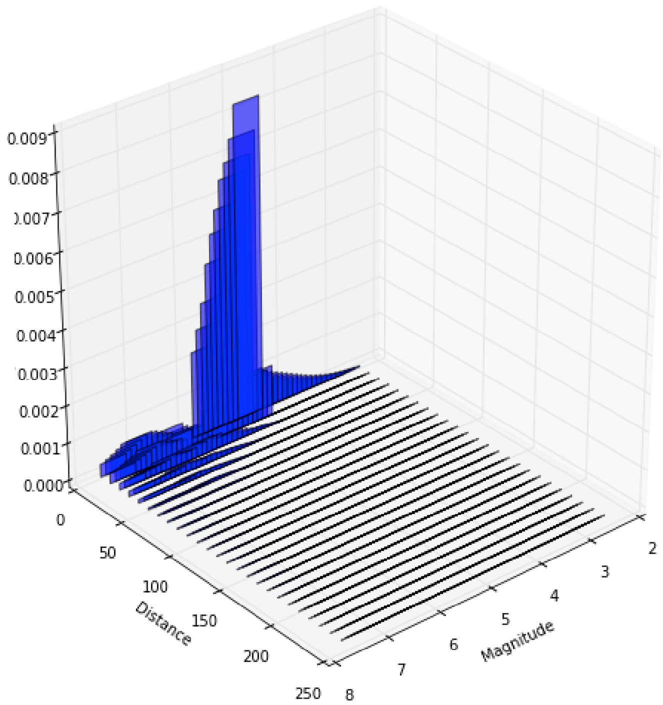

Fig. 1. Figure A: disaggregation plot (magnitude, distance, percentage) at 10% exceedence probability for exposure period of 5 years.

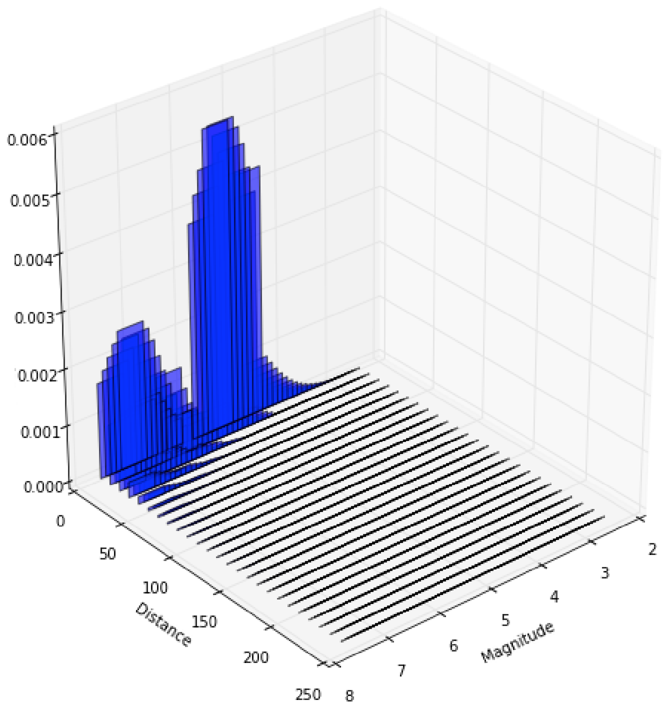

Fig. 2. Figure A: disaggregation plot (magnitude, distance, percentage) at 10% exceedence probability for exposure period of 30 years

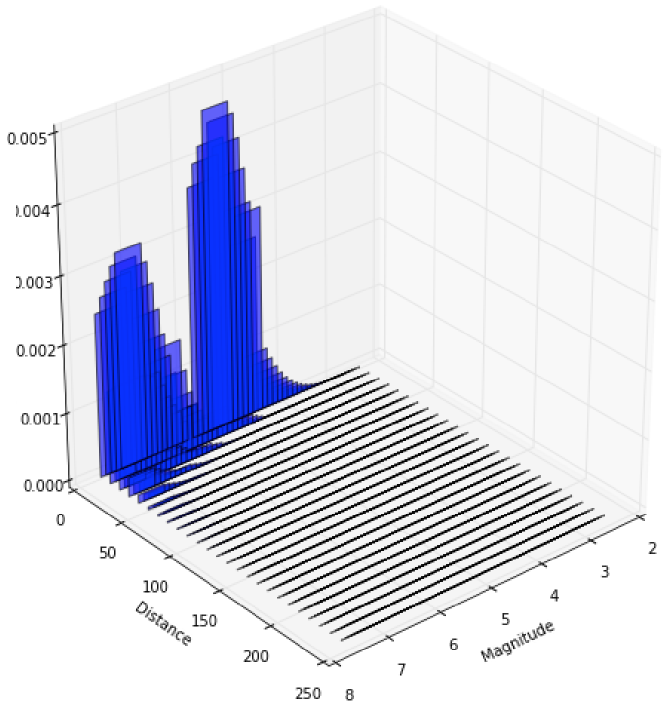

Fig. 3. Figure A: disaggregation plot (magnitude, distance, percentage) at 10% exceedence probability for exposure period of 50 years

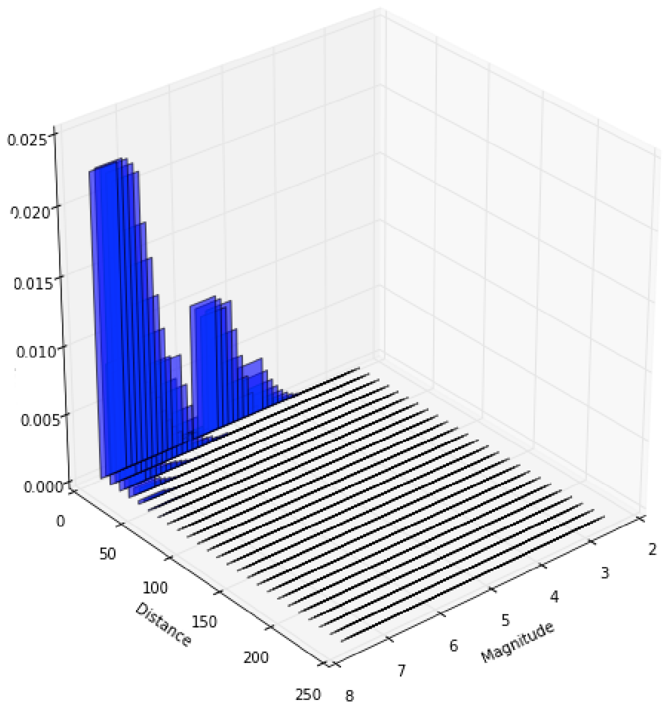

Fig. 4. Figure A: disaggregation plot (magnitude, distance, percentage) at 10% exceedence probability for exposure period of 1000 years