Rebuttal letter manuscript “Generating reliable estimates of tropical cyclone induced coastal hazards along the Bay of Bengal for current and future climates using synthetic tracks”

Dear editor, dear reviewers,

On the June 21, 2021, we have submitted the following manuscript to the Journal of Natural Hazards and Earth System Sciences titled: "Generating reliable estimates of tropical cyclone induced coastal hazards along the Bay of Bengal for current and future climates using synthetic tracks" (MS No.: nhess-2021-181). On the October 21, 2021, we were informed that the open discussion was completed. In total, we received comments by two anonymous reviewers which provided positive feedback on the work done and valid suggestions. With this message we would like to acknowledge their time and efforts which we believe have led to an improvement to the quality and clarity of our manuscript. Below you will find a reply to all the specific questions and suggestions, which have also been addressed in the original manuscript.

Kind regards,

Tim Leijnse and co-authors

Anonymous Referee #2

We thank the reviewer for the review, in particular for the useful feedback and interesting references. Our response to the comments are given in blue below.

Literature reviews are not enough:

For STC, Nakajo et al. (2014) is preferable.

Nakajo, S., N. Mori, T. Yasuda and H. Mase (2014) Global stochastic tropical cyclone model based on principal component analysis with cluster analysis, Journal of Applied Meteorology and Climatology, American Meteorological Society, Vol.53, pp.1547-1577.

Thank you for bringing these references to our attention. Nakajo et al. has been added to the list of STC methods mentioned in the manuscript, L57 & L502.

For the second-order hazard due to TC, the paper should include recent studies using datasets of a STC model and climate change experiments, for example, the following references:

Yasuda, T., S. Nakajo, S. Kim, H. Mase, N. Mori and K. Horsburgh (2014) Evaluation of Future Storm Surge Risk in East Asia based on State-of-the-art Climate Change Projection, Coastal Engineering, Volume 83, January 2014, Pages 65–71

Mori, N. and T. Takemi (2016) Impact assessment of coastal hazards due to future changes of tropical cyclones in the North Pacific Ocean, Weather and Climate Extremes (review paper), Vol.11, pp.53-69. doi: 10.1016/j.wace.2015.09.002

Mori, N., M. Kjerland, S. Nakajo, Y. Shibutani and T. Shimura (2016) Impact assessment of climate change on coastal hazards in Japan (review paper), Hydrological Research Letters, Vol.10(3), pp.101-105. doi: 10.3178/hrl.10.101

Yang, J.A, S.Y. Kim, N. Mori, H. Mase (2018) Assessment of long-term impact of storm surges around the Korean Peninsula based on a large ensemble of climate projections, Coastal Engineering, Elsevier, Vol.142, pp.1-8. doi.org/10.1016/j.coastaleng.2018.09.008

Mori, N., T. Shimura, K. Yoshida, R. Mizuta, Y. Okada, M. Fujita, T. Temur Khujanazarov, E. Nakakita (2019) Future changes in extreme storm surges based on mega-ensemble projection using 60-km resolution atmospheric global circulation model, Coastal Engineering Journal, Taylor & Francis, 61(3), pp.295-307.

Yang, J.A, S.Y. Kim, S.Y. Son, N. Mori, H. Mase (2020) Assessment of uncertainties in projecting future changes to extreme storm surge height depending of future SST and greenhouse gas emission scenarios, Climatic Change, pp.1-18. https://doi.org/10.1007/s10584-020-02782-7

Sooyoul Kim, Jihee Oh, K.D. Suh and H. Mase (2017) Estimation of climate change impacts on storm surge: Application to Korean Peninsula, Coastal Engineering Journal, 59, 170004, 10.1142/S0578563417400046.

Thank you for these interesting references. We now refer to many in L62 and L503 (Mori et al 2016a, Mori et al 2016B, Mori et al 2019, Yang et al 2020).

In 55, the authors mention "local design values" for wave heights. But you investigate them in the relatively deep water. What kind of "local design" is the purpose? It should be clear.

Thank you for pointing this out. We do indeed determine the wave heights in relatively deep water. Ideally, we would have done that in shallower waters, what we would like to do in the future, but that was now not yet computationally feasible to do for the entire BoB. We have changed the text now to 'offshore extremes', L59.

Also, in the discussion Section 4.2 we now emphasize that our model results are indicative and should not be directly used in detailed design L518-520.

When we say a "coupled model", we mention physical processes through coupling. How did you make the coupled model physically?

The coupled Delft3D FM-SWAN model exchanges water levels and radiation stresses every hour, we have added this to the text in L196-198. Thank you for bringing this addition to our attention.

When we consider waves in storm surges, we think of radiation stress in the momentum equations for storm surges. Also, we can consider wave runup/overtopping for coastal floods. Why did you consider waves / why did you use a coupled model in your study? Is any typical reason for it?

Good question. We included waves in our study because besides the effects of HTC vs STC tracks on storm surge, we wanted to evaluate the effect on wave heights too. Thereby it was most straight-forward to do that all in 1 model suite (that of Delft3D) where both models run coupled, with as benefit that effects of the waves on storm surge (and vice-versa) as the reviewer mentions are included too. We have added text on the exchange of water levels and radiation stresses in L198.

In 140, why did the authors choose POT for statistical analysis? How did you determine the threshold value for each station/location/region? Did you consider other methods, likely the annual maximum series? Why did you use a Generalized Pareto Distribution? Is it a representative in this region?

Sorry for not having motivated it. We apply the POT approach because of the available sample sizes. The Generalized Pareto Distribution is fitted because it is the asymptotic distribution of the POT data. The POT thresholds are determined per station based on minimum percentiles and using the threshold stability criteria of Caires 2016 (see explanation in section 2.3.4). Annual maximum series were not used because at most locations in the BoB, the yearly probability of a cyclone is below 1.

In 3.1, the authors investigate the wind speed. But I am surprised why the pressure/central pressure of TC is not studied. The driving force of the wave is absolutely the wind speed. BUT that of the storm surge is the wind speed and the pressure of TC. Therefore, the PRESSURE has to be verified for discussing the storm surge. It is the most significant lack point in this paper. Without discussing the central pressure of TC, the discussion of the future change of storm surges has no meaning.

Thank you for pointing this out and our excuses for the confusion. As you rightly state the pressure is a driver and important when assessing the impact of TCs and is fully accounted for in our computations.

For the verification of the synthetic TC tracks in section 3.1 the pressure was not investigated specifically, since pressure is not one of the variables that is directly sampled in TCWiSE. Since wind speed and central pressure of a TC are directly correlated, TCWiSE samples values of wind speed as proxy for intensity, where after using Holland et al. 2008 a corresponding pressure is found. This is done when creating the 2D pressure fields, as explained in section 2.3.2. To show that central pressure (as derived value from wind speed through Holland 2008) is also estimated fairly well we have added Figure B4 in the appendix showing the CDFs for the 9 locations. Error statistics are added to the main text in L268-271.

Furthermore, sorry for not stating it clearly, but the central pressure is included in our storm surge modelling. In Delft3D both the wind speed and atmospheric pressure are included as 2D fields which drive the storm surge. We hope that the revised manuscript clarifies your question (L 201-202).

Since we derive the pressure from the wind speed and apply it in calculating the storm surge in Delft3D, the influence of the TC pressure and the possible changes in the pressure with climate *change (derived from the increase in wind speeds) is fully accounted for in our study when calculating the future climate results. Possible changes in the relationship between TC central pressure and wind speed as a result of climate change are not included (since we use the empirical relationship of Holland). To highlight this, we have included this as a discussion point in Section 4.3, see L 531-532.*

In 275, the validation process has to include the effect of the central pressure of TC on the storm surge.

We agree that the pressure fields are an important forcing for storm surge computations and have added Figure B4 in the appendix and error statistics in the main text as mentioned above.

In 390, I disagree with these words because the central pressure is omitted.

We hope to have clarified, through our replies above and adjustments and additions to the manuscript, that central pressure is not omitted. In the modelling of storm surge in Delft3D, both the wind speed and the central pressure are used to determine the storm surge.