Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2019-284-AC2, 2020

Natural Hazards and Earth System Sciences
Discussions

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

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Received and published: 13 January 2020

Dear referee,

Thank you for your comments, they are very helpful for improving our paper. The replies and corresponding revisions are described as below.

Best regards,

Bingchuan Nie

On behalf of all the authors.

Comments and Replies

Comment: *According to the authors, the main aim of the manuscript is to present a methodology to account for the role of TCI and SLR on storm-induced inundation. This methodology will also consider the role of waves on inundation. After reading the manuscript is not clear which is the originality or innovation of the work. Authors apply a standard approach where they use a widely employed hydrodynamic model suite and just modify forcing/boundary conditions. Then, they apply the methodology to a specific site for 1 reference scenario + 3 modified ones. Thus, in reality, the main contribution of the paper seems to be assessing the changes in storm-induced inundation in a specific site under different scenarios. However, if this is the real objective of the paper, selected scenarios need to be better defined/selected or justified and the analysis must be deeper covered.*

Reply: The frame work which integrating the trend analysis, numerical analysis and GIS-based analysis is new. It seems quite satisfactory and feasible when assessing the storm tide inundation hazard of the study area of interest. It provide a choice for storm tide inundation hazard assessment at other regions.

Moreover, this work is expected to be helpful for promoting the hazard assessment of storm tide inundation, since the non-uniform and non-stationary TCI and SLR impact is usually neglected at present, and the wave influence does not draw enough attention neither when assessing the storm tide INUNDATION. Whereas, our results shows that they are able to deteriorate the storm tide inundation dramatically, and suggests that they should be adopted in the future risk analysis of storm tide inundation.

On the other hand, the study area Southeast China coast considered in this work is a very typical storm tide prone area over China and worldwide just like the north coast Gulf of Mexico. For example, about 70 people dead or missing when typhoon Lekima (201909) made land fall just at the study area about a few months ago. The literature review shows that inundation caused by storm tide at Southeast China coast is scarcely studied, let alone the long-term hazard assessment of storm tide inundation considering potential TCI and SLR. Those typical cases considered can provide reference for the prevention and mitigation of storm tide inundation hazard and future coastal management there.

The introduction section is strengthened to present the innovation more clearly.

Comment: *[Methodology] This section must be improved. Although the main objective is to present and apply the methodology, the half of the section (from lines 80 to 90) is not giving any details about methodology but providing some general text. The rest of the text is just giving a brief outline overview on some used tools/models. At its present form, this section can be fully removed without affecting the manuscript. The best option should be rewriting this section by putting emphasis on describing the general methodological framework (e.g. to describe steps in figure 1) and how to apply it. For instance, authors select as a base case scenario conditions recorded during a TC and then, they propose some scenarios. It is VERY important to properly describe how to build future scenarios to cope with time variation in TCI and SLR. At present this info is partially (insufficiently) covered in section 4, but needs to be included here. All these steps need to be well justified and well described and, since this is an IMPORTANT part of the analysis (scenario selection), this section is the best place to describe how to do it.*

Reply: According to your suggestion, this section is rewritten in the revised manuscript. More details about the non-stationary TCI model including the basic equations for estimating the maximal wind speed of TC under climate change is added. And, the reconstruction of potential TC's wind field at each time step from the maximal wind speed and TC track is added. Thus, how to build the wind fields of future scenarios considering TCI and landing moment is fulfilled. On the other hand, the hydrodynamic surge-tide-wave coupled model in section 3 is integrated within the section of method-

ology.

Comment: *[Hydrodynamic surge-tide-wave coupled model] This section is superfluous. Since authors do not modify the model and the model is widely used and very well-known and referenced, it would be enough to mention it with proper referencing. The most important part is how to select conditions to be simulated and, details on model setting (grid, boundary conditions, etc.). All this info could be integrated within section 2 (Methodology).*

Reply: Yes. The description of Hydrodynamic surge-tide-wave coupled model is simplified along with the description of the grid and boundary conditions details added. And these revisions are integrated within Section 2 in the revised manuscript as the referee suggested.

Comment: *[Case study] The first part of this section needs to be better described and included in methodology (section 2). You mention that you have segmented time series in 50-years long time series which are fitted to an extreme distribution (Weibull). Why Weibull? Is this the best distribution? How can you justify it (r2-values)? Once you have fitted all time series, what to do next (step 3)? Are you doing trend-analysis on fitted parameters to see time evolution of distribution's parameters? If yes, please be explicit. The text does not clearly describe this step.*

Reply: In the revised manuscript, the procedures for constructing the wind field is added according to reviewer's suggestion. The main details are as below. Once the extreme wind speed of different return period is obtained, the maximal wind speed at each time step is ready to be evaluated by multiplying the actual wind speed of Saomai by the amplification coefficient. Where, the amplification coefficient is determined by the maximal wind speed of assumed TCs and that of Saomai focusing on the landing moment. Then, the wind fields at each time step can be reconstructed based on the

TC wind field model. Further, adjust the time coordinate of the assumed TC to make sure the TC lands during astronomical high tide, thus, the track of the TC center can be determined. As for the non-stationary TCI model, it has been described in a previous paper of ours (Wang, L.Z. and Li, J.C.: Estimation of extreme wind speed in SCS and NWP by a non-stationary model, Theor. Appl. Mec. Lett., 6, 131-138, 2016), in which the Poisson distribution for the TC annual frequency and Weibull distribution for wind speed are justified carefully.

Comment: *[Case study] Why did you select a specific TC to do the analysis. Conditions for this TC does not seems to be really strong since recorded wind speed (60 m/s) are weaker than the wind speed associated with a return period of 20 year (figure 3). Please comment about this.*

Reply: Examining the historical typhoons passing by the study area from 1945, it shows that Saomai has the largest wind speed when LANDFALL. Moreover, Saomai struck the coast almost vertically, the most dangerous landing angle. In reality, Saomai did claimed 480 deaths and 2.5 billion USD economic loss according to the annual report of national marine hazard of 2006. That is to say Saomai is most typical TC landing the area of interest, thus, it was chose as the basic TC. In addition, for Saomai, we have observation data available for verification (Wuxi, Q.Y., Li, J.C., and Nie, B.C.: Effects of tide-surge interaction and wave set-up/set-down on surge: case studies of tropical cyclones landing China's Zhe-Min coast, Theor. Appl. Mec. Lett., 8, 153-159, 2018.). Those explanations are added in the revised manuscript as the reviewer suggested.

Comment: *[Case study] The selection of SLR scenario is just an extrapolation of recorded local conditions. This is a very simple approach and it need to be better justified. It has to be considered that using this approach is not accounting for any possible acceleration in SLR and, in this sense, it has not too much meaning to compare*

with IPCC scenarios as authors do in lines 190-193. In any case the ideal situation will be to add to local SLR the expected changes due to CC which would result in rates larger than the used by authors.

Reply: To predict future SLR, two solutions can be usually found in literatures. One is based on the climate models such as CMIP5 and upcoming CMIP6. The other one is purely data-driven prediction, i.e. estimating SLR based on statistically analyzing the long-term measurement of sea level. We chose the latter one because of simpler procedure. In fact, extrapolation of recorded local mean sea level do account for the possible acceleration in SLR, since it is a nonlinear extrapolation.

More details about the comparison between the results by extrapolation and that reported in AR5 are added in the revised manuscript. The AR5 of IPCC reported that the global SLR for 2081-2100 relative to 1986-2005 will likely be 260 to 550 mm for RCP 2.6, 320 to 630 mm for RCP 4.5, 330 to 630 mm for RCP 6.0, and 450 to 820 mm for RCP 8.5. The historical data in Kanmen gauge shows that MSL during 1986-2005 is 6979 mm, while the extrapolation demonstrates that the local MSL during 2081-2100 would be 477 mm higher relative to 1986-2005. That is to say the SLR we obtained is between the situations of RCP 4.5 and RCP 6.0. It should be noted that since the historical data only reflects the response of climate system to influential factors in the past, the real SLR may speed up or slowdown dependent on the factors such as greenhouse gas emission in the future.

Comment: *[Case study] Considering the previous comments, scenarios used by authors need to be reformulated or much better justified. It should be great if authors dedicate a larger effort to this task. They need to consider that since no significant novelty in methodology is provided, the best contribution they can do is to perform a solid assessment. Otherwise it would be an academic exercise without too much practical interest.*

Reply: Justification of the scenarios are added in the revised manuscript. The main points are: 1) the area of interest is very typical over China and worldwide just like the north coast Gulf of Mexico; 2) The basic TC Saomai is typical, since it has the largest wind speed during made landfall at the area of interest among the TC during 1945-2013 and struck the coast with the most dangerous landing angle; 3) scenario S2 in which typhoon has the same extreme of Saomai and makes landfall during the astronomical high tide is the worst situation could occur at present; 4) S3 and S4 are two typical future scenarios considering TCI and SLR based on S2.

Comment: *[Case study] Results presented in Fig 5 could be much better compared if you use the same scale for all figures. Also, which is the relevance of representing water level at the sea, especially when you are also plotting the component associated to astronomical tide? If you mention that one of the advantages of your approach is accounting for the wave contribution, why do not show wave heights? They will be modulated by water level and, thus, you can assess how the hazard component associated to waves does change from one scenario to other one.*

Reply: Yes, we have modified the scales of figure 5 with its subfigures updated. The water elevation is measured from the mean sea level, which is contributed by tide and surge directly and by wave indirectly. The water depth of the storm tide inundation region we care most in this work is so shallow that large waves is hard to survival there. Waves contributed to water elevation and current is obvious mainly in surf zone. The evolution of components of water elevation due to tide, wave and surge and their coupling effect have been already analyzed quantitatively in previous paper of us (Wuxi, Q.Y., Li, J.C., and Nie, B.C.: Effects of tide-surge interaction and wave set-up/set-down on surge: case studies of tropical cyclones landing China's Zhe-Min coast, Theor. Appl. Mec. Lett., 8, 153-159, 2018.). In addition, the spatial distribution of wave impact can also be figured out from figure 6. As for the wave heights and the main physical processes of the tide-surge-wave coupling, one can refer to the second and 26th reply

to referee 1, respectively.

Comment: *[Case study] It has not too much meaning to compare different scenarios at different tide conditions unless you want to specifically assess the role of the astronomical tide. If you want to assess the contribution of TCI and SLR you just need to concentrate in compare any scenario under the same tide condition. Please, simplify.*

Reply: According to the referee's suggestion, the comparison between S1 and S2 which have different tide condition is simplified. And more discussion about the contributions of TCI and SLR to water elevation are added, see the reply to the next comment.

Comment: *[Case study] Are water levels represented in Figure 7 also including the wave contribution (run up)? If so, which is the difference in this contribution between scenarios? Thus, you can account which is the contribution of each component (TCI, SLR, waves to differences in total water level)? Why don't you include all graphs within a single figure (it should be the best way to compare them)?*

Reply: Your comment is very helpful. Subfigures 7 (a) and (b) are merged into one figure in the revised manuscript. As for wave, the contribution to water elevation at surf zone via wave breaking is much more significant than the run up for storm tide inundation. The former can increase the water elevation more than 10 percent as surf zone (Wuxi, Q.Y., Li, J.C., and Nie, B.C.: Effects of tide-surge interaction and wave set-up/set-down on surge: case studies of tropical cyclones landing China's Zhe-Min coast, Theor. Appl. Mec. Lett., 8, 153-159, 2018.).

The qualitative discussion on the contributions due to TCI and SLR, are added in the revised manuscript as below. Since the period of astronomical tide cycle is a few times larger than the duration of TC passing by for a specific location, the increase of tide level can be regarded as the quasi-steady process of increasing the water depth.

That is similar to SLR, but much larger amplitude. It implies that the increasement of water elevation contributed by SLR can be estimated roughly by the tide-surge coupling effect. Taking S4 for example. The MSL of S4 is 0.51 m higher than that of S2. That means increase of water elevation caused by SLR will be a little bit less than 0.51 m, because less wave induced surge occurs for higher water depth. While, the total increase of water elevation by SLR and TCI, 0.80 m, suggests that increase of water elevation caused by TCI could be larger than 0.29m. The qualitative results can be obtained for S3 similarly, i.e. TCI could cause more than 0.16 m of water elevation increasement, while SLR can caused water elevation increasement a little bit less than 0.19 m. In all, both TCI and SLR are important factors should be involved in the future long-term hazard assessment of storm tide. To prove that, results of a few new cases considering MSL and TCI effects independently has been presented in Figure R3 of the reply letter to referee 1.

Comment: *[Case study] Results showed in this section are only relevant if tested scenarios are relevant/representative.*

Reply: As described above, the area of interest is a very typical storm tide prone area over China and worldwide from the view of intensity and occurrence frequency of TC and the damages caused. On the other hand, the four scenarios are representative: S1 the worst scenario did happen in the past; S2 is the worst scenario could occur at present; S3 and S4 are two typical future scenarios based on S2 considering TCI and SLR. Those justifications are added in the revised manuscript.

Comment: *[Conclusions] This section needs to be modified after implementing previous recommended changes.*

Reply: Yes, the section of conclusions with modifications considering the previous changes, especially those correspond to the methodology and whether the scenarios

are representative included is presented in the revised manuscript.

Comment: *Figure 10 is not needed.*

Reply: Figure 10 is the remotely sensed maps of the hardest three hit regions of storm tide inundation, which shows that the downtown of Cangnan city and Aojiang city, Longwan and Dongtou districts of Wenzhou city are of high risks of storm tide inundation considering TCI and SLR. It is believed that those results may knoll alarm clock for the local risk management of future potential storm tide inundation.

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