Response to Handling Editor (Prof. Maria Ana Baptista)

All reviewers agree that your paper should be accepted for publication after minor revision. I believe you should consider changing the title following the suggestion by referee #6. Please check carefully the comments provided by the referees

Thank you very much for handing our manuscript promptly and carefully.

Responding to Reviewer 6's suggestion, we have changed the paper title from '*Probabilistic tsunami inundation assessment of Kuroshio Town, Kochi Prefecture, Japan considering the Nankai-Tonankai megathrust rupture scenarios*' to '*Uncertainty quantification of tsunami inundation in Kuroshio Town, Kochi Prefecture, Japan using the Nankai-Tonankai megathrust rupture scenarios*'. We believe that we addressed the majority of the issues raised by six reviewers.

The detailed point-by-point responses are given in the following pages. All changes that are made during the revision are highlighted with yellow.

Response to Anonymous Referee 1

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.

Q1-1: I found a typo in line 431 where 'exiting evacuation towers' should be replaced by 'existing evacuation towers'.

R1-1: In the revision, we corrected the typos (exiting) at two places.

Response to Anonymous Referee 2

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.

Q2-1: Fig. 11b,d. there is only three CDMC event (filled square). I wonder where are the other 9 cases.

R2-1: For the case of the vertical evacuation tower in the Saga district, tsunami inundation did not happen for 8 cases out of the 11 CDMC tsunami source models. These cases are not included in Figures 11b and 11d. To clarify this in the manuscript, the following sentence is added: '*In Figure 11, the inundation results based on the 11 CDMC source models are also presented, noting that the vertical evacuation tower in the Saga district is inundated for 3 cases out of the 11 models (i.e. model 4, 5, and 10; see Figure 2) and thus only three square markers are shown in Figures 11b and 11d.*'

Q2-2: Line 231, The reference elevation of bathy is defined as the standard altitude in Japan. It may not be clear to readers. Is it equivalent to Mean high water levels or others? Does tidal variation is negligible in this site?

R2-2: We appreciate the referee for pointing out this unclear aspect. In our tsunami simulations, bathymetry-elevation data provided by the Cabinet Office of the Japanese Government are used as they are. The bathymetry-elevation data are defined by Tokyo Peil (Japanese standard altitude) which is close to mean sea level within 0.2 m, depending on location. Additionally, as mentioned in the manuscript, no sea-level (tidal) variations are considered in our tsunami simulations. The reference sea level does not correspond to the mean high water level. We do not intend to mean that the effects of tidal variation are negligible. To clarify this point in the manuscript, the following sentence is added: '*However, the effects of tidal variations (e.g. mean high water level) are not considered in the simulations.*'

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.

Response to Anonymous Referee 4

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.

Q4-1: The main comment that I have with regard to the manuscript is with regard terminology that would be confusing to most readers. "Probability" is used in the manuscript to describe tsunami inundation hazard assessment. This can be easily confused with probabilistic tsunami hazard analysis (PTHA), which is a very different method than what is performed in this study (see Grezio et al., 2017 for details). PTHA is an aggregation of tsunami rates and heights from different sources, including uncertainty (i.e., aggregate aleatory uncertainty is integrated into the rate calculations). Probability in PTHA is also given in terms of a particular exposure period. Neither of these aspects are considered in this study. I strongly recommend that this type of study be termed as "uncertainty analysis" or "uncertainty quantification".

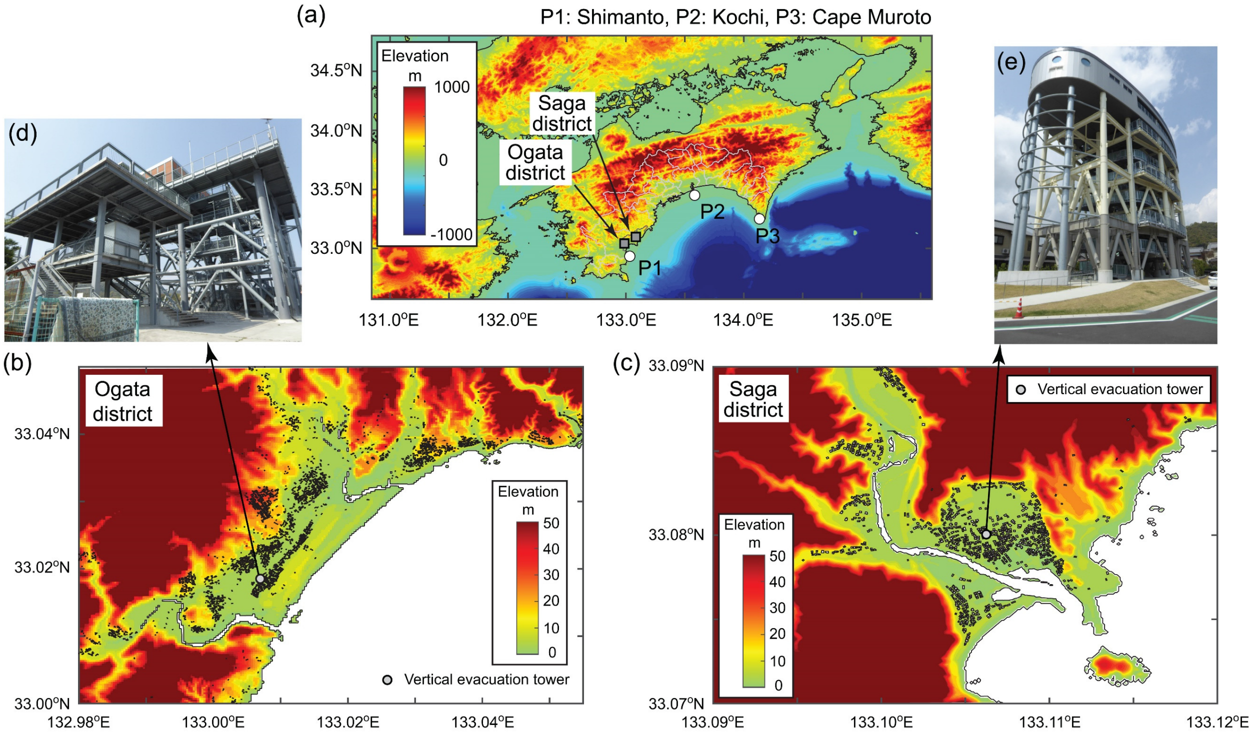
R4-1: We understand the concern by Referee 4. Our study is conditional on earthquake scenarios falling within a specific range of magnitude, and does not address on the occurrence probability of these events nor cover the entire range of the magnitude. In the revised manuscript, we change the paper title to: '*Uncertainty quantification of tsunami inundation in Kuroshio Town, Kochi Prefecture, Japan using the Nankai-Tonankai megathrust rupture scenarios*'.

Q4-2: Probability is also used to describe the scaling relationships. These are better termed as empirical or statistically derived scaling relationships. They are only probabilistic in the sense that the residuals are distributed according to some probability distribution. For example, standard linear regression assumes that the residuals are normally distributed, but this type of regression is rarely if ever called probabilistic. The same would hold if the residuals were distributed as a lognormal, Poisson, etc. distribution.

R4-2: Agreed, and as above, we understand the concern by Referee 4. Throughout the manuscript, we avoid the expressions like '*probabilistic*' tsunami hazard assessments and '*probabilistic*' scaling relationships.

Q4-3: It would be helpful for the authors to clarify the names of the study areas, especially for those unfamiliar with geography in Japan. In the title, Kuroshio Town is referred to. In Figure 4, does Kuroshio Town encompass the Ogata and Saga districts? It would be helpful if the boundaries of these districts, Kuroshio Town, and Kochi Prefecture were included in Figure 4.

R4-3: We created a figure requested by Referee 4 by showing political boundaries for Kochi Prefecture and Kuroshio Town as below. In our opinion, it is not easy to see the boundaries and these boundaries interfere with other mapped objects in the figure (and even with high resolutions, it would be difficult to see these boundaries in the properly formatted paper). Since we have indicated these political boundaries in Figure 1, it may be better to mention this in the Figure 4 caption so that readers can check the locations of Kochi Prefecture and Kuroshio Town. For this reason, we added the following sentence in the caption of Figure 4 in the revised manuscript: '*The Ogata and Saga districts are located in Kuroshio Town, Kochi Prefecture. The locations of Kuroshio Town and Kochi Prefecture are indicated in Figure 1.*'

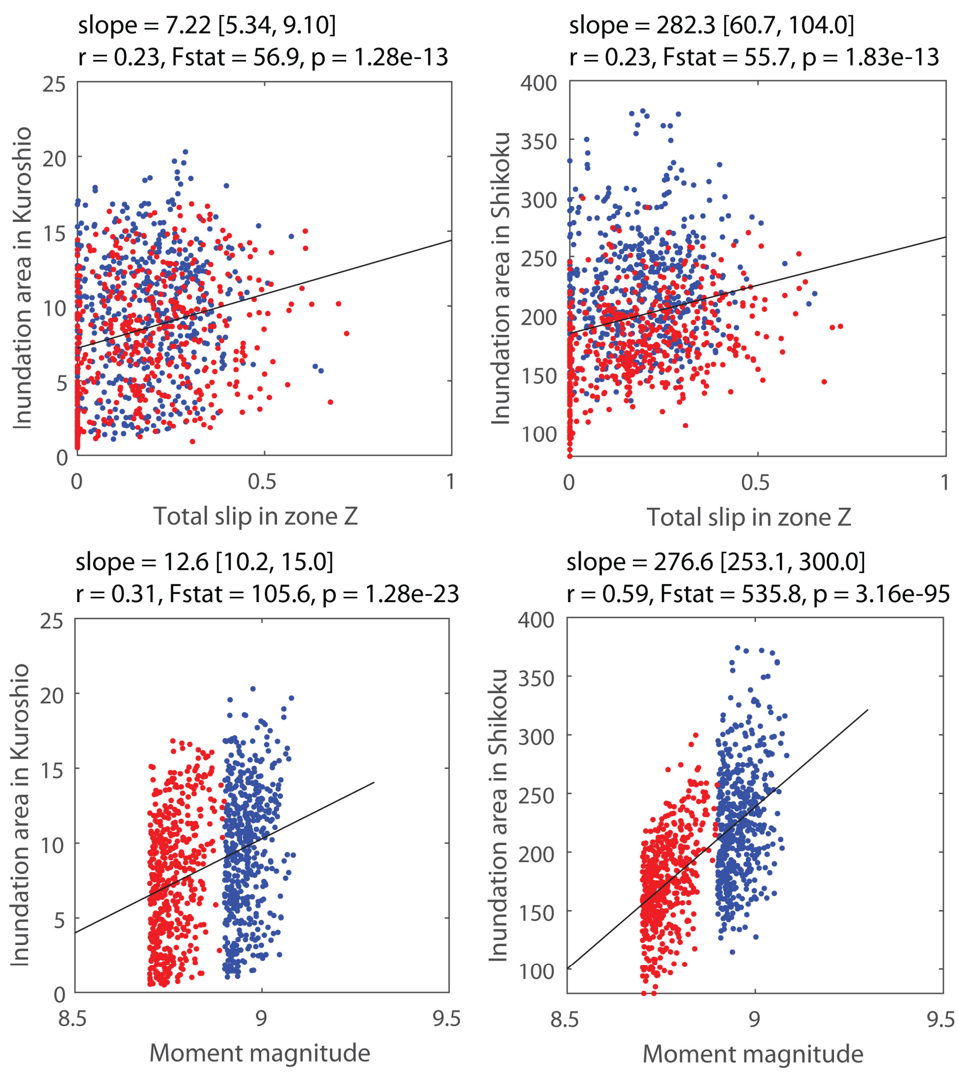


Q4-4: The scatter plots (Section 4.2) exhibit very weak correlation, likely because the effects of Green's Law have not been considered. Some discussion as to this effect would be helpful. Also, results of the regression F-test would be helpful in these cases to determine whether dependence on a particular parameter is statistically significant.

R4-4: As Referee 4 points out correctly, the scatter plots for slip ratios for segment Z (Figures 9a,b) and those for moment magnitude (Figures 10a,b) do not show strong dependency on inundation areas for Kochi (regional) or Kuroshio (local). These are consistent with r values indicated in the figures. The effects of Green's law are implicitly considered by the nonlinear shallow water modeling but is not clear in our results. This is because the we didn't compare tsunami height change from offshore to onshore, although we analyzed Mw-inundation area relation in Section 4.2.

Furthermore, as suggested by Referee 4, we checked the linear regression statistics of these scatter plots shown in Figures 9 and 10. (Note that the detailed discussions of the regression analysis results, such as developing statistical relationships for prediction purposes are not of our direct interest). We report that even for Figures 9a and 9b, the positive linear dependency is observed via linear regression analysis; the confidence interval of the slope parameter does not include zero, F-statistics is large, and the corresponding *p* value is small (less than 0.01). Nonetheless, from visual inspections, when the r value is relatively small, e.g. less than 0.2 in absolute values, the linear dependency between the hazard metric (i.e. slip ratios, magnitude, tsunami potential energy, or other parameters we tested) is not obvious and obscured by the large scatter of the data points. In the revised manuscript, we added the following sentence: '*Although some of the identified trends between the inundation areas and slip rations are weak (e.g. Figures 9a and 9b), the slope coefficients of these relationships are found to be significant (i.e. non-zero) based on the p-values.*'

Just as illustration of specific cases, we show such linear regression plots for slip ratio in segment Z and for moment magnitude below. In the figures, slope parameters (with confidence interval), r value, F statistics, and *p*-value are indicated.



Q4-5: L30, 32: Please provide references for magnitudes of historical earthquakes.

R4-5: We took magnitude values from Fujiwara et al. (2020).

Reference:

Fujiwara, O., Aoshima, A., Irizuki, T., Ono, E.,Obrochta, S. P., Sampei, Y., Sato, Y., and Takahashi, A. (2020). Tsunami deposits refine great earthquake rupture extent and recurrence over the past 1300 years along the Nankai and Tokai fault segments of the Nankai Trough, Japan. Quaternary Science Reviews, 227, 105999.

Q4-6: L35: "which occurred on a megathrust that was originally thought to only rupture in smaller segments"

R4-6: The segmented rupture model was a standard for the Tohoku region (and elsewhere in Japan) prior to the 2011 Tohoku earthquake and tsunami. This fact is indicated in the Cabinet Office's report. In the revised manuscript, we cite an opinion paper by Stein and Okal (2011), which raised issue associated with such a segmented model.

Reference:

Stein, S., and Okal, E.A. (2011). The size of the 2011 Tohoku earthquake need not have been a surprise. EOS, 92, 227–228.

Q4-7: L114: The slip models shown in Fig. 2 do not look like they are derived from a circular crack model. Is this a surface-rupturing crack? Some clarification is needed. It would also be helpful to compare CDMC models with those using the authors' stochastic slip model.

R4-7: Based on the report on the Nankai-Tonankai tsunami model by the Cabinet Office, the circular crack model was used to calculate the average slip as a function of stress drop. But the report indicates that the earthquake slip is distributed (or determined) based on the assumed asperity models as well as geodetic information on individual segments (slip rate is not uniform along the strike).

Q4-8: L138: fault -> rupture

R4-8: Agreed and this change is made in the revised manuscript.

Q4-9: L144: Not sure what "synthesis" means here.

R4-9: We change it to 'simulation'.

Q4-10: L158: "mu" is not typeset in equation.

R4-10: Thank you very much for pointing this out. We corrected this error in the revised manuscript.

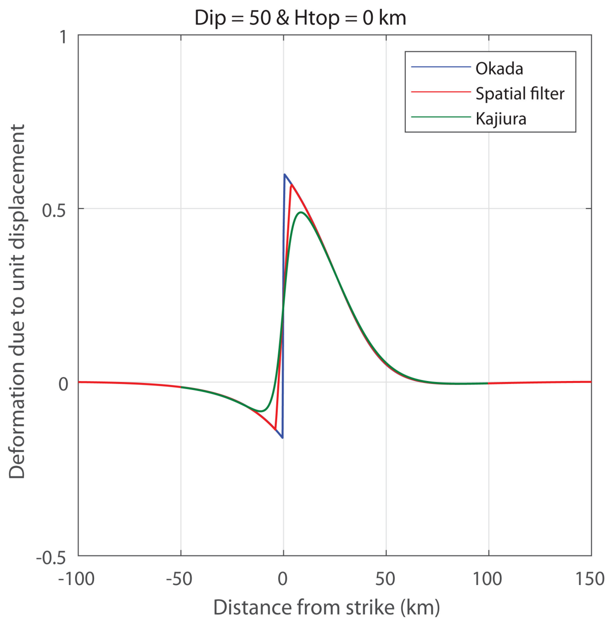
Q4-11: Section 3.2: Please indicate how displacements are calculated for surface rupturing earthquakes, compared to imbedded earthquakes.

R4-11: We used Okada equations to calculate elastic displacements (vertical and horizontal) due to the fault movement and used Tanioka-Satake equations to obtain the final vertical displacement. Subsequently, we applied the spatial smoothing filter (9 grids by 9 grids, with grid size of 810 m), as was done for the 2012 CDMC models. In the 2012 CDMC models, the top of the fault plane was assumed to reach the ocean bottom (i.e. 0 km depth in calculating the Okada displacements; as mentioned in Section 2.2). We used the same fault plane geometry as the 2012 CDMC models. Therefore, for the subfaults that are most offshore along the Nankai-Tonankai Trough, earthquake ruptures are not embedded. In other words, for these subfaults, the discontinuous rupture profiles are obtained along the trough.

Q4-12: L244: How does this filter compare to Kajiura's 1/cosh(kh) filter?

R4-12: We compare the Okada deformation (vertical only), spatial filtered deformation (15-grid size with 0.5 km grid size, which is approximately similar to 9-grid size filter with 0.81 km grid size), and Kajiura filtered deformation. To make a suitable reference to the published paper, we used the same rupture scenario as considered by Glimsdal et al. (2013). More specifically, the rupture scenario is: unit reverse displacement of a rectangular fault with L = 100 km and W = 50 km. The dip angle is set to 50 degrees and the top of the depth is set to 0 km (i.e. surface rupture). Glimsdal et al. compared the Okada-based cross-section profile with the Kajiura-based cross-sectional profile for this rupture (see Figure 2 in their paper). In addition, a comparison for dip = 5 degrees (representing a shallowly-dipping reverse fault rupture, like subduction events) is also included in the figure below.

The spatial filter that is used in this study has less significant than the Kajiura filter (red versus green). Note that when the spatial filter size is doubled, the spatial filter and the Kajiura filter become more similar in profile and amplitude. Therefore, in our calculations, sharper deformation profiles are used, compared with the Kajiura filter. We emphasize that the set-up for the spatial filter is the same as in the Cabinet Office's model and we wanted to use the same set-up for our investigations.



Reference:

Glimsdal, S., Pedersen, G.K., Harbitz, C.B., and Løvholt, F. (2013). Dispersion of tsunamis: does it really matter? Natural Hazards and Earth System Sciences, 13, 1507–1526.

Q4-13: Figure 8: Indicate that these are probability density histograms (correct?).

R4-13: We used the normalization to make summed vertical values to be equal to 1. In the revised manuscript, we added '*The sum of the vertical bin heights is 1*' in the figure captions.

Response to Referee 5 (Dr. Reza Amouzgar)

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.

Q5-1: Page 8, Line 230-231 Authors: The reference elevation of the bathymetry and terrain data is the standard altitude in Japan (Tokyo Peil), and no variation of sea levels is taken into account in the tsunami simulations. Is this reference equivalent to mean high water level? (Does it assume the tsunami arrival times coincide with high water mean tide? or it is relative to the mean sea level). If the mean sea level is assumed for the simulations, based on the bathymetry/topography of the region, if the tsunami occurs at a higher tide, how it may affect the physics of the wave and inundation?

R5-1: We assumed mean sea level for simulations as usual tsunami simulations. As the tsunami length is quite long having smaller wave steepness, there is less nonlinear effects on wave deformation in a high-tide situation. The close-to-linear effect on the inundation can be expected. To clarify this point in the manuscript, the following sentence is added: '*However, the effects of tidal variations (e.g. mean high water level) are not considered in the simulations.*'

Q5-2: Page 12, Line 250-252 Authors: The numerical tsunami calculation is performed for a 3-hour duration which is sufficient to model the most critical phase of tsunami waves for the Nankai-Tonankai scenarios. Comment: That is fine to carry out the simulations with 3 hour duration. But I wanted to know for the area of your study, have you attempted one/or more of the simulations for a longer duration to see the wave interactions with the coast and the effect on local inundation. Sometimes, depending on the location, the amplification of the wave is possible.

R5-2: Longer simulations do not cause significant differences in maximum tsunami height nor tsunami inundation except for small height tsunami regions. As initial tsunami heights are quite large, edge waves and other effects become minor for the considered scenarios. We also know that 3 hours duration is enough for megathrust tsunami simulations based on the 2011 Tohoku tsunami.

Q5-3: Page 14, Line 430-431 Authors: Since the exceedance of the critical inundation depths was rare (i.e. 5 and 1 out of 1,000 cases for the towers in the Ogata and Saga districts, respectively), the exiting two vertical evacuation towers were judged to be satisfactory. Comment: That is outstanding outcome from this approach to show the towers are in a safe elevation except for very rare events. In deterministic approach usually a safety factor of 50% maybe recommended as a

typical engineering value, for example, to the tsunami wave heights value because of the uncertainties. How do you relate that in the probabilistic approach? (comparison of deterministic approach with inclusion of a safety factor with the probabilistic approach)

R5-3: This is good point to discuss. Applications of the current tsunami simulation results for a full PTHA and engineering design issues are not yet discussed. The safety factor can be considered in several stages of designing a structure. For example, the primary uncertainty involves the estimation of tsunami height (and this was the main focus of this study). The estimation of tsunami force for a given tsunami height is another important aspect (which is not considered in this study). Additionally, structural design also considers the uncertainty associated with material and others. We think that existing studies like Chock (2016) and Chock et al. (2016) would be suitable for answering Referee 5's inquiry, however, we don't have clear answers to this question.

References:

Chock, G.Y.K. (2016). Design for tsunami loads and effects in the ASCE 7-16 Standard. Journal of Structural Engineering, 142, 04016093.

Chock, G.Y.K., Yu, G., Thio, H.K., and Lynett, P.J. (2016). Target structural reliability analysis for tsunami hydrodynamic loads of the ASCE 7-16 Standard. Journal of Structural Engineering, 142, 04016092.

Q5-4: Page 21, Figure 5 and Page 22, Figure 6: Comment: In these two figures, at what grid resolution are you presenting the wave profiles and maximum tsunami heights? Is it 10m, 30m, 90m? We know at shallower depth and closer to the coast finer resolutions will better present the wave profile. Have you tried a sensitivity test, to see how the resolution may affect the wave amplitude and phase?

R5-4: The results shown in Figures 5 and 6 are based on 90-m grids. We agree with Referee 5 that at shallow depths, using finer grids will be able to capture more accurate wave profiles. We have not compared offshore wave profiles based on different grid resolutions. To indicate that the results shown in Section 4.1 are based on 90-m grids, the following sentence is added in the revised manuscript: '*The temporal surface wave profile results shown in Figure 5 are based on 90-m resolution*' and '*The extracted near-shore maximum tsunami height results shown in Figure 6 are based on 90-m resolution*.'

Q5-5: Page 24, Figure 8: Comment: In Figure 8 (a), in legend part, the horizontal red dash line (Average of 2012 CDMC models) is intersecting with the histogram. If possible, try to edit that in the final edition.

R5-5: Agreed. In the revised manuscript, we avoid this intersection in Figure 8.

Q5-6: Page 24, Caption of Figure 8: Authors: Figure 8: Histograms of tsunami inundation area in Shikoku and Kuroshio Town for the two magnitude ranges M8.9-9.1 (a,c) and M8.7-8.9 (b,d) (left: Shikoku Island and right: Kuroshio Town). Comment: It might be easier for the reader if as follows: Figure 8: Histograms of tsunami inundation area in Shikoku (a,c) and Kuroshio Town (b,d) for the two magnitude ranges M8.9-9.1 and M8.7-8.9 (left: Shikoku Island and right: Kuroshio Town).

R5-6: Agreed. We made changes to the figure caption to make the figure more understandable.

Q5-7: Page 25, Line 550, Caption of Figure 9: Authors: Figure 9: Scatter plots of slip ratios in segments Z (Hyuga-nada), A-B (Nankai), and C-E (Tonankai-Tokai) versus tsunami inundation area in Shikoku and Kuroshio Town for the two magnitude ranges M8.9-9.1 (a,c,e) and M8.7-8.9 (b,d,f). Comment: It might be easier for the reader if as follows: Figure 9: Scatter plots of slip ratios in segments Z (Hyuga-nada), A-B (Nankai), and C-E (Tonankai-Tokai) versus tsunami inundation area in Shikoku (a,c,e) and Kuroshio Town (b,d,f) for the two magnitude ranges M8.9-9.1 and M8.7-8.9.

R5-7: Agreed. We made changes to the figure caption to make the figure more understandable.

Q5-8: Do you have any comments about the computational time, parallelization, and computer resources for this study? For example, for one hour of simulation considering one scenario what would be the estimated time? There are 1000 of simulations for the regional scale and local resolution which will require significant resources and time. I didn't find in the manuscript, as I understand it is not the scope of this study. TUNAMI code or any model which solves the shallow water equation (SWEs) is computationally more efficient compared to other wave models with more complex physics. This is more important in probabilistic methods which need lots of simulations. Therefore, a code which require less computer resources/time (as used in this work) maybe more practical for this purpose compared to the alternative ones, for example Boussinesq models.

R5-8: We did not discuss the computational aspects of the Monte Carlo tsunami simulations in the paper because this was not the main focus of this study. We are willing to mention some details of these computational aspects in this interactive review communication (not in the main manuscript), partly because it may be difficult to 'generalize' our experience in a way that is useful to readers.

The Monte Carlo tsunami simulations for 90-m resolution (1000 scenarios) can be performed using several recent PCs by running multiple cases at a time. The simulations for 10-m resolution were much more demanding partly because of larger spatial domains to evaluate and finer time integration steps (0.1 s). For our calculations, we used BlueCrystal Phase 3 at the University of Bristol (https://www.acrc.bris.ac.uk/acrc/phase3.htm). Using a single node per tsunami simulation, it took about 7-10 days; we run many simulations simultaneously using multiple nodes. Because of queuing wait times, various communication failures and limitations in usage, which were specific to the BlueCrystal system, it took approximately 4 months to complete all simulations.

The TUNAMI code we used was developed by C. Goto and the original development was dated in 1997, as indicated in the accompanying TUNAMI code manual (in Japanese).

As mentioned by Referee 5, for large-scale tsunami simulations, using well-tested nonlinear shallow water equation codes, such as TUNAMI and COMCOT, is a practical approach. Having said that, it would be desirable to use more advanced numerical solvers of the governing equations for tsunami wave propagation and inundation.

Response to Anonymous Referee 6

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.

Q6-1: You should change the title from "Probabilistic inundation assessment". Because what you are doing is not a probabilistic assessment' in common understanding. This term was long ago privatized by studies (PSHA, PTHA) aiming to assess probability of hazard occurrence in time. Looking at your title, reader would expect to find typical PTHA products – hazard curves, hazard maps for various return periods – but won't find them in your study. There is no time dimension in your study. Please change the title to avoid misleading readers and search engines. For example, "Tsunami inundation assessment for Kuroshio Town from stochastic rupture scenarios along the Nankai-Tonankai megathrust".

R6-1: We understand the concern by Referee 6. Our study is conditional on earthquake scenarios falling within a specific range of magnitude, and does not address on the occurrence probability of these events nor cover the entire range of the magnitude. In the revised manuscript, we change the paper title to: '*Uncertainty quantification of tsunami inundation in Kuroshio Town, Kochi Prefecture, Japan using the Nankai-Tonankai megathrust rupture scenarios*'.

Q6-2: It is not clear from the text if Authors have simulated tsunami propagation and inundation for the 11 CDCM source models themselves, or CDCM scenarios were calculated elsewhere. In the latter case, differences by inundation and coastal wave heights (Fig.5 and following figures) between the stochastic and CDCM models may be attributed not solely to slip distribution but also to the generation, propagation and inundation modeling stages. To avoid such mixing, Authors have to simulate CDCM scenarios exactly within their framework.

R6-2: To clarify Referee 6's inquiry, for the 11 CDMC scenarios, we have used the tsunami input files as provided by the Cabinet Office to obtain tsunami simulation results for the CDMC models. The input data were provided in a form of a series of deformation profiles over 10-s duration (i.e. kinematic source models). Therefore, the differences we reported can be attributed to the differences in the tsunami source characteristics.

Q6-3: Authors have limited the lower bound of magnitude range to 8.7. Looking at the histograms on Fig. 11 and 12, one may assume that smaller magnitudes could also trigger dangerous inundation (that is also well known from the history). Hence, by not considering scenarios with M<8.7, Authors effectively constrain their analysis from below and neglect the large amount of hazard-relevant

scenarios. I am not invoking Authors to complete their scenario database but just propose to make a correspondent note in the text (e.g., in conclusions).

R6-3: Referee 6 is right that we did not do tsunami simulations for events with magnitudes less than 8.7. This choice was made because our main objective for the research project was to consider extreme tsunami scenarios that are similar to the deterministic scenarios (M9.0 to M9.1) considered by the Cabinet Office for tsunami disaster risk mitigation purposes. In the revised manuscript, we added one more sentence to indicate this; since we would like to address the point raised in Q6-4 below, we respond how we made changes in the revised manuscript in R6-4 below.

Q6-4: Lines 47-52: These four sentences are, in my opinion, very important. I propose to replicate them (with necessary adaptation) in the conclusion part.

R6-4: Agreed. In the revised manuscript (in Conclusions), the following sentences are added: '*Our motivations in comparing the stochastic tsunami inundation results with the deterministic 2012 CDMC models were to quantify the variability of tsunami hazards at municipality/township levels and to account for local extreme situations. To enable a consistent comparison with the CDMC tsunami source models (M9.0 to M9.1) in terms of released seismic moment, the lower magnitude limit of the stochastic sources was chosen as M8.7.*'

Q6-5: Line 158: Symbols for the rigidity lost in the equation.

R6-5: Thank you very much for pointing this out. We corrected this error in the revised manuscript.

Q6-6: Line 235-237: Was the breakwater modeling directly incorporated into the NLSW code? Please describe the adopted numerical technique in more details (e.g., modification to volume conservation).

R6-6: As mentioned in the main text, we used Honma's (1940) overflowing formulae which were implemented as part of the TUNAMI code. Based on the formulae, the discharge q that flows over the breakwater or sea dike is estimated by considering two overflow conditions: $q = 0.35 \times h_1 \times (2gh_1)^{0.5}$ for complete and incomplete overflows ($h_2 \leq 2/3h_1$) and $q = 0.91 \times h_2 \times (2g(h_1 - h_2))^{0.5}$ for submerged overflow ($h_2 > 2/3 \times h_1$). h_1 and h_2 are the water depths in front of and behind the structure. Since these explanations are available elsewhere (JSCE, 2002), we do not include these equations (we would like to keep consistent levels of details).

Reference:

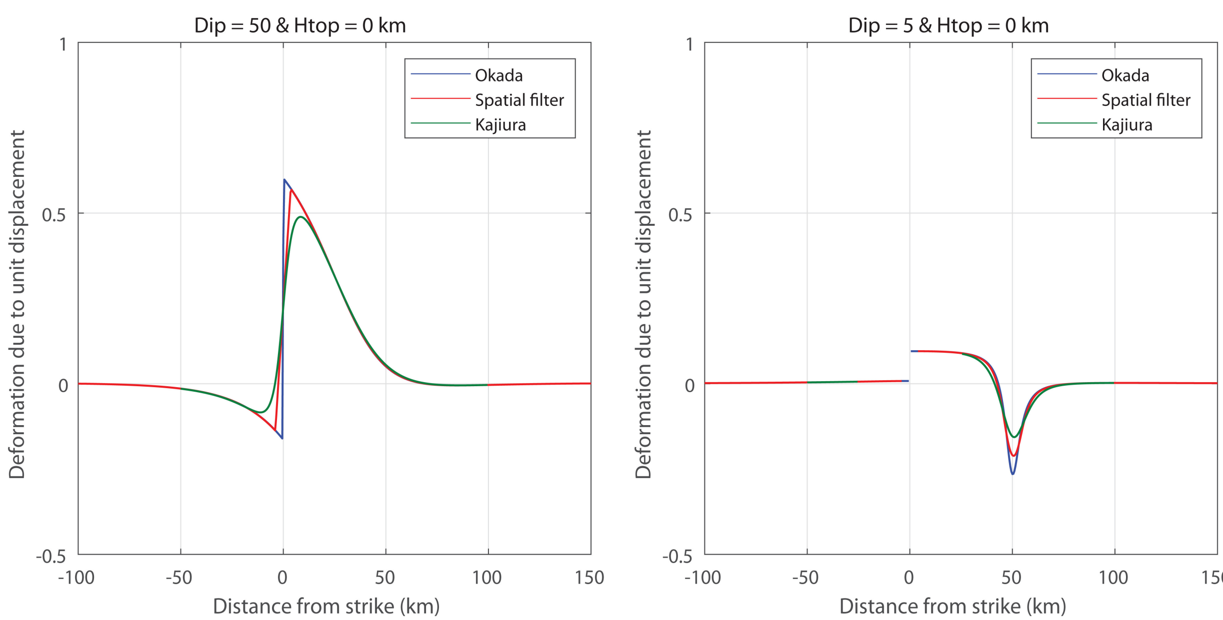
Japan Society of Civil Engineers (JSCE) (2002). Tsunami assessment method for nuclear power plants in Japan. https://www.jsce.or.jp/committee/ceofnp/Tsunami/eng/JSCE_Tsunami_060519.pdf.

Q6-7: Line 244: Spatial smoothing 9-by-9. Why "9-by-9“? Looks grid-dependent. Any benchmarking against Kajiura or Nosov&Kolesov methods?

R6-7: This simple spatial smoothing filter was used by the Cabinet Office. This is the main reason that we also used the same filter function. We applied the filter at 810-m grid resolution, which is the same as the approach suggested by the Cabinet Office.

Regarding the spatial filtering of deformation profiles, we compare the Okada deformation (vertical only), spatial filtered deformation (15-grid size with 0.5 km grid size, which is approximately similar to 9-grid size filter with 0.81 km grid size), and Kajiura filtered deformation. To make a suitable reference to the published paper, we used the same rupture scenario as considered by Glimsdal et al. (2013). More specifically, the rupture scenario is: unit reverse displacement of a rectangular fault with L = 100 km and W = 50 km. The dip angle is set to 50 degrees and the top of the depth is set to 0 km (i.e. surface rupture). Glimsdal et al. compared the Okada-based cross-section profile with the Kajiura-based cross-sectional profile for this rupture (see Figure 2 in their paper). In addition, a comparison for dip = 5 degrees (representing a shallowly-dipping reverse fault rupture, like subduction events) is also included in the figure below.

The spatial filter that is used in this study has less significant than the Kajiura filter (red versus green). Note that when the spatial filter size is doubled, the spatial filter and the Kajiura filter become more similar in profile and amplitude. Therefore, in our calculations, sharper deformation profiles are used, compared with the Kajiura filter. We emphasize that the set-up for the spatial filter is the same as in the Cabinet Office’s model and we wanted to use the same set-up for our investigations.



Reference:

Glimsdal, S., Pedersen, G.K., Harbitz, C.B., and Løvholt, F. (2013). Dispersion of tsunamis: does it really matter? Natural Hazards and Earth System Sciences, 13, 1507–1526.

Q6-8: Line 247: TUNAMI code family has different members. Which TUNAMI version was employed?

R6-8: The TUNAMI code we used was developed by C. Goto and the original development was dated in 1997, as indicated in the accompanying TUNAMI code manual (in Japanese). It is not the TUNAMI N2 version (Imamura et al., 2006; http://www.tsunami.civil.tohoku.ac.jp/hokusai3/E/projects/manual-ver-3.1.pdf).

Q6-9: Line 284: Any explanations for P1/P2 versus P3?

R6-9: These are our observations. We do not have any explanations to mention here specifically. The observations are to indicate that for some locations, national-level tsunami source models may be able to capture local extreme cases, while these may not be applicable to other locations.

Q6-10: Line 306-307: I do not agree with this interpretation. The blue 50% line is in average above 1.0.

R6-10: Agreed. The two sentences with regard to the interpretations of the results shown in Figure 7 are changed as follow: '*The maximum tsunami heights based on the 2012 CDMC models are closer to the 50th percentiles of the stochastic tsunami simulation results for both M8.9-9.1 and M8.7-8.9 cases, indicating overall similarity of the tsunami hazard levels along the coastal line of Kochi and Tokushima Prefectures. The comparisons shown in Figure 6 and Figure 7 indicate that the largest of the 2012 CDMC results does not exceed the 90th percentiles of the stochastic tsunami simulation results for the majority of the locations, and thus the 2012 CDMC results are among the realizations but are unable to capture extreme scenarios of local tsunami hazards and their variability.*'

Q6-11: Line 308-309. I do not agree with this interpretation. For M8.7-8.9 cases, CDMC results are also closer to the 50% (solid) line. For me it is obvious from Fig. 7.

R6-11: Please see R6-10.

Q6-12: Lines 333-334: "reasonable degree of similarity" in what?

R6-12: We wanted to make an observation that from a regional perspective (Figures 6 and 7 [overall] versus Figure 8a), the 2012 CDMC results are contained by the results from the M8.9-9.1 stochastic tsunami simulation results and are more towards higher percentiles, whereas from a local perspective (Figures 6 and 7 [point IDs around 25-30] versus Figure 8b), the 2012 CDMC results are more towards the 50th percentiles of the stochastic tsunami simulation results. We agree that this sentence/observation is not clear. We decided to remove this sentence in the revised manuscript.

Q6-13: Lines 387: Sentence not closed.

R6-13: On Line 387-388 of the original manuscript, we read the sentence: '*Once the earthquake rupture models that correspond to the identified critical inundation areas, various stochastic tsunami simulation results, such as regional maximum tsunami heights and local maximum inundation depths, can be extracted*.' We think that the sentence is understandable and has a complete structure as a sentence.

Q6-14: Figure 11: Y-axes show "Probability" of what? 'Probability of inundation' usually implies probability of occurrence within given period of time ('return period' in classical PTHA studies). I suggest to avoid using the term "probability" at this plot. Alternatively, Authors could plot "Scenarios count", or rename "Probability" into "Fraction of scenarios".

R6-14: Agreed. We changed the vertical axis to 'relative frequency' (normalized bin counts to make summed relative frequency values equal to 1). To clarify the meaning, in the revised manuscript, we added '*The sum of the vertical bin heights is 1*' in the figure captions.

Q6-15: Same Figure: Add vertical dashed line showing the height of the evacuation platform. Information on tower basement elevation is not needed as long as inundation depth is presented.

R6-15: In revised manuscript, we indicate the height of the evacuation platform. We retain the elevation at the tower location because this information may be useful for some readers if they use different elevation models.