Natural Hazards and Earth System Sciences Discussions

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/nhess-2021-397-RC2, 2022

Comment on nhess-2021-397

Anonymous Referee #2

Referee comment on "Climate-induced storminess forces major increases in future storm surge hazard in the South China Sea region" by Melissa Wood et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-397-RC2, 2022

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1 Summary

In this manuscript, Wood et al. present a study of the storm surge hazard in the South China Sea region using a hydrodynamic model. They forced their model through statistically generated storm database (Previously published, and under-review dataset). The manuscript falls into the category of full-length paper. The subject of the paper is very important but at current state lacks clarity.

In general the manuscript is not very well-written. The text is often hard to follow. I would ask the authors to put substantial effort to improve the readability of the manuscript (see comments in 2.1).

In terms of scientific content, some parts are weak (or weakly explained). Validation of the model section needs much more substance, and modelling limitation needs to be acknowledged further in the discussions. (see comments in 2.2). I also welcome more detail of the storm dataset used in this study. Particularly the detail of the future dataset was not accessible.

Although the paper title says the study is about "South China Sea", my opinion it is more focused on the Vietnam coast, particularly the storm surge evaluation section. The model resolution is rather low on the other parts of the south china sea coastline - even inferior to global models (GTSM). I let authors to decide if the title needs a revision.

Overall, given the importance of the topic and potential of the paper, I do not want to reject the manuscript. My decision is **Major revision**.

2 Major comments

2.1 Writing and the manuscript structure

In my opinion there is a large scope to improve the overall writing of the manuscript. Following is a list of comments -

- Introduction was rather hard to follow. For example, the §1 opened with the future surge hazard, then immediately switched to current surge hazard in §2, and then revert back to future surge hazard in §3. My suggestion would be to reorganize it into §2 ☐ §1 ☐ §3.
- Two method discussed at L75 and L80 are not same thing it seems. One of them extends data over century, another extends only on decade. The sequence and presentation of these paragraph reads odd.
- L91-105: This paragraph is troublesome as it opens with synthetic storm, follows by reanlaysis forcing, followed by synthetic data again and STORM dataset etc.
- L135-140 should go to a dedicated Dataset section, (Beginning of section 3?) and their properties should be discussed. It is again a bit odd to read about data and method at the end of the Introduction section. Here I was expecting how you are going to structure the manuscript.
- The data/model/discussion section also has similar structure-related issues. I stopped suggesting explicit updates from this point on. I suggest the authors to carefully revise the manuscript with readability (and story) in mind.

2.2 Modelling and validation

- L146: Is this model already published? If so, please provide past reference.
- How did you choose the model resolution? For a regional study, a resolution of 2.3km (finest) seems rather low, as GSTM 2020 model discussed in the introduction has 2.5km at the coast. Additionally, in most cases the resolution is set to 5-7 km, which is twice as coarser than GSTM.
- Is any wave model is incorporated? If so which one and how? The resolution issue is further problematic as Wave models needs metric (10s of meter) resolution to properly model the wavesetup [1]. In case wave model is not coupled, then wave setup is not computed which can furhter augment the storm surges [2]. My understanding is that no wave model is coupled. Please provide discussion and adds in the limitations.
- What do you mean by updated bathymetry in L161?
- What does it mean by '2016 tidal model solution' in L167?
- L181-184: Annual mean sea level values fluctuate, and linearly increase. How do you make it equivalent to model datum (EGM96) by removing a fluctuating value?
- L184-185: Tidal analysis does not remove meteorological influences. At daily scale, for example, S1 tide is forced by diurnal atmospheric pressure loading. At seasonal scale, for example, Sa and SSa components are often influenced by long-term meteorology, freshwater input etc [3].
- L185-187: If possible please compare for a consistent year.
- Modelled tide and tide gauge data are inherently correlated (L200), hence validation using MAE and correlation is fundamentamentally does not capture the error in the various 'waves' (e.g. tidal constituents). Please use complex error for validation and update Table 2 with complex error instead of MAE. You might also provide a map of the station with Complex Error for the 4 main constituents noted in L214. See for example Tanchant et al 2021 [4].

- L239: is 0.25 degree enough resolution? Why it was chosen?
- L231-235: A map of these tracks would be useful. This can be added to, for example, figure 2b.
- L264: Can you show the landfall time in Figure 4? How about other stations? Can those be added as supplementary materials?
- I could not find what was the model timestep used for tide, or surge simulation. Please provide this vital modelling detail.
- I also could not find any detail regarding the model friction formulation. Is it manning? Chazy? roughness length? What is the distribution? Regionally uniform or different? How it was chosen?
- Please update figure 2b, with a better colorbar resolving the nearshore bathymetry, which is very critical for storm surge modelling [5].

2.3 STORM dataset

- At several places, Bloemendaal et al. (in review) were referred, and at L292 it was explicitly asked to see a under review paper. The paper was however not accessible. Is it in a closed review journal? If so, I would ask the authors to add substantiating details in the current manuscript. For example, explain in L291 the delta approach used in Bloemendaal et al (under review). Please feel free to use supplementary materials to add further details.
- It is also of interest to know if the seasonal distribution of the STORM dataset follows the observed distribution. Also, if the storm category distribution matches the observed? - or how well they matches.
- L304-309: I do not follow why CNRM-CM6 model was chosen? Some linkage with previous study is missing here. Where is this decreasing trend of frequency is found?
- L310: does that mean there is an increase in frequency along vietnam coast? and the TC frequency is decreasing in WNP? L343 indicates that for current around 40 year period there is 30,800 cyclones, and for future 35 year period there is 63300 cyclones? The statements in L304 about decreasing frequency, does not make sense with the number presented here. Please explain and clarify. Perhaps it is the selected cyclones, not 'frequency' in the data?

2.4 Statistical analysis (Section 3.3)

I do not understand what does it mean by "annual maxima approach" was selected? As you explain in Appendix, the STORM dataset has only month information in it, no date, time, or year - is it the case? If so, then how do you decide which storm fall in which year? Needs a clear explanation and logic behind the method chosen here.

In Figure 8. please provide estimate of confidence interval. For example, bootstrap method can be used for such large dataset. That will also tell us if presenting 10000 year return period value is meaningful.

2.5 Discussions

- A lot of discussion was put forward on bath-tub model, and flooding, but those were neither shown nor discussed in the results section (or appendix, supp.). Detail will be appreciated. For example, sea dykes are discussed in L506, but no information, height, location is presented. Please consider presenting those information - otherwise it is hard to check this claim.
- How the storm surge timing differs/similars to river flood timing? Does river discharge has any impact? Does inland flooding can module surge level? Please discuss? Please also indicate that inland flooding is not considered directly in the model.
- Please discuss the model resolution issue. (Elaborated above).

3 Minor comments

- L24: What do you mean by "± tides"?
- L25-26: Repetition of the previous line "storm surge hazard" increases, is essentially stated again in the beginning of the next line. This can be shortened. See comment about "regional amplification" in the Major comments.
- L34: This line is a bit jarring to read. I suggest to drop "Because of projected sea-level rise" completely from this line, as you have again said it in L39. Please also re-refer to Kirezci et al. at L39.
- L41: The statement that there has been an assumption of stationarity of storm surge statistics needs to be backed up by references. Do you mean the "storm surge" statistics were assumed stationary, or the "storm" statistics were assumed stationary? Needs clarification.
- L47: "these TC. . . " which one you are indicating? I believe all the TC, not the low-probability ones? is it?
- L49-51: It is a bit odd to read that 3% of the global population is currently exposed to storm surge hazard, while 2.4-4.1% of global population will be exposed to 1/100 year flood. Are these both quantify the same thing? Does that mean the estimate by one of these two author was over/underestimated? Or do they fall into the errorbar of the estimation? This statement also hurt the claim that "it is an increase of 52% compared to today" in L36.
- L51-56: Would it be possible to shorten this line? Perhaps, it can go to as a table in the appendix, with corresponding death counts, year, affected country(ies), and reference.
- L60: Please add 'low confidence' interval defined by IPCC.
- L56: Please avoid inconsistency and mismatching. TCs are feature of Tropical regions. For mid-latitude regions they are extra-tropical cyclones or mid-latitude cyclones, not TCs. So, saying "in particular in tropical and mid-latitude regions" is a bit odd.
- L64-65: By capturing do you mean 'observing'/'recording' ? The next lines tells me so, but "capturing" is a bit of odd choice of word. Do you mean to say that infrequent nature of storm surge is the problem, or having a long-term data with necessary spatial resolution is the problem/challenge?
- L73: "two varient approaches"? ☐ do you mean to say "two variation of an approach" or "two different approach"? Please clarify.
- L79-81: Please add a reference(s) to this claim. (similar to L85)
- L83: 'gridded domain' indicates the underlying model is gridded? Is it the case? Although the final distributed output might be 'gridded', GTSM model referred just after is a Delft3D FM implementation - meaning unstructured grid.
- L87: At L69 you refers to > 100 year record. But the datasets you are referencing GTSM they are not 100y long? are they? ERA-Interim data starts from 1979, that means GTSM for current climate is only 40 year long.

- Table 1. Appears at wrong location. Should be moved around when it is first referred.
- L148: ‘interconnected’ is redundent here.
- L148-151: Please break these lines.
- L151: What do you mean by ‘reconfigured’ ? And why it was reconfigured?
- L172: Last line is redudant as it is standard.
- L221: Table 1 should contain data range for each station.
- L295: drop ’smaller’.
- L296: What do you mean by “uncategorized”?
- L324: What is a ‘steering‘ file?
- L393: What do you mean by ‘dampens’ surge? May be you wanted to characterize opposite way - like, narrow shelf does not allow surge to amplify as much as . . . ?
- Fig6, Section 4.1: It is interesting to see that basically for all the points, 10th largest surge is generated by tracks crossing the point on the west side, except point 1700 - why would be that?
- L575: Does this line refer to the current study? Or another study?

References

[1] Thomas Guérin, Xavier Bertin, Thibault Coulombier, and Anouk de Bakker. Impacts of wave-induced circulation in the surf zone on wave setup. Ocean Modelling, 123:86–97, 2018.
[2] Xavier Bertin, Kai Li, Aron Roland, and Jean-Raymond Bidlot. The contribution of short-waves in storm surges: Two case studies in the bay of biscay. Continental Shelf Research, 96:1–15, mar 2015.
[3] David Pugh and Philip Woodworth. Sea-Level Science: Understanding Tides, Surges, Tsunamis and Mean Sea-Level Changes. Cambridge U. Press, April 2014.
[4] Yann-Treden Tranchant, Laurent Testut, Clémence Chupin, Valérie Ballu, and Pascal Bonnefond. Near-coast tide model validation using GNSS unmanned surface vehicle (USV), a case study in the pertuis charentais (france). Remote Sensing, 13(15):2886, jul 2021.
[5] Y. Krien, L. Testut, A.K.M.S. Islam, X. Bertin, F. Durand, C. Mayet, A.R. Tazkia, M. Becker, S. Calmant, F. Papa, V. Ballu, C.K. Shum, and Z.H. Khan. Towards improved storm surge models in the northern bay of bengal. Continental Shelf Research, 135:58–73, mar 2017.