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 ≤ M ≤ 7.9. The total selected database comprises 11,125 records from 245 earthquakes of 3.0 ≤ 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.

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