The paper compares potential impacts of near field and distant earthquakes. The results show the need to take into account minor but proximal faults when addressing seismic risks in urban areas. In my opinion the paper is of good quality and may interest many readers. I suggest to accept it with the following revisions.

We would like to thank the reviewer for their instructive feedback and comments regarding our manuscript. Below we explain below how we have addressed each of the issues raised.

1) I'm not sure that the large impact of local/crustal earthquakes compared to subduction earthquakes is a new result. Similar conclusions have been reached in the 90s during the Earthquake Risk Management of the Quito city (Chatelain, J., Tucker, B., Guillier, B. et al. Earthquake risk management pilot project in Quito, Ecuador. GeoJournal 49, 185196 (1999) doi:10.1023/A:1007079403225). Other references may exist.
- Thank you for bringing this reference to our attention. While we agree that the local vs distal earthquake impact conclusion is not new, we are the first to show it using a scenario based earthquake risk analysis for the specific case of Santiago. We have included the highlighted reference in section 5.2 of the discussion to clarify this point.

2). Are the selected GMPE's similar to the one selected by the SARA project ? If not, why ?
- Yes, the GMPEs selected in this study are the same as the ones used in the SARA study as well as the study of Villar-Vega et al, 2017 (an output from the SARA project).

3) The exposure model is "aggregated" at the census district level. The active faults are however "near field" and close to faults the ground-shaking intensity is highly distance dependent. The authors should better describe the distance computation between the faults and the exposure assets. Which distance definition is used ? Is this distance definition similar for all GMPE's ?
- To remain consistent across the GMPEs we implement the form of the equations that uses the Joyner-Boore distances - defined as the shortest horizontal distance from each exposure element to the surface projection of the rupture area. We have clarified this point in section 3.2 of the manuscript.

4) My understanding is that the building are homogenously distributed within the district which is not the case in reality. Such spatial homogenization may introduce a bias. The author should discuss the potential effects of this homogenization (and even test it using various random buildings distributions in each district cell)
- The reviewer is correct that the buildings are homogenously distributed within each grid cell (1x1km). We believe this is sufficient for our study because in the end we aggregate up and present the results at the district level. Therefore any variations and/or bias introduced due to the equal distribution is minimised.

5) The authors used the vs30 values from the Bonnefoy et al. (2009). This paper is however not deriving such vs30 values but resonant frequencies from H/V values. I then do not understand how vs30 values have been obtained.
- Thank you for pointing out this error. The site study was initally conducted by Pasten 2007 for his Masters thesis where he made 264 measurements of the Vs30 for the local soils.

6) Epistemic uncertainties are large for such risk computations. Such epistemic uncertainy is taking into account only for the GMPE part (for which several GMPE's are

considered). The resulting uncertainty is however never shown in the paper (since the authors consider an average GMPE model). It would be interesting to show (on Figure 10) the results for each GMPE (and not the average) to illustrate (at least once) the impact of the modelling epistemic uncertainty on the results.

- Yes, we attempt to take into account the epistemic uncertainties in our modelling by using several GMPEs, which we average for our reported loss numbers. To avoid cluttering Fig. 10 in the manuscript we have included a similar figure but with the losses for each GMPE in supplementary Fig. S10.