RESPONSE TO REVIEW #2

We highly appreciate the time and effort that was invested in reviewing our manuscript. After carefully studying the constructive queries and comments, we have thoroughly answered them in this document in an attempt to clarify as much as possible the content of the draft manuscript and/or the changes that will be made in the revised version where needed. Below you will find our comments (in blue) to your feedback (in **black**) to be implemented in the revision of the draft manuscript.

Summary

The manuscript evaluates the compound inundation in Ho Chi Minh City in Vietnam during Typhoon Usagi. Their main purpose was to determine which flood mechanism drives the flood along the Saigon-Dong Nai river system. The analysis was performed by analyzing observed data and remote-sensing products. Their finding suggests that the estuary system is mainly dominated by coastal processes, despite the fact that the typhoon event only brought rainfall inundation.

General Comments

The manuscript presents a challenging problem to assess in a data-scarce region subject to extreme hazard events, especially the interaction between coastal and hydrologic processes. I have mixed feelings if this manuscript has “enough novelty” to be accepted in a peer-review journal. Since the authors did not develop a new technique or method to investigate the proposed issue, and the results are very specific to this region. Thus, they do not either present broader and general results for the region. However, the compound flood assessment is in high demand, and this manuscript could be a good resource in the literature once it goes under a major revision.

First, there is a lack of novelty in the manuscript, not in the approach selected. In the current version of the manuscript, the novelty of applying the skewness of surge to determine which flood driver dominates it is not highlighted enough, for example. Like this, several other components of the methods are “novel enough” to be published but need to get more attention in the introduction. Thus, I highly recommend including a literature review in the manuscript that summarizes other studies that have used similar techniques to the authors and identify the missing gaps of previous works and how this manuscript tries to fill them.

Second, the authors should focus the theme of the manuscript on a “compound flood assessment” rather than a study of the hydraulic/hydrology response of the watershed. The authors are underselling their work and should put more emphasis on the “hot topic” of compound floods, which is, in reality, what the authors are doing since they are also considering coastal processes and their impacts. I strongly suggest rewriting and refocusing on this theme, including the title.

Third, the manuscript format can be improved substantially to follow a “storytelling” rather than a report. For example, the authors have a “Results and Discussion” section, but a “Discussion” section follows this one. The “Discussion” section is, in reality, a sub-section of the results since they focus on the flood impacts at the urban center, whereas the discussion section should be for comparing their results with previous findings and the physics. The authors did a great job discussing their results in the “Results and Discussion” section. Thus, I strongly recommend separating the discussion from the “Results and Discussion” section and making it a stand-alone section called “Discussion”. In addition, the current “Discussion” section should be a sub-section on the new “Results” section.

Lastly, there needs to be a more coherent nomenclature and wording with the current published studies within this field. This could be from a translation from their native language to English. For

example, the authors used the word “continental” to refer to hydrologic effects on the flood. However, current studies use the word “inland” more to differentiate from the coastal process in a compound flood event. Thus, the authors assess the “inland and coastal effects” on the hydrosystem, not the “continental and coastal effects”. Similarly, the word “evacuate” is being used oddly for the field when referring to the riverine water leaving its banks and flooding the community. Also, the authors used the term “extreme water levels events”, whereas the community uses more “extreme flood events”.

Our initial motivation for writing this manuscript was in light of the previous research conducted by Camenen et al. in 2021 (introduced in L41-L45). In this research, which provided a monthly evaluation of the Saigon River's response to this extreme rainfall, there emerges a paradoxical result of a lack of direct response in both water level and discharge. However, the authors of the current manuscript recognize the importance of investigating the finer-scale behaviour of the hydrosystem during and immediately after this event, as the monthly average may not capture the dynamics adequately. This is the reason why we chose our title to be more generally focused on the hydrosystem. However, as correctly pointed out by Reviewer 2, the manuscript addresses a challenging problem of assessing compound inundation in a data-scarce region. We do believe that the most important outcome of this manuscript becomes exactly this one. We appreciate the suggestion to focus the theme of the manuscript on a "compound flood assessment" rather than solely on the hydraulic/hydrologic response of the watershed. You are correct that our study addresses the issue of compound floods by considering both coastal and hydrologic processes. We will reframe the manuscript to put more emphasis on the compound flood assessment, as this is a hot topic in the field. Additionally, we will change the title for the final revision to be: *“Understanding Compound Flooding in Ho Chi Minh City: Assessing the Combined Impacts of Rainfall and Coastal Processes during Typhoon Usagi”.*

We understand the reviewer's concerns regarding the novelty of the study. While we did not develop a new technique or method, we believe our research contributes to the field by applying and adapting existing approaches to the context of this case study. We would like to clarify why we strongly believe the results provided in this manuscript are valuable for the scientific community. The two main points are as follows:

Firstly, for the first time (to the best of our knowledge), we compare and evaluate several distinct datasets of precipitation against in-situ measurements over this region; and, in particular, their capacity of capturing extreme rainfall as brought by Typhoon Usagi. Indeed, the techniques used to obtain this result are not new but we do believe that this result is a valuable contribution to the scientific community interested in this area in particular, but also other areas with similar characteristics. Given the context of data-scarcity this result is especially relevant to the current body of knowledge.

Secondly, we jointly analyse different bodies of free, open-access data (precipitation, water level, topography, land use, wind and flooding hotspots) to holistically characterize the drivers of compound flooding during this extreme event. Each method to analyse the data and provide results is not novel but have never been used in unison for a case study of compound flooding during an extreme event. We believe that the value of this manuscript lies, on one hand, in the smart integration of different sources of data with different analysis methods and, on the other hand, in the outcomes and insights of this integration. We effectively show that even in a region where reliable data is hard to obtain, it is possible to unravel (to a certain extent) the complex interplay between coastal and hydrologic processes and gather meaningful information that allows the establishing of a case study such as this one.

We acknowledge that the results are specific to this region and may not offer broader and general conclusions for other areas. However, the focus on compound flood assessment is highly relevant

and significant, as it addresses the growing demand for understanding the combined impacts of different flood drivers in the region. By analyzing the coastal and hydrologic processes, our study offers valuable insights into the mechanisms driving floods in the HCMC urban, low elevation coastal zone, which is a hotspot of vulnerability. HCMC is often presented as one of the most vulnerable cities in the world with respect to climate change and water-related issues (please see the answer to Reviewer #1 for a more in-depth explanation of the reasons why).

Thank you for pointing out the need for improved manuscript formatting and a clearer storytelling approach. We agree that the current structure could be enhanced to provide a more coherent flow of information. We will make the necessary changes by separating the current "Discussion" section from the "Results and Discussion" section and creating a standalone "Discussion" section. The current "Discussion" section will be incorporated as a subsection in the new "Results" section. This revision will ensure a more logical organization of the manuscript and allow for better comparison of our results with previous findings and underlying physical processes.

We appreciate the comment regarding the nomenclature and wording used in the manuscript. We apologize for any confusion caused by the terminology inconsistency. We will revise the manuscript to use the more commonly accepted term "inland" instead of "continental" to refer to hydrologic effects on the flood, aligning with current studies in the field. Similarly, we will replace the term "evacuate" with a more appropriate term to describe the riverine water leaving its banks and flooding the community. Additionally, we will modify the phrase "extreme water levels events" to "extreme flood events" to align with the standard terminology used in the community.

Finally, we would like to express our gratitude to the reviewer's suggestion that the manuscript could serve as a useful resource in the literature. We will take their feedback into account and ensure that the manuscript undergoes thorough revision, including better highlighting the novelty of our approach, providing a more comprehensive literature review, and emphasizing the contribution of our study to the field of compound flood assessment. Through these revisions, we aim to strengthen the manuscript's value and relevance for publication in NHESS.

Specific Comments

• L24: remove the word “coastal” from “coastal engineers” since it can also help water resources engineers. I will also remove the word “reliable forecasting” there is a lot of effort needed to get to this point, such as computational resources, meteorological forecast inputs, accurate models, and not just the basic understanding of the hydrodynamics of the system.

Thank you for your remark. We will implement this in the revision.

• L25: researchers almost never do decision-making activities, as this statement suggests.

Thank you for your remark. Indeed, we will implement this in the revision.

• L35: give an example of population density from another major city (e.g., New York, Hong Kong, Mumbai, etc.) so the reader can have a fair comparison for this statement.

Thank you for your remark. To provide a fair comparison, an example of population density from another major city will be included to enhance the reader's understanding.

• L55: be consistent with your acronyms. The authors first used LECZ to refer to a low-elevation coastal zone, but in this statement did not use the acronym. Similarly happens with HCMC throughout the entire manuscript.

Thank you for your remark. Consistency in acronyms will be ensured throughout the manuscript. The authors will consistently use "LECZ" to refer to the low-elevation coastal zone and maintain consistency with the acronym "HCMC" for Ho Chi Minh City.

• L60: quantify the “short spatial scale”. Give an example.

Thank you for your remark. Here we refer to short spatial scale to mean the scale of the district size of HCMC namely in the order of magnitude of the kilometer. For example, it can be that it is raining heavily in one district whereas in another one it is not raining at all. We will clarify this in the revision.

• L64: describe what it means to have a negative discharge value on this gauge.

Thank you for your remark. Having a negative discharge at this location means we have river flow towards upstream. This will be clarified in the next version.

• L68: Where the tides dominate in the river? Until what river length from the outlet or it is complete?

Thank you for your questions. Indeed, the tide dominates the totality of the river Saigon from the confluence with the Dongnai river to the outlet of the Dau Tieng reservoir. This information will be included in the revision.

• Figure 1: Need to add a map that shows where HCMC is within Vietnam and then zoom into the basin and the city. Panel (a) add the label for the Vietnam-Cambodia border and the name of the main rivers. What are the grey lines in panel (b)? need to add it to the legend.

Thank you for your remarks. A map depicting the location of HCMC within Vietnam will be added. In panel (a), the label for the Vietnam-Cambodia border will be included in the next version and the name of the main rivers as well. The grey lines are depicting the complex natural and artificial canal network around the main rivers. The legend will be updated to explain this.

• L85-91: the authors give too many details about the classification of the typhoon in this paragraph. I would condense this since it is not pertinent to the manuscript.

Thank you for your remark. We will take this into account in the revision.

• L94: did the authors consider soil type? They only have datasets of topography and land use, but they talk about infiltration and groundwater recharge as one of the main processes during the flood but do not talk anything about the soil types which govern these processes.

Thank you for your remark. Indeed, while datasets of topography and land use are available, the discussion will be expanded to include the influence of soil types on infiltration and groundwater recharge during the flood. We did not fully consider the soil type upstream of the city center in the text but did consider literature concerning this topic (Khai et al. 2015, Tu et al. 2022). Nonetheless, a more thorough literature review on soil type will be added in the revision.

Khai, H. Q. and Koontanakulvong, S.: Impact of Climate Change on groundwater recharge in Ho Chi Minh City Area, Vietnam, In proceedings: THA 2015 International Conference on Climate Change and Water & Environment Management in Monsoon Asia, https://www.researchgate.net/publication/275643904_Impact_of_Climate_Change_on_groundwater_recharge_in_Ho_Chi_Minh_City_Area_Vietnam, 2015.

Tu, T. A., Tweed, S., Dan, N. P., Descloitres, M., Quang, K. H., Nemery, J., Nguyen, A., Leblanc, M., and Baduel, C.: Localized recharge processes in the NE Mekong Delta and implications for groundwater quality, Sci. Total Environ., 845, 157 118, https://doi.org/10.1016/j.scitotenv.2022.157118, 2022.

• L95: as the statement is written, it says that extreme events, like a typhoon, would have an effect on the astronomical tides. However, they do not alter this response.

Thank you for your remark. Indeed, we will remove “astronomical tide” and restructure this sentence accordingly.

• Figure 3: all the components in the diagram are talked about in the main text of the manuscript, with the exception of the “mapping and characterization”. The authors should explain this more. Also, on the figure label, the focus is on the “hydrological system”, but it also talks about coastal processes. I recommend changing the wording toward “estuarine system” which implies both coastal and hydrologic processes.

Thank you for your remark. The authors will provide a more detailed explanation of the "mapping and characterization" component mentioned in the diagram. This component refers to the analysis of the different sources of information and results in order to characterize the impact of this typhoon on compound flooding in HCMC and upstream of HCMC and explain the interaction of the different drivers. We will explain this more clearly in the next version. Additionally, the wording in the figure label will be changed to "estuarine system" to reflect the inclusion of both coastal and hydrologic processes.

• L110: the authors lack a justification for the selection of a 3-day rainfall total for this analysis. All the datasets have a maximum daily time scale. Why not select a daily accumulation rather than a 3-days total? Also, the word “adequate” needs a quantification. What is adequate for the authors might not be for other readers.

Thank you for your remark. Indeed, we did not justify our choice. The reason is that this is the time frame of the heavy precipitation event which starts on the 24th and ends on the 26$^{th.}$. Additionally, using the 3-days total instead of a daily total allows us to mitigate some daily missing data in the in-situ data that we obtained from the HCMUDC - HOS in this data-scarce region. The selection of a 3-day rainfall total for the analysis will be justified in the manuscript for clarity of the reader.

• L112: What criteria the authors used to “deem sufficient” the quality of the observed data?

Thank you for your remark. Indeed, we do not provide an extended explanation on how we selected the rain gauges used for the study. We choose rain gauges data mainly based on the availability of the data. Some gauges presented meaningful gaps of several weeks with no data. These gauges were not used for the study. Only rain gauges with more than 450 daily measurements during the selected period (about 3 years) were used. This amount of measurements was chosen given that during the dry season HCMC experiences very few rainy days. We will clarify this in the revision.

• Table 1: the nomenclature for the correlation coefficient equation is missing. What represents “cov(P,O)”?

Thank you for your remark. The nomenclature for the correlation coefficient equation will be included, specifying the meaning of "cov(P,O)" to be the covariance between the predicted and observed values.

• Table 3 is in the text before being cited. The table should be cited first and then shown. Also, how can the authors visualize a semi-diurnal tidal behavior if the time resolution of the tidal gauge has a daily time step, meaning only one value per day?

Thank you for your remark. The table will be cited first before being presented in the text in the next revision. Regarding the visualization of semi-diurnal tidal behavior, there is a mistake in Table

3. The time resolution of the tidal gauge is hourly and not daily. This will be corrected in the revision.

• L173: what was the time window for the moving average performed for the monthly tide values?

Thank you for your remark. In order to remove the monthly variability in water level time series, we subtract the monthly moving average (window size is equal to 30 days) from the hourly tide gauge or 10-minute river gauge water level time series. We will clarify this in the next version.

• L177: mention the amount of tidal constituent used in the resynthesize analysis.

Thank you for this remark. We use all the constituents (146) except the 6 constituents that include quasi-periodic meteorological effects thus, the total amount of constituents is 140. This will be mentioned in the revision.

• L185: generally, you should not refer to a figure before presenting other ones. For example, the authors cite Figure 7, but only have presented three figures.

Thank you for this comment. The reference to Figure 7 will be removed to ensure other figures are presented before it.

• L216-217: the authors should justify why they used the selected thresholds of dH and dt.

Thank you for this comment. Indeed we did not justify this in this paragraph. These values come from a calibration step which is only mentioned in L221-L228. We will restructure this paragraph such that the source of these values is immediately clear to the reader.

• L260-261: have other studies found similar results with ERA5?

Thank you for this question. At the time of writing the authors did not find any comparable studies using ERA5 in similar regions. However, a thorough literature research will be done and a comparison and discussion of these results versus other previous results will be included in the discussion section of the revised version.

• L303-304; L320-322: are these findings also been found by other researchers? Find additional literature that supports or refutes your findings. That should be part of your new discussion section.

Thank you for this remark. Similarly to the answer to the previous remark, other findings will be compared and contrasted with other this work, supported by additional literature, which will be incorporated into the new discussion section. At the moment, no other findings directly related to the area of study have been found.

• Figure 6: why the observed discharge is higher in the wet season than in the dry if the observed water level is higher in the dry season than in the wet? Discharge is computed from the water level, so they should have the same behavior, which is not the case.

Thank you for this remark. The main driver of discharge as estimated via our method is the slope between the two river stations. During the dry season the water levels at both stations tend to be higher on average than in the wet season but this difference in magnitude on the seasonal average is not necessarily transferred to the instantaneous slope of the water surface. As discussed in the text (L476-480), a proportional change in water level at both stations leaves the slope variable constant and thus, the discharge is little affected. Therefore, it is physically possible that we have stronger slopes with lower seasonal average water levels and estimate higher average seasonal

discharge. This strong dependence on slope is one of the drawbacks of our method to estimate discharge as discussed in the Discussion section. We will clarify this in the revision.

• L343: where the coastal water level is the main driver and not the rainfall?

Thank you for this question. In this sentence we are referring to the Saigon river water levels at least all the way upstream to our Phu Cuong river gauge where we clearly see these effects. The logic behind it comes from the statistical interpretation of our data: in the rainy season, we have very clearly much stronger precipitation, yet the river water levels are lower in this season which, at first, might seem paradoxical. On the other hand, the coastal water levels decrease in this season due to the change in direction of the monsoon wind. Hence, at the seasonal scale the river water levels are controlled by the coastal water levels. This is explained in text in L311-315. We will make it clearer that we see evidence that this downstream control is valid for the whole extension of the Saigon river.

• L381-387: move out from results into methods and data collection. This will explain to the reader why the authors also consider wind data. It was quite strange when I saw wind vectors in Figure 2.

Thank you for this remark. We will displace this paragraph to the methods section, providing a justification for its inclusion. The placement of wind vectors in Figure 2 will then be more clear.

• Figure 7: add a legend to the figure explaining each color of the lines. Also, add the datum to which the levels are referenced.

Thank you for this remark. The color of the lines is already explained in the first sentence of the caption of Figure 7: black is the results for the Phu Cuong location, grey for Thao Dien and blue for Vung Tau. Nonetheless, we will make it more visible in the revision.

There are no datums that can be used as reference for the water level measurements in the river. The tide gauge is the only one to have a station datum as provided in the repository of the Sea Level Center of the University of Hawaii (link to Vung Tau station datum information: https://uhslc.soest.hawaii.edu/stations/?stn=383#datums . However, given the unknown datum of the river stations we cannot compare them. In order to mitigate this problem with perform mean normalization across the gauges such that the tidal signal is fluctuating about zero, as mentioned in the response to Reviewer 1. This allows the comparison between gauges. We will clarify this in the revision.

• L410-424: these are not results and more a description of the study area. I would move them out and into the study area section, including the figures. Maybe the wind vector panels in figure 2 can be swapped with the top three panels in Figure 8.

Thank you for this remark. We will take it in consideration when revising the manuscript.

• Figure 8: panel a) the track line in the legend is green but in the map is purple. Panel b) add the datum of the elevation from the DEM.

Thank you for this remark. Indeed, the DEM datum information is not present in the legend. The SRTM vertical datum is global mean sea level and is based on the WGS84 Earth Gravitational Model (EGM 96) geoid as specified in:

1. U.S. Geological Survey, Earth Resources Observation and Science (EROS) Center. (2018). USGS EROS Archive - Digital Elevation - Shuttle Radar Topography Mission (SRTM) 1 Arc-Second Global. Retrieved from https://www.usgs.gov/centers/eros/science/usgs-eros-archive-digital-elevation-shuttle-radar-topography-mission-srtm-1

We will take implement these changes in the revised version.

• Conclusion: Add a paragraph about the limitation/assumption the method used by the authors may have.

Thank you for this remark. We will implement a new paragraph discussing the limitations of our approach and methods.

Thank you for your feedback and valuable suggestions regarding the manuscript. We appreciate your insights and will address each of your concerns in the revision.