ANSWERS TO REVIEWER 2

This manuscript proposes a regional exposure database featuring the population and residential building stock for the countries of Central Asia. This new dataset has a regional scale, with the appropriate resolution to be used in multi-hazard and risk assessment. Moreover, the authors provide estimates for exposure for the year 2080 that can support current and future risk mitigation strategies. To achieve this, the authors use high resolution population and building datasets to generate gridded exposure datasets. The introduction clearly states the research goals and main novelties. The methodological aspects of the research are very interesting. The authors rely on local data and expert knowledge to provide reliable building characterization and replacement costs. The manuscript presents relevant information for other risk scientists. This information is clearly laid out in maps, figures, and tables, making the paper useful, appealing, and easy to read. The exposure layers for 2080 are a significant addition to the research outputs. These rely on future scenarios of population and urbanization to propose different possibilities for future exposure in Central Asia. The results sec on is brief, clear, and very well written. I praise the authors for presenting the results of their work so well. The manuscript contains a couple of typos, which can be easily corrected, but it is also missing several references, some of which are essential to the research. I strongly suggest thorough proof-read by the authors. Regarding the methodological aspects of the research, there are only a couple of points that need to be explicit in the manuscript. One is regarding the SSPs, specifically the motivation for the scenarios chosen and the uncertain es that they account for in this research (i.e., it seems that future population and urbanization are being accounted for, but not future sustainability, resilience, or vulnerability of the residential building stock). The other point that needs to be clearly addressed is the expected reduction in the future number of buildings, which is most likely due to the fact the future abandoned or unoccupied buildings are not being accounted for in the 2080 exposure dataset. Based on this assessment, I believe the manuscript would be an excellent contribution to the scientific community, and that its main contents are up to the standards of NHESS. I also want to extend my congratulations to the authors for their hard work. My recommendation is to accept the manuscript, subject to minor revisions. Please find my comments line to line below, which I hope can help the authors greatly improve the quality of the final publication.

Thank you very much for your suggestions. We replied to all comments, included the missing references and proof-checked the document. We added more details on how the future exposure projections were developed, the choice of SSPs and how the progressive replacement of deprecated building typologies was accounted for. We also included new figures that show the exposure layers developed for different building typologies. Please find attached our responses to each comment.

Introduction:
Lines 30-35: Reference Yu et al. 2019 is missing in the references section. The authors need to include it.
The reference was included, thank you.

Lines 45-50: Reference UNECE 2017 is missing in the references section. The authors need to include it.
The reference was included, thank you.

Lines 55-60: The authors mention that the only regional-scale exposure dataset of residential buildings available at the time (April 2023) is provided by Piiiore et al., (2020). I believe the Global Earthquake Model Foundation has provided an updated exposure model for Central Asia in terms of population, building counts and replacement costs (See Yepes-Estrada et al. 2023 and https://github.com/gem/global_exposure_model). The authors should consider mentioning the update as well.
The reference was now included, thank you for pointing it out.

Lines 75-80: The authors state that the first regional-scale exposure dataset for Central Asia was developed by Pittore et al., (2020) using ground-based and remote sensing datasets. The Global Earthquake Model Foundation also has updated exposure datasets covering Central Asia using a bottom-up approach (Yepes-Estrada et al. 2023). Herein, however, the authors maintain that they are proposing the 'first regionally-consistent exposure database for Central Asia'. Were the first efforts and methodologies to map the residential exposure in the region inconsistent? How so? There are novelties in the new dataset the authors propose, but this assertion needs further explanation to be included in the manuscript.

With respect to the work of Pittore et al. (2020), we update the model using recent country-based data and we use a constant spatial grid of higher resolution (500m) which covers the entire Central Asia region. Also with respect to Yepes-Estrada et al. (2023), we use a higher resolution grid (500m with respect to approximately 30km). Our resolution is, in particular, higher in rural areas, and supports the impact and risk assessment not only for earthquakes but also for floods, under certain limitations that are discussed in the manuscript. We therefore included the Yepes-Estrada work in the references and modified the wording explaining that this is not the first exposure model produced for Central Asia but is the first high-resolution exposure dataset that provides exposure to earthquakes, floods and landslides at a constant spatial resolution of 500m.

Methodology

Lines 120-125: The authors mention that comparing Facebook data versus national census data results in differences in the total population that exceed 20% in 7 oblasts. What is the total number of oblasts under study? What is the difference between the total population estimates from the census and Facebook? Would it be possible to elaborate? The use of Facebook data is an interesting approach and I think it would be interesting for other researchers to see how it compares to national databases.

At the time of the analysis, Facebook data had been recently released (June 2019) and provided data at a higher resolution (30m) with respect to other datasets such as WorldPop. We compared it with the latest available population census from the year 2021 for Uzbekistan, 2020 for Kazakhstan and Kyrgyz Republic and 2019 for Turkmenistan. Population data was retrieved for 2018 in Tajikistan. However, this data was older than the Facebook dataset and was available only for selected towns and cities, so we did not correct the Facebook layer assuming it to be more reliable. The comparison showed that at the regional scale, the Facebook dataset contains a 5% less population than the sum of the national scale census retrieved. At the national scale, the population in the Facebook dataset was also consistently lower than in national census, with a difference of 1.5, 4, 5 and 8% respectively for Kazakhstan, Uzbekistan, Turkmenistan and Kyrgyz Republic. We noticed that larger discrepancies were associated with the presence of older census data (e.g. for Turkmenistan) while smaller differences are found in Kazakhstan, and Uzbekistan. Large differences are found between the Facebook layer and the national census for Kyrgyz Republic despite the fact that the census was relatively recent. We included more details on the database and its usage, including the total number of Oblasts used for the analysis and the overall difference between the two datasets at regional and national scale and the differences observed between the two datasets.

Lines 135-140: The Global Earthquake Model building taxonomy by Brzev et al., 2013 has been updated by Silva et al., 2022 (https://doi.org/10.1007/s13753-022-00400-x.).

The reference was updated, thank you.

Lines 160-165: The authors used the Facebook population data and adjusted it to respect local estimates of the population. Then the buildings were taken from Pittore et al. (2020). Both datasets end up distributed in variable-resolution grids. These disaggregation techniques can result in an inconsistent number of occupants per dwelling and occupants per building in each cell, especially if datasets developed independently are being combined. Do the time stamps of the population and building datasets agree with each other? (i.e. the population of 2020 and the number of buildings by 2020). Was the final number of occupants per dwelling and building consistent with the subnational local estimates after the distribution? It would be a great addition to the methodology section to briefly mention the checks performed after the disaggregation methods.

Overall, the timestamps between the dataset are not always matching, which can introduce discrepancies. Regarding the population, we only used recent census data to correct the Facebook dataset, which is dated 2019. In particular, the census dates were 2021 for Uzbekistan, 2020 for Kazakhstan and Kyrgyz Republic, 2019 for Turkmenistan. For Tajikistan, we did not use the census because it was from 2018. Here, we maintained the Facebook constant grid. Since the population datasets were all recent and of similar age, we performed a simple check to make sure that the population never had negative values.

As for the buildings data, we updated the Pittore et al. (2020) dataset (that uses buildings data from the time period 2012-2016 and population ancillary data referred to 2015) using country-based data for Kazakhstan and Uzbekistan, referred to 2020. This was done maintaining the same resolution of the original layer. Finally, we disaggregated the buildings layer on the regular grid of the population dataset.

The larger discrepancies are expected, in principle, for the countries for which the Pittore et al., dataset was not updated, i.e. Kyrgyz Republic, Tajikistan and Turkmenistan. In order to make sure that there were not discrepancies between the two datasets, we performed the following simple checks:
- We made sure that no points were associated with null population and not-null number of buildings, or vice-versa
- We checked that the average number of occupants per building (i.e. the fraction between population and number of buildings in each point of the grid) was consistent with the average occupancy defined for Central Asia typologies (Table 1 of the manuscript). No points had an average number of occupants per building larger than 200, which would have required additional checks.

A sentence on this was added to the manuscript, underlining the importance of tackling temporal variability of exposure and checking that the results of the disaggregation are realistic and consistent among the variables which have known relation (e.g. building occupancy and population).

Development of exposure layers for 2080
Lines 210-2020: HAZUS is mentioned throughout the manuscript, but I could not find the reference in the reference section (HAZUS 2021? FEMA 2021? Inventory technical manual?). This is the third reference that is either missing from the manuscript or difficult to find in the corresponding section. There is also a typo: 'manual (2021).g dife Costs'. I strongly recommend the authors to proofread the manuscript carefully.
Thank you, the Hazus reference has been included, we referred to the HAZUS inventory technical manual, available at: https://www.fema.gov/sites/default/files/documents/fema_hazus-inventory-technical-manual-4.2.3.pdf, accessed 20/10/2021, edited by FEMA. We also carefully proofchecked the manuscript and apologize for the typos and flaws.

Lines 235-245: The SSPs chosen to develop these layers were SSP1, SSP4 and SSP5. This is a very interesting choice. However, the authors mention that these scenarios envisage different development drivers, but provide no further motivation for selecting these. For example, why was the SSP2 not included? The 'middle of the road' considers less strong deviations from the current fertility trajectories, hence it is the likely scenario in terms of the future population and urbanization. Could the authors elaborate on what motivated this choice? Was supporting risk management strategies a part of this motivation? How so?
The number of SSPs to be considered in the analysis was limited to three by the need to perform subsequent risk analysis which were time consuming for other research partners (Salgado et al., and Berny et al., this volume). We decided to use three 'extreme' SSPs in order to highlight the larger variations expected, and to create an upper and lower bound for expected exposure changes with respect to more 'middle of the road' scenarios. The choice of SSP1 was also motivated by the willingness to highlight the role of governance and international cooperation and show how it affects exposure. However, we agree with the reviewer that other scenarios such as SSPs are very relevant (and could represent a more likely future pathway). We included a sentence in the manuscript to explain this and open to future work that analyzes in more detail the implications of different SSPs for exposure assessment and disaster risk reduction.

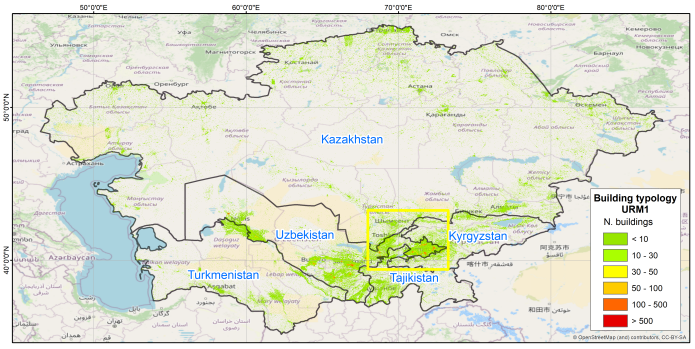
Lines 245-265: It is unclear how the SSPs are used beyond the scenarios of future population and urbanization. The uncertainty regarding the future population is taken from the SSPs. The different future population scenarios are used to infer a future number of buildings, and the urbanization layers inform location and density. However, there seems to be no uncertainty considered in the future characterization of the buildings, which influences the final vulnerability and sustainability of the future exposure datasets. This would be a very difficult task, and the authors rightly rely on expert judgement alone to propose a single set of rules for future building characterization. There is no mention of this set of rules changing depending on the SSPs. If this is the case, the authors need to be explicitly clear in the manuscript that the SSPs only inform the population figures and building allocation. This is important given the choice of SSPs by the authors. For example, the SSP1 (sustainability with low emissions) and the SSP4 (high inequality with high emissions) are clearly opposite scenarios. For example, a reader could understand that sustainability and a more resilient built environment were at the core of creating the future building characterization for an SSP1 scenario, but it doesn't seem to be the case. I think this section really needs more clarity on this point.
Thank you for your comment. We agree with you that, right now, it is somehow unclear how SSPs influence the assessment of future exposure. We explicitly say that SSPs are used only to inform on population variation and residential buildings allocation, and that conversion rules defined with local experts are

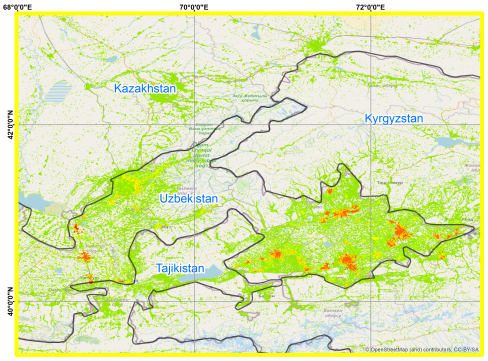
assuemed to be the same in all SSPs. We also included a few sentences to explain the limitations of the approach, underlining the need for performing more sophisticated analysis in the future where scenarios are based not only on the number of populations and buildings, but also on the different economic system envisaged in each SSP and the socio-economic consequences of its adoption. I think that implementing an analysis of this kind would probably lead to higher discrepancies in the exposure layers, highlighting the beneficial or negative impact of the economic model on the overall exposure to disasters.

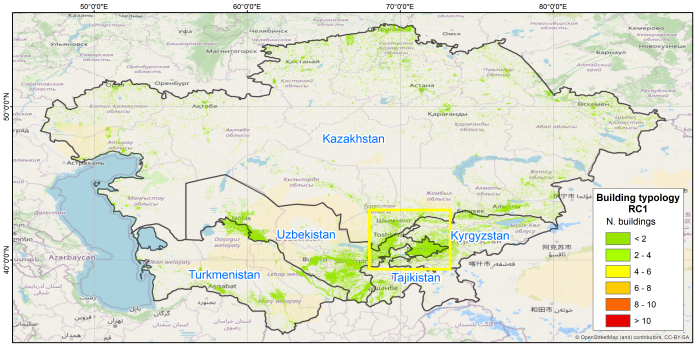
Results

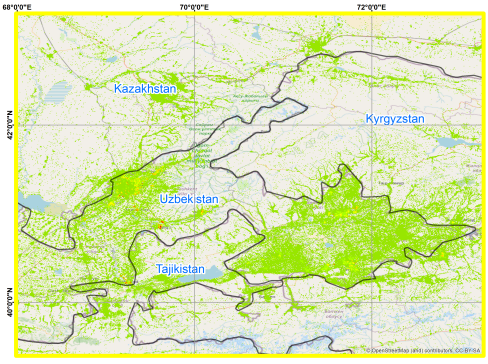
The results section is brief, clear, and very well written. I praise the authors for presenting the results of their work so well. I would even recommend the authors include a figure like Figure 3 for all the sub typologies but only if it is possible within their time constraints.

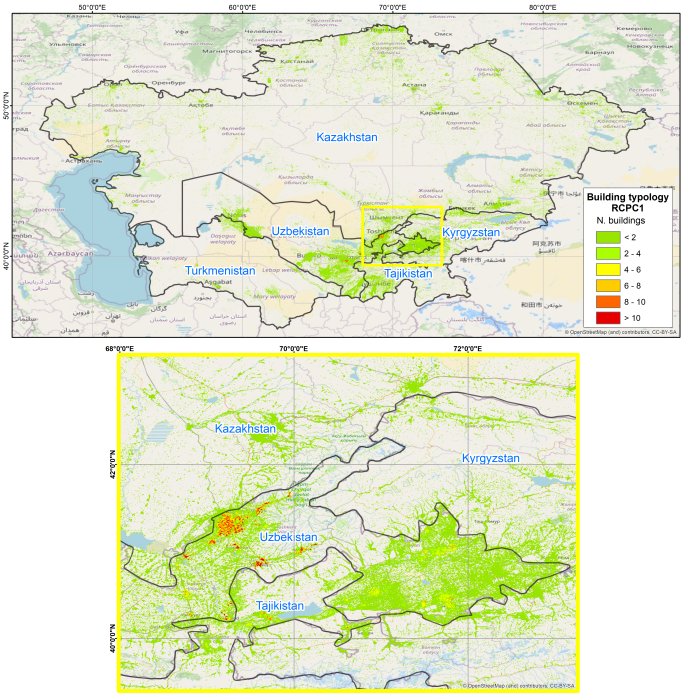
We generated new plots for Figure 3, which now shows the spatial distribution of three selected sub-typologies (URM1, RC1 and RCPC1) at the regional scale and for a selected area.
Figures are found below:











The only comment here is regarding Table 5. The building variation is mostly negative in the scenarios, given the population prospects of Central Asia. However, it was not explained in the methodology why the building count is decreasing. I believe that it is because the dataset is meant to include occupied structures only and abandoned or unoccupied structures are not being considered. This needs to be explicit in the methodology.
This is important because a sustained population decrease does not always lead to a sustained decrease in the exposed number of structures. For example, despite a steady decline in population, the number of total dwellings exposed in Albania rose by more than 25% between 2001 and 2011 (www.instat.gov.al). In other words, residential exposure in Albania (new dwellings become new buildings) is increasing despite the decreasing population, as there are other factors beyond population driving changes in residential exposure. Moreover, less population might lead to fewer occupied structures, but abandoned or unoccupied structures are still at risk of flooding, shaking, and causing debris, which will incur an economic loss for society. If the datasets the authors proposed account only for occupied structures, that is perfectly reasonable, but the limitations of this approach should be mentioned as well.
Thank you for the comment, we clarified the methodology explaining in a more explicit way how unoccupied structures are considered in the analysis. In our work, we distinguish between urban and rural areas. In urban or expected-to-be-urban areas, we only account for occupied structures, and we replace old structures with the new typologies according to set of rules previously defined. In rural areas the deprecated building types are maintained in the 2080 exposure layer in order to avoid underestimating the risk related to weak typologies which might still be in use, or not demolished, despite their age. In urban areas, abandoned or unoccupied structures are not being considered as they are assumed to be demolished during the process of building new structures. However, we agree with you that this need to be clear as it might lead to underestimate the overall impact (which might include unoccupied buildings as well). This was clarified in the text.

Discussion
Lines 345-350: The authors mention that the final data is provided in GED4ALL format. Is it open access already? Consider using ‘will be provided’.
At the time of the submission, the link for data sharing was not active. We included it in the data availability statement together with the license under which data are made available.

References
Please include the missing references
The missing references were added, thank you.