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Natural Hazards and Earth System Sciences Discussions
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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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Review of Peruzza et al. "When probabilistic seismic hazard climbs volcanoes: the Mt Etna case, Italy. Part II: computational implementation and first results"

General Comments

This manuscript is the second of two papers describing a probabilistic seismic hazard assessment (PSHA) for the Mt Etna region. Whilst the focus of the first part (Azzaro et al., 2017) describes in detail the source model, this manuscript presents the broader implementation of the PSHA, specifically addressing topics such as ground motion modelling, elevation, site response and calculation details. By itself the manuscript presents the components of the model well, highlighting specific issues raised in the application to volcano tectonic environments and presenting observations from observed seismicity and ground motion that have guided the modelling process. The comparison of two software (CRISIS and OpenQuake) adds additional value to the manuscript as it can highlight potential details pertinent to the implementation of the models that are not always addressed in PSHA papers. Whilst there are some scientific criticisms of certain modelling decisions (discussed in the detailed comments below) I believe the overall approach of the work and the content of the manuscript is of a quality that is sufficiently sound for publication within this journal. I therefore recommend the manuscript for publication subject to minor revisions, mostly seeking clarity or discussion on issues raised below.

The manuscript itself it generally well written, with references found to be complete. Some minor typos or small phrases of awkward writing are present and these are highlighted where possible. I encourage the authors to check the manuscript for other small errors before resubmission. Unfortunately, the image quality appears to be quite low for some figures (indicated below) and I would encourage the authors to revise them, export them to a higher quality and ensure that all text within the images is legible.

Comments in Detail

Abstract:

Line 16: Change “more standard PSHA” to just “standard PSHA”, and change “which are most broadly due to” to either “which are mostly due to” or “which are broadly due to”.

Introduction:

Page 2, Line 1: This opening statement might be helped by giving examples of two or three damaging earthquakes that are volcanic in origin (can be in Italy or elsewhere).

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Discussion paper

Page 2, Line 19: “poissonian” needs an upper case “P”

Page 2, Line 30: “effects” should be “affects”.

2 Seismic Source Model

Page 3, Line 13: “engine of earthquake occurrences” seems an unusual phrase. Suggest “. . . basic assumptions behind the physical processes driving earthquake occurrences”

Page 3, Lines 14 – 16: The mention of volcanic tremor here seems to be quite pertinent, yet this is the only place in either of the two papers where this gets mentioned. Volcanic tremor events are usually quite different in their spectral characteristics then those arising due to brittle failure (or those due to general tectonic loading on existing faults). The ground motion model subsequently in this paper does not seem to distinguish between the different types, suggesting that both are present in the database use to fit the GMM. This would inevitably influence the total variability of the GMM. The consideration of tremor events, which one would assume might be more prevalent during periods of raised volcanic activity, may warrant more discussion as possible caveats of the approach presented in these two papers.

Page3, Line 18: “high-quality instrumental network which geometry and characteristics are essentially remained unchanged” – Change “which” to “whose”, and “are” to “have”.

Page 3, Line 27: Change “independency” to “independence”.

3 GMPE at Mt Etna

General comments:

1. The choice of the functional form of the GMPE here is not well justified given the description of some of the phenomena observed in the ground motion dataset. The most important inconsistency is in the treatment of focal depth. In the first paragraph of the section the authors indicate that the strong motions have a clear dependence

on hypocentral depth, with shallower events richer in frequency content than deeper ones. However in their functional form it appears that they are adopting a model with a fixed pseudo-depth, and an explicit dependence on hypocentral distance. This largely negates the depth dependence as deep events at close epicentral distances can produce similar levels of shaking at shallow events at larger epicentral distances. Some justification is needed by the authors as to why this is preferable to a GMPE in which the hypocentral depth is considered as a separate term in the model.

2. The coefficient of the anelastic attenuation term (c3) is positive but very close to zero. This is problematic as it means that at longer distances the attenuation trend will reverse and ground motions will increase with distance. This flattening at longer distances is already visible in Figure 3 at about 100 km and will reverse the attenuation at greater distances. Given this unphysical behavior and the fact that the coefficient is barely significant there is little justification for including an anelastic term in the ground motion model.

3. The use of nonlinear least square fitting accompanied by bootstrapping is unconventional compared to the more common nonlinear mixed effects regression approach. Could the authors comment on why they adopted this approach rather than mixed-effects regression?

Page 8, Lines 29 – 30: I'm not sure that the reference to the filename of the tusa_langer_2016.py file is relevant to the reader (and it is always possible that in the future the location of the code archive can change). I suggest to remove this particular mention and simply refer the URL to the main OpenQuake archive or repository from which the software can be accessed.

4 Accounting for topography

Page 9, Line 5: Replace “we have done” with just “introduced”

This section is an interesting development on the conventional assumptions made in

PSHA. There are two issues though that the authors may wish to comment upon. The first is the potential influence of topographic amplification effects that may mean that two sites at similar elevations and source-to-site distance may experience different amplitudes of shaking (and different frequency content) depending upon the local gradient of the slope or the proximity to steep edges. Is there evidence for topographic effects in the residuals of the ground motion model?

Secondly, the assumption of hypocentral distance, though simple and practical, has its own inconstancies when the hypocentres are located above the reference surface. If the source is located above sea level within the volcanic edifice yet the site is located away from the base then a linear path from the source to site cannot be constructed as it would be for events below the surface, as it implies travel of the waves above the free surface. This is perhaps another argument as to why hypocentral distance may be an unsuitable metric in the present case and an explicit hypocentral depth parameter may be able to better account for changes in elevation

5 Accounting for Site-Specific Response

Generally a good section, although some comment from the authors as to whether the local site conditions of the volcanic edifice are consistent with the typical conditions needed in order for HVSR to be a good indicator of amplification (i.e. strong impedence contrasts, minimal lateral heterogeneities etc.) is needed.

6 Results

Pages 12 – 13 (Lines 31 – 3): There is some information that is unclear here, possibly suggesting a slight inconsistency in the methodology. The authors describe the use of floating ruptures on fault surface, yet they don't indicate the choice of magnitude scaling relation or how they distribute the location of the hypocenter within the rupture. Given the magnitude frequency distribution is characteristic then one would expect that the finite rupture dimensions would be close to the total area of the fault. Thus, floating ruptures would not necessarily move the rupture to a great extent. If the hypocenter is located mainly at the centre of the rupture then the resulting sources will be distributed very closely around the centroid of the full fault surface. If the intent is for the hypocentres of ruptures on the fault source to be constrained to a small area within the middle of the source volume, as one might do if one wished to infer bilateral propagation of ruptures on the fault, then the results make sense given that from the GMPE a more uniformly radial pattern is expected. However, as GMPE itself does not require information regarding the rupture finiteness, it may be preferable to consider aleatory uncertainty in distance by considering the hypocentres as points on the fault plane. This would mean that hypocentres, rather than ruptures, can be floated across the fault plane and the pattern of the map should look less radial (though the peak level of the hazard in the centre of the fault will be lower). Some clarification is needed from the authors as to how they have generated the finite ruptures being floated, the distribution of hypocentres within the rupture plane and whether the resulting distribution of sources conforms with the modelling intention (e.g. whether bilateral propagation of ruptures is desired).

Page 13, Line 2: “In this cause” – should be “In this case”

7 Conclusions

Page 14, Line 18: “strongly spatially uncorrelated” – Not sure how one can discern a strong uncorrelation from a weak uncorrelation? Simply “spatially uncorrelated” is sufficient.

Figures/Tables:

Table 2: Exponential notation shown in the probabilities columns is broken over two lines. Try to avoid this (or fix in the final typeset).

Figure 1: Image quality is low. Please use a higher resolution in the final submission.

Figure 3: Image quality is very low. Please use a higher resolution in the final submission.

Figure 5: Screenshots are usually of too low a quality for journal publication. Text is not legible. Higher resolution maybe difficult to obtain and it is not obvious that these figures add specific value to the manuscript. If quality cannot be improved consider removing this figure.

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