

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 Graeme, Thank you very much for careful and helpful revision. We answered positively to all the questions you raised, and hope that the manuscript 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

R1. 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

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are broadly due to”. Done

R1. 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). We modified the text to account for this comment, referring more clearly to the issue of seismic hazard in volcanic zones and providing some references of major case-studies worldwide. Page 2, Line 19: “poissonian” needs an upper case “P” Page 2, Line 30: “effects” should be “affects”. Done

R1. 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” Done 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 than 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 used 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. The referee’s comment is right, but the sentence on the “volcanic tremor” is a mistype which had to be deleted. Actually the events we used to fit the ground motion model are all volcanic-tectonic (VT) events caused by double-couple fracturing and fault slip. In our study, no volcanic tremor events have been taken into account and the database used to estimate the ground motion model, and then the GMPE, include just VT earthquakes. We now modified the text accordingly. Page 3, 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”. Done



R1. GMPE at MT Etna 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. We did not enter into much details with respect to the empirical GMPE as they can be found in the paper by Tusa & Langer (2016). The model presented here is close to a standard formulation “ITA10” (by Bindi et al, 2010) widely used in the Italian Territory. Nonetheless we now added some more explanation which may match the reviewer’s concern. We point out that the “pseudo-focal depth h ” was not fixed a-priori but identified during the inversion process in the same way as the other coefficients of the model. Besides, please note that we focus on the shallow events, i. e., events having a focal depth less than 5 km, and about the 95% of the data we used have focal depth less than 2.5 km. Depth dependence for the given data is therefore of minor importance, besides the area very close of the epicenter – for which the data coverage is indeed poor. This problem is not neglected, however, as we propose a specific treatment for this case on the base of synthetic simulation. 2. The coefficient of the anelastic attenuation term (c_3) 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. Again, the coefficient c_3 was

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“inherited” from the form of ITA10. We agree, being it close to 0 it can be probably neglected, but then a reader might ask why we did not consider it as it makes part of the standard formulations. We now state that is parameter could be in reality neglected, as we found it indeed being close to 0. The “unphysical behavior” at larger distance (>100 km) is a result of extrapolation of the model to a distance range not covered by the data. Problems of extrapolating non-linear models outside the parameter range of the data set are common, and must be strictly avoided. We stress this now. By the way, from a physical point of view we must be aware that mechanisms of attenuation may change, for instance for the role of reflections at deeper discontinuities, Q-factors/velocities not being constant along the path or other. 3. The use of nonlinear least square fitting accompanied by bootstrapping is unconventional compared to the more common non-linear mixed effects regression approach. Could the authors comment on why they adopted this approach rather than mixed effects regression? We used the non-linear least-squares (NLLS) Marquardt-Levenberg algorithm (Press et al. 1992) because the random-effects regression technique proposed by Brillinger and Preisler (1984) does not converge to a unique solution and depends on the initial model parameters. Additionally, it tends to give approximately the same coefficient values founded through the more classical non-linear least-squares technique. We also added a few sentences to the Boot-strap techniques, which simply consists of resampling the data and repeating the inversion many times. The Boot-strap comes with the advantage not to make any a-priori assumption on the distribution of the data set (commonly Gaussian) and allows a robust estimation of the statistics of the coefficients of the model, i.e., their mean and their dispersion. Here we see that the estimation of the coefficients and their uncertainties are rather stable. Page 8, Lines 29-30: I’m not sure that the reference to the filename of the tusa_lager_2016.py file is relevant (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. Combining R1 and R2 suggestions, we modified the sentence in lines 27-30 page 8.

R1. Accounting for topography Page 9, Line 5: Replace “we have done” with just “introduced” Done 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 inconsistencies 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 We modified the text to address these two issues related to accounting for the topographic surface in PSHA (see Page 9, Lines 13 – 23). We clarify that we are not accounting for topographic site effects and justify the use of a linear source-to-site path approximation. R1. 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 impedance contrasts, minimal lateral heterogeneities etc.) is needed. We added a new sentence aiming at supporting the use of spectral ratios methods in a volcanic environment.

R1 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

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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). We modified the text to account for these comments, describing more clearly the MSR, rupture floating, hypocentre location, and the observed pattern (see Page 13, Lines 17 – 25). Page 13, Line 2: “In this cause” – should be “In this case” Done

R1. 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). All Done 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

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obtain and it is not obvious that these figures add specific value to the manuscript. If quality cannot be improved consider removing this figure. The original images have higher resolution, we slightly modified figure 5 as given in the attached zip file of figures.

Please also note the supplement to this comment:

<https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-121/nhess-2017-121-AC1-supplement.zip>

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

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