Comment on "How Well Does Poissonian Probabilistic Seismic Hazard Assessment (PSHA) Approximate the Simulated Hazard of Epidemic-Type Earthquake Sequences?" by Shaoqing Wang, Maximilian J. Werner, and Ruifang Yu

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KEY POINTS

- Comment on the scientific aspect of a recent paper.
- The link between results and conclusions need better specification.
- The readers of the commented paper will better understand the study and its implications.

The commented paper reports about a study, aimed at comparing epidemic-type aftershock sequence (ETAS) to different hazard models. The study is interesting; nevertheless, there are some issues that the commenters feel compelled to discuss, to avoid generating incorrect understanding in less experienced readers. The comments herein, that are intentionally limited to the comparison of probabilistic seismic hazard assessment (PSHA) and sequence-based PSHA (SPSHA) with ETAS, focus on (1) the link between abstract and conclusions of the paper and the results of the study; (2) the approach taken for the calibration of the compared models and the possible impact on the results; (3) the description and discussion of PSHA and SPSHA given in the paper; and (4) the actual goals of hazard analysis and the relevance of modeling multiple events for current seismic design.

THE LINK BETWEEN THE RESULTS AND THE CONCLUSIONS (ETAS TRUTH OR BENCHMARK?)

Some sentences reported in the abstract and in the conclusions section may generate incorrect understanding of the results presented in the paper. To clarify this issue, on one hand, it is first necessary to quote Wang *et al.* (2021), where the introduction (page 510) reads: "An important goal […] is to continually assess the validity of model approximations to observed hazard." However, this cannot be a goal of the commented study, because it is only aimed at assessing how well methods such as PSHA (Cornell, 1968) and SPSHA (Iervolino *et al.*, 2014) can recreate the ETAS-based hazard. This is evident from the body of the text, where it is made very clear that all data used for the comparisons are generated via ETAS (Ogata, 1988). On the other hand, the final statement of the abstract and the conclusions (pages 508 and 524) is: "We conclude that realistic multigenerational earthquake clustering [ETAS] has both obvious and more subtle effects on long- and short-term hazards and should be considered in refined hazard assessments."

Given that all data are generated via ETAS, its superiority with respect to the other compared models is, in fact, an assumption of the study rather than a conclusion from the results. Consequently, although there is consensus on ETAS being an advanced model for the generation of synthetic catalogs and the reconciliation of short- and long-term hazard, terms such as "realistic [ETAS]," "approximate [other] method," and so on, could have been used more prudently. The ETAS model, as any other model, does not exist in nature and it may be questioned whether the found bias (see next section for comments on this term) in reproducing the ETAS results, holds in nature as well. The capability of a model in describing actual seismicity can only be proven via real earthquake data.

THE SIGNIFICANCE OF THE RESULTS AND THE APPROACH TAKEN FOR THE CALIBRATION OF THE COMPARED MODELS

Some conclusions (e.g., page 508) of the study read: "(1) at (low) design-oriented probability (i.e., 10% or 2% in 50 yr), the approximate PSHA methods give peak ground acceleration (PGA) estimates to within ±7% of the ETAS hazard; (2) at high probability level (>~45% in 50 yr), the methods interestingly overestimate the ETAS hazard, a subtle consequence of the

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poor Poisson approximation to the skewed ETAS distributions […].” The criterion according to which these differences are considered poor is not given, whereas according to a classical engineering rule of thumb differences around 10% are negligible. Moreover, the reader of the paper should be warned at least of three possible issues about these figures.

First, the results are obtained comparing the hazard from a completely specified ETAS model with those by competing models the parameters of which are estimated from the ETAS-generated data. Therefore, at least part of these differences may be attributable to an estimation uncertainty issue that can arise in the parameters of the compared models (van Stiphout *et al.*, 2011). In other words, differences like those observed could also be obtained by adopting as a competing model an ETAS model whose parameters are estimated from the ETAS-simulated data, only because the estimated ETAS will not be exactly equal to those of the model used to generate the data.

Second, the results are also inherently dependent on the method used to calibrate the compared models; that is, declustering according to Reasenberg (1985). To warrant that this approach is fit for purposes in the context of this study, it should be verified that the data remaining after delustering are compatible with the realization of a homogeneous Poisson process, which is the goal of such a procedure (see also next section). It is not clear from the paper whether the necessary check was performed, whereas it is not granted a priori that this declustering method works satisfactorily when applied to ETAS data (except for the background term); see Luen and Stark (2012).

Third, it could be also argued that the method to estimate the parameters of the Omori–Utsu law of SPSHA, could possibly lead to underestimating the long-term seismicity. In the study, these parameters are estimated from the first one hundred days of the fifty largest clusters in the ETAS catalog, claiming that this approach is at least as good as (Iervolino *et al.*, 2014), which considers 90 days as the duration of the seismic sequence. In fact, the longer sequences are likely generated by the largest magnitudes, whereas modified Omori law has, for mathematical convenience, magnitude-independent parameters. For this very same reason, the length of the sequence to be considered in SPSHA should be regarded as a parameter of the model requiring calibration based on the actual set of data used to perform the estimation. In other studies, it was verified that varying the duration of the sequence does not affect the result appreciably, but this does not hold true in any region (i.e., Iervolino *et al.*, 2018; Chioccarelli *et al.*, 2021) and may be even more inappropriate for the largest sequences generated via ETAS, unless otherwise proven.

DESCRIPTION OF PSHA AND SPSHA (ASSUMPTIONS OR MODELING CHOICES?)

In the paper, it is stated (page 511) that SPSHA (like PSHA) is developed under “several simplifying assumptions,” the first of which being that “mainshock or clusters follow a stationary Poisson process.” It must be recalled that the Poisson process is a modeling choice, representing the main (intentional) advantage of both PSHA and SPSHA. It enables to retain the robustness of calibration and mathematical ease implied by this stochastic process. Moreover, in SPSHA, it avoids the issues of catalog incompleteness with respect to aftershocks, which is even more relevant than for mainshocks. To make this modeling choice compatible with real earthquakes, in PSHA and SPSHA, the foreshock and aftershock removal, carried out via declustering, let the remaining data resemble the realization of a Poisson process, and this is a formally rigorous procedure (Gardner and Knopoff, 1974). (Incidentally, it is also important to warn the reader that ETAS has a background term that is analogously modelled via a homogeneous Poisson process.) The second listed assumption of SPSHA is that aftershocks are “smaller (in magnitude) than the mainshock.” In fact, this is a direct consequence of the declustering method, which assumes that the mainshock is the largest magnitude event in the sequence; consequently, the aftershocks could not be otherwise modeled. The third alleged simplifying assumption is that aftershocks do not trigger their own aftershocks. The commenters believe that this issue should be put in an aftershock classification perspective, rather than a modeling assumption. The modified Omori law works in such a way that all aftershocks are classified as contingent to the mainshock identifying the sequence. In other words, Omori’s law does not neglect any aftershock, it only classifies them as branching from the mainshock only.

The paper also states that SPSHA cannot be used to estimate multiple exceedances, which is factually incorrect. SPSHA features a two-level (hierarchical) modeling: one between sequences (i.e., the homogeneous Poisson process for the occurrence of mainshocks and sequences) and the modeling within each sequence (conditional nonhomogeneous Poisson process, with Omori-type mean function). This fully allows to generate synthetic catalogs including aftershocks: (1) the occurrence of mainshocks is simulated by means of a homogeneous Poisson process, (2) conditional to the magnitude and location of each mainshock, the occurrence of aftershocks can be via a nonhomogeneous Poisson process with Omori–Utsu mean. In fact, even the authors note in the paper that SPSHA can be seen as a first-generation ETAS, and as such it can generate synthetic data in the same manner.

All of that said, the only actual simplifying assumption is that the effect of potential foreshocks is neglected, and although strategies to include them in SPSHA are available, foreshocks are often considered of secondary importance by earthquake engineering (Yeo and Cornell, 2009).

THE ACTUAL GOALS OF HAZARD ANALYSIS FOR CURRENT (STATE-OF-THE-ART) SEISMIC DESIGN

In the paper, it is stated (page 522) that “none of the approximated methods reproduce the multiple exceedance curves implied by the ETAS model […].” The readers of the commented paper should be first made aware that the current implementation in seismic codes worldwide (e.g., CEN, 2004) of a simplification of performance-based seismic design (Cornell and Krawinkler, 2000), does not explicitly account for damage accumulation. The structural checks are made with respect to one seismic event with a given return period only, for a number of structural engineering reasons that cannot be discussed here at length. This is why PSHA and SPSHA are primarily interested in the probability of observing at least one exceedance in a given time interval, although, as discussed earlier, SPSHA allows to model multiple exceedances.

In the same context (page 508), the commented paper also claims that "[…] cumulative seismic risk assessment with damage-dependent fragility curves should account for ETAS-like sequences." Regarding this statement, the commenters believe that more caution is warranted here. Establishing superiority of one hazard model with respect to alternatives, in accounting for damage accumulation, cannot be established based on hazard comparisons alone. The metric should be the structural failure (or better the loss) probability in a time interval of interest, that is considering the effect of structural fragility (and possibly consequences); see also Marzocchi *et al.* (2015) on related issues. In fact, it could not be ruled out that the differences brought by hazard models are further reduced when considering the seismic vulnerability of engineering systems.

Finally, the paper claims (page 510) that "Using the Omori–Utsu formula or the ETAS model, studies have conducted APSHA in various regions […]. However, these studies did not attempt to connect the short-term hazard with the long-term average hazard." Actually, examples of such evaluations are given in Iervolino *et al.* (2016, 2020). The second paper just recalled (interestingly cited in the commented paper) precisely attempts to connect the short-term (Omori type) and long-term hazard.

FINAL REMARKS

A number of clarifications should be given to the community interested in seismic hazard analysis to appropriately interpret the results of Wang *et al.* (2021) and draw conclusions on the compared models.

1. The study assumes ETAS as a benchmark, simulating the data for the comparison with this model, therefore: (a) the results do not enable speculations on the superiority of ETAS; (b) the differences found between the other models and ETAS seem of limited relevance, if any, especially those of primary engineering interest; that is, the probability of at least one exceedance of a ground-motion intensity measure. Based on this, one can argue that models simpler to calibrate and run are preferable, as opposite to the conclusions of the commented paper.
2. Calibration for comparability requires care if the (synthetic) data are generated with one of the models. The way declustering was applied to ETAS-generated data could have required further analysis to prove its soundness in the context of the study. Along the same lines, it may also be argued that the observed differences between ETAS and other models can be, at least partly, attributable to the adopted calibration method and/or (simply) to the fact that the parameters of the ETAS model are fixed, whereas those of the other models are estimated.
3. The study lists a number of alleged assumptions of PSHA (SPSHA), which are in fact precise modeling choices, which are followed by consistent approaches to treat earthquake data (i.e., declustering). The only assumption of the current formulation of SPSHA is to neglect the foreshock contribution, whereas—with respect to aftershocks—SPSHA can be seen as a first-generation ETAS.
4. Current performance-based seismic design does not contemplate structural failure due to multiple partially damaging events; thus, PSHA and SPSHA only target the probability of at least one exceedance. In any case, the comparison in case of damage accumulation should not stop at hazard, but consider the structural failure or loss probability, and these metrics could further align the different hazard models considered.

Notwithstanding these issues, which are mostly related to the communication of the study and its results, something relevant in the context of research papers on a journal such as *BSSA*, the commenters believe that the discussed paper is an interesting piece of research on the important topic of seismic hazard and risk assessment.

DATA AND RESOURCES

All data and resources used in this study came from published sources listed in the references.

DECLARATION OF COMPETING INTERESTS

The authors acknowledge that there are no conflicts of interest recorded.

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