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Reply on RC2

Francesco Visini et al.

Author comment on "An updated area-source seismogenic model (MA4) for seismic hazard of Italy" by Francesco Visini et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-60-AC2, 2022

Dear Referee 2,

Thank you for your useful suggestions and comments.

In the following we answer point by point to your comments here.

REF: - Seismic rates: five "models" are defined and given the same weight in the logic tree. I believe there is not much difference in them from the practical point of view, but I agree that seismic activity is a very important parameter controlling the final hazard values, so it is justified to consider that wide suit of alternatives. I wonder however if the authors could give an opinion in which one actually they believe more. In my opinion I would go for approach one (i).

ANSW: We appreciated the Reviewer getting this important point. For many zones there is not much difference from the practical point of view, however the seismic activity is a very important parameter controlling the final hazard values. We understand that the Reviewer can prefer the approach 1, as this can catch the finest spatial variation of the GR parameters. From our point of view, the two "most representative" options, from a qualitative point of view, are the approaches 1 and 2.

REF: - Note that in line 265 you cite "Mt" as magnitude threshold, when elsewhere is cited as M_0. Not sure if this is a typo or you actually mean it.

ANSW: We will check and correct the use of Mo and Mt through the text.

REF: - Style of faulting. I would be very interesting to know which criteria is considered to classify the different styles of faulting. I assume this is done based on the rake, but which rake values have you used to classify the ruptures? Aki criteria? Additionally, for the later calculations with OpenQuake I believe you have to state a fault-plane (strike/dip) for each of the different types (reverse, normal and s-s), which values do you use or are these

random?

ANSW: According to Pondrelli et al. (2020), to which we refer for the nodal planes calculations, to define the three main tectonic styles we adopted the rake-based criteria given in Akkar et al. (2014), which attributes each focal mechanism to either reverse, normal, or strike-slip. In particular, normal solutions have a rake between −135 °and −45°, reverse solutions between 45° and 135°, and other rake values are classified as strike-slip. We will add in the appendix the values used for strike/dip; in the zone where calculation of the average fault planes were not feasible these are: strike="0" or "90" with dip="90" for Strike Slip, strike="0","90", "180" and "270", with dip="60" for normal faulting, strike="0","90", "180" and "270", with dip="30" for reverse faulting. For the other cases, these values are listed in the appendix.

REF: - Hypocentral distribution: This analysis is very nicely performed. The paper states (line 291) that this uncertainty is considered as aleatory. I wonder how this is considered in the calculations. I assume this is done by some built-in procedure in the OpenQuake code itself. Is that right? A Montecarlo? If so, how this affect the 60 realizations? Could you provide some extra information about this?

ANSW: Currently, our text may induce some confusion in the reader. The aleatory uncertainty is recalled to indicate there is a probability function to describe the occurrences of the hypocentral depths. We will modify the text to be clearer, as this is a consideration of how to treat the next occurrences, not an OpenQuake procedure.

REF: - Rupture mechanism: In the same line as above (291) it is said that this issue is also considered aleatory. Please, provide some extra information so the reader can follow properly the way the uncertainty is taken into account and how it is eventually affecting the 60 final realizations. I wonder for example on how you consider the dip and strike of the ruptures in the calculations (line 300), are they horizontal or you are using some fixed values according to the rupture mechanism, for instance 30, 60 and 90 for reverse, normal and strike-slip? or is it a variable considered random?

ANSW: as above, we will modify the text to be clearer, as this is a consideration of how to treat the uncertainty, not an OpenQuake procedure. We will specify how the finite fault is dimensioned and positioned in the space according to the strike/dip we evaluated for each zone. In particular, for many zones, strike/dip were evaluated by adopting the Pondelli et al (2020) procedure, but where this was not possible, we adopted fixed values that represent a balance between the need of exploring multiple orientation and computational machine time. Computed values are in the appendix, however we will add the "fixed" one and specify this part better in the manuscript.

REF: - GMPEs: To properly follow the results and discussion it is necessary to provide some extra information about the GMPEs used in the calculations, particularly the "distance parameter". I also missed some information about the significance of these GMPEs to be used in Italy, about the distance and magnitude range considered in them, the rupture mechanisms, number of records, … and very importantly: for what type of ground are you using the GMPEs (I assume rock-type, but this should be stated in the paper to properly interpret the results). I know this paper is not about GMPEs but these are crucial information for understanding the results. I also wonder about the differences between the GMPE of Bindi et al 2014 and Bindi et al 2011. Are they derived from the

same database?

ANSW: Some extra information about the GMMs used in the calculations will be provided in the revised manuscript. In particular, a brief description of the procedure followed in MPS19 to select and weigh the GMMs will be added together with basic characteristics of the 3 models, such as distance metrics and calibration datasets (e.g., dataset including European and Middle-East records for Bindi et al. 2014 and Italian dataset for Bindi et al. 2011). We will also specify that hazard computation in our study, as well as in MPS19, was performed for rock-site conditions, i.e. EC8 site category A or Vs,30>800m/s.

REF: - Macroareas (line 380): Macroareas (a set of grouped source areas) are used to calculate b-values. This procedure is followed so the fitted b value results statistically stronger than the one doing the fitting in each of the zones. This a practical procedure, however it may miss significant b variations from zone to zone. It would be good to support the use of this "concept" a bit more.

ANSW: We will expand this part to better explain why we used macroarea. In particular, our main concept is that for zones characterized by a "low" seismic activity, the b-value can be biased and results as an artefact of the low number of data available. Using macroarea, where zones were grouped according to their tectonic features, for example zones characterized by extension, we can estimate a statistically robust b-value. To maintain b variations from zone to zone we also used the approach 1, in which the b-value is actually estimated for each zone. Actually, we do. Not know what is the best solution: b-variable at the level of the single zone or at the level of large tectonic features. This is the reason why we used a logic tree approach.

REF: - I suggest the authors to write at some point in the paper the "return periods" of the key annual probability of exceedance levels targeted (for example: 10% of exceedance in 50 years, also refer as 475-yr return period; and so on). This is not crucial, of course, but it helps the reader, particularly among the engineering community.

ANSW: We agree with the Reviewer and we will add the "return periods" of the key annual probability of exceedance levels targeted.

REF: - **Discussion and Conclusion: I believe this section could be much improved.** I suggest you to separate Discussion from Conclusion. As it is written now, is seems a bit erratic. It is just a matter of organizing ideas and end properly with a short Conclusion.

ANSW: We accept this suggestion and we will modify the section.

REF: - line 356: please provide a bit more information on the "community-based effort". Was a procedure like SSHAC followed? Did it follow a sort of expert judgment method?

ANSW: We will explain better in the text what we mean with "community-based effort". In particular, this was referred to the MPS19 project, which involved more than 150 Italian researchers at various stages of the project. Many of them were involved in the building of their seismicity model or earthquake rupture forecast. There were 11 groups of researchers that produced 11 ERFs, MA4 is one of them. Other researchers were involved

in the selection of the GMMs and others on the testing procedure. We will better explain this concept in the beginning of the section.

REF: - line 363: refrain the use of "true tectonics", use instead "actual" or "known" for example.

ANSW: We will modify it according to the suggested term.

REF: - line 364: Documentation is crucial in the process of defining source zones for PSHA. It supports the zone model and provides a ground for further refinements in future updates. The paper lists somehow the different data used in the process of defining the zones; however, it would be very good to provide detail information on each zone about the method/criteria used to define each of the boundaries (and may be add this info as an electronic supplement), as other authors have done elsewhere (eg., Vilanova et al., 2014; García-Mayordomo, 2015)

ANSW: This is a good point, also raised by the REV1. We are going to include georeferenced maps of the geophysical data and shapefiles we used to draw the zones in a separate appendix. We will add in the text an explanation of how polygons were drawn. We will add these details for groups of zones, for example Alps, Northern Italy and so on.

REF: - References: There are few typos, eg., lines 478, 480,

ANSW: we will fix these typos.

REF: - Figure 6. I believe the y axis should read cumulative annual rate. Additionally, could you use a clearer scale for the x axis so it reads integers and halves (eg, 4.5, 5.0, 5.5,..). The graphs would look better if you also reduce a couple of marks the length of y-axis. In the caption, use approach i instead of "method 1".

ANSW: yes, it is a cumulative annual rate. We will modify the graphs according to these comments.

REF: - Figure 8. It would be good to also stated the "return period" of each p.o.e. NOTE there is a typo in the titles of the maps as it says 50 years twice, when it should read just "PGA 10% of p.o.e in 50 years". I assume is PGA on rock, but it would be good to say it.

ANSW: We will add the RP of poe and modify the graphs according to these comments. We will correct the typo.

REF: - Figure 11: NOTE the typo on the right hand map (in the title). It should say 2%.

ANSW: we will fix this typo.

REF: - Figure 13: Typo in the caption, it says “againt”.

ANSW: we will fix this typo.