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Author comments on "A sanity check for earthquake recurrence models used in PSHA of slow deforming regions: the case of SW Iberia" by Margarida Ramalho et al.

Title: A sanity check for earthquake recurrence models used in PSHA of slow deforming regions: the case of SW Iberia Author(s): Margarida Ramalho, Luis Matias, Marta Neres, Michele M. C. Carafa, Alexandra Carvalho, and Paula Teves-Costa

Interactive comment by Robert J. Geller (Referee)

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RC2 - 1. This paper is headed in the right direction, but only makes a tiny amount of progress. The key issue is stated in very hesitant terms in lines 43-45, namely: "One of the evolutions suffered by PSHA studies and now recognized as essential, is the evaluation of the uncertainties on the results (e.g. Frankel, 2004, Stein et al., 2012, Mulargia et al., 2017), being a consequence of our incomplete knowledge of the earthquake generation and propagation mechanisms." The question though is whether this uncertainty can be dealt with within the PSHA framework (this seems to be the authors' position) or whether the entire PSHA framework is fundamentally flawed and should be abandoned (this is the position of Mulargia et al., 2017, with which, as a co-author, I agree with). If one agrees there is, at present, no scientific basis for selecting the earthquakes to be fed into PHSA as the input, then the output of PSHA will just be a bunch of numbers, with no physical validity. I'd like to see the authors confront and discuss this issue head-on in their revised paper.

R: We agree that the PSHA has its limitations, but it is still used mainly for several seismic hazard maps. For this reason, given the societal relevance of PSHA, in our work we propose a sanity check to score the existing models in a slow-deforming region.

Following this thought, we decided to better express our opinion adding this sentence to the paper: line 43 "As written before, despite PSHA limitations and flaws, it is the used method by society and scientific community, and here is this paper contribution to the topic.".

RC2 – 2. Using the published hazard maps to conduct a "sanity check" is one way to show the existence of problems and inconsistencies, and I support the eventual publication of this work, but it seems to me that this was a fully expectable result, given that (as I understand it) the hazard maps were constructed by assuming that future seismic activity would be the same as past seismic activity.

R: For the specific case of our study area, we focused on different available models relying on the classical PSHA approach of source zonation because there isn't any published model (using alternative techniques to PSHA) to be scored with the neotectonic one. However, in this paper, we don't discuss PSHA models themselves, but we concentrate on the earthquake generation models used to build them. PSHA map differences may also result from the choice of the Ground Motion Equations that are not discussed at all. In fact, given a set of different PSHA models, it is not easy to assess the causes for such differences, a relevant question not addressed in the paper.

In Slow Deforming Regions (as it is our study area), assuming time ergodicity is the usual approach and indeed it is used in the investigated earthquake generation models. At this stage of our research, we are still not entirely convinced that time ergodicity directly affects differences in hazard maps, as assumed by the referee (if we understood this point correctly).

We think that the novelty of our approach rests in determining the earthquake rates from a neotectonics model, which is largely independent of the past-seismicity spatial distribution and mainly relies on long-term slip rates off-fault deformation. This method is more coherent with the expected long-term tectonic evolution of the study region. More, it doesn't assume that the

future seismicity follows the spatial pattern of the available seismic catalog. In any case, we think that the scope of the paper is well expressed in the introduction, section 5.3 and conclusions.

To better clarify this issue in the paper: in figure 2 "*These maps show the inconsistency of PSHA models, as two options for the chosen models*.".

RC2 - 3. Seth Stein wrote a book about this problem "Disaster Deferred: A New View of Earthquake Hazards in the New Madrid Seismic Zone" (2010, Columbia University Press). You can find references to some of his papers on New Madrid in that book. I think the Frankel (2004) paper you cite (lines 762-763) is basically a reply to arguments by Stein. It might be useful for you to discuss that controversy in more detail in your paper, as the basic issue seems to be similar to the one involved in hazard maps for Portugal.

It will take a long time for the hazard estimation community to figure out how best to deal with problems of the type discussed in this paper. I hope the authors' revision is done in a way that they address the underlying issues in more depth, and more profoundly.

R: We quote the work by Frankel (2004), which was published following a controversy with Seth Stein and others on the New Madrid earthquake hazard. However, our paper is by no means a contribution that that controversy. However, since the New Madrid earthquake zone may be qualified as a Slow Deforming Region, one sentence addressing the debate was included in the discussion section.

"*Taking into consideration the space geodetic information, the New Madrid seismic zone also classifies as a Slow Deforming Region (e.g. Newman et al., 1999). Inferred tectonic strain imply that the 1811, 1812, if M=8, have very low probability of occurrence (ibid.). The interpretation of this earthquake sequence generated a long-standing debate (e.g. Frankel, 2004, Stein, 2005, Frankel, 2005). The New Madrid events show that time ergodicity, a common assumption for earthquake generation in Slow Deforming Regions, can fail dramatically*."

Frankel, A. (2005). Reply to "Comment onHow Can Seismic Hazard in the New Madrid Seismic Zone Be Similar to That in California?'by Arthur Frankel". Seismological Research Letters, 76(3), 366-367.

Stein, S. (2005). Comment on "How can seismic hazard in the New Madrid Seismic Zone be similar to that in California?" by Arthur Frankel. Seismological Research Letters, 76(3), 364-365.