Response to Referee 3 (Dr. Thomas Ulrich)

Thank you for providing us with comments and suggestions on our manuscript. In this response, the referee's comments are shown with bold letters to distinguish from our point-by-point responses. In the revised manuscript, all changes are highlighted with yellow.

R3-1: In this study, the results of the 2x500 scenarios are mostly analyzed in terms of 10/50/90 percentiles. One could argue that many interesting aspects of the scenarios remain unexploited and that such an approach is suboptimal for quantifying the hazard variability. For instance, families of scenarios, characterized by similar patterns might be identified by analyzing the results, using e.g. using a clustering algorithm. E.g. in fig5a, we can see that some waves start with a trough while others with a crest. It is likely that similar features might be identified and linked to specific earthquake characteristics or inundation patterns. Could you comment on this idea?

R3-1: Our intention of using 10th, 50th, and 90th percentiles is to represent a range of tsunami hazard results from the 1000 Monte Carlo tsunami simulations. We do not have strong theoretical reasons for this choice, and other percentiles could be selected. As part of our investigations, we do examine the distributional aspects of various quantities, such as magnitude (source parameters), wave profile-related metrics (peak and trough amplitudes), and inundation-related metrics. Our general observations are that they are usually not normally distributed and show significant skewness towards right. As such, we tend to focus on three representative percentiles, such as 10th, 50th, and 90th percentiles, to capture the range. We acknowledge that these percentiles may not be optimal; for different tsunami hazard parameters, we wanted to use the same percentiles (although their distributional characteristics differ depending on parameters, locations, etc.).

We appreciate Referee 3's suggestion for clustering analysis in linking tsunami wave profiles and tsunami inundation metrics. The suggested idea is interesting but we have not looked into such a relationship.

We would like to clarify our intension in presenting the results in the manuscript. Our ultimate goal was to relate earthquake source characteristics with tsunami hazard metrics (and eventually we would like to evaluate tsunami loss for a building portfolio). For the tsunami hazard metrics at local community levels, we chose inundation areas, because we found that inundation areas are highly correlated with local/regional tsunami loss (Goda et al., 2019). For the earthquake source parameters, we used conventional parameters, such as magnitude (Figure 10a,b) and earthquake slip (Figure 9), as well as other parameters, such as tsunami potential energy (Figure 10c,d), as promoted by Melgar et al. (2019). We did not focus on peak or trough offshore tsunami height amplitudes as they are intermediate results from our overall goal, but we thought that showing results, such as offshore tsunami profiles and tsunami heights, is useful in conveying tsunami hazard characteristics – this is the reason why we included Figures 5-7.

References:

Goda, K., Mori, N., and Yasuda, T. (2019). Rapid tsunami loss estimation using regional inundation hazard metrics derived from stochastic tsunami simulation. International Journal of Disaster Risk Reduction, 40, 101152.

Melgar, D., Williamson, A.L., and Salazar-Monroy, E.F. (2019). Differences between heterogeneous and homogenous slip in regional tsunami hazards modelling. Geophysical Journal International, 219, 553–562.

Q3-2: The probabilistic approach generates many more earthquake scenarios than a deterministic approach, but some scenarios may look unrealistic. E.g. scenario of Figure 13b, showing 2 distant asperities hardly connected, and linked by an extended zone of limited fault slip. Do you think that such a scenario may be representative of a megathrust earthquake and how to ensure that all scenario earthquakes are realistic?

R3-2: Our synthetic scenarios are obtained through stochastic simulations and as such all of individual scenarios may not have 'realistic' features. As ensemble, these source models have desirable source characteristics similar to previous source inversion models for major subduction events. We do apply various constraints to remove scenarios with unrealistic features; these constraints are based on empirical findings from past inversion analyses (e.g. aspect ratio, slip concentration, asperity size, and spatial distributional feature) as well as seismotectonic aspects in a subduction zone of interest.

Having mentioned that some of the stochastic source models may appear unrealistic, we are also cautious about having a very strong opinion on how future earthquake slips should look like. Our view is that we may not have seen all possible rupture patterns to date and thus our knowledge and understanding may be limited. In other words, we take a loose view/approach and we use stochastic source modeling methods as a guide to generate possible scenarios that could be used for tsunami hazard analysis. We again emphasize that our methods are constrained by various geological and geophysical knowledge (via, for instance, fault plane model and source parameter scaling relationships). We do not claim that our models are perfect nor complete, but we think that our model results are useful for tsunami disaster risk management.

For the particular case shown in Figure 13b, we think that a different interpretation is possible. From the viewpoint of final slip distribution, we see two asperities, which are separated distinctly (i.e. one in western Nankai and the other in eastern Tonankai). In our simulation, we performed kinematic rupture simulation (in a simple way) and thus the rupture starts near one of the asperities and the rupture front propagates towards the other asperity – therefore, there is a time lag between these two local asperity ruptures in our model. Such a lagged rupture may be possible for the Nankai-Tonankai Trough earthquakes as there exists a segmentation boundary near the tip of Kii Peninsula between the Nankai and Tonankai regions. In fact, such a lagged rupture case was also considered in the Cabinet Office's report. We are aware that the segmented rupture modeling is challenging, and more physics-based approaches are necessary to tackle/resolve the problems in a fundamental way. This is beyond the scope of our study.

Q3-3: The earthquake displacements are calculated based on the Okada approximation. Accurate 3D tomographic models exist for Japan, which could be combined with advanced modeling technics. I would expect the computed displacement field to be significantly affected by the accounting of a more realistic 3D Earth structure. Could you comment on this limitation?

R3-3: We agree with Referee 3 that using of 3D Earth model together with more advanced models for computing rupture deformation will improve the accuracy of tsunami modeling and tsunami hazard assessment. As pointed out by Referee 3, the Okada formula are based on a half space and uniform property of the Earth. Use of such advanced models would be particularly useful for capturing the variable rigidity at shallow depths and near the trench, and thus would be able to characterize the tsunami generation mechanism in the subduction zone accretionary wedge.

Q3-4: Many plots use a rainbow color map, which is characterized by many known flaws. See e.g. http://www.fabiocrameri.ch/endrainbow.php. You could use perceptually uniform colormap, which would prevent misled interpretation of your data.

R3-4: We appreciate the suggestion. We will consider different color schemes in our future studies.

Q3-5: Some of your figures present large areas of saturated color (e.g. fig 13l). I think it would be wise to avoid that. (or at least write the maximum value in a corner of the figure).

R3-5: We understand Referee 3's point. Our intension in selecting such a maximum value for plotting purposes is that we wanted to ensure that the important ranges of the data/results are shown by different colors (i.e. not saturated color, like dark red or dark blue). For example, when we show local elevation levels (e.g. Figure 4b,c), we set the maximum elevation at 50 m; this value is selected because it is unlikely that the maximum run-up height exceeds this level (noting that the run-up of 41 m was observed during the 2011 Tohoku event). Similarly, we used the maximum value of 10 m for inundation depth in Figures 13 and 14. Based on the tsunami damage observations during the 2011 Tohoku event (De Risi et al., 2017), severe damage would happen to many structures when inundation depths reach 10 m. We also wanted to use the same color range for different cases (from *M*8.7 to *M*9.1, different percentile levels, and different locations) to facilitate the visual comparison.

Reference:

De Risi, R.*, Goda, K., Yasuda, T., and Mori, N. (2017). Is flow velocity important in tsunami empirical fragility modeling? Earth-Science Reviews, 166, 64–82.

Q3-6: L80: 'A megathrust subduction event occurs when the accumulated slips are released forcefully and triggers intense ground shaking and a massive tsunami.' I'm not certain that every megathrust event releases a massive tsunami.

R3-6: With this sentence, we did not mean that this is always the case. In the revised manuscript, we add '*is capable of*' in front of '*trigger(ing)*' to indicate that such generation of shaking and tsunami is still a possibility, not certainty.

Q3-7: L158: the mu character appears as a square.

R3-7: Thank you very much for pointing this out. We corrected this error in the revised manuscript.

Q3-8: L200: Could you comment on the choice made for the assumed rupture velocities and rise time (e.g. by adding proper references?).

R3-8: Agreed. The 2012 Cabinet Office's tsunami hazard model (i.e. 2012 CDMC model) considered the rise time of 1 minute and the rupture propagation velocity of 2.5 km/s. We mentioned this information in Section 2.2. Then, in Section 3.1, we mentioned our simulation set-up. We modeled the rise time for *M*8.9-9.1 scenarios as truncated normal variate with mean equal to 60 s (same as the 2012 CDMC models), standard deviation of 10 s, and lower/upper bounds of 50 and 70 s. For *M*8.7-8.9 scenarios, we shortened the rise time and shifted the distribution of the rise time by 10 s. For the rupture propagation velocity, we modeled the propagation velocity for all scenarios as truncated normal variate with mean equal to 2.5 km/s (same as the 2012 CDMC models), standard deviation of 0.5 km/s, and lower/upper bounds of 2.0 and 3.0 km/s. Except for the mean rise time for *M*8.9-9.1 scenarios and the mean propagation velocity, these adjustments are not based on any specific references (i.e. our assumption).

In the revised manuscript, we added '*(based on the 2012 CDMC models)*' and '*(note: the standard deviation and the lower/upper bounds are assumed)*' to indicate which information is based the 2012 CDMC models and which information is our assumption.

Q3-9: L317: The definition of the slip ratio is not very clear. Is it ponderated by the area times the average slip on each segment?

R3-9: We calculated the slip ratio for a segment of interest as summed slip over the segment divided by total slip over the entire fault plane. In the revised manuscript, the sentence is modified as follow: '*The second source parameter is the slip ratio, which is calculated as the summed slip within a specified segment divided by the total slip over the entire fault plane*'.

Q3-10: L320: ET is wrongly formatted.

R3-10: Thank you very much for pointing this out. We corrected this error in the revised manuscript.

Q3-11: L373: ‘Out of all 1,000 scenarios, there are 5 cases and 1 case where the maximum inundation depths at the vertical evacuation towers in the Ogata and Saga districts exceed the critical water depths. The chances of such exceedance are low, and these scenarios can be regarded as very extreme.’ I agree that 6 scenarios out of 1000, is not much. But it is worth noticing that the likelihood of each scenario is unknown. Please comment if necessary.

R3-11: Thank you for pointing out this. Referee 3 is right that our work does not address on the occurrence probability of earthquake scenarios considered. The same issue was pointed out by other referees. Following these suggestions, we change the paper title to: ‘*Uncertainty quantification of tsunami inundation in Kuroshio Town, Kochi Prefecture, Japan using the Nankai-Tonankai megathrust rupture scenarios*’. In the paper, we stress that our assessments are conditional.