

Interactive comment on “Assessing potential storm tide inundation hazard under climate change: a case study of Southeast China coast” by Bingchuan Nie et al.

Anonymous Referee #1

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General Comments

This paper presents the results of four simulations of one typhoon (Saomai) under different scenarios: (1) the real case, (2) enhanced tide, (3) enhanced typhoon wind speed and (4) sea level rise. Simulations are performed using a coupled hydrodynamic and wave model (ADCIRC and SWAN) implemented for the Southeast China Coast. The results are presented in terms of water elevations and inundation areas near the typhoon landing location for the different scenarios. Main outcome of the paper is that with increased wind speed and sea level rise the land areas inundated are going to be larger.

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The paper is not acceptable for publication for several different reasons: (1) the four scenarios are not well designed and based on very weak hypothesis; (2) one of the strengths of the paper would be to take advantage of the wave-current models coupling by showing waves results for the different scenarios, that are instead completely absent; (3) the paper is poorly written; (4) the results are for just one typhoon and one specific location and it is complicated to get what is the advance in science presented in this paper or the impact of the results presented.

There are several caveats in the design of the future scenarios (Sections 4.2 and 4.3). Tropical Cyclone Intensification (TCI) – this is taken into account by substituting the wind speed of the actual typhoon with the 50-year and 100-year return period wind speed. The reasoning between the choice of those return periods and the link with TCI should be better justified with supporting literature. Sea Level Rise (SLR) – the authors claim their approach in estimating sea level rise is advanced because they consider local changes in mean sea level, i.e. they extrapolate until 2050 the mean sea level from a tide gauge dataset from 1960 to present. There is an extensive literature on global and regional sea level rise projections that is overlooked in this paper. Taking the data from one tide gauge and projecting in the future with a polynomial fitting curve do not take into account that the rate of sea level rise will change in the future due to unprecedented external factors, i.e. global warming. An accurate estimate of SLR should consider the contribution to mean sea level from steric sea-level rise, dynamic sea-level change, glaciers and ice mass loss from glaciers, Greenland and Antarctic ice sheet contributions, as well as land-water storage changes and the glacial isostatic adjustment. There are regional projections available, see Jackson, Luke P., and Svetlana Jevrejeva. "A probabilistic approach to 21st century regional sea-level projections using RCP and high-end scenarios." *Global and Planetary Change* 146 (2016): 179-189.

Section 3.1 is not needed. ADCIRC-SWAN are standard models already described in the literature and if the authors have not changed anything in the equations, there is no

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need to state them.

The paper claim they are using a wave-hydro coupled model, but then the results are presented only in terms of water elevation and inundation maps, with no mention of waves. It would have been interesting to see for example the importance of the wave-currents interactions, or at least maps of significant wave height under the different scenarios.

The significance of the results, if any, is not conveyed to the reader. The results are just for one typhoon and for one SLR level and two enhanced wind speed scenarios. The paper does not present how these results can be generalized, for example focusing on the processes, or presenting a brand new methodology.

Specific comments

L2. “trend analysis, numerical analysis and GIS-based analysis” – authors should be more specific

L4. “non-stationary” – it is not clear how the authors take into account the non-stationarity of tropical cyclones and mean sea level.

L9. Maximum water elevations should be compared with values during present conditions, to understand the magnitude of changes.

L22. It is worth mentioning typhoons impacting the Chinese coast, as Haynan killed 6300 people, but in the Philippines.

L23-33. This literature review does not hold together. Risk assessment, vulnerability and hazard assessment are randomly mentioned, but the previous studies are not presented in a coherent way, it is not clear what the present work is adding to previous literature.

L24. “empirical model” – authors should be more specific

L28. “surrogate model” – authors should be more specific

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L32. “the precisions of water elevation prediction models in the aforementioned works are limited resulting in quite coarse hazard assessment” – this should be supported by evidence. Hazard assessment usually means to evaluate the probability/evidence of an event. Not clear what is a “coarse” hazard assessment. With a coarse resolution model?

L36. “continual renovation” – it does not sounds as the right term

L44/45 “They found . . . 28

L54. Cite Walsh, Kevin JE and McBride, John L and Klotzbach, Philip J and Balachandran, Sethurathinam and Camargo, Suzana J and Holland, Greg and Knutson, Thomas R and Kossin, James P and Lee, Tsz-cheung and Sobel, Adam and others. (2016). Tropical cyclones and climate change. Wiley Interdisciplinary Reviews: Climate Change, 7 (1), 65-89 and references therein.

L54. “NWP” - missing the acronym definition

L60/61 “increasing rate. . . level”: this sentence is not clear, in 0.06 m units are not for a rate.

L67. What is the global TCI? How does it compare with the regional one the authors are using?

L73 “1.73 billion USD” – reference needed

L159. What is the length scale? From where V_m , U_0 , R_g and R_m are taken?

L163. From where P_c and P_n and track of TC are taken?

L192-193. The AR5 IPCC sea level rise projections are given as the mean sea level 2081–2100 average relative to 1986–2005 average. While the authors are comparing 2006 with 2100. How is it imposed the SLR in the model?

Table 1. Scenarios S3 and S4 are not well designed, as it is impossible to understand

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if the changes are due to MSL changes or wind changes.

L201. Is the maximum wind kept constant during the simulation? Or how the wind of the real typhoon is adjusted to reach the 50- and 100-year return period?

L210. Reference needed.

L212-2019. Poorly written. Not clear what “coupling effect” means and how it affects the surge (shown in Figure 6). In addition, what are the processes behind the “non-linear tide-surge effect”? High water depth (due to high tide) means reduced bottom friction and possibly increased surge. At the same time, reduced wind stress (inversely proportional to the water depth) can lead to a decrease in the surge. So, it looks like in this case the wind could be more important than the bottom friction effect since the surge reduces. The authors should explain better the effects of the different processes, including waves.

L230. Again authors should explain why it is smaller/larger.

Figure 5. The authors should use the same limits for the colorbar to help the visual comparison of the different scenarios. Figure 5 for scenario S3 and S4 is not described in the text.

L227-237. It is very difficult to understand which are the processes that are into play. Also because in scenario S3 and S4 both the wind and MSL are changing.

Figure 7. The two panels can be merged in one figure to compare better the different scenarios.

Figure 8/9. Is the red line the actual coastline? Is the blue area where wetting and drying is allowed in the model? The authors should mark the landing location of the typhoon too.

Technical corrections L26. (2017) analysed

L41. 2011) and

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L49. 2005) examined

L54. considered. As

L211. 2018) concluded

L264. From figure

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