Nat. Hazards Earth Syst. Sci. Discuss.,
doi:10.5194/nhess-2017-121-RC2, 2017

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.

Anonymous Referee #2

Received and published: 30 May 2017

General Comments

This submitted paper is an interesting contribution to volcanic risk reduction. In particular, the PSHA approach developed in this study is remarkable because probabilistic studies are very rare in this type of context. The context of the research is well explained and the aims of the study are clearly presented and well connected to companion paper (Azzaro et al., 2017). The case study of Etna region is pertinent in order to lead such a study because of the quantity of data available. Nevertheless coverage of uncertainties could be improved, because it is a key-point of the use of PSHA studies. Altogether the manuscript is very readable, but certain passages could be improved (avoiding repetitions, precision of elements, bibliography, ...). Some minor typos or

Printer-friendly version

Discussion paper

small phrases of awkward writing are present. Moreover the definition of figures must be improved because many of them are not well readable.

The imbalance between different sections is strengthened by the fact that few quantified element comes to support the conclusions: when comparison between figures is cited to quantify differences between level 1, 2 or 3 seismic source model approach, few quantified data and no uncertainties are presented to support the conclusions on the scenarios. For instance results could be enhanced by the estimation of uncertainties of different scenarios (= different branches of logic tree) that are compared. In conclusion this study is very interesting and the PSHA study in a volcanic context with development of Etna-specific GMPE or accounting of site-specific response must be indicated as key points of this work. Nevertheless it could be improved: it is in the current state too illustrative and should contain more quantified data and in particular to specify associated uncertainties in order to strengthen the argumentation.

Comments in Detail

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

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.

Introduction

I have no comment on this section.

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.

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

Definition of Figure 1 must be improved.

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.

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, . . .

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.

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

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).

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

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.

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.

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

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

Line 28, Page 11: algoritm => algorithm

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).

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 λ(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 OpenQuake, ...). 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.

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.

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.

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, ...

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.

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.

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

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.

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.

Line 25, Page 13: “47-284” => “47-284 years”

Line 20, Page 14: “DPC” => “Dipartimento della Protezione Civile”.

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

---

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2017-121, 2017.

Printer-friendly version

Discussion paper