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:

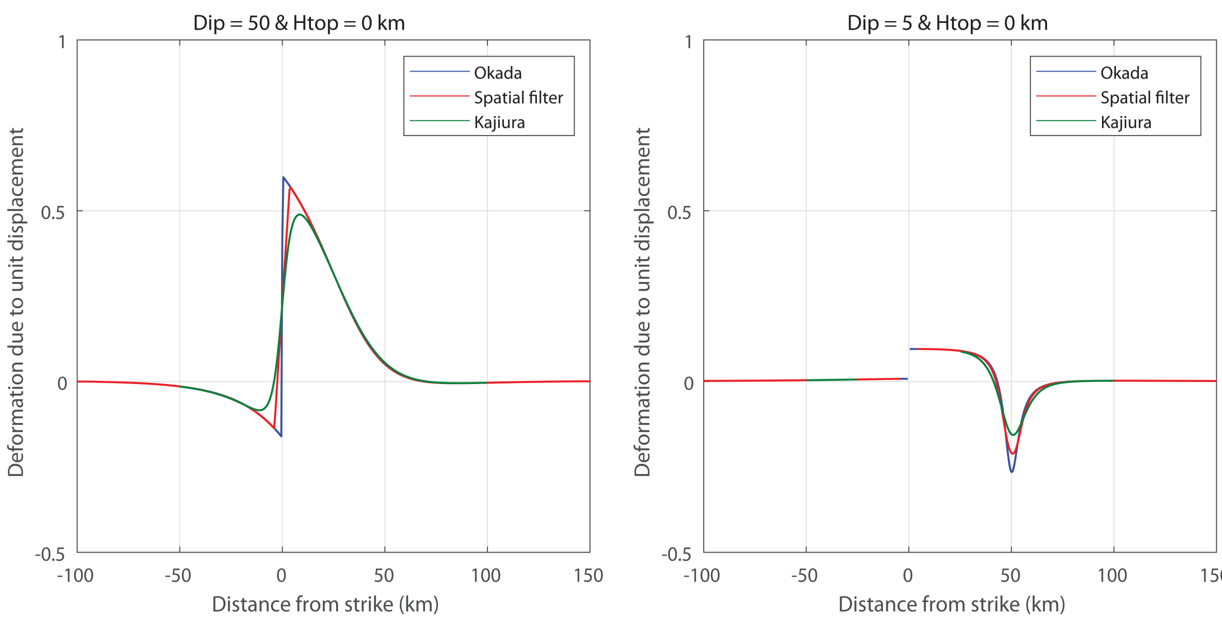
Japan Society of Civil Engineers (JSCE) (2002). Tsunami assessment method for nuclear power plants in Japan. https://www.jsce.or.jp/committee/ceofnp/Tsunami/eng/JSCE_Tsunami_060519.pdf.

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 50^{th} 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.