Natural Hazards
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Sciences
Discussions

Interactive comment on "Spatial Seismic Hazard Variation and Adaptive Sampling of Portfolio Location Uncertainty in Probabilistic Seismic Risk Analysis" *by* Christoph Scheingraber and Martin Käser

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Review of "Spatial Seismic Hazard Variation and Adaptive Sampling of Portfolio Location Uncertainty in Probabilistic Seismic Risk Analysis" by Christoph Scheingraber and Martin Käser Referee: Robert J. Geller (anonymity waived)

This manuscript (ms) presents a method for stochastic treatment of portfolio location uncertainty in Probabilistic Seismic Risk Analysis (PSRA). PSRA rests on Probabilistic Seismic Hazard Analysis (PSHA). So the evaluation of this ms is a two-step process.

First, the validity of PSHA must be verified. If this can be done, then the merits of this particular ms must be evaluated.

It might seem odd that a referee is saying in 2019 that the validity of PSHA must be verified, as it is a widely used method and probably a majority of scientists and risk assessors support it. But the mere existence of a consensus means nothing in science. Objective and quantitative testing must be conducted to show validity. However, since the first paper by Cornell (1968), PSHA has become a ubiquitously used method without ever having been objectively vetted and verified.

Recently Mulargia et al. (2017) suggested that the use of PSHA should be abandoned. They pointed out that the basic assumption of PSHA, namely that the frequency of occurrence of past earthquakes can be conflated with the probability of future earthquakes, has never been validated and is probably wrong (also see Stein et al., 2012; Kagan et al., 2012).

Notwithstanding the fact that PSHA has been shown to be fundamentally flawed, many workers, including the authors of this ms, continue to use PSHA. However, this is in no way a justification for accepting such mss for publication. Science is based on data, not on consensus. I therefore recommend that all such mss, including the present ms by Scheingraber and Käser, should be rejected, unless and until they can refute the criticisms of Mulargia et al (2017).

The above recommendation may seem harsh to these authors, as innumerable papers on PSHA or based on PSHA continue to be routinely published. But unless at some point papers on PSHA start to be rejected, they will go on being published forever.

Specific comments on this paper:

1) This referee's impression is that PSHA gives ever more unreliable results as the grid is made finer and finer. Notwithstanding the inherent flaws in PSHA, if the regions were made larger and larger the central limit theorem would probably mean the results were

more safely usable. Maybe this is like the tradeoff between stability and resolution in inverse theory.

2) The quality of the writing is generally good, but there is one typo on page 25. The family name of the first author is "Petersen" not "Mark Petersen." Ditto for the other coauthors. And in the body of the text this paper needs to be called out as "Petersen et al.," not "Mark Petersen et al."

3) Both authors list "Munich Re" as an affiliation on the title page, and the acknowledgements list "Munich Re" as a sponsor. Thus (see line 20 on page 22) the declaration of "no competing interests" is incorrect and must be rewritten. Munich Re sells consulting services and insurance products based on PSRA, and this fact must be appropriately stated in the "competing interests" declaration. The biomedical field is far ahead of the physical sciences in this regard. See the following links. I suggest the authors consult an expert in research ethics to make sure their declaration is in conformity with currents standards. http://www.cell.com/pb/assets/raw/shared/forms/di_form.pdf https://www.nature.com/authors/editorial_policies/competing.html

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