

Dear Jess Delves, Kathrin Renner, Piero Campalani, Jesica Piñón, Stefan Schneiderbauer, Stefan Steger, Mateo Moreno, Maria Belen Benito Oterino, Eduardo Perez, and Massimiliano Pittore

Dear editor, Robert Sakic Trogrlic,

I believe some of the content in this paper is relevant for publication in NHESS, and certain findings may have implications for disaster risk management. However, due to the manuscript's disorganized structure and specific imprecisions, substantial revisions are necessary before moving forward. I found it challenging to identify any clear innovations or methodological advancements from the manuscript. Simply applying existing state-of-the-art (or at least state-of-practice) tools to a different problem or case study does not inherently provide novelty. Nevertheless, deriving new insights—even from straightforward applications—can be valuable and appropriate. Unfortunately, the authors do not present a compelling case for any new insights gained through their application of established tools to a new case study.

Despite significant shortcomings, I opted not to reject the manuscript at first, as I recognized the hard work that went into assembling these ideas and the potential for application and replication of the study. My recommendation is that major revisions be undertaken prior to publication, as the manuscript in its current form does not meet the quality standards expected.

I have provided the authors with a first review to assist in improving the quality of their manuscript. I am confident that with substantial revisions addressing the feedback I outline below, the quality of the paper could be enhanced to meet NHESS standards. I advise that all authors contribute to a comprehensive revision of the manuscript and reach an agreement on its content before resubmission. The responsibility is not solely that of the main author. I believe the manuscript will improve significantly with the notable experience and careful proofreading of senior scientists.

However, given the nature of these comments, the decision ultimately rests with the Editor as to whether the manuscript can indeed undergo major revisions or if it should be rejected with the possibility of a new submission.

A. MAJOR COMMENTS

1. Style:

The paper appears to be more of a project report rather than a scientific manuscript, evident from its general structure and specific phrasing (e.g., lines 36, 43, 352, etc.). The scope of the study seems directed towards a specific audience, distinct from the scientific community, which is acceptable. However, the basics of the various methodologies and assumptions should be clearly stated, even if only in a supplementary section. Below are some related suggestions.

2. Originality:

I have concerns that this paper may have been submitted elsewhere (referencing https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4882187) or that it may be part of an existing project report or deliverable that is currently available or will be published in the future, raising potential originality conflicts. If this is the case, this should be noted in the manuscript.

3. Structure

- 3.1.** Considering the final results the paper intends to convey, the structure of the submitted manuscript requires revision. Numerous sub-sections lack sufficient detail (e.g., the approaches followed for hazard-specific vulnerabilities) and surface late in the document. It would be beneficial to consolidate sections by creating “chunks” for each hazard-related physical risk. This means having risk as a header, with hazard and physical vulnerabilities (the adopted methods) presented together. Such an organization would harmonize text that currently mixes these components (e.g., lines 173-175 & 235-240) in the “Hazard” section, while exposure and social vulnerability could be presented separately.
- 3.2.** The current arrangement of having discussion as Section 9 (after Section 8: Results) and then Section 10 on ‘Climate Change’ is not appealing. I advise revising this structure.
- 3.3.** The SEVA and Climate Change analyses were not integrated into the final results and therefore feel distracting. I suggest either completely removing them or presenting them as an Appendix, treating them as auxiliary analyses rather than part of the main text. The same applies to the details for the flood model for the five catchments (Fig. 4), as this is also distracting.

4. Material and methods

- 4.1.** My understanding is that the left subplot in Fig. 7 derives entirely from Worldpop (the full citation is missing). However, it is unclear which spatial disaggregation technique was used to derive building counts on the right. No equations or methods were provided. The existing information does not sufficiently connect with the risk assessment. How do these counts compare to existing datasets such as IOM? What methodology (i.e., equations) was used to adjust these initial numbers? Why were existing datasets like the Global Human Settlement – GHSL not utilized, given they offer estimates on the number of buildings and even future populations at higher resolutions?
- 4.2.** My understanding is that population (from Worldpop) was used solely to derive building counts, with no risk-related metrics developed (e.g., casualties, human displacement); only monetary risk metrics were assessed. This should be clearly stated from the outset.
- 4.3.** Line 292 states that GEM taxonomy Version 2.0 is focused entirely on seismic vulnerability applications, including various attributes. This statement is inaccurate. For Fig. 8, only material and number of stories were included in the ten baseline classes. The authors claim that GEM and DTM building types were merged, but the basis for this is unclear. How was this validation conducted? If a test was performed, why not present it as supplementary information?
- 4.4.** The manuscript does not provide the spatial distribution of repair costs for residential buildings and crop values, nor does it include this information in a figure or table, or detail its derivation. This is crucial considering that a key result is the assessment of the Annual Average Loss (AAL). One option might be to include another subplot in Fig. 7 showcasing these repair cost values.
- 4.5.** Regarding damage functions for crops, the only mention is in line 115: “for the impact of fluvial flooding on agricultural areas and for landslides, a simpler binary fragility model was used due to a lack of consistent alternatives.” This aspect requires more detail. What thresholds were used to determine loss? Was a hazard intensity or distance-based metric assumed for vulnerability to landslides?

4.6. Vulnerability of buildings to landslides: In line 354, what exactly does “given distance” from the location assigned to a landslide event refer to?

4.7. SEVA.

4.7.1. Line 342: notes that coping capacity may be one of the highlights and novelties of the manuscript. However, little detail on the methodology and its impact on final results is provided. This should be commented on in the conclusions.

4.7.2. Line 361, which proxies?

4.7.3. terms “indicators, components, categories, dimensions” is not well separated, and sometimes used interchangeably.

5. Main results:

5.1. In the multi-hazard map shown in Fig. 12, why was the 85th percentile selected? This assumption has not been discussed or justified. Notably, the same percentile for different (and very different) return periods has been combined. How can this be technically justified?

5.2. The final results presented in Fig. 13 come from aggregating the individual results in Table 1. My understanding is that 5 out of the 6 AAL columns were derived from assessing the risk to residential buildings, while only one column reflects the potential losses of crops due to river floods. This indicates that the vulnerability of crops was not assessed for torrential rains, winds, landslides, or earthquakes. This separation raises concerns.

5.3. The final result in Fig. 13 was obtained by dividing the physical risk by the area of the colline. This appears to be an arbitrary assumption; various results would arise if the AAL risk were divided by average gross income, total population, or any other aggregated parameter. This approach needs revision, commentary, and stronger justification.

5.4. Although I previously suggested moving the impact of climate change to an Appendix, I am concerned about lines 470-472, where the authors mention that Fig. 15 is derived from averaging three “future projections” (global climatic models) and “three return periods.” Is this map an average of three return periods and three climatic models? The authors should provide a justification or methodology for this assumption. How should these results be interpreted, and are they genuinely useful for practical applications?

6. References

6.1. Official citations for datasets and other relevant sources are missing (e.g., Worldpop, IPCC).

6.2. Given the difficulty in assessing impacts, open sources, such as the Climate Impact Explorer (<https://climate-impact-explorer.climateanalytics.org/impacts/>) and the ISIMIP repository (<https://data.isimip.org/>), could have offered valuable initial insights. Additionally, as pointed out by Reviewer 1, several existing datasets have been completely neglected.

6.3. Several studies on flood vulnerability and risk in the African context were conducted under the CLUVA project. I recommend that the authors include and comment on these works (e.g., Jalayer et al., 2014, 2016).

6.4. The entire section “5.4 Earthquakes” lacks bibliographic references. This omission implies that the authors claim all content therein as their own, which is likely not true. Also, in line 254, the authors mention five hotspot localities without further elaboration. If this is not part of the final results, why is it included?

- 6.5. The references of Paul et al., (2022); and Paul and Silva, (2025) are missing. These studies contain valuable references that should be contrasted with the authors' findings. There has been no thorough bibliographic revision concerning multi-hazard risk. I fully agree with Reviewer 1 on this point.

7. Data availability

Access to geodata products is not only pertinent to reviewers; potential future readers may seek access to replicate the methodology using the developed tools in other study areas. This is crucial for adhering to and upholding the FAIR principles (Findable, Accessible, Interoperable, and Reusable) (Wilkinson et al., 2016).

B. Minor comments:

1. Figures. In general, all figures with maps should include clear copyright statements.
 - 1.1. Fig. 2: should be improved. It is not self-explanatory. I advise to include references for the adopted models.
 - 1.2. Fig. 10: The legend was never mentioned in the text. Also, it is unclear why this damage function is included when the adopted function for earthquakes is not shown. As this is not an original development, I recommend removing this figure.
 - 1.3. Fig. 7: I advise to use a continuous colour bar, and not discrete intervals.
 - 1.4. Fig. 12: delete the title. The figure's caption is enough. I advise to update the figure showing the two lower subplots and the table bigger.
 - 1.5. References style: The footnote 1 (line 288) is not a correct citation. Please note that in that website, the citation suggested is mentioned.
2. Please, add every individual author's contribution, as required by NHESS.

C. Editorial comments:

1. A more thorough review of the English writing is recommended, focusing not only on grammar but also on the presentation of ideas. I am particularly surprised that this was not noted by the senior co-authors of the manuscript, who should have provided careful supervision and strict approval before resubmission.
2. The use of several adjectives, such as "accurately" (line 456), "precise" (line 449), and "effectively" (line 457), is inappropriate given the uncertainties associated with global climatic models. I advise using more straightforward language.
3. Please ensure uniform font style throughout the document. The text currently displays mixed font styles that do not comply with the NHESS template (e.g., line 212).
4. In line 299, the phrase "finally..." implies that the presentation of the figure in pie charts is part of the methodology. This is misleading, as it represents merely a selected format of presentation.
5. There are several disconnected sentences that appear as if different texts were mixed together without harmonization by a careful reader.
6. Similarly to Reviewer 1, I am surprised that no local authors were invited to contribute to this study. I recommend that the authors consult the EGU statement on Scientific Neocolonialism.

Best regards.

References

Jalayer, F., Risi, R., Paola, F., Giugni, M., Manfredi, G., Gasparini, P., Topa, M., Yonas, N., Yeshitela, K., Nebebe, A., Cavan, G., and Lindley, S.: Probabilistic GIS-based method for delineation of urban flooding risk hotspots, *Natural Hazards: Journal of the International Society for the Prevention and Mitigation of Natural Hazards*, 73, 975–1001, <https://doi.org/10.1007/s11069-014-1119-2>, 2014.

Jalayer, F., Carozza, S., De Risi, R., Manfredi, G., and Mbuya, E.: Performance-based flood safety-checking for non-engineered masonry structures, *Engineering Structures*, 106, 109–123, <https://doi.org/10.1016/j.engstruct.2015.10.007>, 2016.

Paul, N. and Silva, V.: Probabilistic seismic risk assessment of Africa, *International Journal of Disaster Risk Reduction*, 119, 105303, <https://doi.org/10.1016/j.ijdr.2025.105303>, 2025.

Paul, N., Silva, V., and Amo-Oduro, D.: Development of a uniform exposure model for the African continent for use in disaster risk assessment, *International Journal of Disaster Risk Reduction*, 71, 102823, <https://doi.org/10.1016/j.ijdr.2022.102823>, 2022.

Wilkinson, M. D., Dumontier, M., Aalbersberg, Ij. J., Appleton, G., Axton, M., Baak, A., Blomberg, N., Boiten, J.-W., da Silva Santos, L. B., Bourne, P. E., Bouwman, J., Brookes, A. J., Clark, T., Crosas, M., Dillo, I., Dumon, O., Edmunds, S., Evelo, C. T., Finkers, R., Gonzalez-Beltran, A., Gray, A. J. G., Groth, P., Goble, C., Grethe, J. S., Heringa, J., 't Hoen, P. A. C., Hooft, R., Kuhn, T., Kok, R., Kok, J., Lusher, S. J., Martone, M. E., Mons, A., Packer, A. L., Persson, B., Rocca-Serra, P., Roos, M., van Schaik, R., Sansone, S.-A., Schultes, E., Sengstag, T., Slater, T., Strawn, G., Swertz, M. A., Thompson, M., van der Lei, J., van Mulligen, E., Velterop, J., Waagmeester, A., Wittenburg, P., Wolstencroft, K., Zhao, J., and Mons, B.: The FAIR Guiding Principles for scientific data management and stewardship, *Scientific Data*, 3, 160018, <https://doi.org/10.1038/sdata.2016.18>, 2016.