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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 I: model components for sources parametrization" *by* Raffaele Azzaro et al.

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The manuscript describes the construction and characteristics of seismogenic source models for use in probabilistic seismic hazard analyses (PSHA) in the Mt Etna region. The models are derived from short-term instrumental seismicity, long-term historical seismicity and from the fault geology. Volcanic regions such Mt Etna pose an important challenge in PSHA modelling due not only to the complexity of the source process, but also to the very nature of the forces controlling the seismicity, which may be by-definition non-Poissonian in nature. As a practical approach, illustrating the possible means by which a seismogenic source model can be constructed in such a region using available seismic and geological data, the paper is a valuable contribution to the topic.

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Discussion paper

It is easy to see how it can be incorporated into models in other volcanic environments. The specific methodologies within the approaches are generally applied in a sound manner and build upon the current state-of-practice in PSHA source modelling. On this basis, I believe the paper represents a solid contribution to the scientific literature and should be considered for publication within this journal.

There are, however, some specific topics where the material presented seems somewhat incomplete. The main topic omitted in this paper is the characterization of epistemic uncertainty in the source model and the manner in which this is formulated for a PSHA calculation. Whilst some discussion on this topic can be found, albeit briefly, in section 6 of the accompanying Peruzza et al. manuscript, complete omission of the epistemic uncertainties and the combination of the different model approaches presented here diminishes the value of this manuscript as a stand-alone paper on source modelling in volcanic regions. I recommend the authors to consider adding some additional discussion here as to how epistemic uncertainty should be treated and outline the basic formulation of the logic tree. Some overlap with Peruzza et al. (2017) is tolerable in this case.

Secondly, the authors explain in section 2 that destructive historic events have occurred both in periods of activity as well as times of quiescence, and that the recurrence models for slip in larger characteristic events behave in a manner typical of those under tectonic stress rather than local magmatic stress. Whilst it is possible to accept this at face value given the lack of correlation mentioned, it is not so true to say that this applies to all the other seismicity. Within the distributed sources and area zones the recurrence is dependent upon the rate of all seismicity, which will be more closely linked to cycles of eruptive activity and quiescence. In a PSHA risk mitigation context, this means that if considering the probability of exceeding ground motion in a given time period (e.g. 5, 30, 50 years) one needs to account for the probability of an eruptive episode and the probability of exceeding the given levels of ground motion conditional upon the occurrence of the eruptive episode. This is in addition to the baseline hazard during periods of quiescence. Of course, this widens discussion regarding the quantification of the probability of eruptive episodes, but it may be important for putting this work into practice. This is not a critical flaw in the methodology described, but may be a theoretical limitation of the assumptions made in the application of the recurrence models for the area and distributed seismicity sources.

For the manuscript in general, the writing is of a quality sufficient for publication. Some notable typing errors are indicated below, but I would recommend the authors to check further before compiling the final manuscript. Figures and tables are clear and well captioned.

Comments in Detail:

Lines 19-20: “We derive a magnitude-size scaling relationship specific for this volcanic area” – Change “specific for” to either “specific to” or “specifically for”

Lines 20 – 21: “Pace et al. (2015)” is “Pace et al. (2016)” in bibliography

Lines 25: “These analyses to not account regional M>6” – “Do not account for regional . . .”

Line 29: “However, apparently less evident . . .”, can be changed to just “Less evident but equally . . .”

Line 36: comma needed after first “which”

Line 37: Should be “computation codes developed for the whole of Italy”

Line 62: comma needed after “widespread” and then removed after “eastern flank”

Line 82: “. . . seismic hazard applications regards the question of . . .” is better phrased as “. . . seismic hazard applications is the question of . . .”

Line 84: “It is a matter of fact that destructive earthquakes in the Tiempe area historically occurred both during flank eruptions and not” – Perhaps change the “and not” to “as well as during periods of volcanic quiescence”.

Lines 104 -105: “It has to be noted that moderate values of magnitude for heavily damaging events are a feature of seismicity in active volcanic areas such as Etna, whereas in tectonic domains crustal earthquakes producing the same effects are generally associated with M > 6.” This comment has particularly significant implications for seismic hazard analysis and I would encourage the authors to: i) add a citation, ii) if known, briefly summarise what are believed to be the potential factors that may explain this observation.

Line 132: Needs comma after “somehow uniform”

Line 170: Replace “global” with “European”.

Line 180: The use of the detailed areal sources and the extended sources are not clear. Are these alternative branches on an epistemic uncertainty analysis as the comment regarding uncertainty would apply? If so, then the authors need to clarify how the two different models are weighted. If not, then it is unclear how the authors are partitioning the moment rate between the two models.

Section 4.1.2: The assertion of a Gutenberg Richter model for the various faults is not entirely consistent with the observation shown in Figure 6. In nearly all cases the observed rate of earthquakes around M 3 is greater than that implied by the GR models, which suggests some kind of hybrid characteristic model. This may be shifting the trend toward lower b-values. Did the authors consider a hybrid model in which larger events occur more frequently than predicted by GR? The trend is less obvious for the Timpe zone, which reflects a common perception of GR-behavior across zones spanning larger spatial domains.

Section 4.2: The usage of distributed seismicity in this context should be debated more than is done so here. Given the relative brevity of the seismic catalogue, when looking at b-value variation on a fine spatial resolution it may be increasingly likely that the values in any given cell may reflect a transient process. Even if the variation in b-value is cannot be attributed to statistical artefact, can the authors rule out the possibility that they are related to transient properties of the state of stress around particular elements in the complex volcanic system (including interaction with fluids), even if the period is quiescent? How representative might these values be of recurrence on a multi-decadal timescale?

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