21.06.2022

Dear Sanish Bhochhibhoya and Roisha Maharjan,

Dear Editor Prof. Dr. Heidi Kreibich,

Please find below the comments after having read your revised manuscript.

In general, the authors significantly improved the quality of the first submitted paper. The authors accepted several suggestions, but not as many as I was expecting. I have found that the style of writing the paper is still not mature enough. There are some basic concepts that are mixed up and the paper still follows a quite disorganized structure. Therefore, in my opinion the paper should not be accepted as it is. The paper still requires several modifications and hence it is still in the "major revisions" category. I feel like I have been suggesting changes about the most basic issues, such as terminology, structure, colours, legend, captions, but also about more profound topics such as clear comparisons between the author's results and exiting studies and a compressive discussion (which are still missing). Hence, I honestly feel that the manuscript should have been sent to some colleagues asking for feedback before submitting to the Journal in a first place. I will elaborate more about the former ideas in the following while referring to the formerly listed comments of the first revision:

A. General comments.

1. English quality has improved, but there are still some remaining sentences to fix. Hopefully, that can be handled at a later stage (after a new revision, if the editor finds this pertinent).
2. Ok. The suggestion was accepted by the authors. Even though the authors have correctly rewritten the parts where the expression "a method is proposed" (or similar) had been initially stated, there are still several parts where the authors should have better emphasized that many of the inputs used in their study do not come from their own data, models or assumptions, but do come from existing ones (i.e. all that is related to physical seismic risk).
3. Ok. The suggestion was accepted by the authors. A discussion section is presented. However, the content therein is not entirely satisfactory, as will be elaborated later on.
   Line 409: "This variability in the result is due to differences in variables and hazards considered during the analysis". The authors used the word "hazards". Why? Was not the seismic ground shaking the only hazard considered? Or do you refer to the distinctive input seismic hazard levels considered by the two mentioned studies? This is distracting.
   The following comment is related to the suggestion **B.3.**6: The reference "Schiappapietra and Douglas, 2020" is not a correct citation for the role of spatially correlated ground motions on seismic risk assessment. That study only focused on the physical phenomenon, but not on their effects on risk. Then, this should be removed or relocated. Also, they say "spatially-correlated distribution". *Distribution of what?* The entire sentence is not clear enough and it is presented more as background information (something people would write in an Introduction (*or in the suggested chapter*) and not in a Discussion). Then, the authors say: *"However, in our study, we have used the conventional method of probabilistic seismic risk assessment due to its simplicity*" as if the incorporation of spatially correlated ground motions was an alternative method to PSHA, when in reality, they can be complementary. This shows the lack of understanding of these basic concepts. The OpenQuake engine (used by the authors) has already the model of Jayaram and Baker (2009) included, as well as some of the GEM recent manuals have a short explanation of their importance.

   > *Jayaram, N.; Baker, J.W. Correlation model for spatially distributed ground-motion intensities. Earthq. Eng. Struct. Dyn. 2009, 38, 1687–1708.*

   The intention of having suggested commenting on this topic in the Discussion section was more focused towards rather acknowledging the related limitations and outlook.

In the discussion section, you have included the sentence: *"The rescaling is necessary to integrate social vulnerability with physical risk although the rescaling of the estimates may have resulted the loss of spatial information of physical damage results*". I think I do understand what you try to say, but due to the terminology used in the text, "loss" might not be a good word selection. I suggest changing and making the sentence clearer.

4. Ok. The suggestion was accepted by the authors. There is a generalized improvement in the manner this is presented in comparison with the first version.

B. Specific comments.

1. "Abstract"
   In general, the quality of the new abstract has been significantly improved with respect to the initial version of the paper. However, its last part is presented more a too detailed summary. Providing a short summary in the abstract is always nice, but not to the degree of mentioning the number of variables. This could be moved to the last part of the introduction.

   There is a persistent imprecision in the abstract. The sentence *"the assets used were five types of buildings under the exposure model"* is not accurate. The exposed assets are not "types", but real objects (in this case residential buildings) that are classified into simplified typologies for the hazard-dependent vulnerability of interest (in this case, seismic ground shaking). In reality, the assets are the Residential buildings of Nepal (classified into five types), not the types.

   Also, the entire sentence *"In this paper, the physical or seismic risk was evaluated from the exposure model, hazard curves, and the vulnerability model of the country"* can be misleading. There are no unique exposure or vulnerability models for any region in the world. Hence, using "the" in "the exposure model" and "the physical vulnerability functions" is incorrect. They are not unique invariable models. Also, since the ones used by the authors basically follow the same building classes and corresponding fragility functions, this should be rephrased to "<u>an existing</u> exposure model for residential buildings". Otherwise, it might lead to the wrong belief that the exposure and vulnerability parts of the paper are your contribution (which is not the case).

   Moreover, the words "hazard curve" do not really fit here. No hazard curve was really presented in the entire paper. The authors do not present any result related to these computations, (only presented existing source models and recalled the selection of GMPE by others. This might lead to the wrong belief that hazard curves will be presented, or even, that a *new* probabilistic seismic hazard assessment will be presented (which is not the case). Therefore, the authors should also state something similar to: "<u>an existing</u> probabilistic seismic hazard assessment".

1.1. Ok. The suggestion was accepted by the authors.
1.2. Ok. The suggestion was accepted by the authors.
1.3. Ok. The suggestion was accepted by the authors.
1.4. This is not sufficient. This is correctly done at the beginning of section 3.2.3, but not in the abstract as requested.
1.5. The inaccurate sentence written in the former version was corrected. However, aligned with the previous comment, the expression "residential buildings" was not accepted in this section by the authors. Also, the explicit request on writing "seismic ground shaking" (as it is the only hazard evaluated) was not accepted either. The latter is very important as explained in the first review.

1.6. Ok. The suggestion was accepted by the authors.

2. “1. Introduction”

<u>New comment:</u> the expression “hazard management” is incorrect. Do you refer to other concepts different from the hazard (e.g. disaster risk)?

2.1. The suggestion was not accepted. I still find that the structure of providing global characteristics in between two subsections with issues about Nepal is disorganized.
2.2. Ok. The suggestion was accepted by the authors.
2.3. Ok. The suggestion was accepted by the authors.
2.4. Ok. The suggestion was accepted by the authors.
2.5. Ok. The suggestion was accepted by the authors.
2.6. Ok. The suggestion was accepted by the authors.

3. “2. Theory and background”.

3.1. Ok. The suggestion was accepted by the authors.
3.2. Ok. The suggestion was accepted by the authors.
3.3. Ok. The suggestion was accepted by the authors.
3.4. Ok. The suggestion was accepted by the authors.
3.5. Ok. The suggestion was partially accepted by the authors. Corrections of terminology were done. However, the text of interest was moved to another section. It should have remained here, as the hazard component is part of the “material” used in your work. (See comment B3.6). Nonetheless, it is Ok to leave the issue related to the Poissonian assumption of PSHA in the Discussion.
3.6. The suggestion was partially accepted. No comment on the spatial correlation was provided in one of the advised sections. Something similar was mentioned in the discussion part as discussed above.

Moreover, sections 3.2.2 and 3.2.1 integrally present the work of others (the seismic zonation, or the selection of GMPE). There is nothing that the authors have done by themselves in those subsections. Also, even though the actual outcome of these two sub-processes is the probabilistic seismic hazard (at certain return periods), there is nothing written about these outcomes in this section. Outcomes such as the acceleration values obtained from hazard curves, either computed by you (during the recalculation of the work of Chaulagain et al, 2015) or by other authors are missing here. It is true that the authors provided those values in the Discussion section (between lines 417 and 422). However, providing such background information at that very last stage (the first time in the whole text that acceleration values manner are mentioned in this new version of the manuscript), is extremely weird and disorganized. Moreover, the probability of exceedance used in your calculations is not clearly stated. I suspect it was 10%, but this guess can be ambiguous by other readers considering that the work you are based on (Chaulagain et al, 2015) also provided results for 2%. Considering these reasons, I recommend that the authors merge Section 3.2.2 and 3.2.1 into a single one: “*Seismic hazard assessment*” or something similar, also including the lines from 417 to 422 (of course, re-writing if needed).

4. “3. Materials and methods”.

4.1. Ok. The suggestion was accepted by the authors
4.2. “ 3.1. Social vulnerability assessment”

4.2.1. Ok. The suggestion was accepted by the authors
4.2.2. Ok. The suggestion was accepted by the authors. Understood.
4.2.3. Ok. The suggestion was accepted by the authors.
4.2.4. Ok. The suggestion was accepted by the authors.

4.2.5. Figure was not relocated as advised.
4.2.6. Ok. The suggestion was accepted by the authors
4.2.7. Ok. The suggestion was accepted by the authors
4.2.8. The suggestion was not accepted by the authors. Although the authors provided an explanation about used test, there is no real reason of using the same outcome of the software (using 3 decimals in 0.000) right after they wrote other numbers with different number of digits.
4.2.9. The full name of SPSS was written in the text as suggested. However, it is still missing to provide it within the Reference list. Thus, this suggestion remains incomplete.
4.2.10. The suggestion was not accepted by the authors.
4.2.11. Ok. The suggestion was accepted by the authors

4.3."3.2. Seismic Risk Assessment"
4.3.1. Ok. The suggestion was accepted by the authors
4.3.2. Ok. The suggestion was accepted by the authors
4.3.3. Ok. The suggestion was accepted by the authors.
4.3.4. Ok. The suggestion was accepted by the authors, but the word "similar" does not mean "identical" (your case). I still suggest rewriting this. It is great you have written "Main Himalayan Thrust" in Sect. 2.2.
4.3.5. Ok. The suggestion was accepted by the authors.
4.3.6. Ok. The suggestion was accepted by the authors
4.3.7. Ok. The suggestion was accepted by the authors
4.3.8. Ok. The suggestion was accepted by the authors (the figure with the seismic sources was improved). However, commenting/ the newly added information is missing in the text (it still has the same description as before). I had suggested including a sentence that was not accepted to be included.
4.3.9. The suggested reference "Rao et al., (2020)" was not cited to support the explicitly suggested topic. Instead, it was cited in a very generic sentence on page #1. These authors were not the first ones to work on such a topic (the one you cite them for on page 1), and their work is neither mostly recognized for that topic. Adding suggested references randomly just add more noise to the paper. This comment also applies for the suggested reference "Gomez-Zapata et al., (2021)" that was used to reinforce a statement about fragility functions (page 4). These two references should be relocated to reinforce the specific topics for which they were suggested in the first review round (their main topic), not generic aspects of any seismic risk assessment. For instance, the central topic of the first one is not "disaster", and the latter one did not propose the concept of "fragility functions", right? You could also consider moving them to the discussion if you feel that the topics exposed could be taken into account in the future (for instance, as an outlook.
4.3.10. Ok. The suggestion was accepted by the authors.
4.3.11. Suggestion was not accepted by the authors.
4.3.12. Ok. The suggestion was accepted by the authors
4.3.13. Ok. The suggestion was accepted by the authors
4.3.14. Ok. The suggestion was accepted by the authors
4.3.15. Ok. The suggestion was accepted by the authors
4.3.16. Ok. The suggestion was accepted by the authors. However, there is no need of separating Fig 5 and 6. Fig. 6 could be just listed as Fig."e)". Moreover, in line 287, the authors have written: "After defining fragility functions, it is also important to assess the correlation between the logarithmic means and standard deviations" is not accurate. The fragility functions are implicitly defined by their

logarithmic means and standard deviations. It is not a second step and has nothing to do with “correlation”. This must be corrected using a simpler expression.

4.3.17. Ok. The suggestion was accepted by the authors. This is a significant improvement.

4.4. “3.3 Integrated risk assessment”

4.4.1. Ok. The suggestion was accepted by the authors

4.4.2. Suggestion was not entirely accepted by the authors. Despite giving explicit hints, the caption was not modified.

4.4.2.1. The authors chose to do it in the text, not in the caption.

4.4.2.2. Suggestion was not accepted by the authors.

4.4.2.3. The authors combined this comment with 4.4.2.4, which is Ok. The suggestion was accepted by the authors. The authors made an attempt to include a similar sentence to the suggested one. However, it is not well written (the words “present” and “although” do not fit there, and the overall sentence sounds incomplete). Hence, due to the way it was presented, it does not yet fulfill the aim that was requested in the first revision.

4.4.2.4. See above.

4.4.2.5. Ok. The suggestion was accepted by the authors.

4.4.3. Ok. The suggestion was accepted by the authors

4.5. Ok. The suggestion was accepted by the authors

4.6. Ok. The suggestion was accepted by the authors. For the related paragraph, I still recommend to cross-referencing the sections using parenthesis or chapters in which you presented each component.

5. “Results and discussion”

5.1. Ok. The suggestion was accepted by the authors and there are two different sections now.

5.2. “4.2. Seismic Risk Assessment”.

5.2.1. This comment is one of the most relevant suggestions. However, the suggestion was not accepted by the authors. The authors fully rely on the outcomes of the mentioned study in the revision (Chaulagain et al, 2015) not only for the seismic hazard (the only aspect they comment on), but also on the exposure model, fragility functions, and even on the replacement costs and loss ratios (basically everything). In the discussion section, the authors should have commented on this issue as well as what could be results if these components were updated, or if other models were used. As suggested, the authors should be made it clear that these steps are not their contribution, but only the incorporation of the social vulnerability part. Nonetheless, I can clearly see that the results between Chaulagain et al, 2015 (fig 7) and the author’s work (fig 13) are different, but once again, the authors do not do such a comparison while discussing why their result is improving that existing study. This suggestion was just neglected.

5.2.2. Ok. The suggestion was accepted by the authors.

5.2.3. The suggestion was not accepted by the authors. The figure has the same caption as before.

6. Conclusions

6.1. Ok. The suggestion was accepted by the authors.

6.2. Ok. The suggestion was accepted by the authors.

C. Technical corrections

1. There is an inconsistency: The authors have included the DOI and year for most of the references. However, the journal names have been deleted from each reference. This is unacceptable.
2. Ok. The suggestion was accepted by the authors
3. Ok. The suggestion was accepted by the authors
4. Ok. The suggestion was accepted by the authors
5. Ok. The suggestion was accepted by the authors
6. Ok. The suggestion was accepted by the authors
7. Suggestions were not accepted by the authors.
8. Ok. The suggestion was accepted by the authors
9. Ok. The suggestion was accepted by the authors

D. New general comment (second revision)

The results obtained in this study should be freely available to the reader. Please make sure to provide one or several data repositories with the input data and outputs. This should be cited in the manuscript. This is because the results should be transparent and reproducible.

E. New Specific comments (second revision).

I think that figure 4b is not informative enough for the purposes and aims of exposure modelling at this regional scale. Although box whisker plots might be informative to visualise the median and extreme values, this might not be that interesting for exposure modelling. It is evident the authors wanted to do an alternative plot to Fig 3b provided by Chaulagain et al, 2015. However, the percentages shown by the referred paper are more interesting. In this sense, please note that the values you report in line 282: “135.73, 603.36, 239.91, 340, 46.33” as “average” of the types RCC with pillar, Mud-Bonded, Cement-Bonded, Wooden-pillar, and Adobe respectively cannot easily “read” from that figure, specifically for RCC with pillar and Abode types. This is more a “form” comment. Please be aware that the total number of buildings per class is more interesting than the median values across the administrative divisions. You can think of including the respective percentages and total building counts per class.

F. New technical corrections (second revision)

1. The authors use sometimes “MIN-MAX”, “MINMAX”, “Min-Max”. Please select only one notation and harmonize.
2. Line 330: “that affect the earthquake risk”. The verb “affect” is out of context here. This sentence is in general not needed and actually can lead to confusion: as if the 2 mentioned variables might induce to the modification of something pre-established, which is not the case.
3. The caption of Figure 13 is not comprehensive. It must be more descriptive and self-explanatory.
4. The quality of most of the figures that remained the same (e.g. Fig 10, 11 in the new version) has decreased in comparison with the ones in the initially submitted version (i.e. Fig. 7 and 8). Please improve the colour quality of those figures.
5. Line: 442: “doesn’t” is not proper English. Please change it.