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Discussions

Interactive comment on "Quantification of uncertainty in rapid estimation of earthquake fatalities based on scenario analysis" *by* Xiaoxue Zhang et al.

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The paper presents a method to estimate earthquake fatality and to quantify the uncertainty in the estimation. The method is statistical analysis of the empirical fatality data. A sophistication is related to the classification of scenarios and further statistical modelling of the fatalities given a scenario – it is a two-stage modelling process. The separation of the data into different scenarios may reduce the scatter of the data within a scenario and thus tend to behave 'better' for statistical characterisation. This is a valid method – however, this itself seems to be insufficient to claim the innovation as a new research – it is more like an incremental research. This comment is based on the deficiency of the hazard modelling aspect of the proposed method and the lack of the demonstration of the robustness/quantitative performance of the method. In addition,

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the method is quite local (specific to China), which casts the doubt with regard to its applicability to other seismic regions. From this perspective, the paper is suitable for Chinese journals, not international ones. These comments are elaborated below.

The accurate mortality estimates are important but are not the sole source of critical elements in rapid fatality estimates. The hazard and exposure elements are also important. The proposed method only uses the macroscopic earthquake information (magnitude and source intensity). Modern rapid earthquake impact assessment methods use site-specific estimates of ground shaking, local site conditions, and if available, real-time assimilation of ground motion data and/or human-based intensity observations. The critical differences between the method proposed in this method and these modern methods are the use of local seismic shaking information in estimating the earthquake impact (which is distributed across space). The lack of this element in the proposed method is deficient.

The robustness of the performance of the methods is not well demonstrated. The split of the data into calibration and validation data is fine but this can be more rigorous – for example, a comprehensive cross validation should be carried out and quantify the performance.

The title is misleading because, essentially, the proposed method and its applications are mainly for China, not other parts of the world. Of course the method can be adopted for other parts of the world. However, the paper is insufficient to establish the innovation against existing methods, such as USGS. Methodologically, it is not clear how novel this method is. It appears that there are improvements with regard to the existing methods in China but this claim is not necessarily justified against, for instance, PAGER method and something similar. Ideally, the proposed method should be compared with the state-of-the-art methods (for the same conditions) – it is certainly possible that the proposed method performs better than global ones because the former is calibrated using local data. These comparisons should be done on a fair, objective basis.

It is unclear how the epicentral fortification intensity is defined. Some texts in Section 3 are repetitive and redundant. Figures 2-5 – the vertical axes should be in log10, not natural log so that readers can convert the numbers into the original scale. Figure 6: what is ‘oder’? The validity of normal distribution is not clearly explained (in light of data characteristics). Is this a valid assumption? Robustness of the results shown in Table 3 is not well quantified.

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