Response to Reviewer #1 of our manuscript entitled

Coastal extreme sea levels in the Caribbean Sea induced by tropical cyclones submitted to *Natural Hazards and Earth System Sciences.*

Ariadna Martín, Angel Amores, Alejandro Orfila, Tim Toomey, Marta Marcos

December 20, 2022

This is my second review of the paper. I want to thank the authors' work and effort answering my recommendations to the first version of the manuscript, which I believe helped to improve the paper. The validation section was completely re-written, giving from my point of view a better foundation to the results. I still think the investigation contribution is interesting and scientifically relevant, with useful results for coastal risk assessment associated to tropical cyclones in the Caribbean Sea. However, in my opinion some aspects need to be improved. For this reason, my recommendation is a minor revision before considering its publication in the Natural Hazards and Earth System Sciences journal

Specific comments:

(1) — L57. Review the sentence structure.

A — The sentence has been rewritten

(2) — L61-63. In my previous review, I asked to clarify how the PDFs were built. Although authors improved the description, I still have problems understanding this procedure, which is important, as it is the base of using a 1000 TCs to represent the complete dataset. I suggest including the maximum wind speed and the spatial distribution of the TCs track PDFs in a two panels figure in the supplement material. In each case, show the PDFs from the complete dataset (25494 TCs) and from 1000 events sub-set, as this was selected as the proper number of TCs to represent the complete dataset. Please see my comment (23) to Figure 1.

A — We explain the methodology in the manuscript and refer to ? for more information. We agree that the information requested by the reviewer is necessary but, to our understanding, it is the same that is included in Fig.3 and Fig.S1, where we show the wind speed distribution for the subset and the complete dataset respectively. Panels e) and f) of Fig.1 shows the same for the spatial distribution. Thus, we do not see the need to repeat the information.

(3) — L62. Remove "(".

A — Done

(4) — L63. . . . "for the maximum wind speed" . . .

A — Done

(5) — L82. "area". "Caribbean Sea".

A — Done

(6) — L96. "five real TCs".

A — Done

(7) — L141-142. "GPD fitted to all measurements". I suggest replacing measurements for "synthetic values" or similar, as return levels are not constructed from observations.

A — Done

(8) — L156-158. I bring again this comment, as I fail to explain myself in the previous review (comment 9). Indeed, the TCs prevailing travelling direction in the Caribbean is toward the west-northwest. As commented by Torres and Tsimplis (2014), "Due to the diminution of the Coriolis force close to the equator, any tropical cyclones passing toward the south of the basin are weak. South of 10°N there is less than 1% chance of a hurricane strike per year [Pielke et al., 2003]". Therefore, my suggestion was to consider if you wanted to include a comment about the relation between the weak Coriolis force toward the south of the Colombian Basin and the smaller number of TCs per decade seen in this region.

A — We thank the reviewer for this suggestion. We now understand the point and have included the text accordingly in the manuscript, along with the reference provided

(9) — L179-181. Please clarify this sentence. You compare landfalls per year, but figure 3 shows landfalls per decade. Besides, where the reader can see the results form IBTrAcs? It is in one of the Knapp et al. papers or is your own calculation, which is not shown?

A — The comparison between STORM and IBTrAcs is extracted from ?. Here we just emphasise some important aspects of the study that should be taken into account. For clarification, we have added the reference in the manuscript.

(10) — L184. For the first time in the paper the variable "Hs" appears, therefore please indicate its name. This variable is usually used to define "significant wave height". By definition, "significant wave height" indicates the mean wave height of the highest one-third of the waves. Through the paper, Hs is not used following the pervious definition, e.g. Figure 5ab, "a) and b) represent the 99th percentile of the maximum Hs ...". Please consider changing this variable (could be "wave height") and review the correct use of the term "significant wave height" in all the manuscript

A — We have added the symbols H_s, T_p and D_p the first time their name is mentioned (L 119-120). Here we use the same definition of H_s: mean wave height of the highest one-third of the waves, as it is the variable that provides the model. When referencing e.g to the 99th percentile of the maximum of H_s, one has to understand that the model provides H_s for each time step for each TC for all the grid points. To represent that on the map, first we use the maximum value of H_s affecting each coastal point by each TC, obtaining (# of coastal points) x (# TCs, in our case 1000) values. Then, we compute the 99_{th} percentile of that values to obtain a single value for each coastal point.

(11) — L192. The agreement is not so good in SSE. As you mention in line 288, your SSE includes "only the hydrodynamic response" to wind, pressure and waves. Therefore, the lack of a better agreement is probably because the observed SSE includes the tide, while your simulated SSE does not. Although the Caribbean has a microtidal environment, this can be important for extreme SSE. I recommend that for the comparison shown in Fig 4b, you use the tidal residual from the observed sea level time series. This can also have an important effect when fulfilling the 0.4 m peak criteria used to detect cyclone-related SSE in sea level time series (L190).

A — We apologise for the missing information. Indeed, for the comparison between observed and modelled SSE, we have used the non-tidal residual of the tide gauges. Prior to the validation we removed the tides using UTide function. We have added this information to the methodology section (L 80-82)

(12) — L210. Please verify the referenced figure.

A — It is correct

(13) — L222-225. I found more interesting the SSE results when they are shown as relative terms (Fig.S2 - contribution percentage), when compared to absolute values as shown in Fig.6. This is because in my view, the paper provides a statistical perspective of the TC effects in the Caribbean Sea, what I found more important than the absolute values presented. Please consider to switch these two figures.

A — We are grateful for the suggestion. We have considered switching Fig 6 and Fig S2. However, we believe that this would be inconsistent with the rest of the results shown in the manuscript, which are given in absolute terms. This is why we prefer to keep the figures as they are now. Nevertheless, we agree the information is relevant, but we hope that the interested reader can access easily to the supplementary material when needed.

(14) — L230. I do not see how figure S3 supports the relationship between the distance to the eye and the atmospheric pressure contribution to SSE. Please see comments to Figure S3 (28).

A — The reviewer is right. In figure S3 we are only illustrating the relationship between the distance of the TC to the coastline and pressure effect. The text has been modified accordingly.

(15) — L237. "the model's spatial resolution".

A — Done

(16) — L243. Consider replacing "northern coast" by "northern Caribbean boundary", or similar. This because the northern coast can be understood as the Atlantic coast of the Greater Antilles.

A — Done

(17) — L248. In my previous review, I made a suggestion to re-arrange the Summary and discussion section. The authors' response provided an outline of this section with a good structure. However, in my view, a problem remains. In the paragraph that starts in L263, you start discussing the wave results, but from line 265 to 275, you give some examples of historical major impacts from hurricanes in the region. However, not all these impacts are limited to the wave height effect, but probably including storm surges, and others effects. Therefore, I suggest moving this section before of the last paragraph, which starts in L314.

A — The have now rearranged the summary. Following reviewer's suggestion we have moved the references to the impact of TC in the Caribbean. However, instead to moving this part to the end of the summary, we have used it as a starting point. As it is now the references are not inconsistent in terms of the causes of the TC impacts (not necessarily waves only).

(18) — L253. It is mentioned here for the first time in the paper that the "Caribbean basin family generates off the cost of Honduras". This exact place of generation of the Caribbean TCs family was not discussed previously in the results section. Besides, in my view, this statement is inaccurate, as in Figure 3e, TCs effects from this family are seen even in the Venezuela basin. I mention this issue again, as my comment 22 of the previous review was not clearly answered by the authors.

A — There might be a misunderstanding here. We refer to the formation area, not to the impacting area in this sentence, which can indeed affect other regions such as the Venezuela basin.

(19) — L261-262. This line was included, answering to my comment (24) from my previous review. I recommend including the reference, so readers can know the origin of this statement.

A — Done

(20) — L279-280. Please consider including a value or range of the wave height 30-year return level found by Montoya et al (2018), as I believe it might serve as a reference to the 100-year return levels found in your research.

A — Done

(21) — L282. "Colombia Basin".

A — Done

(22) — L298. Please verify the referenced figure.

A — Done

Comments to figures:

(23) — (23) Figure 1. Please see my comment (2). ¿Why the vertical axis in panel c) is in meters, if the spatial domain was divided into 2 degrees bins? Below the colorbar there is a title "Normalized Spatial distribution (%hurricanes (TCs)/pixel)". ¿How this distribution was normalized? Besides, in L62 you clarify that the PDF is built using the 3-hourly time step for each TC passing though each pixel of the grid. Therefore I am unsure if panels e-f are showing the "% of TCs/pixel" or the "% of TCs hits/pixel", understanding a "hit" as each time the 3-hourly TC position is placed in the pixel. Based on this, if necessary, update L67.

A — The reviewer is right, the units in panel c) are wrong. This, along with a clarification on how the distribution was normalized has been added to the legend. With regards to how the PDF was built is express on L63: "counting each time step".

(24) — Figure 2. A buoy used to validate Hurricane Ernesto (black dot) is shown in the northern boundary of the study area, which is probably a mistake. Try to improve the description of the location of buoys and tide gauges in the legend.

A — There is no mistake in the location of the buoy. The fact that it seems to be outside the domain is due to the size of the dot, which is chosen to be wider than the boundary line. We have also modified the figure caption to improve the description.

(25) — Figure 3. Title of panel (a) and in the legend replace "mean" for "median", as stated in L153. Consider keeping the range of the color scale for panels a, e and f between e.g. 50-200 km/h, so an easier comparison between these results can be done by the reader. Same comment applies for the color scale range in Fig. S1c and d. In the third line of the legend replace "if" for "it".

A — We have corrected the title. We have not made any changes to the colour scales because the geographical patterns are hardly visible if we expand them as suggested by the reviewer.

(26) — Figure 4. My comment (18) from the previous review was about a probably too long wave period (14-18 s) reported inside the Caribbean Sea. In the answer to that comment, you included a validation of the period comparing the model results and buoy data. I suggest including in Figure 4 the validation of the maximum period between the model and the buoy data as forced by TCs, in a similar way as the wave height is presented in panel (a). Besides, include a grid and/or line of equal observed and simulated values, to facilitate the results assessment. At the end of the legend I suggest to include "... between the eye of the TC and each instrument at the moment of the largest observed value", or similar clarification.

A — We have added a panel to the validation figure (Fig.4) to show the wave period validation for all the buoys used for the H_s. We have clarified the legend accordingly. Regarding the grid in the figure, we feel that it hampers the comparison rather than facilitating it, as it makes the plot more blurry.

(27) — Figure 6. First line of the legend: "a value that".

A — Done

(28) — Figure S3. (a) Seems to be the same as Fig S1a, but with different color scale values; I do not understand the maximum value of 100 TCs. (b) Seems to be the same Fig S2d; the title inside indicates meters what is not coherent with the color scale legend (%). (c) I am not sure that I understand this figure. ¿It tries to support that the larger the number of TCs affecting a node, the larger the atmospheric pressure contribution to SSE? Please see my comment (14).

A — The name in panel b) has been corrected. As for the rest of the comments, yes panel a) and b) are shown in other figures. Please, refer to our response to comment (14) for Fig. S3.

Response to Reviewer #2 of our manuscript entitled

Coastal extreme sea levels in the Caribbean Sea induced by tropical cyclones submitted to *Natural Hazards and Earth System Sciences.*

Ariadna Martín, Angel Amores, Alejandro Orfila, Tim Toomey, Marta Marcos

December 20, 2022

In their manuscript "Coastal extreme sea levels in the Caribbean Sea induced by tropical cyclones," the authors Martín et al. use a new database of synthetic tropical cyclones as forcing to simulations of wind waves and storm surges in the Caribbean Sea. It is shown that the wind waves and storm surges vary significantly at coasts around the basin, due to differences in storm evolution, local bathymetry, and other characteristics.

Main comment:

(1) — The authors should be commended for revising the Validation to use a consistent atmospheric forcing and to include comparisons to as many observations as possible. That said, the validation for the water levels is unconvincing, with some large errors between observation and model. The authors have selected a threshold water level of 0.4 m to identify observations to include in the validation. This value is somewhat arbitrary? Can the set of available observations be expanded if this threshold is relaxed?

A — We agree with the reviewer and we had indeed tested different thresholds. Of course, a lower threshold would result in larger number of values, but then the question is whether those maxima are indeed related to the passage of TC or to any other disturbances present in the observations. The selection of the threshold is therefore a trade-off of the number of peaks and the real signals. Despite the limitations of the results we believe that this threshold is good enough. We have tested it and no improvements were found with other cases..

(2) — This reviewer also wants to push again on the novelty of the study. The authors have done a better job of emphasizing the lack of comprehensive studies in this region with large numbers of storms and a large geographic coverage, and thus this study does fill a gap in terms of available data. But what does it add to our scientific understanding of storm-induced hazards in the region? As-is, the Discussion confirms findings from observations and other studies. The largest waves affect the Lesser Antilles, West Indies, and northern Caribbean ... which is known from historical storms Hugo, Maria, Irma, and David and recent studies by Pillet and Montoya. The largest water levels are found in Cuba, Mexico, and Belize ... which matches the findings by Torres and Dullaart. The atmospheric pressure has its largest effect along the storm track, whereas the wind forcing has its largest effect in shallow coastal areas ... again, this is known. Is it possible for the authors to extract more understanding from this great new database?

A — The information we provide in the manuscript may not seem groundbreaking. However, as we emphasise in the introduction (L37-39), the importance of the study lies in the numerical quantification of these results. In addition, having done a study that encompasses the entire Caribbean coastline, our results focus on highlighting those areas most affected and understanding why, while doing an effort to compare these results with historical data. In addition, the results of this work can be used for the study of smaller

or more localised areas where until now there have been insufficient data. The data we provide from the model can be used for coastal protection studies or to extend our results to more specific areas with higher spatial resolution. All the data are freely available.

Other specific comments:

(1) — For the Validation, it is not clear if tides are included. This reviewer guesses not- can this be clarified?

A — Prior to the validation we removed the tides using UTide function. We have added this information to the methodology section (L 80-82)

(2) — Relatedly, for both the Validation and Results, if the tides are excluded, then maybe 'sea surface elevation' is not the best term. It would be better to refer to 'storm surge' or 'non-tidal residual.'

A — The standard term for the position of the water surface is sea surface height (see for example the outputs in Copernicus Marina Data Store. Our model provides the variable elevation, so we have modified the term as sea surface elevation. We agree that when dealing with observations it is more common, and probably more understandable, to use either storm surges or tidal residuals. But we feel that SSE is also suitable for the modelled data in our case and see no reason to change it.

(3) — Lines 75-84 and Table 1: For the water levels, it is mentioned that the gauges were selected if they had observed peaks larger than 0.4 m. For the wind-waves, how were the buoys identified in Table 1 - was there a similar threshold for the peak in significant wave height?

A — In the case of wave validation there was no need to use a threshold because we found numerous buoys that could be used simply by visual inspection, unlike for the sea level time series. We used a threshold for the SSE validation because it was difficult to find an event with available data. So, instead of checking all the periods visually (which would have taken a long time), we did the selection in an automatic way, thus needing a definition of event.

(4) — Lines 93-94: There are newer ways to reduce momentum transfer in overland regions based on land-use/land-cover data. The method in this study (with a uniform 20 percent reduction) is likely okay because the computational domain does not contain a significant amount of overland regions, and the analyses do not focus on them. Can these points be noted here?

A — We agree but, in our case, this is not really a relevant factor, as those winds do not generate waves or storm surges, once the TC has made landfall. We only included it for completeness of the method.

(5) — Lines 117-118: Can the authors provide a reference to support this statement?

A — A reference to Bertin et al. (2015) has been included in the revised version of the manuscript.

(6) — Lines 16 and 156: "over" is a spatial relation, better to use 'more than' here.

A — Done

(7) — Line 20: 'nations' should be plural.

A — Done

(8) — Lines 26 and 186: "since" is a temporal relation, better to use 'because' here.

A — We understand that both are acceptable.

(9) — Line 82: "are" should be 'area'.

A — Done

(10) — Line 84: 'Figure' should be capitalized.

A — Done

(11) — Line 97: "lastest" is misspelled.

A — Corrected

(12) — Lines 101-102 and 106-107, and page 9 footnote: Can the URLs be moved into the list of references?

A — We are unsure about the criterion of the journal in this respect. We expect that this will be assessed at the editorial level, if the manuscript is accepted for publication.

(13) — Line 109: Here, "Fig." is abbreviated, but on the preceding page, "Figure" is spelled fully. Please be consistent. See also line 152, etc.

A — We have corrected this.

(14) — Line 122: "In order" can be deleted.

A — Done

(15) — Line 163: "Tcs" is mis-capitalized.

A — Corrected

(16) — Line 186: "fullfil" is misspelled.

A — Corrected

(17) — Line 187: "Figure Fig." is redundant.

A — Corrected

(18) — Line 220: When the letter 'm' is shown in italic font, this reviewer assumes it is a variable, e.g. 25 times m. If it is meant to be a unit (meters), then it should not be in italic font.

A — Done

(19) — Line 239: Add spaces between the years in this list.

A — Done

(20) — Line 250: "a" can be deleted.

A — Done

(21) — Line 268: "hurricane" should be capitalized.

A — Done

(22) — Lines 299-300: Can this sentence be rewritten for clarity?

A — .We have slightly rephrased the sentence

(23) — Line 300: ”In fact” can be deleted.

A — Done

Comments to figures:

(24) — Figure 1 caption: In the second sentence, starting with ”Where” is awkward - should this instead be a continuation of the first sentence? In the third sentence, the word ”represents” should not be plural.

A — Done

(25) — Figure 3: For panels (a) and (b), why not use ‘intensity’ or ‘maximum wind speeds’ as labels for both plots?

A — Done

(26) — Figure 3 caption: ”when if is within” should be corrected.

A — Done

(27) — Figure 6 caption: ”taht” is misspelled.

A — Done

(28) — Figure 7 caption: ”Levels” should not be capitalized.

A — Done