

I am pleased to see the improvements made to this manuscript upon revision. The current version thoroughly details the seismic hazard approach developed and discusses the credibility of the method. However, based on their description, I am unclear why the authors have proposed a new smoothing method when other approaches with better performance are available. Therefore, I recommend further discussion on the credibility of this new smoothing approach, as detailed below.

Major concerns:

1D Gaussian function: I am puzzled as to why authors have proposed a new smoothing method based on a one-dimensional distance framework. This assumption might distort the distribution of seismic activity. Consider Figure 1d, for instance; the method presupposes a uniform seismic rate around the target site's periphery (full ring), even though there are no seismic sources to the southeast or northwest. Furthermore, there are already several established smoothing methods utilizing two-dimensional (e.g., Frankel, 1995; Woo, 1996) and even three-dimensional (e.g., Chan, 2016) distance frameworks. The rationale behind introducing this new approach remains unclear to me.

References:

Chan, C. H. (2016). Importance of three-dimensional grids and time-dependent factors for applications of earthquake forecasting models to subduction environments. *Natural Hazards and Earth System Sciences*, 16(9), 2177-2187.

Frankel, A. (1995). Mapping seismic hazard in the central and eastern United States. *Seismological Research Letters*, 66(4), 8-21.

Woo, G. (1996). Kernel estimation methods for seismic hazard area source modeling. *Bulletin of the Seismological Society of America*, 86(2), 353-362.

Discussion on credibility of the approach: I am pleased to see the discussion on the results of this time-dependent PSHA, highlighting the impact on seismic hazard from each event. At the same time, I would recommend further discussion on the credibility of this new smoothing approach. When introducing a new forecasting method, establishing its credibility is crucial, and retrospective validation could be an effective means to achieve this.

Validation of the declustering approaches: To validate the declustering approaches, the authors compared the results with the 2011 Lorca earthquake seismic series as defined by Cabañas et al. (2011), detailed in Lines 320-327. However, it is challenging to assert that this series, defined by a previous study, represents the

ground truth. In my view, the definition of an aftershock (how to determine if two events are related) is still contentious. I would appreciate some discussion on this topic in the manuscript if possible.

Minor Comments:

- b-value calculation (Line 227): Are there sufficient events to support the calculation of the b-value? Cases with an insufficient number of events (for instance, fewer than 100, as noted by Aki, 1965) could lead to greater uncertainties in the b-value. It would be beneficial to address this issue in the paper.

References:

Aki, K. (1965). Maximum likelihood estimate of b in the formula $\log N = a - bM$ and its confidence limits. Bull. Earthquake Res. Inst., Tokyo Univ., 43, 237-239.

- Table 9: Some parameters are also from Table 9?

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