# General comments

The manuscript by Gomez-Zapata et al. address a topic of interest in catastrophe risk modelling, regarding the balance between an adequate resolution level for the exposure aggregation and the computational resources needed for performing the risk assessment. A methodological proposal based on CVT is presented, followed by a case study that consider subduction earthquake scenarios that trigger tsunamis with different characteristics, having these two perils different resolution level requirements. 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.

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.

# Specific comments

* Authors take for granted the conclusions of some studies to support the findings of this manuscript. However, in some cases (see the following point), for the characteristics of the events considered in the case study these do not hold valid and need revision.
* 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. 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.
* 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
* 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.
* 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.
* 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.
* 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.
* 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?
* 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.
* 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.