Author comments / Letter to the editor and reply to Referee #2 comments (Robert J. Geller) Submitted on 06 Aug 2021

Title: A sanity check for earthquake recurrence models used in PSHA of slow deforming regions: the case of SW Iberia
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Interactive comment by Robert J. Geller (Referee)

General comment: PSHA or no PSHA: our position

In the revised document we made all efforts to clarify our position in relation to Robert Geller (RG) comments, namely that this paper is not a "sanity check" on PSHA but on one of its components, the earthquake recurrence models. In fact, our work can be considered in line with PSHA, and it is one suggestion for its improvement in slow deforming regions, despite the criticism that the PSHA has been subjected to. If by this assumed position in favor of an improved PSHA the paper is rejected, then we do not expect any more papers on PSHA to be published from now on and we will move to another subject. Our understanding is that the discussion is still open, and our paper could be considered a contribution to it, from the PSHA side.

Despite the author's position, we tried to acknowledge the major criticism to PSHA earthquake recurrence models and to point out its limitations. We follow some of the references that propose to abolish PSHA in endorsing the need that a wide range of users concerned with the need to build safety are involved in the decision making where the uncertainties and assumptions of the models used are clearly stated.

Winston Churchill once said that: "democracy is the worst form of government – except for all the others that have been tried."

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1a. The title of the paper is "A sanity check for earthquake recurrence models…." When we talk about a sanity check for a human being, we're talking about finding out whether or not the person is insane. We don't normally order a "sanity check" on a human unless we strongly suspect the person is insane. When we make a "sanity check" of PSHA this is because we strongly suspect PSHA is fundamentally flawed, i.e., that it is an incorrect theory that therefore gives incorrect results.

R: This paper is not a "sanity check" on PSHA and it will not provide a definite opinion on it. In the paper the term "sanity check" is used with the same meaning that was used, for example, for probabilistic tsunami hazard assessment in Basili et al. (2021). It means that the model predictions are tested against observations and/or independent parameter estimates, like we do in the paper. One sentence in the introduction was added clarifying this issue. The discussion on the flawness of PSHA is deferred to the new Discussion section.

1b. Two papers (Stein et al., 2012; Mulargia et al., 2017) are cited here, but not correctly. Lines 43-45 say "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. This is a misrepresentation, for the 2017 paper in particular. The first paper (Stein et al., 2012) points out that, for whatever reason, many damaging earthquakes occurred in regions designated as being at relatively low risk by PSHA-based hazard maps. We did not say that this discrepancy is due to "our incomplete knowledge of the earthquake generation and propagation mechanisms." We just note that the discrepancy exists.

R: We acknowledge the incorrect interpretation made in the text and the introduction section was corrected.

2. The second misrepresentation is more serious. Mulargia et al. (2017) do not say the problems of PSHA are because of "our incomplete knowledge of the earthquake generation and propagation mechanisms." Rather, we say that the fundamental assumption of PSHA (that past seismicity rates can be conflated with future probabilities) is wrong, and that the use of PSHA should therefore be abandoned. The "sanity check" made in the present paper should therefore be framed as one trial to see if the position of Mulargia et al. (2017) is correct or not.

R: As mentioned above, the proposed paper cannot be considered as a trial for the position of Mulargia et al. (2017). First of all we tried to make clear in the paper that PSHA has two major sources for uncertainty (and possible causes for it to be

"wrong"), one related to the earthquake recurrence models, in space and time, and the other related to the Ground Motion Prediction Equations that translate the earthquake occurrence to ground motion. Our paper discusses only the first of these two that depends above all on the physics of earthquakes. While criticizing the lack of physical constrains in the models used for earthquake generation in slow deforming regions, we suggest procedures to improve them considering its relationship to plate kinematics, in line with the moment release balance made by Bird and Kagan (2004). This subject is discussed on the new Discussion section.

3. The authors (lines 24-43) discuss the widespread use of PSHA by society. This discussion should also mention that the widespread use of PSHA is an example of a kind of "group-think," in as much as PSHA is being used by almost everyone without having been validated by anyone.

R: We express our opinion on the difficulties that occur in the validation of PSHA studies in the new Discussion section.

4. Conclusions section: What is the conclusion: sane or insane? This should be clearly stated.

R: As mentioned above this paper is not a "sanity check" on PSHA and it will not provide a definite opinion on it. This position should be clearer by the changes made in the Introduction and the new Discussion sections.

5a. Discussion section: A brief "discussion" section should be added.

R: Done. Maybe not so brief as suggested by R. J. Geller but, required to address the main points questioned.

5b. In particular, I recommend that Klemeš (1989) (as well as, perhaps, Klemeš, 1986 as well) should be cited and discussed. His work discusses hydrology, but the problem in our field is the same. We have the PSHA machinery into which we put numbers, and like magic, other numbers are output. But this is just a mathematical pastime and the numbers that come out are just numbers, nothing more, unless the model used by PSHA has been validated. In fact, not only has it not been validated, but, as pointed out by Mulargia et al. (2017) it appears to be wrong. So what should everyone involved in risk and hazard estimation be doing instead of blind (or conditional) reliance on PSHA? Please discuss this.

R: We found the Klemeš, 1986 reference worth to be mentioned in the new Discussion section. However, our interpretation of its arguments differs from R. J. Geller ones.

As a final note, not included in the paper, we may note that Mulargia et al. (2017) provides some hints on the procedures to replace PSHA. It uses terms as "earthquake resistance" and "sufficient strength" adding to "including beyond current-code earthquakes". However, there is no indication whatsoever on the methods to define the earthquakes used to evaluate the "resistance" or "strength" of the buildings, nor the ways to choose the earthquakes "beyond the codes". Our belief, expressed in the discussion, is that even if PSHA will not be the single tool to make the required for land use management and for the establishment of building codes, it will still remain has one of the tools to be used and for this reason, any effort to improve it should be considered. Incorporating physics on a blind statistical model was our task in this paper.