Review of the manuscript: "**Coastal extreme sea levels in the Caribbean Sea induced by tropical cyclones**" by Ariadna Martín et al.

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.

(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.

(3) L62. Remove "(".

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

(5) L82. "area". "Caribbean Sea".

(6) L96. "five real TCs".

(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.

(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.

(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?

(10) L184. For the first time in the paper the variable "H_s" 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, H_s is not used following the pervious definition, e.g. Figure 5ab, "a) and b) represent the 99th percentile of the maximum H_s …". Please consider changing this variable (could be "wave height") and review the correct use of the term "significant wave height" in all the manuscript.

(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).

(12) L210. Please verify the referenced figure.

(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.

(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).

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

(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.

(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.

(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.

(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.

(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.

(21) L282. “Colombia Basin”.

(22) L298. Please verify the referenced figure.

Comments to figures:

(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.

(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.

(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”.

(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.

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

(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).