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

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Dear Graeme, we appreciated very much your constructive criticism and helpful hints, aimed at improving really the paper; you are acknowledged for your contribution. We answered to all the comments you posed and modified the text accordingly. Hereinafter the detailed list of your comments and our replies, a zip file containing the revised manuscript with tracked changes, and the new figures that have been modified. On behalf of all the authors Raffaele Azzaro

R1. First general consideration: 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.

We thank you for this comment and yes, we agree, some overlapping with Part II paper is useful both for having a stand-alone paper, and for commenting the epistemic uncertainties in source modelling. We entered some new lines in the introduction and a new block of text now marked as Chapter 6, where we also added the picture representing the logic tree approach (taken from Part II paper).

R1. Second general consideration: 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.

The reviewer comment is right, in a generalized view of the problem. We added some lines at the end of chapter 2 to widen the discussion and references.

R1. 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 Timpe area historically occurred both during flank eruptions and not” – Perhaps change the “and not” to “as well as during periods of volcanic quiescence”.

Done

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.

Done

Line 132: Needs comma after “somehow uniform” Line 170: Replace “global” with “European”.

Done

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.

The extended area source marked in red (SZ Timpe) is given only for a comparison of the parameters with the more detailed SZs; now we clarify this in the text.

R1. 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.

The effect commented by the referee is indeed visible only for FF, that is the SZ having the lower number of earthquakes. We do expect also some completeness problems, as far as the seismicity is located at depth and low magnitude events can be missed. We don’t consider a hybrid model at this stage of hazard parametrisation, but we will consider it for future implementations.

R1. 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?

The reviewer comment is right, it is not possible to calculate the b-value of a cell as a function of time since earthquakes aren't sufficient if split into different time windows. Therefore we can consider only the spatial variation of b-value. This said, we cannot rule out the possibility that transient properties of the state of stress are influencing the b-value. We acknowledge this limitation in the text, by declaring that the basic assumption we accept in this analysis is the representativeness of few years of high quality seismic monitoring during an interseismic period for the long-term seismic rates of faults: this assumption on annual/multi-decadal timescale recurrences is, in our opinion, supported by the global agreement of short term occurrences (red dots in Fig. 6), with the ones obtained with the whole catalogue (blue dots). We did some effort for better explaining this part, a point raised by Reviewer 2 too.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-127/nhess-2017-127-AC2-supplement.zip

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