Review of manuscript NHESS-2017-41 "Integrating faults and past earthquakes into a probabilistic seismic hazard model for peninsular Italy" by Alessandro Valentini, Francesco Visini & Bruno Pace

Main comments

This manuscript describes an approach to model seismic hazard in Italy using a combination of active fault data and gridded seismicity based on the instrumental and historical earthquake catalog. A database of active faults has been compiled, and important historical earthquakes have been assigned to their causative faults. Two models are considered for the magnitude-frequency distributions (MFDs) of the faults, either a truncated Gutenberg-Richter (TGR) MFD or a characteristic Gaussian (CHG) MFD. The gridded source model accounts for off-fault seismicity, and its MFD is computed in a way that it is complementary to the MFD of the fault source model (using a threshold magnitude, avoiding double-counting of earthquakes assigned to faults, and an additional weighting function that reduces gridded seismicity in the vicinity of faults). The authors explore the impact of the two MFD models, as well as the contribution of fault sources and gridded seismicity to the total hazard. They also define a preferred source model, in which the most appropriate MFD model for each fault is selected.

The approach to model fault sources is state of the art, and the integration of fault sources and gridded seismicity contains some innovative elements. The manuscript is mostly well written (with some exceptions, which are pointed out in the detailed comments below), the figures are clear, and the references are appropriate. The conclusions are supported by the results.

However, a number of improvements need to be made before the manuscript can be published. Below, I have listed a number of detailed comments. I summarize my main comments here:

- A major shortcoming is that the paper does not contain any reference to other published fault source models for Italy, notably DISS (Database of Individual Seismogenic Sources, http://diss.rm.ingv.it/diss/). At the very least, the authors should indicate how their fault source model relates to DISS, and what are the main differences (concepts and/or data).

- *We will add these information in the section 2.1 "Fault Source Model" at line 84: "Although for the Italian territory there is already a database that contains the results of the investigations of the active tectonics during the past 20 years (Database of Individual Seismogenic Sources, DISS, http://diss.rm.ingv.it/diss/), made by three main categories of seismogenic sources: individual seismogenic sources, seismogenic areas, macroseismic sources, it does not work well to elaborate a PSHA model using individual seismogenic sources, as in this work. In* *fact, the DISS Authors (Basili et al., 2008) say that the individual seismogenic sources database cannot guarantee the completeness of the sources themselves and are not meant to comprise a complete input dataset for probabilistic assessment of seismic hazard. For this reason, we are not restricted to just use of the DISS, but trough a synthesis of published works over the last twenty years (see supplements for complete references) we defined a database as complete as possible, in terms of individual seismogenic sources, and parameters to have input dataset for PSHA."*

- Although the authors refer to the SHARE project, and even use certain aspects of it, they do not compare their results to the fault-based hazard map (FSBG model) created in this project.
- Similarly, although a comparison with the current national hazard map is described in general terms, this comparison is not shown.

- *We attach in supplement a figure (Figure S1) showing the comparison among SHARE (FSBG) model, the current Italian national seismic hazard map (MPS04) and our model (Mixed model), using the same GMPE's. The new figure we'll be included in the manuscript, at Chapter 3. The figure shows how the impact of our fault sources input is more evident then the FSBG-Share model (the branch using fault sources and background) and the comparison with MPS04 confirm a similar pattern, but with some significant differences at the regional-to-local scale.*

- In my opinion, it is also essential to show the summed MFDs of the different source models, and comparing those to each other and to the observed MFD based on the full catalog. Without this information, it is not possible to evaluate the performance of their model. Notably, it is indicated that the rate of M 5.5-6.0 earthquakes in the TGR end member is higher than in the CHG end member, but this is not shown.

- *Thanks for your suggestion. We attach in supplement a figure (Figure S2) showing and comparing the summed MFD's of the fault source inputs (TGR, CHG, Mixed), the distributed source input, the total model (distributed + fault) and the CPTI15 catalogue, for Apennines and surrounding areas. This new figure highlights also the differences in the rate of M 5.5-6.0 earthquakes between TGR and CHG model. The new figure we'll be included in the revised version of the manuscript.*

- I have some doubt whether maximum magnitudes are correctly modelled, as it is indicated at some point that an earthquake assigned to a fault could have a magnitude larger than the magnitude range in the MFD for that fault, which should not be allowed.

- *What we wrote at lines 442-444 was a mistake: we never have a magnitude larger than the magnitude range in the MFD for a fault. So, the right sentence is: "if an earthquake* *assigned to a fault source (see Table 2 for earthquake-source associations) has a magnitude lower than the magnitude range in the bell curve of the CHG model distribution, the TGR model is applied to that fault source." We'll update the text in the revised version of the manuscript.*

- To improve clarity, the authors should more clearly explain in advance what they intend to do. Two main cases are:

  - They first describe the fault-source model and the distributed source model, and only later explain that these are not independent models, but are complementary, together accounting for all seismicity in Italy;

  - *You are right, we'll write in the revised version of the manuscript, as you suggest, that the two models are not independent but complementary, both in magnitude and frequency distribution. Moreover, as also suggested by the second reviewer, the fault-source and distribute source are not 'models' s.s., so we'll rename them as 'input'.*

  - They first show hazard maps produced with the TGR and CHG MFD models, but only later explain that these are two end members, and that their preferred model is the Mixed model, in which a particular MFD model is assigned to each fault.

  - *Thanks for your suggestion. We'll be more clear in the introduction of the revised version of the manuscript that we consider the TGR and CHG MFD models as end members, and the Mixed model as a sort of an "expert judgment" model, useful for comparison analysis.*

Detailed comments

Abstract

L. 30: "the spatial pattern of our model is far more detailed" → "the spatial pattern of the hazard maps obtained with our model is far more detailed". Unfortunately, this is not demonstrated in the paper, as there is no direct comparison with other hazard maps.

We'll show the differences between our approach and the others by a figure (Figure S1 in the supplement) where we compare our results with SHARE (FSBG) model and the current national hazard map (MPS04), using the same GMPE's. The new figure we'll be included in the manuscript, at Chapter 3.

1. Introduction

L. 52: “Combining seismic hazards from active faults with background sources” → “Combining active faults with background sources”. I also note that the plural “seismic hazards” is used in other places in the manuscript, but it should be singular, as the paper deals with only one type of seismic hazard, namely ground-motion seismic hazard.

Thanks for your suggestion: we’ll remove the plural.

2.1 Fault Source Model

L. 92: “thrust faults could be considered in a future study”: Is there a particular reason for not including thrust faults in the present study? And for which areas in Italy will this have the largest impact?

We decided to not include thrust faults in the present study because for them we have to solve some problems, mainly connected to the definition of individual seismogenic source, not yet solved in Italy for such kind of structure. For example, for thrust faults we do not have a good knowledge of the geological slip rate as for normal active fault, we need to introduce a different way to make the segmentation and different segmentation rules, and maybe there is need to consider them as complex sources in OpenQuake. The areas in Italy where we think they will have the largest impact are NE sector of the Alps, Po Valley, offshore sector of the central Adriatic Sea and SW Sicily. In this paper we want to focus on the impact of the integration of faults and earthquakes data, without the assumption to be complete in terms of individual seismogenic source database, but on the contrary suggesting a way to integrate two incomplete database in the best way, without throwing data. We will add in the manuscript a phrase explaining our choices.

L. 101-102: “Slip rates control fault-based seismic hazards … and provide a time scale …”: Strange phrasing. Slip rates do not provide a time scale. I’m not sure whether the authors mean to say that slip rates may be measured over different time scales or that slip rates may vary through time or both.

Thanks for your suggestion: we will rephrase this sentence as: “Slip rates control fault-based seismic hazard … and reflect the velocity of the mechanisms operating during continental deformation ...”

L. 112-124: This paragraph discusses slip rate variability through time, and states that slip rates have been determined for different time scales. However, (1) it is not clear how this time variability is handled in this study (it is not mentioned anymore further in the paper), and (2) Table 1 only lists minimum and maximum slip rates, without indication of the corresponding time scale. Is the time scale the same for all faults in this table?

Thanks for your suggestion: this paragraph is not clear and so we will re-write it in the revised version of the manuscript. The aim is to highlight that we are conscious of the problem of the possible slip term variability through time, but we are able to solve it with the data in our database. The assumption we do is that we use the minimum and maximum values of slip rate, determined in different ways and different time scales (see the numerous neotectonics, palaeseismological and seismotectonics cited papers), to calculate a mean value that we assume as representative of the long term behaviour (about last 15 ka for the Apennines).

L. 141: “the function with the lowest log-likelihood”: Shouldn’t this be the highest log-likelihood? Usually, one seeks the maximum likelihood, not the minimum likelihood

Yes, it is the highest log-likelihood. We’ll correct in the revised version of the manuscript.

L. 145-150: Is this an appropriate way to determine the overall standard deviation of the slip rate distribution in an area? I think it would be more appropriate to apply the Central Limit Theorem. If you consider each fault slip rate (x) as a sample from a population with mean μ and standard deviation σ, then μ can be found as $\mu_{\bar{x}}$(mean value of the sample means), and σ as $\sqrt{n}\,\sigma_{\bar{x}}$ (with n the number of samples and the standard deviation of the sample means).

Thanks for this suggestion. We applied the Central Limit Theorem for the three areas and the standard deviation is 0.11, 0.33 and 0.83 for Northern, Central-Southern, and Calabria-Sicilian area respectively. Instead using our approach we obtained 0.25, 0.29, and 0.35 for the three areas respectively. The obtained values for Northern and Calabrian-Sicilian areas are a little bit different, we think because the sample population is not enough large to apply the Central Limit Theorem; in fact n has to be > 30, while in our case n is equals 20 and 14 for the Northern and Calabrian-Sicilian area respectively. For this reason we decided to leave the standard deviation computed with our suggested approach.

L. 166-169: there seems to be overlap between criterion ii (sharp bends) and criterion iv (bending ≥ 60°).

Yes, you are right, we wrote in a wrong way. The ii criterion is “(ii) intersections with cross structures (often transfer faults) extending 4 km along strike….”. We will correct the manuscript.

L. 180: “thinnest ST” → “smallest ST”. Can you comment on the small ST value of 2.5 km? Is this in a volcanic zone?

No, it is not in a volcanic zone. The value of 2.5 km is due to the presence of “Alto Tiberina Fault”. It is a structure well known in literature: a low angle normal fault acts to detachment *for the seismogenic faults located in the hanging-wall. We'll add a sentence in the revised manuscript at line 180 as: "with the thinnest ST is Monte Santa Maria Tiberina (id 9, ST = 2.5 km) due to the presence of east-dipping low angle normal fault, the Alto-Tiberina Fault (Boncio et al., 2000), located few kilometres west of the is Monte Santa Maria Tiberina fault."*

L. 181: "Observed maximum magnitude data have been assigned to 47 fault sources". Is this based on Table 2?

Yes, it is. We have written it in the manuscript at line 181:" Observed maximum magnitude data have been assigned to 47 fault sources (based on Table 2)".

L. 197-198: "a value that corresponds to the maximum observed magnitude (Mobs)". I'm not convinced it is correct to consider Mobs as one of the possible Mmax values, and treat it the same as the other estimations. In fact, the only thing we know for sure about Mmax is that it cannot be lower than Mobs. For that reason, Mobs is often used as a lower truncation of Mmax distributions (e.g.,EPRI method for Stable Continental Regions). Not doing this can have strange consequences, as in lines 442-444, where it is stated "If an earthquake assigned to a fault source has a magnitude lower or higher than the bell curve of the CHG model distribution, …". However, the second case (observed magnitude higher than modelled Mmax distribution) should not be allowed in the PSHA model.

We partially agree with you. In some cases the observed Magnitude (Mobs) is useful to better constrain the potentiality of an individual seismogenic source, as some examples like Irpinia Fault (id 51 in the database) where the 1980 earthquake helps to better constrain the Mmax computed by only scaling relationships. Obviously it is important to avoid cases where there is an inconsistency between the fault geometry and the observed magnitude, and so our rationale was:

1) *we calculate the maximum expected magnitude (Mmax1), and the relative uncertainties, using only the scaling relationships (detail in Pace et al., 2016, FiSH paper);*
2) *we compared the observed magnitude of the associated earthquakes in the catalogue (Mobs), and if the Mobs is contained in the range Mmax1 +-1 standard deviation, we consider the Mobs recalculating the Mmax (Mmax2) and the new uncertainties;*
3) *if the Mobs is lower then Mmax1 we consider a GR behaviour for the source, without using the Mobs in the Mmax2 calculation;*
4) *if the Mobs is larger then Mmax1 we review the fault geometry or the earthquake source association.*

We'll improve the manuscript in order to better explain our rationale.

L. 199: "modifying the along-strike dimension if the rupture length exceeds the length predicted by the aspect ratio relationships". This is not very clear. Maybe rephrase as "reducing the fault length if the aspect ratio (W/L) is smaller than indicated by the relation between aspect ratio and rupture length for observed earthquake ruptures in the Abruzzo (Peruzza & Pace, 2002)".

Thanks for this suggestion. We'll rephrase as you suggest.

L. 202: "we use the criterion of "segment seismic moment conservation"": is this a criterion or a concept, and can you briefly describe what it implies?

We agree that a brief description could be useful. At line 203 we'll add a sentence as: "… which divides the seismic moment that corresponds to M_{max} by the moment rate given a slip rate:

$$T_{mean} = \frac{1}{Char\_Rate} = \frac{10^{1.5M_{max}9.1}}{\mu VLW}$$

where T_{mean} is the mean recurrence time in years, Char_Rate is the annual mean rate of occurrence, M_{max} is the computed mean maximum magnitude, μ is the shear modulus, V is the average long-term slip rate, and L and W are the geometrical parameters of the fault, along-strike rupture length and down dip width respectively."

L. 206-207: "we use two magnitude-frequency distributions" → "we use two magnitude-frequency distribution models". I also recommend introducing the acronym MFD here, as the term is used frequently in the remainder of the manuscript.

Thanks for the suggestion: we'll introduce the acronym MFD in the abstract and replaced all "magnitude-frequency distribution" in the manuscript.

L. 208: "Gaussian bell curve centred on the Mmax": Perhaps it is worth mentioning that this Gaussian curve applies to the incremental MFD values, not to the cumulative MFD values that are shown in Fig. 2c.

We'll modify the sentence into: "symmetric Gaussian bell curve (applied to the incremental MFD values) centred on the Mmax of each fault, with a range of magnitudes equal to 1-sigma".

L. 209-211: It is not explained how the a- and b-values are determined for each fault when the TGR model is used. I assume this is done with the FiSH code, but it would be good to briefly describe the underlying concept (relation with slip rate).

We'll add a phrase to better explain how the a- and b-values have been determined: "For MFD, the b-value is constant and equal to 1.0 for all faults, obtained by the interpolation of the earthquakes in the CPTI15 catalogue, as the events on the single sources are *insufficient for statistics. However the a-values have been computed by Activity Rate FiSH code, balancing the total expected seismic moment rate with the seismic moment rate that was obtained by the pair M_{max} and T_{mean}, evaluated by the fault geometry and the slip rate of each individual source (details in Pace et al., 2016).”*

2.2 Distributed Source Model

L. 233-234: “If the causative source of an earthquake is known, the impact of that earthquake does not need to be included in the seismicity smoothing process” → “If the causative fault of an earthquake is known, that earthquake does not need to be included in the seismicity smoothing procedure”. It should be explicitly mentioned before that the fault and distributed source models are conceived as <u>complementary</u> source models, not as alternative source models (competing models in a logic tree). In the latter case, they should be independent.

Thanks for this suggestion. We’ll better explain before that we consider the two source models complementary but not alternative, and so not independent.

L. 263: I think the * symbol in the equation should be left out. If I understand correctly, rather than a multiplication, $\lambda(i_x, i_y)$ represents the seismicity rate in grid cell (i_x,i_y)

Yes, you are right, it was a typo.

L. 276-278: I don’t understand the description of the Voronoi partition procedure: if the Italian territory is divided in a grid with 0.05° lon/lat spacing, then how can the number of grid cell centres be varied? Perhaps the centres of the grid cells represent the <u>possible</u> centres of Voronoi polygons, and you <u>vary the number of Voronoi polygons</u> from 3 to 50, for each case drawing 1000 random subsets of Nv grid cell centres?

To be more clear we’ll modify the manuscript as:“… the Voronoi tessellation of space without tectonic dependency. The whole Italian territory has been divided into a grid with a longitude/latitude spacing of 0.05°, and the centres of the grid cells represent the possible centres of Voronoi polygons. We vary the number Voronoy poligons, Nv, from 3 to 50, generating 1000 tessellations for each Nv.”

L. 297: “β = 2/3 b”: I think this should be “ = b. ln(10)”, which is ~2.3 b.

Yes thanks, it was an oversight. It is “= b. ln(10)” because we are taking into account the equation with magnitude and not seismic moment.

2.3 Combining fault and distributed sources

L. 299-300: It would be better to describe this concept before the two source model components are described (see general remark).

Thanks for the suggestion. We'll introduce this concept before in the manuscript.

L. 307: Add some statement that this assumption is explained in more detail in the following paragraphs.

Ok, at the end of the line 307 we'll add a sentence as:"… this assumption is explained in more detail further on."

L. 338-340: Is this valid for all types of faults or only for dip-slip faults?

It is valid only for dip-slip faults, and because we want be more general with this concept, we'll modify the lines 338-340 as: "Static stress changes produce areas of negative stress, also known as shadow zones, and positive stress zones".

L. 360: Perhaps add that it is a linear function.

Ok, we'll add it. We'll modify line 360 in: "we introduced a slip rate and a distance-weighting linear function.."

L. 363: Write the equation more completely:

We'll, thanks.

However, there is still a problem with the second line, which does the opposite of what is intended (going to 1 as d increases): instead of 1/d it should be d/d_{max}…

Thanks, you are right, we'll correct.

L. 366-367: What is the rationale for varying d_{max} in function of slip rate?

We made a simple assumption, higher is the slip rate, higher is the deformation field and so higher is the value of d_{max}. We'll explain our rationale in the manuscript.

L. 369-371: This is hard to understand. Maybe rephrase as "Because we considered two fault source models, one using only TGR MFDs and the other only CHR MFDs, and because the MFDs of distributed seismicity grid points in the vicinity of faults are modified with respect to the MFDs of these faults, we also obtain two different models of distributed seismicity."
In my opinion, it is also necessary at this point to show the summed MFDs of the different (sub)models, i.e. summed MFD of the TGR fault source model, of the CHR fault source

model, of the TGR distributed source model, of the CHR distributed source model, and of the combined TGR and CHR source models.

Thanks for the suggestion, we think that rephrasing as you suggested is clearer. As said in the previous comment, we'll add a new figure to show the MFD's of the different models.

3. Results and discussion

L. 382: "designed under the traditional Poisson hypothesis": Rephrase

We'll rephrase in: " To obtain PSH maps we assign the calculated expected seismicity rates, under Poisson hypothesis, to their pertinent geometries…"

L. 386: "well-known": this is not the most relevant property for choosing OpenQuake. Perhaps widely used, open-source, tested, …?

We'll remove "well-known" and add at line 387 before "The ground motion…" this sentence: "We used this software because it is an open source software developed recently by GEM with the purpose of providing seismic hazard and risk assessments. Moreover, it is widely recognized within the scientific community for its potential."

L. 402: Explain more explicitly that the TGR and CHG fault source models are end members that are only used to explore the epistemic uncertainty, and that in the preferred fault source model a choice is made between the two MFD models for each fault.

Thanks for your suggestion; we'll better explain our choices.

L. 403-404: "Although both models have the same amount of seismic moment release": this has not been demonstrated.

Here, we were discussing about the two fault source models. In this case the same amount of seismic moment release is an assumption that we made before to compute the MFD's, as before explained.

L. 409-411: "The rates of earthquakes with magnitudes between 5.5 and approximately 6, …, are generally higher in the TGR model than in the CHG model": Please demonstrate by showing the summed MFDs.

Will be shown in a new figure (now Figure S2 in the supplement).

L. 443: “a magnitude lower or higher than the bell curve” → “a magnitude lower or higher than the magnitude range in the bell curve”. See also my remark at lines 197-198: a higher magnitude should not be possible!

We’ll improve the manuscript, better describing our approach: see the answer in the general comments.

L. 468-471: It has not been explained exactly how the TGR MFDs have been constructed. See my remark at lines 209-211.

We’ll add this information at line 209-211. See our reply at these lines.

L. 505: Perhaps replace “TGR model” with a brief description like you do for the CHG model in the following line.

Thanks for your comment, we agree. We’ll add at line 505 a sentence as:” the Truncated Gutenberg-Richter model, where the maximum magnitude is the upper threshold and M_w *= 5.5 is the lower threshold for all faults…”.*

4. Conclusions

L. 558-559: “pattern similar to that of the current national maps at the national scale, but some significant differences in hazard are present at the regional-to-local scale”: this has not been discussed in the main text. It would be instructive to show both maps side by side and describe the comparison in some more detail in §3.

See our reply at general comments and the new figure (now Figure S1 in the supplement). As suggested, the new figure we’ll be included in the manuscript, at Chapter 3.

L. 563-565: See my comment for lines 409-411. It would also be interesting to compare the summed MFDs to the observed MFD based on the full catalog, to see which of the two MFD models is closer to the observations in this particular magnitude range (M 5.5 to ~6.0).

See our reply at general comments and the new figure (now Figure S2 in the supplement).

Figure captions

Fig. 9 : Explain acronym "poe"

In the caption we’ll add this sentence: ”The dashed lines represent the 2%, 10% and 81% probability of exceedance (poe) in 50 years.”

Fig. 12: How are the contributions of the component source models computed? The perfect symmetry between the contributions of the fault source model and the distributed source model gives me the impression that they do not correspond to the contributions one would obtain from a deaggregation.

Yes, you’re right it is not a deaggregation. It is the contribution of each source model in the total. For example, if the PGA value in a given point of the grid is: 0.15, 0.20 and 0.35 for the distributed, fault source and total respectively, the contribution will be 43% and 57% for the distributed and fault source respectively. Probably could be right to better explaining this in the manuscript, and so at line 482 we’ll add a sentence as: “Note that the contributions are not given by deaggregation but are computed how the percentage of each source model in the PGA value of the total model.”

Cited papers

Basili, R., G. Valensise, P. Vannoli, P. Burrato, U. Fracassi, S. Mariano, M. M. Tiberti, and E. Boschi. 2008. 'The Database of Individual Seismogenic Sources (DISS), version 3: Summarizing 20 years of research on Italy's earthquake geology', Tectonophysics, 453: 20-43.

Boncio, P., Brozzetti, F. and Lavecchia G. 2000. Architecture and seismotectonics of a regional Low-Angle Normal Fault zone in Central Italy. Tectonics, 19 (6), 1038-1055

Pace, B., F. Visini, and L. Peruzza. 2016. 'FiSH: MATLAB Tools to Turn Fault Data into Seismic- Hazard Models', Seismological Research Letters, 87: 374-86.