Responses to Dr Mario Salgado (Reviewer 1)

Thank you for your helpful review. Please find our answers to each of your comments below.

1. **General comments.**

   1.1. *>> "The topic of the paper fits well under the scope of the Journal and in my opinion, it should be accepted after addressing some minor comments that I include below"*

   Thank you. We really appreciate it.

   1.2. *>> "The manuscript is well organized and written (although a minor final review of English is suggested, there are a couple of typos and sentences that are not easy to read) and a careful review of recent references has been made".*

   Thank you for the nice comment about the structure of the paper. Following your advice, we have accordingly asked a native English speaker to provide us a strict language review. The new version has been significantly been improved in that regard.

2. **Specific comments**

   >> "Although the focus of the manuscript is mostly on the variable resolution level of the exposure databases, authors include too loss analyses for different earthquake (and tsunami) scenarios to assess the sensitivity of the different aggregation levels in the results".

   Indeed, the main focus of the paper is the construction of variable resolution building exposure models and to test their impact and benefits on risk assessment. Hence, we do not perform, *on purpose,* sensitivity analyses for different controlling parameters of the earthquake and tsunami scenarios to keep less degrees of freedom upon the main goal of the paper: exploring the uncertainties in the losses carried by the resolution of the exposure model.

   2.1. *>> "These loss analyses make some assumptions which consequences are not negligible and have been studied recently (even in some of the documents cited in the manuscript). For instance, for earthquakes with the characteristics used in the case study, for which each rupture has zones below Lima, assuming no cumulative damage (ground shaking and then tsunami wave) or not assessing the quasi-simultaneous occurrence of the losses can have consequences in the obtained results, mostly for the tsunami case."*

   In order to solely study the impact of the exposure resolution for the two-hazards, we make some assumptions that are clearly described since the beginning. Such as "*We present decoupled earthquake and tsunami scenario-based risk estimates for the residential building stock of Lima (Peru)".* We cite the recent study we are based on for that assumption (Petrone et al., 2020). However, in order to raise awareness of our assumptions, we have accordingly cited some recent studies that have explored the effects of cumulative damage (see end of the Introduction section).

We then totally agree with you: addressing cumulative damage is fundamental. The new manuscript version is better stressing this issue in the Discussion. Addressing this important but complex issue was beyond the scope of this paper which is already quite long. We have been working in another complementary publication regarding a novel methodology for multi-hazard risk. This has been already presented in the EGU-2020 conference (Gomez-Zapata et al., 2020). We expect to submit the associated actual journal paper with a rigorous methodology very soon. This explains the very last sentence presented in the Discussion section: *"Furthermore, it is worth investigating the usefulness of in mapping cumulative damage and losses in hazard sequences i.e. when a first hazardous event modify the fragility of buildings that are then affected by a successive event."*.

2.2. *>> "The EQ footprint was generated using only 1 GMPE which is known to be a highly sensitive component in the risk results. A discussion about how capturing the epistemic uncertainty (by any of the traditional methods typically used in PSHA) may (or not) affect the proposed"*

We totally agree. This selection is crucial. This is a special research topic of our team. However, the influence of the choice of GMPE, and other epistemic uncertainties, on the final loss estimates is not within the scope of this study. As formerly stated, we reduced on purpose the degree of freedom to analyse the impacts of the selection of the exposure models. The selected GMPE predicts the log median $ln\overline{Sa}$ and standard deviation σ of the spectral acceleration Sa(T) at periods T ∈ [0.01~PGA, 0.3s, 1.0s]. We explore the impact of using spatially uncorrelated or cross-correlated ground motion fields, which is nevertheless a step forward capturing epistemic uncertainties related to ground-motion modelling.

In the revised version we will added a note for clarity in this regard as well as the importance and uncertainties linked to this assumption. We cite a study that advised the use of logic trees for capturing the uncertainty in the GMPE selection. (e.g. Scherbaum et al., 2005). We point out once again the reference of Weatherill et al., (2015) in this section. This is because of its clarity when it points out: "*the choice of GMPE may influence upon the results depending on whether the inter- and intra-event components are homo- or hetero-skedastic, or due to the manner in which soil nonlinearity is accounted for in the functional form"*. Please note that in the latter reference the GMPE selection was neither within its scope.

2.3. *>> "The consideration of site-effects was performed using by combining two models with different resolution level (i.e. the city's microzonation and the Vs30 values when needed), made available in another study for Lima. However, being this a parameter that defines somehow the weights in the proposed aggregation scheme, a discussion of the possible impact of merging two datasets with different resolution to account for the soil response should be included in the manuscript".*

Indeed we have used the shear wave velocity in the uppermost 30 meters depth (Vs_{30}) as reported in Ceferino et al. (2018b). The resulting resolution of that dataset is 30 arc-seconds (~ 1km). We agree with the reviewer that that resolution might be coarse and a short note has been accordingly added in the Discussion section to raise awareness in this regard.

On the other hand, the weights used to derive the aggregation schemes are assigned to the customised focus maps (see section 3.3). Please note that only the population density and tsunami inundation are used to define the two types of focus maps presented. The spatial distribution of Vs30 values was never used to derive them. Perhaps the presentation of Figure 5 has caused that confusion. This figure is only displaying one of the resultant CVT-modes on top of the spatial distribution of Vs30 values in Lima/Callao, but is not an input to their derivation.

2.4. *>>"The nonlinearity of the soil response is assumed as negligible. However, the microzonation for Lima identifies zones with soils that typically have large nonlinear effects, particularly when subjected to large EQ intensities as the ones expected for events with Mw 8.5-9.0 (see zones III and IV). This aspect should be revised and discussed with more detail by the authors, instead of only pointing out to a reference which at the same time contradicts the findings and statements of others used".*

Please note that that the sentence the reviewer refers is included in the section "Construction of the focus maps" as a manner to justify why we did not include the expected ground motion to derive the focus maps. Nothing similar was included in Section 3.2 that refers to the hazard scenarios.

Having said the former, we are not pointing out that seismic amplification due to the soil site condition might not occur. And due to the scope of our paper, we do not perform any site response analysis (1D, 2D, 3D) after having obtained the simple GMPE-based ground motions. Nonetheless, we agree with the reviewer that providing a clarification in this regard will benefit the quality of the paper. Accordingly, we have provided a note in the discussion section that will benefit the understanding of the assumptions within our procedure for this large-scale study. Thus, we have decided to include in the updated version of the paper a recent citation (Aguilar et al., 2019) that clearly describes the importance of addressing soil-amplification in Lima.

2.5. *>> "Details of the bathymetric data for the case study are missing. These should be included in full since they have a direct effect in the outcomes of a tsunami scenario analysis".*

Yes, the bathymetric and topographic data are absolutely essential for tsunami simulations. However in the given study we do not aim at the validation of the model for a given scenario which would require the most adequate topography model available. Rather we investigate the tsunami impact for varying magnitudes and strive for a systematic comparison by varying only very few parameters. But of course it is a crucial point and following this comment we included more details on the data used for the simulations.

Accordingly we have updated the new version of the manuscript as follows: *"The model bathymetry and topography were built from several data sets. The ocean part is based on the GEBCO bathymetry (General bathymetric chart of the ocean, GEBCO_08 Grid, see http://www.gebco.net). The coastal topography is given by SRTM values (Shuttle radar topography mission, 30m resolution, see https://www2.jpl.nasa.gov/srtm/) whereas in the pilot area Lima/Callao additionally the measurements of the TanDEM-X mission (see* (Krieger et al., 2007) *given at a spatial resolution of 12m were provided to the RIESGOS consortium. In this region, the available data sets were combined to a joint product and augmented by nautical charts in shallow areas by the project partner EOMAP. All these data were bilinearly interpolated to the triangular mesh and slightly smoothed to allow for stable simulations".*

2.6. *>>"Section 3.5 should include, for a better understanding, a graphical distribution of the nodes (geocells) after using one or another aggregation scheme. Even if Table 1 shows some interesting information, it is not easy to imagine the changes from one to another. Something similar to what is shown in Figure 4b, but for each of the aggregation schemes.*

We have been considering this suggestion carefully but providing independent printed figures for each resultant CVT-based for the other models (with 10,000 and 50,000 geocells) would not necessarily lead to a better visualization upon printed figures. It is already quite difficult to see independent cells in figure 4b (only 5,000 geocells). Considering the large size of the study area as well as the limited are for a printed figure, some geocells will be basically displayed as point clouds. We consider that the subset study areas that are provided in Fig. 10, 14, 15 may be useful enough to provide a visual comparison between the various models upon the actual printed version, and most importantly, along with their respective normalized loss metric.

Nevertheless, following your advice, we have decided to provide the data models and scripts that we have constructed during the elaboration of our study. These models are supplementary data to the paper. They are assigned an independent DOI, and are accordingly cited in the new version of the manuscript. We consider that this is a transparent approach that could also benefit future readers who will be able to better understand, reuse and cite these datasets. Examples can be found accessing the following revision links:

https://dataservices.gfz-potsdam.de/panmetaworks/review/f932840b5c130da18c3a9d407e85f086ce0874b80edbd796e0f096ba94d89cc4/

https://dataservices.gfz-potsdam.de/panmetaworks/review/0470cd1366982c5e319c5c39ca1c2e524b213d8f9b7868c98aad804fceba33d0/

2.7. *<< "The conclusion of line 588 could be reached by performing a graphic and direct comparison between the curves by De Risi and Suppasri. What was the purpose of adding that comment? How did the results of this manuscript change the perception or expected outcome of these two TS vulnerability models?"*

We greatly appreciate this suggestion. It has allowed us to realise that perhaps the differences between the models might not only due to the parameters that made up the fragility functions, but also due to the aggregation of different building classes (Suppasri) into a less diversified one (de Risi). This might further impact the risk loss estimates because of the selection of the replacement cost values for these building classes (lines 434- 436). Although we had already pointed out these types of limitations (see lines 539 – 541), a sentence about this aspect has been included in the Discussion section. This comparison has also allowed to realise the importance of locally calibrated financial consequence models for any type of hazard-related physical vulnerability.

The reviewer should however note that the Suppasri et al., (2013) and De Risi et al., (2017) schemes, comprise different building classes (see figure 6 in the submitted paper). For instance, Suppasri define three types of wooden building classes (W1, W2, W3) whilst De Risi only accounts a single wooden class. As explained, this is because the storey range is only addressed in the first one. Moreover these models were derived in a very different manner whose implications are described in our paper as well in each of them. They even have different validity ranges of tsunami inundation height (20 m versus 10 m respectively). They have different number of damage states (six in Suppasri, five in De Risi (it omits the damage state # 1) (see table 4 of the respective publication). The latter was mentioned in lines 435- 437 in our submitted paper. Therefore, we consider that it would not very meaningful to the reader if we provide a graphical comparison of these two fragility models. Accordingly, in the journal paper we would prefer to avoid doing so.

Nonetheless, you can find on Figure 1 such a graphical comparison for the wooden building classes. Please note that the De Risi models are the same throughout the three subplots and the selected range of validity is 10 m. The damage state D2 of De Risi model was slightly modified only for graphical purposes (to avoid the 0.0 value that is reported in Table 4 of that study).

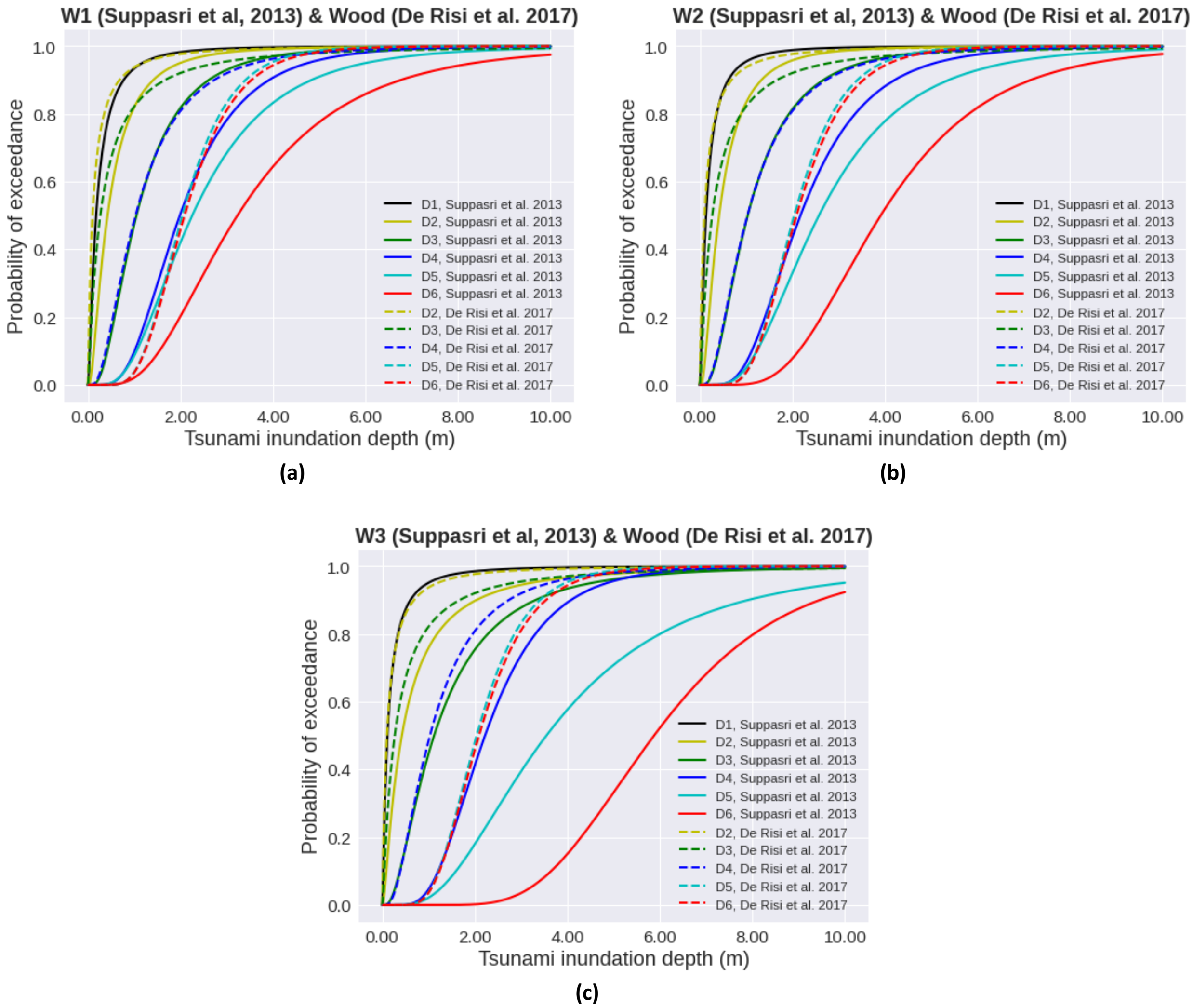

Figure 1. Graphical comparisons between flow-depth-based empirical fragility functions used in the submitted manuscript for wooden building classes (a) W1 (one story), (a) W2 (two stories), (c) three stories within the (Suppasri et al., 2013) scheme and the single material-based "wooden" class according to De Risi et al., (2017).

2.8. *>> "It would had been interesting too to include as a set of TS vulnerability functions, the ones derived for a neighboring location in the Pacific by Medina et al. Even if that works only considers one typology, having a more local overview may be insightful and allow having a better understanding of the risk results as a function of choosing one or another vulnerability set".*

We absolutely agree. That is the reason why we included lines 534- 536 in the submitted version of the paper the reviewer: *"The comprehensive adaptations of such as "foreign" fragility models (e.g., Suppasri et al., 2019) in Peru, as well as the need of future development of analytical models for the South American context (e.g. Medina et al., 2019) would benefit future risk assessment studies for Lima".* Moreover a graphical comparison of some of these fragility functions is presented (for collapse damage state) in Paez-Ramirez et al., (2020) and we want to avoid receptivity. We have decided to cite the latter study for such a purpose.

Furthermore, as the reviewer is correctly pointing out, this comparison would not be very meaningful at this stage because Medina's model is derived analytically and only considers a unique building class. Contrary, Lima has a more heterogeneous building portfolio. Nevertheless, as a follow-up of our response presented in point 2.1, we let the reviewer know that expansions of the Medina's models (for more building classes) are being used in the aforementioned study about multi-hazard risk addressing cumulative damage we are currently working on.

2.9. *>> "Authors mention that future research on EQ and TS fragility models should address several aspects, among which the hazard-sound aggregation entities are included (L 603). Vulnerability/fragility models are typically developed for typologies which are insensitive to the aggregation level and/or the scale of the analyses. Please elaborate more about how the hazard-sound aggregation entities can/should be used for enhancing the vulnerability models."*

Starting in line 603 we stated in the submitted version: *"… importance about keep working on seismic and tsunami fragility models that consider particular construction practices, local hydrodynamics,* ***and remarkably, the relevance of hazard-sound aggregations entities for exposure modelling and loss mapping****".*

We guess that that you refer to the sentence in bold. We are not pointing out that fragility/ vulnerability functions should be derived or improved from aggregation entities. We do not see how our study can contribute to that idea. We have included that sentence in the conclusion section as manner of wrapping up the advantages of using CVT models (i.e. hazard-sound aggregation entities) for exposure modelling (i.e. aggregation of the building stock), risk computations, and loss mapping (i.e. visualization of the results). Therefore, we believe that apparently there was a slight misinterpretation of our statement. Nonetheless, it is important because we can clearly see that these two sentences must be separated to ensure independent interpretations by other readers. Accordingly, in the corrected version we have modified as follows: *"Nevertheless, these aspects highlight the importance of improving on seismic and tsunami fragility models that consider particular construction practices, local hydrodynamics.* ***This study also highlights the relevance of hazard-based aggregation entities for exposure modelling, risk computations and loss mapping****".*

We deeply thank the reviewer for the time invested in providing us the very constructive feedback and comments.

With best regards

The team of authors

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