

The purpose of the paper is to develop fragility curves, for Chinese buildings classified in two main categories (masonry, RC) and two sub classes (A and B based on their seismic resistance level), using results from >80 papers reporting damage data from many Chinese earthquakes. Both empirical and analytical methods were applied to this regard. Empirical and analytical fragility curves for the two building typologies are derived based on median values. In addition the authors proposed relationships between PGA and macroseismic intensity IM using the derived fragility curves by eliminating the fragility values from the fragility–intensity and from the fragility–PGA relations.

Knowing that there are numerous if not huge uncertainties involved in all parts of their work the authors tried to make reasonable hypothesis in order to tackle the problem and derive some useful results in the Chinese context. For example they found by their analyses that reasonable results should emerge if the building types used for analytic calculations and those used in the empirical field studies are close enough, which however is not so obvious. All their work is based on Chinese data and China's official standards proposed by CEA regarding the structure typologies, intensity scales and damage states.

The paper is well written and very interesting as it synthesizes a great number of data from Chinese earthquakes and damages to buildings. All figures and tables are useful for understanding the work done.

General comments:

1. The state of the art chapter is not as complete as it should be. Important references from the international literature are missing, namely the work in GEM, PAGER, SYNER-G, PERPETUATE etc.
2. The methodologies applied in the different steps are clearly described and the reviewer has no major comment on that except that to his opinion the various uncertainties are treated, probably inevitably, in a quite simplified way. A comment on that should be useful.
3. The classification of the buildings in only two categories and two sub classes is an over-simplification, probably a reasonable one, for sub or underdeveloped countries, but maybe not for China any more. To the reviewer's opinion if the results of this interesting and useful work, mainly considering the huge efforts made to collect and synthesize all these data, will be generalized for any building type in China, and furthermore used for risk analysis, the final outcome will be heavily biased.
4. The accuracy of the results (fragility curves) depicted in Figures 7 (empirical) and 8 (analytical) are to the reviewer's opinion "too optimistic". The derived fragilities seem to be very low for these intensity levels, either in terms of IM or PGA and in particular for masonry structures (A or B). There are many reasons for that depending on the scatter of the data but also to the method used in particular regarding the treatment of uncertainties. The authors should compare their curves with other curves from the international literature (i.e. GEM, PAGER, SYNER-G etc). In any case they should comment on that important point.
5. According to the authors IM-PGA empirical expressions are generally region-dependent and have large scatter. This is not entirely correct. If region-dependency should mean soil conditions dependency as well, then this should be probably partially fine; but region-dependence is a much broader definition (i.e. spatial variability of ground motions etc) and to the reviewer's opinion this simplification is a certain source of huge uncertainties. PGA values are strongly dependent on site and local soil conditions. Furthermore the typology of buildings and their seismic quality in terms of seismic resistance is another crucial parameter, which again is practically "crushed" and downgraded in the regression analysis. This is obvious in the results where the difference between the different approaches is very

small. In few words the reviewer is very sceptical to the use of IM-PGA relationships in earthquake engineering and risk analysis in particular. Saying that the criticism is not made on the methodology and tools applied but on the philosophy (i.e. principles) of this methodology and the accuracy of the wished outcome.

6. To the reviewer's opinion if the results of the present work i.e. the IM-PGA tables, should be used as recommended values for IM-PGA ranges in China, it should be clearly stated that this is just for preliminary evaluations and the scatter may be very important.

Minor comment:

In table 7 (Recommended intensity-PGA relationship in China (GB17742-2008/1980)) there is an obvious error in the suggested value for Intensity X.