Dear Maria Ana Baptista

We have uploaded a new version of the manuscript.

It is this version, we have had considered all the comments made by the reviewers and included the proposed corrections. In particular, the main changes in this version are:

1) We have included new equations that help to better understand the methodological development, the maximum magnitudes associated with each type of source have been differentiated,
2) the figure captions have been revised,
3) 30 new references have been included,
4) the discussion and conclusions have been separated
5) the English has been reviewed by an expert.
6) In addition, an annex with the parameters of the faults considered in the application is included.

After the changes included we hope that the manuscript may be considered for publication.

Sincerely,

Alicia Rivas Medina

**Referee #1**

General: The paper is generally well written and presents a new alternative for combining faults and zones in a PSHA evaluation. The language is generally acceptable, but here and there the Spanish “accent” is coming through, and sometimes wrong wording is used. A thorough “language washing” by a native English-speaker is needed before publication.

On Mmax: This review is concerned with using the catalogue only up to Mmax recorded in the catalogue. What about blind faults that are not mapped and may trigger larger magnitudes away from the mapped faults. The authors should be more clear about this situation. This issue is exemplified in Fig. 6 where the Granada and Almeria hazard is so widely different from the zonation based hazard map. Fig. 6 should also be expanded with 1 Hz hazard difference in which the difference between methiods could be even higher.

On Mmax: After reading I am still somewhat uncertain how MmaxC and Mmax relate.

The authors should make some additional effort in clarifying the different Mmax used for faults and areas. May be use more clear annotations Mmax(fault)= MmaxF;

Mmax(zone)=MmaxZ and Mmin correspondingly.

On Mmax: May be I have overlooked, but how is Mmax established quantitatively from the catalogue?

QAFI database not defined/referenced.

Fig. 6 and 8: What is the Mmax used in the reference model? Due to the big differences it is important to be very clear about the reference computation. The implications in an application is significant.

Thank you for your comments and remarks.

MmaxC refers to the maximum magnitude value that can be recorded in the catalog completely. This means that the catalog may contain events with higher magnitude value, but due to the long recurrence of these events, it can not be assured that the period of records covered by the catalog includes several recurrence periods of these events to make it possible to derive an (statistically) meaningful recurrence period value. The MmaxC value is used to constraint the distribution of seismic potential between faults and zones, but not to estimate seismic hazard.

The maximum expected magnitudes for each source are included in seismic hazard assessment. For fault sources, the maximum magnitude is obtained from the length of the fault plane. For zones (that contain the seismicity related to blind faults) it is more difficult to establish the maximum magnitude value, as it depends on the study area. It the application shown in this work, it is considered Mmaxzone=MmaxC+0.5. For our study area this implies a Mmaxzone value of up to 6.5. This is considered sufficient due to the short record of events with that magnitude in the catalog.

The term Mmax is changed, distinguishing the different Mmax used for zones and fautls: Mmax(zone) and Mmax(fault)

We only show the seismic hazard maps expressed in terms of PGA. The hazard maps for different spectral ordinates are not shown to reduce the length of the paper and because they do not display significant differences.  Nevertheless, we include the response spectra for selected locations in figure 10.

The case of Almeria and Granada, high acceleration values obtained with the HM are associated with documented faults, nearby these cities. The Mmax value for these faults is derived from the fault length ,

In the CM, the maximum magnitudes for each zone are modelled using a magnitude distribution that considers the maximum recorded magnitudes in the catalog and the maximum magnitudes expected in the faults (see details in Gaspar-Escribano et al., 2015).

We make an effort to clarify these points in the text. We also include a new figure with the UHS spectra obtained for different cities (Granada, Almeria and Murcia) with both methods to show the impact of the source model in different spectral accelerations.

Several references are included (including one to QAFI).

## Referee #2

Thank you for your comments and remarks, which imply a considerable improvement to the manuscript. We give response to all the points raised by referee 2 below.

*# General comments*

This paper is interesting and covers an important topic of interest to NHESS readers.

However, there are several significant weaknesses in the paper, which I estimate will require *major revisions* to address.

Although the major revisions may involve considerable work from the authors, I do think it should be possible to revise the paper to meet the standards of NHESS publication.

Before going into detail below, I note some 'overall' problematic aspects of the current draft:

- Poor referencing. The paper contains no references in the Introduction, despite discussing other work. Other areas also need more references (e.g. often non-obvious 'facts' are stated, without information on where they come from). I note a number of examples below, but not all – in general, the authors need to be more rigorous about justifying statements with references.

We include 33 new references in the document, specially in the introduction.

- There is insufficient explanation of the earthquake catalogue analysis methods (i.e. the discussion on page 3 in the paragraph around line 20 needs to be expanded).

This paragraph is changed, including the appropriate references and explaining the method. The graph of figure 2 illustrates this point.

- A number of statements in the paper are unclear or lacking in rigor (details below).

- I think there is a (fixable) logical flaw in the method. The authors are assuming (equations 8 and 9)

1: region_seismicity = zone_seismicity + fault_seismicity; and furthermore, at various points in the analysis they assume (equations 10 and 4):

2: all of these seismicity sources can individually be represented with GR curves;

The combination of (1) and (2) would be fine *if* all the seismicity sources have the same 'b-value', but the authors suggest different 'b-values' on fault vs region/zone.

Mathematically, I don't think (1) holds under this assumption (i.e. you can't sum 2 GR curves with different b values and expect to get another GR curve). I have suggested a possible fix below, which looks like it may be reasonable based on the data in the paper – but I can't be sure. Anyway, the authors should address this somehow.

Equations 8 and 9 (new 12 and 13) add cumulative seismic moment rates and cumulative seismicity rates. Both are point values and not functions (as GR curves).

The b-value give the ratio between events of different magnitudes and thus it changes the rate of earthquakes produced of each magnitude, but not the total earthquake rate nor the total released moment rate. We adjust the b-value that keeps those values (cumulative seismic moment rates and cumulative seismicity rates) constant for each source.

We agree with you when you state that the GR curves cannot be just added, as they have different b-value.

On the other hand, I expect these issues can be addressed. Furthermore I note there are good points to the paper: it treats an important topic, provides a good application of the methods (which I found much easier to read than the methods description itself), and includes a reasonable attempt to quantify the uncertainties (Section 2.4).

# *Specific Comments*

- The Introduction does not contain any citations of other work. However, there is repeated mention of 'other studies' {e.g. p1 Line 25; p1 Line 27; p2 Line 3; p2, Line 22; p2, Line28 ... and many other instances, I will not list them all}. Throughout this section, the authors need to provide references to justify their descriptions of other work (even when they refer to general issues that are not specific to a particular study).

References have been included for all cases

- P 3, Line 15: ** Magnitudes values above MMaxC present recurrence periods higher than the catalog OP or constitute a sample an insufficient number of records to apply statistics. ** – This statement needs to be rephrased to correct the grammar. More importantly I think it is factually incorrect.

For illustration, suppose we have a catalogue of 100 years duration, and this catalogue contains zero events with Mw>=X. Then I claim that we **can apply statistics** to place bounds on the rate of events with Mw>=X, and furthermore, that **the return period of such events MIGHT be less than the observational period**. Both of these points contradict the author's statement. For example, with zero events in 100 years, an exact Poisson test of the true rate of events with Mw>=x (e.g. poisson.test(x=0,T=100) in the R software) gives a 95% confidence interval on the true rate of [0, 0.037] events/year.

So the return period is probably greater than around 1/0.037=27 years. This makes sense – if the return period were very small then it is likely an event would occur in the data. Furthermore, it is obviously possible that the return period is < 100 years (we don't get a '100 year event' in every hundred year observational period).

I think the authors probably want to say that it is increasingly difficult to constrain rates of rarer events – that's correct - but the authors need to weaken the statement accordingly.

We wanted to refer to the fact that when recurrence periods are long, there are a few records of those events in the catalog, and consequently, the catalog presents a small sample, with limited statistical significance to establish recurrence periods. We changed the phrase for:

"Magnitude values above MMaxC present recurrence periods higher than the catalogue OP. These values usually constitute a sample that does not include a high enough number of records to clearly establish the recurrence period, as this makes it increasingly difficult to constrain rates for rarer events."

However, we want to indicate that we refer to recurrence periods (as the inverse of the earthquake occurrence rate) and we do not consider return periods at this point of the work (it is considered in hazard calculations).

- P 3, lines 17-18: The authors should explain the 'Stepp (1972)' approach to estimating the reference years for different return periods. Although Figure 2 is related to this, I cannot understand the method from the figure.

This paragraph is changed, references are included and the method is explained to understand figure 2 better. The reference to Stepp was a mistake that we have changed.

- P3, Lines 19-20: **Then, it is possible to estimate ...** – the authors should explain how they do this estimation. There could be a range of methods (depending on the extent to which one makes parametric assumptions, like that the data-generating process has a GR distribution).

We include two equations to explain how both parameters are estimated. However, we want to indicate that we extract these data from the catalog, and we do not assign a GR distribution in this part of the calculation.

- P 3, Line 24: ** This way we avoid miscalculation problems ...**. I think this statement is too simplistic, and should be rewritten. In reality, errors in the estimated rates may be quite significant even for events with true return period significantly less than the catalogue duration. There are statistical methods for quantifying such uncertainties in different contexts.

For example, for individual rates one might assume a Poisson process and estimate confidence intervals as I suggested in an earlier comment. Or for a full magnitude frequency distribution, one might estimate uncertainty in the rates for any magnitude by assuming a GR distribution, and using maximum likelihood with profile likelihood confidence intervals - or using Bayesian techniques, etc. If the authors google-search 'Gutenberg Richter maximum likelihood estimation' they will find many related references.

The text has been modified: "In this way, we avoid using magnitudes with long recurrence periods that have not been recorded in the catalogue within the completeness periods". Again, we make reference to recurrence periods and not to return periods.

- P 4, Section 2.2: It's not clear to me whether the authors assume there is only 1 fault per region (this is how the math appears, and consistent with the use of the word 'fault' rather than 'faults' in most places), or if there are multiple faults and GR curves which are summed over (that would seem sensible, but no summation is mentioned), or if there are multiple faults which are all treated with a single GR model (but there is no summation mentioned in equation 3, and the word 'fault' is mostly used, rather than 'faults'). This needs to be made much clearer to the reader, with 'summations over faults' included in the equations as appropriate. Table 4 makes me think it is 'multiple faults with single GR model', but it is far from clear.

We consider as many faults per region as available in the fault database. All faults are assigned the same b-value and different occurrence rate (as each fault presents different moment rate). We corrected the ambiguity in the manuscript.

- P 4, line 12: It is unclear how 'v' is estimated (I suppose from other geophysical studies? Or paleo work?). Please provide references. Also, how do you choose the shear modulus? References.

The slip rate (v), can be obtained from paleoseismicty studies and GNSS measurements. We include a reference to database of active faults providing this information.

The shear modulus value is $\mu = 3.2 \times 10^{10}$ Pa (Walters et al., 2009; Martínez-Díaz et al., 2012)

- P 4, line 16: Note that 'm=0' is not the lowest value of m. Maybe just say 'from very low up to the maximum ...'.

The text is modified

- P 4, line 26: Related to the above comment, you might make the lower summation index in the first right-hand-side term be "negative infinity" rather than zero. Also, I think you don't model earthquakes with m < M_min later in the paper? If so, I suggest mentioning that here. Indeed you might re-write this part to avoid mention of events below M_min, perhaps it is just confusing?

We do not include events with magnitude below Mmin to estimate seismic hazard, but we take into account the moment rate related to events with magnitudes below Mmin to estimate the seismic moment rate in the interval Mmin-MmaxC.

$\sum_{\mathrm{M_{Min}}}^{\mathrm{M_{MaxC}}} \dot{\mathrm{n}}(\mathrm{m}) \cdot \mathrm{Mo}(\mathrm{m}) = \dot{\mathrm{M}}\mathrm{o}_{\mathrm{fault}} - \sum_{\sim 0}^{\mathrm{M_{Min}}} \dot{\mathrm{n}}(\mathrm{m}) \cdot \mathrm{Mo}(\mathrm{m}) + \sum_{\mathrm{M_{MaxC}}}^{\mathrm{M_{Max}}} \dot{\mathrm{n}}(\mathrm{m}) \cdot \mathrm{Mo}(\mathrm{m})$ of Eq. (6)

The moment rate assigned to each interval (0- $M_{Min}$), ($M_{Min}$, $M_{MaxC}$), ($M_{MaxC}$, $M_{Max}$) depends on the b-value estimated for the fault. Hence, we must take into account this interval until a b-value is fixed.

This part is changed for a better understanding.

- P 4, line 22: I'm not sure if you have defined the $noverline{d}$ variable. Also I don't understand the notation "Mo|" in the numerator (what is the bar? what is Mo? Should it be for faults only?)

The notation is changed and all parameters are defined

- P 5, line 9: Here, I think you should remind the reader that the 'region' parameters are assumed known, based on Equations 1 and 2.

A comment is included

- P 5, line 14: **Note that the b-value of the zone appears in this equation can be equaled to the b-value of the region as both sources present similar seismic nature.** Mathematically I think this is problematic. Question: Is a distribution defined by summing 2 GR distributions with different 'b-values' still a GR distribution? I don't think so. However, that seems to be an implicit assumption of your method (i.e. because the regional seismicity is the sum of 2 different GR distributions, fault and zone, each having different b values). This suggests an internal contradiction in the methods, which should be fixed. Suggestion for a fix: Consider modifying your method so that b-fault is equal to b-region and b-zone. Then I think the issue would be avoided? Furthermore, from Table 4, that would look to be an OK approximation, given the statistical uncertainties in Table 1.

This topic is answered in "General comments". We remark that we do not equal the GR distributions, but only the total seismicity rate and moment rate, both values are concrete and constant for each source independently the ratio of different magnitudes that are calculated later on.

- P5, Section 2.4. I like this analysis overall. However, from Table 1 it seems like you are using a single b value in the simulations (?), in contrast to the above-mentioned 'distinct fault/region b values'. The testing here needs to be consistent with the methodology.

So if you change the analysis as I suggested above then this section is probably fine –but otherwise, you should treat these 'distinct fault/region b values'

The uncertainty analysis focuses on how the input data (seismic catalog and fault database) affect the uncertainty in the end result.

Concretely, table 1 shows how the magnitude interval, the number of records and the b-value of the seismic catalog affects the uncertainty in the seismic moment rate of the region, a parameter that will have a strong influence on the final result for all sources. Thus, table 1 only considers the uncertainties of the seismic catalog, which is used to model the seismic potential of the region.

The uncertainty related to the input parameters of faults is tackled in the last paragraph of the section.

- P7, line 4: Need a reference for the shortening rate.

- P 7, line 14: Reference for the QAFI database?

The text is changed and the references included

- P 7, line 16: Not clear to me how you estimate M-max if there are multiple faults per region. Do you assume they all rupture at once?

For each fault we obtain a different Mmax, as a function of the length of the fault plane

- P 7, line 29: '.. lacks a COV estimate because the sample of records is not significant ..' – I think you need to re-word this. What do you mean by 'not significant'? It's unclear – the word 'significant' is suggestive of 'statistical significance', but that does not appear to be what you mean. I think you just mean 'there is very little data'. However, I don't see why you can't calculate a COV coefficient (albeit a very uncertain one).

Zone 30 only has 7 seismic records. Any statistical analysis with this sample size is not representative. The text is changed to clarify this point.

- P 8, line 13: 'This result is characteristic of this method ...' – sounds like there must be other studies that show this, please add references.

The text is changed

- P8, line 20: "These results agree with real observations" – can you cite the study?

A reference is included

# *Technical corrections*

- P1, Line 6: 'estimated' should be 'estimates'

- P2, Line 18: suggest changing 'a part of' to 'some of'

The text is changed

P2, Line 18: 'must be linked to faults' – is this always true? What about if there are not catalogue events on the faults? Suggest rewording.

We understand that is that fault is active and generates earthquakes with not very long recurrence periods, the events associated to the fault are contained in the seismic catalog. We have rewritten the text to clarify this point.

- P 2, line 26: 'fixing a Mc value results certainly complicated' - This doesn't make sense, suggest rewording. I'm not 100% sure what you want to say – do you mean 'it is difficult to choose Mc non-arbitrarily'?

Yes, it is complicated to assign a Mc value non-arbitrarily'. We have changed the text

- P2, Line 28: ** The approach presented in this paper, as all probabilistic seismic hazard models, face the challenging question of estimating the expected ground motions with the basis of a short period of observations of earthquake occurrences and limited geological data (with significant uncertainty) to construct recurrence models.** This needs to be re-worded to correct the grammar. One suggestion: ** The approach presented in this paper faces the challenging question of how to estimate the expected ground motion exceedance rate, using a short period of earthquake observations and limited geological data (with significant uncertainties). This challenge is faced by all probabilistic seismic hazard models. **

- P 3, L5: **The zone is defined as the source which seismic potential is residual, excluding the seismic potential of faults **. There are grammatical issues here, as stated it does not make sense. I think you mean 'The zone is the same as the region with fault seismicity removed'. However, you may choose to make different edits (considering also repetition in the subsequent sentence).

- P3, Line 10: This sentence needs rewording (grammar). What about: "The seismicity rate of the region is derived from the seismic catalog using events that are contained in the period for which ...."

- P 3, Line 11: You use 'CP' here, but on line 13 you use 'PC'. Please double check for consistency throughout the paper.

- P 3, Line 13: **The complete periods PC(m) (for different magnitude up to a maximum magnitude of completeness value, MMaxC.) are included in the observation period (OP) of the catalog. ** Is it really correct to say they are 'included'. I suspect you mean to say they are 'inferred using' or are 'less than'?

- P 4, line 1: I suggest you replace the word 'cumulative' with 'total'.

- P 4, line 20: 'establish' instead of 'stablish'

- P 4, line 20 and below. You refer to $ndot{Nmin}$ – should it have a subscript 'fault', considering later notation (Equation 8)?

- P5, line 9: 'An new' should be 'A new'

- P5, line 19: I think this should mention 'faults', e.g. 'by extrapolation of the faults recurrence model'

- P6, line 11: should say 'moment rates for different magnitude values, ...'

- P6, line 12: similar problem as above

- P 7, line 26: 'It is appreciated that ..' – this sounds strange – suggest you change to 'Note that ..'. Also, it's not clear

We have changed the text

## Referee #3

Thank you for the numerous remarks. The changes that you propose are accurate and improve the paper considerably. All your suggestions are included in the manuscript. Here we answer to your questions

The paper it is a valuable and original contribution in the subject of fault source modelling in PSHA. The proposed approach has been used in a SH calculation in Spain with satisfactory results. It would be interesting to check the viability of the model in other parts of the world with faster moving faults.

Nevertheless, there are few issues that, if tackled appropriately, will greatly improve the paper, and I think they should be addressed obligatory before publication in NHESS (please see list below).

Additionally, I attached the manuscript with many comments, suggestions, and corrections, all of them highlighted in yellow and commented using Adobe reader tools.

- Improve the English. Please, for a new submission make sure that the entire article is revised by an English native speaker. The style could also be greatly improved. Avoid the excessive use of "extra comments" in brackets (e.g., page 1, line 9; p.2 lines 17, 30, 31; p.3 line 4, 12;: : : and so many more: : :). I attached the manuscript pdf highlighting that, typos, few mistakes and so.

English has been corrected by a native person in this version.

- Citations. In the introduction there is not a single cite about other previous approaches about incorporating faults as sources in PSHA neither globally nor locally. The authors must cite other previous work in the subject (e.g., Youngs and Coppersmith, 1985; Wesnousky, 1986; Anderson and Luco, 1983; Bungum, 2007; : : :.. and much more recently the different UCERF versions (e.g., Field et al., 2009, 2014), the SHARE project (Woessner et al., 2015),: : : among others. If there are, cite also other previous work done in your study area.

Previous work is cited and referenced (more than 33 new references in the paper)

- Seismic Moment Equation. It is necessary to cite and write the equation used for calculating the seismic moment from magnitude (Mo as function of (Mw), Hanks y Kanamori, 1979, IASPEI, 2005) for a completely comprehension of equation (2) and, later, equation (4). Additionally, you should state before that with the variable "m" you always refer to magnitude in the moment magnitude (Mw) scale.

A reference and the equation of Hanks and Kanamori are included to facilitate understanding the issue

- Equation 4. I assume this equation is an original contribution from this work? Please, you need to show how do you get to equation 4 from Anderson (1979) integral. Need to explain what is parameter d (in relation to Mo f (Mw).

Two equations are included to explain how eq 4 (eq 8 in the new version ) is obtained,. All terms are explained

- Equation 10. Further explanations should be given on how you get to equation 10.0

Eq 10 is explained with more detail

- Figure captions. They are very short. More explanations in the captions are needed to fully understand the figures. Particularly figure 2 (see attached pdf).

Figures are explained with more detail

- Figure 5. This figure can be greatly improved from what it is now (a dumped screen). A inset locating geographically the studied area is necessary.

We include this 3D view instead of a map to get an idea of the depth dimension of faults. A location map is included

- Fault sources. The paper will improve greatly if you further explain how did you select the faults and what are there characteristics in terms of slip rate, Mmax, kinematics, etc.

An annex listing the data of faults and faults segments used is included

- Ground Motion Prediction Equation. The GMPE you chose for your calculations was produced to address near fault-source effects. I understand that the point of the paper is to use your methodology for sharing the seismic potential between zones and faults, but because you have chosen this particular GMPE for your calculations you should explore the impact of using it in your hazard results compare to the use of a general GMPE one. I mean, this is important because in your results it is clearly shown that the hazard increases a lot near the faults, as you state p.10 l.3: "The results show an increment of expected accelerations near fault traces (in a factor of 2)." And later: "This increment is achieved at the expense of decreasing expected accelerations in areas located farther away from faults." This statement should be properly discussed in the Discussion section.

The comparison between the results obtained with both methods must be performed in terms of relative acceleration values because the absolute acceleration values are conditioned by the specific ground motion prediction equation used. To avoid the influence of the GMPE in both results, the same GMPE is used to apply both methods.

However, it remains open the possibility of integrating several GMPE in a logic tree framework to capture the epistemic uncertainty related to path effects. At the same time, it could be included the site effect in the analysis to see if the different source models affect different soil types similarly or not, This paper focuses on the impact of source models in hazard results. The impact of other factors (site effects, GMPE) can be the subject of future studies. This is indicated in the Discussion section.

- Discussion and Conclusion should be separated sections. The paper will improve greatly if you discuss your results in terms of earthquake rates contribution from faults vs zones, instead of accelerations. This way it would be showed the real (pure) impact of your approach in the hazard, without consideration of the GMPE. Subsequently, you can explore the impact of using a different GMPE in the calculations.

The Discussion and Conclusion sections have been separated. The issue regarding the use of GMPE is tackled therein.

- Results. This section should be rewritten. It contains many statements that are better placed in a "Discussion" section. See attached pdf.

The cited sentences are moved to the Discussion section

- References list. There are a couple of references listed but missing in the manuscript.

The reference list is reviewed

- Table 2. Mmax relates to the Max event from the fault. When there is more than one fault in the region, which value do you state on this table? In the range Mmin-Mmax you show the accumulated moment rate from all the faults in the region? How can be regions with MaxC blank? : : : More information is needed to understand this table properly.

Table 2 includes the values of the region, not of faults. Regions without values refer to regions (28, 29, 33 and 40) with no faults identified within their limits. Thus, the distribution of potential between faults and zone is not done in these regions. This is clarified in the table.

- Table 3. More information is needed in the caption to understand it properly. Information on some regions is missed. The MmaxC values are strikingly low: : : What happened to the larger values (big historical events)?

Table 3 shows the values of the catalog that will be used n the distribution of seismic potential. Zones with very low MmaxC value, do not present events with higher magnitude in the catalog (as in region 30). For the rest of the regions, it is included the maximum recorded magnitude equal or lower than MmaxC. Recall that this interval is only to make the distribution of seismic potential, but the higher magnitudes recorded historically are considered in the seismic hazard calculations up t the maximum expected magnitudes.

Please also note the supplement to this comment:

https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-28/nhess-2018-28-RC3-supplement.pdf

All these comments have been taken into account

## Referee #4

This manuscript presents an attempt at modeling the contribution of fault sources and zone sources for probabilistic seismic hazard assessment (PSHA). The authors start with explaining the methodological approach used to develop a hybrid model made up of faults and zones and then illustrate an application of this hybrid model in southern Spain. In this application, they also include a classical zone model to compare their hybrid model with.

The basic idea of this work is not new. It is at least as old as the seminal paper by Cornell (1968) on seismic risk analysis.

Since then, there have been countless PSHA works that deal with the combination of zone sources, fault sources, and point sources. None of these previous works is cited in the introduction, and with much disappointment of the reader, none of them is cited in the discussion. Indeed, not any other paper is cited in these two sections of the manuscript. On the one hand, this has to be regarded as an unethical issue related to the lack of recognition of others' work.

The Cornell method (1968) is a zoned probabilistic method, based on the consideration of seismogenic zones with homogenous seismic potential, which was raised precisely by its author in view of the difficulty of modeling the faults as independent seismic sources. It has been a method of widespread use in the last decades. Although in recent years, with the increasing increase of fault information, combined methods of zones and faults have begun to be proposed, such as those referenced in our current version of the manuscript. Obviously there may be many other works in this methodological line not mentioned in our work, but we are not presenting a paper on the state of the art in the subject. We present a methodological approach that aims to be a contribution in this line of hybrid methods, and we say so in the manuscript. Some representative references have been cited by way of example. The qualification of "unethical issue related to the lack of recognition of other work" b is therefore unacceptable. We raise the following question: does each time a paper on a specific topic is published consider a lack of ethics not mentioning all the existing works on that topic? Where is the limit considered?

On the other hand, this represents a major flaw that prevents the readers to understand what is actually different, innovative, and possibly better in this work with respect to other previous approaches. More specifically, it is very clear that the hybrid model presented by Rivas-Medina and coworkers is surprisingly similar to the Fault Source and Background (FSBG) model presented by Woessner et al. (2015) which, having being applied to all of Europe, obviously also covers the area of the application to southern Spain in this work. The only difference being the fact that Rivas-Medina and coworkers present their approach as if they have just (re)invented the wheel.

The hybrid FSBG model presented by Woessner et al (2015) and applied in Europe resolves the distribution of the seismic potential between zones and faults adopting a Mc cutoff magnitude of Mw 6.5, above which earthquakes associated with faults are considered, taking as a background seismicity the one corresponding to magnitudes in the MW range (4.5-6.4), which is associated with the zone, that is, the method of Woessner et at (2015) considers a fixed cut magnitude, and precisely our approach is aimed at avoiding the adoption of a fixed Mc value, based on an essential question that is formulated on page 2 of the manuscript, where it is literally indicated:

By not fixing this magnitude, the approach to distribute the seismic potential is obviously complicated and what we propose is a procedure that we detail in the paper, including its formulation. Therefore, our methodology differs substantially and essentially from that of Woesner et al (2015), both in the initial hypothesis and in the procedure to be followed.

It must be added that a value of M = 6.5 is practically the Mmax that can generate the active faults in Spain, therefore it would not make sense to establish this value as Mc, but it is not easy to establish another alternative value either.

It is surprising that the reviewer describes the methodology proposed here as "surprisingly similar to the Fault Source and Background (FSBG)" and denotes the lack of grasp of the essential aspects of both methodologies. To this we must add the notable difference of results in the application to the south of Spain.

The comment: "The only difference being the fact that Rivas-Medina and coworkers present their approach as if they have just (re) invented the whee" is also offensive. Obviously, we are not trying to invent the wheel, but rather to propose an approach in an open line of research that is not based on the consideration of a Mc to distribute the seismic potential between zones and faults. This is, in fact, a recognized problem among all the experts that work on seismic hazard towards which considerable efforts are being devoted, and our work is intended to be a contribution in this regard. So we have raised it humbly and repeatedly in the manuscript.

In the work of Woessner et al. (2015) three source models implemented in a logical tree are presented: Area source (AS); Sismicity + fauts (SEIFA) and Fault souce (FS) & BackGroup (BG). Of the three previous models, only the last one deals with a hybrid model of faults and zones. The authors consider a cutoff magnitude (Mc = 6.5) between the faults and the zone (background seismicity). This idea, which is not novel in that work either, was proposed by Frankel et al. (1996), is the most important difference between the presented here and the work of Woessner et al. (2015). In fact, this issue is addressed in the introduction to this paper, since our approach is precisely not to use a previous cut magnitude.

The approach presented in this paper is part of the PhD thesis of Alicia Rivas Medina, the first author of the paper. The public defense of the thesis was on March 2014. The pdf of the thesis was uploaded to the institutional, open repository of UPM on April 2014. It is accessible in http://oa.upm.es/23328/.

As regards the merit of this work, I found it is affected by a number of methodological flaws and possibly some miscalculations that challenge the overall validity of the results. Too many details are also missing to correctly understand how the model is developed and how the application to southern Spain was carried out. Mentioning the use of the software CRISIS is not enough for an explanation of the method and justification of strategic choices.

To be more specific, I'll touch in the following some of the main issues (I use P for the page number and L for the text line to identify the position of the text I'm referring to).

P3L18. The approach by Stepp (1972) for estimating the completeness periods needs various data manipulations that should be explained in more details to let the reader understand and replicate what was done here. This lack of details prevent the reader to appreciate the validity of the results.

The completeness period was mistakenly referenced as Stepp (1972). The correct reference is provided in the text.

P4L16. Here it is stated that the seismic moment rate of faults is calculated by summing up the seismic moment of earthquakes with magnitudes "close to m=0" up to a certain given maximum. To get an accurate seismic moment rate estimate for the faults, all earthquakes with moment M0>0 must be considered. Notice that magnitude m=0 corresponds to seismic moment M0=1.27E+09 Nm using the relation by Kanamori and Brodsky (2001). Basically, this mistake leaves out a lot of seismic moment from the moment rate estimate when the Eq. (5) and Eq. (6) are used. This significantly impacts into the correct estimation of the number of earthquakes for which the hazard is then calculated.

The seismic moment associated with an earthquake of M = 0, (M0 = 1.27E + 09 Nm), is a completely insignificant value when compared to the seismic momentum rate accumulated in a failure annually.

For example, if we assume a slow failure with slip rate = 0.1 mm / year and a failure plane size of 45x10 km, the cumulative annual seismic moment rate is 1.35E + 22 Nm.

This means that the moment released in an earthquake of magnitude M = 0 supposes 0.00000000001%, a completely insignificant value in a year, even more so in the periods of recurrence associated with slow faults. There should be many earthquakes of magnitude M = 0 to modify the result very slightly.

Figure 5. Although this figure is not very clear (a map view would have been much better), it shows that several faults are cut by zone boundaries. How was the fault moment rate assigned to the zones in these cases?

There is no case in which zones cut faults, in fact the author of that zoning is also the author of the fault database (Garcia Mayordomo et al, 2010), and this zoning was designed to avoid that case. Maybe it is a misperception by the projection of the image

Table 3 and Table 4. The b-values reported in these tables are utterly absurd. Are they really the b-values of the GR relation? If not, please explain what they actually are, otherwise I suspect that they result from gross calculation errors. In general bvalues are ca. 1 everywhere in tectonic regions all around the world. I've seen b-value estimates in various works ranging from 0.7 to 1.3, but here they are in the range 1.7-2.4 that has never been seen anywhere.

The values shown in tables 3 and 4 are not values of b, but values of Beta, as clearly indicated in these tables. Do not confuse these two parameters, although there is an equivalence between them (Beta = b * ln (10)). Values of b (0.7 - 1.3) are equivalent to beta values (1.6 - 3.0). These equivalences are well known among those who work on issues of seismic hazard

P7L15. The use of the Stirling et al. (2002) fault scaling laws is questionable and potentially the source of additional miscalculations. First of all, which one of the Stirling et al.'s (2002) relationship was used here? Stirling et al. (2002) provide relationships between Mw and either length (L) or area (A). In the second case, A is obtained from L multiplied by an assumed fixed width. In both cases, L is the surface rupture length. Since the hazard model is concerned with seismic shaking that is generated at depth, the surface rupture is not the most suitable observation to relate with. Rather, a relationship between Mw and rupture dimension at depth should be used. The relationships by Leonard (2014) do provide such parameters and are based on a much larger and more updated dataset than Stirling et al. (2002). In addition, Leonard’s (2014) would allow for differentiating between strike-slip and dip-slip faults. In all cases, the statement that the maximum magnitude is estimated from the fault geometry is too general. More detailed explanation is needed to let the reader understand and possibly replicate was has been done here.

It is a subjective opinion of the reviewer that it is preferable to use the relationships of Leonard (2014) to Stirling et al's (2002). The latter has been, together with that of Well and Coppersmith (1996), one of the most used for the purpose in question.

P8L5. Here it is stated that the GMPEs by Campbell (2013) are used. I suspect it is the Campbell and Bozorgnia (2014) in the references. These are GMPE developed for shallow crustal earthquakes in the western US in the moment magnitude range of 5.0 to 8.5. How well do they apply to earthquakes in the range starting at magnitude 4.0 in southern Spain? What criteria have been used to select this GMPE? The paper by Delavaud et al. (2012) delineates a robust procedure to select the appropriate GMPE to be used in Europe, why was this paper ignored? In addition, the GMPEs are different depending on the sense of movement. The QAFI database provides indication of the sense of movement of faults, but how was the sense of movement determined for the zones?

The Campbell and Bozorgnia model (2014) uses 15,521 records from 322 earthquakes of $3.0 \leq M \leq 7.9$. The total selected database comprises 11,125 records from 245 earthquakes of $3.0 \leq M < 5.5$.

The work of Delavaud et al. (2012) is prior to the model of Campbell and Bozorgnia (2014), so this model can not be assessed in that article. In addition, the GMPEs proposed (in the first places) Delavaud et al. (2012) do not consider the source effect with as much detail as the model of Campbell and Bozorgnia (2014), in this application it does not make sense to define the sources precisely if simpler models are later employed in the GMPEs.

Nevertheless, the focus of the paper is the definition of the source model and the distribution of potential between zone and faults. The choice of the GMPE is a secondary issue in this regard.

P9L27-28. What is said here does not prevent double counting at all. To prevent double counting every earthquake that is assigned to its causative fault should be removed from the rate estimate of the zone and vice versa to ensure it is counted only once.

The problem, precisely in a hybrid model of zones and faults, is to identify which earthquakes are associated to the zone and which to the fault. The events of the seismic catalog are not classified between zones and faults, but that a recurrence is established for the faults from the slip rate and another for the residual zone from the Catalogue. But this in turn will contain earthquakes that will have occurred in the fault, and if they are not easily identified they will be counted twice: one explicit in the area and another implicit in the fault. Most of the hybrid models, including that of adopting a solution to identify the events in the two types of sources: establish a Cut Magnitude Mc and consider Mw <Mc for the zone and Mw> Mc for the fault. But as we have already indicated, our approach tries to avoid this simplification and proposes a procedure for sharing, avoiding duplication. This question is key.

The results of the various calculations are very poorly presented. The fault parameter estimates are not provided at all, and other results are provided only in aggregated form. It is thus very hard to judge the validity of the hazard results if the results of the intermediate calculations are missing.

We include an annex containing a table with the fault parameters included in the study. These data are taken from the QAFI database. Including in this table all the intermediate results for each fault would be too lengthy.

In general, I found the English grammar and the organization of the manuscript to be rather poor. For example, there are sentences in the results that belong to the discussion. I've already commented on the lack of referencing. The symbols used in the equations are never explained. There is often confusion between symbols used for seismic moment and earthquake magnitude (M, M0, m, which is which?). The same also for the b-value (b or beta?). The units in the vertical axis of diagrams in figures 3 and 4 are unclear. Overall, the figure captions do not help much to understand what the figures show.

English has been corrected by a native person in this version.

My conclusion is that this manuscript is not fit for publication. I also cannot see how this manuscript can evolve quickly to an acceptable standard for the readers of NHESS and thus recommend it be rejected.

I don't give more technical suggestions here on how to improve the manuscript because they won't be useful until the major flaws are fixed. References Cornell CA (1968) Engineering seismic risk analysis.

Bull Seismol Soc Am 58:1583– 1606. Delavaud E., Cotton F., Akkar S., Scherbaum F., Danciu L., Beauval C., Drouet S., Douglas J., Basili R., Sandikkaya M., Segou M., Faccioli E., Theodoulidis N. (2012). Toward a ground-motion logic tree for probabilistic seismic hazard assessment in Europe. Journal of Seismology, 16(3), 451-473, doi: 10.1007/s10950-012-9281-z. Kanamori, H. & Brodsky, E.E., 2001. The physics of earthquakes, Phys. Today, 54(6), 34–40. Leonard, M., 2014. Self-Consistent Earthquake Fault-Scaling Relations: Update and Extension to Stable Continental Strike-Slip Faults. Bulletin of the Seismological Society of America, doi: 10.1785/0120140087. Woessner J., Danciu L., Giardini D., Crowley H., Cotton F., Grünthal G., Valensise G., Arvidsson R., Basili R., Demircioglu M., Hiemer S., Meletti C., Musson R.W., Rovida A., Sesetyan K., Stucchi M., and the SHARE consortium. (2015). The 2013 European Seismic Hazard Model - Key Components and Results. Bulletin of Earthquake Engineering, 13, 3553-3596, doi: 10.1007/s10518-015-9795-1.

## Referee #5

The Manuscript by Rivas-Medina et al. "Approach for combining faults and area sources in seismic hazard assessment: Application in southeastern Spain" addresses an important methodological question of how to incorporate individual faults and fault systems into the PSHA. This problem is even more actual for the PTHA (tsunami) studies due to the even larger impact of different faulting styles stimulating researchers to treat as much individual faults as possible. In particular, Authors suggest not using some constant accepted magnitude for distributing seismic potential between faults and background seismicity but employing more flexible criterion for the threshold magnitude based on completeness analysis.

As present Manuscript pretends to become an important methodological paper, I think it has to be significantly improved to meet the quality expected for such kind of paper, and propose major revision.

General comments:

References: The manuscript suffers from clear lack of proper referencing. It is, actually, unique in this sense. Especially, introduction lacks at least minimal comprehensive historical review with proper citations. One single statement at 2.22 (without citations) is definitely not enough.

Figures: Please provide proper explanatory captions. Figures should be selfexplaining. In current state too much concise. Also give explanations to each abbreviation.

Language: please check with native speaker. Some sentences are hard to understand (e.g., the two last sentences of the abstract). I suggest to decouple Discussion and Conclusions for more clarity.

Few concrete suggestions and comments below:

2.12 – "source" vs "source zone" possible misunderstanding

2.6 – unbiased estimates 3.11 - CP(m) or PC(m)? 3.12 – higher? 3.16 – rephrase

From the three equations 8-9-10 it is not clear how to update the b-values for individual faults, please explain in a better way.

In Table 3 caption and also through the manuscript: b-values mixed with beta-values.

Thank you for your time and remarks, with help improving the manuscript significantly.

More than 33 references have been included in the manuscript and figure captions are completed

The English is reviewed,

The discussion section is separated from the conclusions.

A. Grezio

Thank you for your time and the remarks on the paper. We acknowledge that your comments significantly improve the original manuscript. Below we provide response to your points with more detail.

GENERAL COMMENTS:

The paper "Approach for combining faults and area sources in seismic hazard assessment: Application in southeastern Spain, by Alicia RivasMedina et al." can add interesting discussions points on the seismic hazard issues because of the hybrid source model. However, major changes are required to improve the paper. The background knowledge of the application area (Southeastern Spain) is poorly described and the choice of Mmaxc should be discussed more deeply.

SPECIFIC COMMENTS:

Abstract - Lines 9-11 page 1: Instead of " . . .model composed by faults as independent entities and zones (containing the residual seismicity). The seismic potential of both types of sources is derived from different data: for the zones, the recurrence model is estimated from the seismic catalog. For fault sources, it is inferred from kinematic parameters derived from paleoseismicty and GNSS measurements" a suggested re-writing could be (in the list put first fault and then zones): ". . . model composed by faults as independent entities and zones (containing the residual seismicity). The seismic potential of both types of sources is derived from different data: for the fault sources it is inferred from kinematic parameters derived from paleoseismicty and GNSS measurements, and for the zones the recurrence model is estimated from the seismic catalog".

The text has been modified

Lines 15-17 pag 1: The concept of the Max Magnitude in the abstract (stated by the following words "This is derived from a completeness analysis and can be lower than the Mmax generated by the faults, taking into account that their the recurrence period can be higher than the observation period of the catalog") starts a discussion but it is not fully developed, it is just mentioned here and needs to be better explained in the "Discussion and Conclusions" section or when the results are presented. This part in the abstract should be removed and/or re-written.

This is removed from the abstract and the text changed

Line 19 page 1: It is required an explanation of ". . .a seismic hazard model using the traditional zone". Is there any reference of this model? What do the authors mean by the word "traditional"? This part should be re-written.

1 Introduction - There are NO REFERENCES in the Introduction. This is an anomalous way of presenting a scientific paper in a peer review journal because the paper gives a narrow view of the subject. Please add appropriated references in the text. The Introduction looks more a discussion to motivate the paper than a wide presentation of the seismic hazard problem in the application region. Also, there is no mention of the previous seismic studies carried out in Southeastern Spain. Line 27 page 1: References are required near the words ". . . in the last years, as more studies".

References have been included

Line 28 page 1: Please write the acronimo as "GNSS (Global Navigation Satellite System)".

It has been added to the text

Line 2 page 2: Please add reference after " In most practical cases".

References have been included

Line 3 page 2: Please add reference after “Other approaches”.

References have been included

Line 22 page 2: Please add reference after “ Some authors ”.

References have been included

Lines 28-32 page 1: The sentences “The approach presented in this paper, as all probabilistic seismic hazard models, face the challenging question of estimating the expected ground motions with the basis of a short period of observations of earthquake occurrences and limited geological data (with significant uncertainty) to construct recurrence models. The purpose of this work is not to solve this challenge, but rather, to propose a model that contains different types of seismic sources (faults and zones) and distributes the seismic potential appropriately, avoiding double-counting and considering periods of completeness.” present the purpose of this study in a superficial way. Those lines should be re-written. If you want to start a probabilistic hazard assessment, firstly you consider the potential Max magnitudes generated by the faults, and then you associate a low probability of earthquake occurrence with them on the basis of your study and considering the relative uncertainties. Certainly you don’t exclude those max magnitudes just because of the completeness of the catalogue. A probabilistic hazard assessment should overcome those limitations. Again the choice of the Mmaxc should be properly explained, doubts on that should be solved and motivated by the authors in the paper. This part should be re-considered for the discussion in the last section.

We agree on that hose limitations are inherent to the probabilistic method. With this paragraph, we just wanted to clarify that we do not try to solve them, but only to take them into account in the proposed approach.

The maximum magnitudes associated to each fault are calculated, and their occurrence rates are estimated using a GR recurrence model. This model, considering Mmax of faults, is included in seismic hazard calculations. The MmaxC value is only considered for the distribution of seismic potential, but not for the input recurrence model incorporated in the hazard model.

2 Source hybrid approach (zones & faults) for hazard estimation - This section is presented in a schematic way, but should be completed with the description of the "Classical Method" which is also used in comparison with the Hybrid Model.

We consider that it is better to focus the paper in the specific features of the proposed approach. The manuscript is structured accordingly focusing on the description of the source hybrid approach. However, we include the reference to the classical method included in this study: IGN-UPM Working Group (2013).

Lines 10-16 page 3 : The definitions should be clearer, in particular CP(m) and PC(m) need a longer explanation.

The text has been modified

Line 20 page 4: Please write GR after Gutenberg-Richter and may be write a reference for that (Gutenberg and Richter 1944 or 1954).

The text is modified

Line 22 page 4: In Eq 4 it should be written what “d” and “beta” are.

The text is modified

Line 18 page 5: The authors state “ Considering that the fault may generate events with magnitudes larger than MMaxC , the corresponding distribution of seismic potential in the interval (MMaxC , MMax ] is calculated by extrapolation of the recurrence model with the last b-value adjusted”. This concept should be further discussed to overcome the limitations raised by the choice of the Mmaxc.

This issue is included in the Discussion

3 Application of the approach in southeast Spain - It is not clear how the results in the application region were computed with the Classical Method.

Line 14 pag 7: Please explain what QAFI database is and/or write references for that.

A reference to QAFI is included, as well as an annex with fault information

Line 9 page 8: The "Classical Method" should be better explained, it is just mentioned for the Fig 6.

Details on how seismic sources are used in the CM can be found in IGN-UPM Working Group (2013) (reference included in the paper) We have not provided further details on this paper because it is not the objective of this paper.

4 Discussion and conclusions - The discussion of the results and the conclusions should be include the point raised previously.

Line 6 page10: Please add references for 2009 L'Aquila and 2011 Lorca events.

References are included

References - They are a poorly and unsatisfactory list of the other paper on this subject. The References are simply the ones used to carried out the computation.

New references are included

FIGURES - Figure 2: more explanations are needed in the caption, in particular about "AR".

The figure is explained with more detail

TECHNICAL CORRECTIONS:

Line 17 page 1: delete "the" before "recurrence".

The text is modified

Line 10 page: difficult to read. Instead of the sentence "using the records which origin time and magnitude are contained in the period for which the catalog can be consid ered complete" a suggested change could be: "using the records with origin time and magnitude contained in the period for which the catalog can be considered complete".

The text is modified

Line 27 page 8: This part on region 30 should be immediately after "return periods", to complete the part on hazard map.

The text is modified

Line 31 page 8: the sentence with "The hazard curves" should start a new paragraph, on the hazard curves.

The text is changed