Reviewer 3

General

The paper presents the approach and the results of a fully probabilistic earthquake loss model for Central Asia which was developed to respond to the need of a consistent, comparable and harmonized methodology across different perils in a multi-risk perspective (hazard and floods) and different regions. Such loss model is intended to provide a scientifically sound basis – albeit preliminary - for stakeholders dialogues for disaster risk mitigation policies. The methodological framework illustrated in the paper is based on an event-based risk assessment approach accounting for a full treatment of the uncertainty and for state-of-the-art approaches for the characterization of the seismic hazard, vulnerability and exposure components of the loss model.
The efforts made by the authors to respond to the reviewers' remarks are acknowledged and clearly an improved version of the manuscript has been produced. Nevertheless, I believe that a few points still need to be clarified and explained in more detail in the manuscript, as commented below.

We thank the reviewer for this time and insightful comments that have allowed improving this manuscript. We are providing in blue a response to each comment, as well as an annotated version of the manuscript which shows how these have been addressed.

Remarks

1)For the seismic risk, a unique ground motion intensity measure (IM), i.e. Peak Ground Acceleration, is adopted regardless of the considered asset (buildings or infrastructures). I understand the reasons behind such a choice, especially in a context like the one in Central Asia with limited information on existing fragility and vulnerability curves. However, the choice of an efficient/sufficient/practical IM is crucial in a seismic risk model and it is widely recognized that PGA may not be optimal as an efficient predictor for damage and, eventually, losses, especially for some building typologies. Such aspects shall be commented by the authors. The adoption of a inefficient ground motion IM may be another reasons for the poor comparison with the reported losses (see Section 3.2).

We thank the Reviewer for this comment. We acknowledge it is a legitimate concern and we agree that choice of IM is a crucial point in any earthquake risk assessment. Nonetheless, as also fairly mentioned by the Reviewer, there is

limited information or even functions available for the region under study while most of the ones that are available are provided as a function of PGA. We are aware of this limitation, but it was a choice beyond the preferences of the authors. To address this comment we have added a sentence in section 3.1 of the revised version of the manuscript.

Furthermore, I noted that, in some parts of the manuscript, authors refer to "acceleration" (e.g. line 112, etc..) and "acceleration footprints" (line 278), while in other parts, as "PGA" (line 228, 250, etc..). I recommend to use a consistent notation and adopt "Peak ground acceleration" which is the correct one (maximum values of acceleration are indeed considered). For this reason, the label of the horizontal axis of Figure 2 shall be modified as "PGA".

In the revised version of the manuscript, we have made this consistent. (i.e., the first time it is called Peak Ground Acceleration (PGA) while in any later cross reference it is called PGA).

Figure 2: it is interesting and alarming that urban schools displays a vulnerability as low as unreinforced masonry.

This is a correct observation by the Reviewer. The school vulnerability function was developed based on the weighted average of a sub-set of vulnerability functions in the database. According to the exposure model, a large percentage of the buildings in Central Asia are built with unreinforced masonry or adobe. More specifically and based on a regional survey carried out as part of the project, all inspected schools are constituted by LBM (load-bearing masonry) or Precast concrete (80 and 20%, respectively). While in both urban and rural areas a large percentage of buildings was constructed between 1960 and 1990, urban areas have a larger fraction of modern schools (15% constructed after 2000) while rural areas have 15% of buildings constructed before 1960. Such large percentage of the adobe and unreinforced masonry evidently shifts the school buildings high up, close to these two building classes.

2) Referring to the social losses, does the methodology of this study allow to estimate only fatalities or other types of social losses (e.g. homeless)?

This study only considered the estimation of fatalities. Other types of social losses such as homeless are out of the scope of the study.

3) In my opinion, in Section 3.2, the approach adopted to calibrate the vulnerability functions by comparison with the observed losses during historical earthquakes is not explained with sufficient details. How do the authors adjust/modify the vulnerability functions to pass from the purple histogram to

the green histogram of Figure 3? Authors should provide details on this because it is central in the methodological framework.

In Section 3.2 of the revised version of the manuscript, we have added additional text to clarify this matter.

4) Among the interpretations provided by the authors to explain the differences between the observed and modelled losses for the 7 historical earthquakes (see section 3.2), might issues related to damage accumulation in seismic sequences be relevant?

We thank the Reviewer for this comment, and we agree with the appreciation. In section 3.2 of the revised manuscript, we have added additional text clarifying that the losses estimated in this study are associated only to the mainshock.

5) Figure 4. It is not clear whether, in the approach adopted by the authors specifically for historical earthquakes, the PGA footprint are provided by suitably selected empirical GMMs or, when available, they correspond to publicly available ShakeMaps. Of course, ground motion fields from GMM and from ShakeMap approach are different from a conceptual point of view.

We have added additional text on the revised version of the manuscript to clarify that these shakemaps correspond to PGA modelled after suitably selected empirical GMMs.

6) The aspects related to the choice of the spatial correlation model for ground motion intensity measure (PGA, herein) are not discussed in detail by the authors (see correlation coefficient in Eq. 3) and I believe that the discussion is relevant, as the choice of the correlation model may affect the results of spatially aggregated loss estimates.

In this study we have adopted the event-based risk assessment framework by Ordaz (2000) for which this discussion about the spatial correlation coefficient is explained in the original reference. We have added additional text in the revised version of the manuscript noting that the selection of the correlation model is likely to affect the spatially aggregated loss estimates.

7) I have some concerns on the outcomes of the pseudo-deterministic calculations for the 5 earthquake scenarios of Table 6. In particular, I see from Table 7 that the fatalities for the Mw5.4 Bishkek earthquake are of the order of 1000. It appears a rather large value for the relatively low Mw (which are the reasons for such high fatalities compared to other earthquake scenarios?) and, for validation purpose, this value should be compared with previous historical earthquakes with similar epicenter location and size to check the degree of

realism of such impact estimates. Also for the Tashkent earthquake of Mw5.6, the loss appears rather large and should be validated somewhat.

As explained in the manuscript, the selection of these five earthquakes was carried out based on the loss disaggregation, finding those that have estimated losses very close to that one of the 1000yr return period. This means that in some cases, ruptures were located closer or further away than the capital cities. In the case of Bishkek, the earthquake is very close to the city which combined with a relatively high vulnerability yields this number of estimated fatalities.

8) Figure 1 is scarcely readable and it should be enlarged/enhanced from a graphical point of view.

We consider that the relevant legends in Figure 1 are readable. However, if needed in the proofreading process these can be enlarged.