JOURNAL OF COASTAL AND RIVERINE FLOOD RISK

Review and rebuttal of the paper

Probabilistic Tsunami Hazard Assessment from Manila Trench to Shantou City in China

Tingyu Zhang, Miguel Esteban, Takahito Mikami, Tomoya Shibayama

Editor handling the paper: Jeremy Bricker

The reviewers remain anonymous.

First review round

Reviewer 1

This study conducts a probabilistic tsunami hazard assessment for Shantou, China based on the seismic hazard from the Manila Trench. It conducts 5000 tsunami simulations for different earthquake fault parameters which are used to estimate the nearshore tsunami amplitude for the 100, 1,000 and 10,000 year return periods. These are fed into an inundation modeling tool with several global mean sea level rise scenarios added showing the outsized effect of sea level rise on the inundation potential. The manuscript is concisely written and the results are pretty clear. However, I have some concerns that need addressing as follows.

The authors would like to thank the reviewer for the time taken to provide constructive criticisms. These are addressed below in a point-by-point manner. Should the reviewer have any further comments, the authors are happy to further improve the manuscript.

The study could be of interest to field but the introduction and abstract does not clearly delineate the novelties of the research. It reads more like a technical report following a standard approach. At one point another study that has looked at tsunami risk for Shantou was cited (Yingchun et al., 2007), but the limitations of that study are not mentioned and why the present study is required. In addition, another relevant study, Li et al. (2016) is referenced much later in the manuscript. Overall, the literature review is insufficiently thorough and does not adequately highlight the research problem.

The authors agree that the previous version of the manuscript was not clear enough on these points. The authors have thus made a number of changes:

In the abstract, the first sentences now read:

"Despite the well-know tsunami hazards originating from the Manila Trench, a detailed assessment of present and future risks to the South coastline of China has not been made. Thus, the present paper paper analyzes the tsunami hazard at the southern coastline of China, and more specifically at the city of Shantou. "

The authors also agree that Li et al. (2016) is a very important study, which greatly informed this result (and hence why it features prominently in the discussion). However, the difference between this study and that of Li et al. (2016) is not clearly identified in the introduction. This has now been modified to read as follows:

"It is obvious that SLR changes need to be considered when discussing tsunami risk (Shibayama and Esteban., 2023; Hirschfeld et al., 2024; Esteban et al., 2024), especially where the risk is at present generally considered to be low, such as in the case of Shantou.

The present research will thus quantify the tsunami risk for the city of Shantou, considering future SLR. This will be done by conducting a Monte-Carlo-type probabilistic tsunami hazard assessment (PTHA) using the tsunami propagation model COMCOT (Liu et al. 1998) from the most likely source, the Manila Trench. (…) While other authors (notably Li et al., 2016) have also conducted tsunami *propagation simulations, for a true assessment of the risk posed by such events it is necessary to also perform inundation simulations. A hazard curve explaining the relation between return period and tsunami height will thus be obtained, and tsunami inundation simulations will be conducted using Nays2DFlood for 100, 1,000 and 10,000 years return periods waveforms for a variety of SLR scenarios"*

The practical objective of the study to obtain tsunami inundation maps for Shantou is described clearly, but no scientific objective is provided.

Besides these general comments that need addressing, I have other specific comments below:

Specific comments:

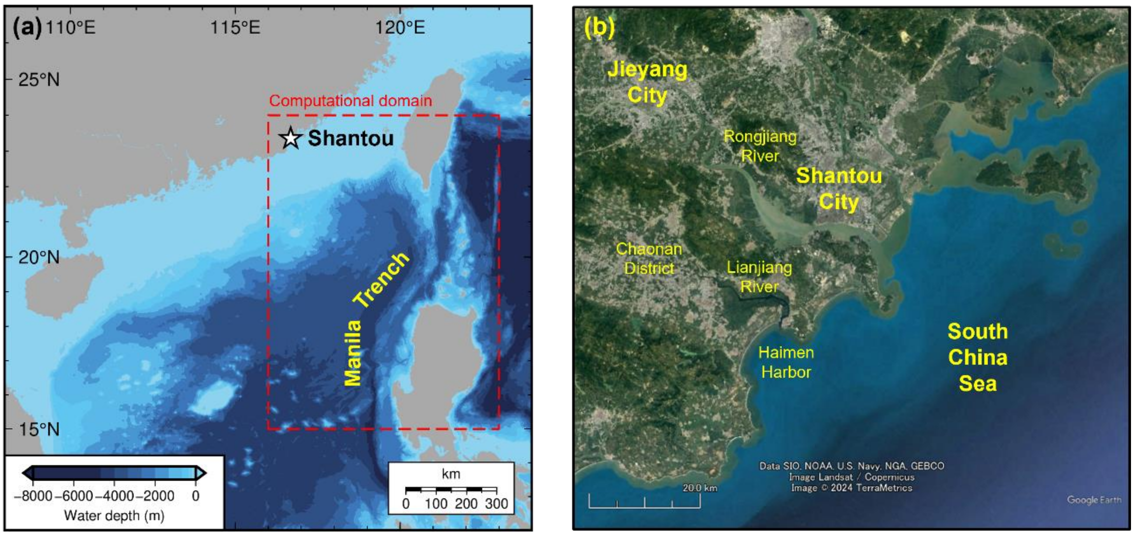
What is Nays2DFlood? Reference for it in the Introduction section (P2:L27) and a description of it in Methodology section (P3:L6) is required.

Citations have been added to the Introduction and Methodology. A description of Nays2DFlood is now provided in Section 2.5:

"Nays2DFlood is a flood flow solver using the International River Interface Cooperative (iRIC) software developed by Professor Yasuyuki Shimizu at the University of Hokkaido. This solver treats unsteady 2-dimensional plane flows using general curvilinear coordinates and adopts the computational scheme of the Nays2DH Solver, including the CIP momentum advection method. This solver is able to conduct tsunami inundation simulations. Previous research using Nays2DFlood is described, for instance, by Chen et al. (2021) and Ali et al. (2017)."

Figures 1 and 3 can be improved. Based on these figures alone, the location of Shantou is geographically unclear. Its location can be indicated in Figure 3. I suggest that these figures be combined into one with Figure 1 being a zoom-in for Figure 3. Google Map is not ideal, and preferable to use a other mapping tool to better highlight the salient features of the region for the present study.

The authors agree that the figures were not the best. They have now been combined into an improved figure, as suggested by the reviewer.



Section 2: Did the authors test that the nonlinearity between tsunami and SLR is small enough to ignore in their methodology?

This was not tested for the propagation simulations across the South China Sea. However, note that in this case the water depth is very deep, and the tsunami height only 1-2 m. Hence, and according to classic tsunami propagation theory equations, it is unlikely that there is any effect.

"For the case of the inundation simulations conducted using Nays2DFlood, the bathymetry was altered for each SLR inundation scenario conducted, meaning that any nonlinearity effects are included in the results (see Koyano et al., 2020, 2022)"

Section 2.1: It is standard to use spatially varying Manning's roughness coefficients overland based on land use data to simulate macroroughness effects from vegetation, urbanization, etc. Why only a spatially constant value of 0.025 was used?

The use of a spatially varying Manning's n coefficient over land is clearly better than using a uniform value (despite the fact that many authors in the past have assumed uniform values). However, for the case of the underwater bathymetry it is difficult to vary this value spatially. Hence, a uniform value of 0.025 was employed, in line with other authors.

Section 2.4:a. Suggest change "Return time period" to "Return period".

Changed as suggested

Section 2.4:b. Is the "tsunami metric value" just the "tsunami height"?

Changed to "tsunami height"

Section 2.5. a. Regional sea level rather than global mean sea level scenarios should be used.

The authors agree that, in principle, the regional SLR projections should be used instead of the global. However, the authors were not able to find any local SLR projections for Shantou, and regional levels appear to be very similar to global SLR. Nevertheless, this is a limitation of the study that is now acknowledged in the limitations:

"Finally, another limitation of the present study resides in the use of global SLR projections, rather than regional level ones. Li et al (2024) analyzed tidal gauge data along the South China Sea coast, and found that SLR was an average of 4.0 mm / year, similar to the global SLR rate, although the study did not include the city of Shantou. Other analysis by Li et al (2024), using tidal and satellite observations also shows rates of regional SLR to be 3.4 mm per year in the period 1980-2021, although again this does not include the city of Shantou. Given that the regional rate of SLR thus appears to be similar to that at the global level, but that no local SLR measurements are available for Shantou, the authors chose to use the global level, although this represents a limitation of this study (as it may be that projections for Shantou might be slightly different). "

Section 2.5. b. It is unclear how the COMCOT simulations link to Nays2DFlood and the SLR scenarios. What is the advantage of Nays2DFlood over just using COMCOT?

In this study, COMCOT was only used for propagation simulations from Manila Trench to Shantou. Due to the large number of simulations required for PTHA, nesting and inundation simulation were not performed with COMCOT. However, for estimating the inundation area in Shantou, simulations with a much finer grid size and appropriate roughness coefficient settings are required. Nays2DFlood was developed for simulating floods over land, so the users can easily set calculation conditions including roughness coefficient, boundary conditions, etc. with a fine computational grid. So, in this study, inundation simulations were conducted with Nays2DFlood using the results of COMCOT propagation simulations as boundary conditions.

Section 2.5. c. "iRIC" and "CIP" acronyms need to be spelled out on first use.

These acronyms are now specified in the text the first time they are used.

Section 3.1: Based on what objective data was it determined that the current tsunami risk for Shantou is "low"? Low relative to what?

This can be considered to be an engineering judgement, given that a tsunami height of 0.5 in Japan typically leads to limited/no damage. Nevertheless, the authors agree the sentence is a bit vague. It has now been improved to "The PNTA for a return time period under 1,000 years is relatively low, which indicates that the current tsunami risk at Shantou is modest, particularly considering the threat the Manila Trench poses to other areas around it. (Wu and Huang, 2009). "

Section 3.2: Please clarify how the waveforms were selected. The waveforms that provided the closest value of PNTA to the 100, 1,000, and 10,000 year return periods respectively are used?

Yes, this is correct.

Figures 9, 10, 11, 12: Indicate the units

Apologies for this. The units are now indicated.

Section 4.1: This information on previous research should be mentioned up front in the introduction. From this description the present study appears less comprehensive than Li et al. (2016). Need to identify any novelties and advantages of the present study.

The authors agree that Li et al. (2016) is a very important study, which greatly informed this result (and hence why it features prominently in the discussion). However, the difference between this study and that of Li et al. (2016) is not clearly identified in the introduction. This has now been modified to read as follows:

"It is obvious that SLR changes need to be considered when discussing tsunami risk (Shibayama and Esteban., 2023; Hirschfeld et al., 2024; Esteban et al., 2024), especially where the risk is at present generally considered to be low, such as in the case of Shantou.

The present research will thus quantify the tsunami risk for the city of Shantou, considering future SLR. This will be done by conducting a Monte-Carlo-type probabilistic tsunami hazard assessment (PTHA) using the tsunami propagation model COMCOT (Liu et al. 1998) from the most likely source, the Manila Trench. (…) While other authors (notably Li et al., 2016) have also conducted tsunami propagation simulations, for a true assessment of the risk posed by such events it is necessary to also perform inundation simulations. A hazard curve explaining the relation between return period and tsunami height will thus be obtained, and tsunami inundation simulations will be conducted using Nays2DFlood for 100, 1,000 and 10,000 years return periods waveforms for a variety of SLR scenarios"

Reviewer 2

The authors would like to thank the reviewer for the time taken to provide constructive criticisms. These are addressed below in a point-by-point manner. Should the reviewer have any further comments, the authors are happy to further improve the manuscript.

Abstract. This first sentence doesn't provide reader background information about this research, for example, research gap. Instead, it mentioned ocean trench in Philipphines, which seems nothing with the next sentence about Shantou city. Please consider revise and add more information about research motivation at the beginning of abstract.

The authors agree that the first sentence of the abstract did not provide much valuable information. It has now been rephrased to:

"Despite the well-know tsunami hazards originating from the Manila Trench, a detailed assessment of present and future risks to the South coastline of China has not been made. Thus, the present paper paper analyzes the tsunami hazard at the southern coastline of China, and more specifically at the city of Shantou. "

Page 2. How about local sea level rise projection?

The authors agree that, in principle, the regional SLR projections should be used instead of the global. However, the authors were not able to find any local SLR projections for Shantou, and regional levels appear to be very similar to global SLR. Nevertheless, this is a limitation of the study that is now acknowledged in the limitations:

"Finally, another limitation of the present study resides in the use of global SLR projections, rather than regional level ones. Li et al (2024) analyzed tidal gauge data along the South China Sea coast, and found that SLR was an average of 4.0 mm / year, similar to the global SLR rate, although the study did not include the city of Shantou. Other analysis by Li et al (2024), using tidal and satellite observations also shows rates of regional SLR to be 3.4 mm per year in the period 1980-2021, although again this does not include the city of Shantou. Given that the regional rate of SLR thus appears to be similar to that at the global level, but that no local SLR measurements are available for Shantou, the authors chose to use the global level, although this represents a limitation of this study (as it may be that projections for Shantou might be slightly different). "

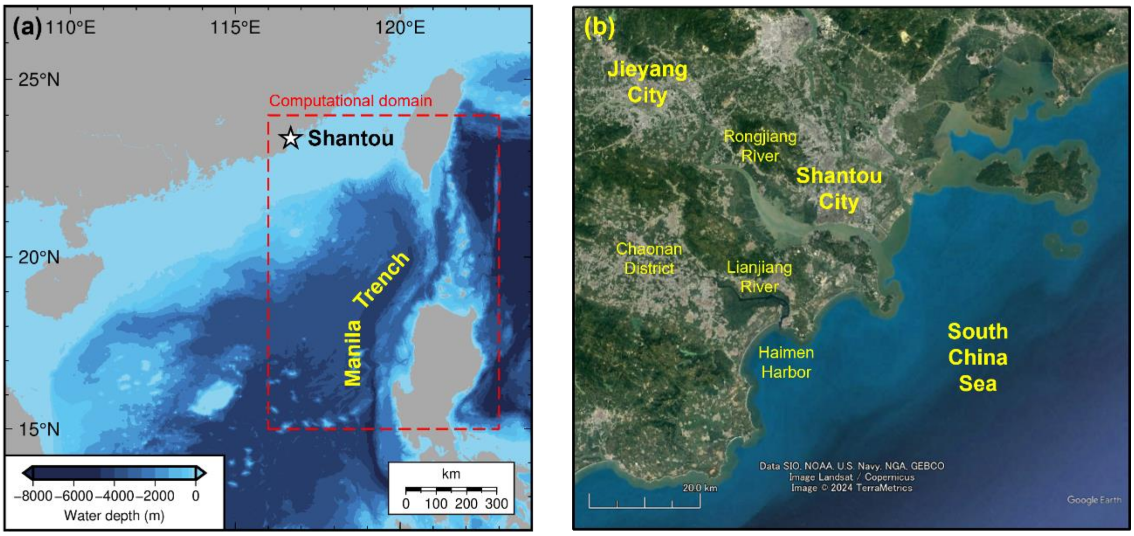
Page 2. Here it is not clear if SLR is a contributing factor or not to the increasing tsunami risk. I recommend adding more literature review about how and to what degree SLR affects tsunami risk in other areas.

The authors agree with the reviewer in that this was not properly referenced. Some more citations have been added, to clearly emphasize the importance of this work.

The sentence at the end of the introduction now reads: *"It is obvious that SLR changes need to be considered when discussing tsunami risk (Shibayama and Esteban., 2023; Hirschfeld et al., 2024; Esteban et al., 2024;), especially where the risk is at present generally considered to be low, such as in the case of Shantou."*

Page 3. It doesn't clearly show the Shantou city in the map. Please highlight this city on the map. And it would be better to show the location of Manila trench as well. This can give reader a feeling of the relation of manila trench and Shantou city.

The authors agree that the figures were not the best. An improved figure is now provided (see below)



Page 3. Could you elaborate figure 2? For example, I'm not clear on 'calculate fault parameters'. What does this mean?

The authors would like to thank the reviewer for this question. Figure 2 just presents the sequence of the calculations that were presented, and which are described in detail in the subsections that follow. The calculation of the fault parameters is described in Section 2.2. Earthquake data and fault parameters.

Page 4. Show the location of case study area on the map.

An improved figure is now provided, which indicates the location of all these places (as per the reply to a previoius comment)

Page 5. How about the longitudes? Why between 119E and 122 E according to fig 4?

Due to source directionality, tsunamis originating further south in the Manila Trench do not produce a tsunami of enough height to consider in this study. This research was conducted by the authors as part of a preliminary study of the wider Manila Trench. which is now cited in the text. This is now acknowledged in the text:

"An earlier study by Zhang (2021) indicated that, due to source directionality, only earthquakes originating in this region had the potential to generate tsunamis that could affect the southern coastline of mainland China"

Page 6. Any reasons why the simulation runs in each interval is between 245 and 322?

The simulations were were performed in parallel using six desktop computers (with different processor speeds, but which took an average of 6 hours per simulation run, representing 30,000 hours of computational time over several months). The nature of conducting such a methodology resulted in the authors leaving the computers to run from prolonged periods of time, and then collecting data at regular intervals. As each computer had different specifications, and accidents happened (computers freezing, or some distracted colleague accidentally turning of a computer, etc), there were some differences in the amount of data collected from each computer for a given time periods. The authors were attempting to obtain more than 200 simulation results from each of the intervals (the current minimum was 200) and had decided that the total would be at least 5000 (due to time constraints related to the project). Nevertheless, this resulted in slightly odd numbers for each interval, but rather than disregarding some data, the authors chose to include everything. This is acknowledged in the text (end of section 2.3). The authors hope the reviewer will understand the limitations of the authors, and the substantial computational effort that was required in developing this work.

Page 8. Local SRL varies a lot. GSLR may not be applicable here.

As per earlier comments, the authors could not find any GSLR projections, and regional ones appear to be similar to global SLR.

Page 8. lacking of unit, [meters] on the map

Apologies for this. The unit is now indicated in the caption.

Page 9. Change to' land use type'

Apologies for this mistake. The reviewer is correct. Changed to "land use type".

Page 8. Considering revising the title. Safety threshold is a combination of water inundation and water velocity according to the author's explanation in the text.

The authors would like to thank the reviewer for this suggestion. The title was changed to “Inundation safety thresholds”.

Page 10. Table 5 is a result of Figure 7. Considering choosing one of them to avoid repeated information. The author can mention PNTA as a function of RP in the text.

Agreed. Table 5 was deleted.

Page 10. Why the PNTA has small variance to the results shown in table 5

The authors are not quite sure about what the reviewer means by this question. Nevertheless, they would like to note at this point that Manila Trench is rather far from the area of interest, and that source directionality generally points towards Vietnam (indeed, simulations by Vietnamese authors available in literature, and those in a preliminary study by the authors show how an earthquake from the Manila Trench has a big impact on Vietnam, but is more limited along the coast of Southern China). The considerable distances also means that much energy is lost as the wave spreads across the ocean. The authors hope that this answer satisfies the reviewer, but are happy to further continue discussions if the authors did not manage to properly grasp the question.

Page 10. Why the peaks occur at around 150 minutes?

The reviewers would like to thank the reviewer for this comment. The following clarification has been added to the paper.
“t=0 is the time of the earthquake, so it takes roughly two and a half hours for the tsunami to propagate from the north of the Manila Trench to the south coastline of China)”

Page 12. No clue where it is. Please highlight it on the map.

An improved figure is now provided, which indicates the location of all these places (as per the reply to a previoius comment)

Page 12. All the areas mentioned in the text should be highlighted on the map.

An improved figure is now provided, which indicates the location of all these places (as per the reply to a previoius comment)

Page 12 The same as the previous comment

An improved figure is now provided, which indicates the location of all these places (as per the reply to a previoius comment)

Page 12. Could you explain why it is not linear?

An explanation about this has now been inserted earlier in this section:

"each inundation simulation was conducted by incorporating the SLR into the bathymetry of the area, in order to capture any nonlinear effects, see Koyano et al., 2020, 2022"

Page 12. Why slightly different to other study?

The authors would like to thank the reviewer for this question. The answer is now provided above this point in the text:

"In the research of Li et al. (2016), a wide range of earthquake magnitudes, ranging from 7.0 to 9.0, were considered along the Manila Trench from around 12.5°N to 23°N, using both uniform and heterogeneous slips. In total, nearly 30,000 earthquakes were simulated by Li et al. (2016), considering both a seismic and geodetic catalog. However, the present study considered only Shantou as a case study, and thus focused only on 5,000 earthquakes that could be generated between 15°N and 23°N along the Manila Trench, using a seismic catalog with a uniform slip model. Due to the differences in the simulation conditions, the obtained hazard curves are thus slightly different to those in Li et al. (2016),"

Page 15. Since this study doesn't cover vulnerability assessment. I recommend using word of 'inundation hazard induced by tsunami'

Agreed. The wording was changed to "inundation hazard assessment", as suggested by the reviewer.

Page 15. I think the implication of this study is SLR is a significant contributing factor to the increasing tsunami hazard. Local monitoring and observation system of SLR should be installed, and climate change mitigation and adaptation initiatives and actions are called for.

The authors agree that this paragraph needed to be futher nuanced, and have done so according to the reviewer's comment. The paragraph thus now reads:

"From the tsunami inundation simulations (see Figs. 10 to 12), it would appear that the area to the northwest of Chaonan District can be affected by tsunami waves, and that this hazard could increase in the future due to SLR. The same is true for the area next to the Rongjiang River. A tsunami early warning system and adaptation countermeasures that could prevent tsunamis from propagating along Lianjiang River, which connects Chaonan District and the South China Sea, should thus be implemented, which could for example include constructing storm surge/tsunami barriers at the entrance of key sections of the river. Regarding the area nearby Rongjiang River, instead of constructing dams across the river, placing wave-dissipation armor or planting trees might be useful, given that there are many harbors that exist along this waterway and that these extend until Jieyang city. The area to the east side of Jieyang city can be expected to suffer less inundation than the two areas just mentioned, given that the origin of this flooding would be tsunami propagation upstream of the Rongjiang River, through a small river (Fengjiang River, and where a small gate could be constructed to prevent propagation along the river to the east of Jieyang city)."

Page 15. As the author mentioned one of the limitation of this study is excluding river levee or dikes in the coastal area. This may largely affect the inundation results. I think it is not a good idea to say there are implications for disaster risk management in Shantou.

The authors agree with the reviewer on this point. The title of the section has now been changed to "Implications of future SLR on tsunami hazard".

Page 15. Hazard

Apologies for this. The reviewer is correct. Changed to hazard.

Second review round

Comments from Reviewer 1:

In the author's response to general questions, the authors address some small part of the introduction and highlight the difference to the Li et al. (2016) study - that is, that inundation is explicitly modeled here.

The authors would like to appreciate the time taken by the reviewer and the constructive criticisms provided.

However, they do not address the difference to the Yingchun et al. (2007) study that contains Shantou City.

Apologies for this omission. One of the main differences is that Yingchun et al. (2007) only conducted propagation simulations, while the present study also conducted inundation simulations (and the effect of SLR). The manuscript has now been modified, so that the sentence now reads: *"According to Yingchun et al. (2007), the probability for a tsunami with a height of over one meter to take affect Shantou city in the next 100 years is 43.99%, although these authors only performed propagation simulations, and did not consider the detailed propagation over land close to the city."*

Furthermore, it is still not clear to me why the tsunami cases were chosen to be less comprehensive than those in Li et al. (2016) (30,000 earthquakes compared to 5,000 here). If the data from Li et al. (2016) already show 0.1 m and 1 m for 100 years and 1,000 years (instead of 0.15 and 0.65 m here) why the authors don't just use those numbers?

Li et al. (2016) conducted nearly 30,000 synthetic earthquakes, but this number included both seismic-based earthquake catalog and geodetic-based catalog, and for a wider range of earthquake epicentres. Li et al. (2016) data only provides the return periods from 100 to 5000 years, whereas the authors wanted to see what the return periods would be up to 10,000 years (which can be thought to be unrealistic in disaster risk management, although the authors note that in current disaster risk management philosophy in the Netherlands such return periods are considered in the design of some coastal structures in densely populated areas). Finally, the authors wanted to verify that the results of Li et al. (2016), and indeed arrive at similar answers. In that sense, part of the –limited- contribution to literature of the present manuscript is a verification, although the authors wish to re-emphasize here that Li et al. (2016) did not conduct inundation simulations –only propagation simulations-, and the present manuscript thus has that merit.

In addition, the authors do not provide a scientific objective or rebuttal with regards to my comment on that.

Figure 1 is much improved and other changes to discussion section 4 are reasonable.

The authors are glad that the reviewer is happy with the improved figures and discussion.

Comments from Reviewer 2:

This revised manuscript has addressed most of the concerns raised in the previous version. However, the scientific contribution of the research is somewhat limited, as it primarily relies on existing models for simulating tsunami-induced inundation. Nevertheless, the findings could help raise awareness of the risks among individuals and government agencies, especially given the high likelihood of sea level rise exceeding 2 meters in this region by the end of the century. While the results may be overestimated due to the lack of consideration for flood defenses, future researchers can build upon this work by developing more advanced models and improving data collection for more accurate simulations in this area. In conclusion, I agree to publish this manuscript in the journal of Coastal and Riverine Flood Risk.

The authors would like to appreciate the time taken by the reviewer and the constructive criticisms provided. Indeed, the contribution to literature is modest, and it is hoped that future researchers can build on it, when more data becomes available. Nevertheless, the authors hope that the paper can indeed help to raise awareness about the risks involved in the area.