

**Reviewer** : Line 83-86: In my opinion, sea level is forced by weather or atmospheric conditions rather than climate conditions (which would then refer to sea level rise issues for example), atmospheric pressure but also and often primarily wind conditions carry sea level and storm surges. Highly non-linear effects can sometimes affect sea level between 2 tide gauges, even at a rather short distance (10km apart for example) which can induce not only “desynchronization” but also affect the event’s magnitude especially when looking at extreme events, this can be introduced as a limitation into your study I think and this paragraph should, I think, be rewritten accordingly.

**Author** : Agreed, paragraph rewritten.

**R** : Model data are inherently associated with uncertainties and often struggle to reproduce accurately extremes, such a point should probably be stated as a limitation.

**A** : Agreed, a sentence has been added at the end of section 2.3

**R** : Within the previous review, I suggested that “the text explaining the process is clear enough so as figure 2 can be deleted.” But this has not been considered within your answer.

**A** : Figure 2 deleted

**R** : Also, you calculated other correlation coefficients however the paragraph (lines 111-119) is quite confusing for me. I do not understand your lines 111-113, which data set are you referring to, which “choice of the data to use first” are you talking about.

**A** : We agree that the paragraph is not clear enough when referring to our data sets. We rewrote it to clarify its link to the protocol.

**R** : A Pearson’s correlation of 0.6 indicates, in that context, a rather strong linear relationship for me. Kendall and Spearman’s coefficients are however really low compared to the Pearson’s one but results are still statistically significant according to your p-values. Also, your last sentence (lines 118-119) seems to highlight that your data are completely independent (like if Kendall correlation would be 0) and therefore I find it difficult to motivate the “call for an approach based on copulas”. This same sentence also seems to be in contradiction with Lines 122-124. Rewriting it for better clarity and understanding to the reader is, I think, necessary.

**A** : We rewrote the paragraph to make it more clear but we think that the choice of the copula is motivated.

**R** : I noticed the 38 hours search window is not really being motivated and the threshold value used to find the peaks leading to the “storms” is not explicitly mentioned. The methodology is, in my opinion, well explained but in practicalities, important information is missing on which all your statistical analysis is based such as the threshold value to identify “storms” resulting in a number of “storms” that occurred within your dataset (and per year) which is also missing. I would also suggest changing the word “storms” to “events” as storms are, for me, associated with a strong meteorological definition.

**A** : We changed the storm duration to 33 hours, it did not change the dataset significantly but it is also better justified using the data. We added the value of threshold used. We prefer to keep the term storms as it gives a better idea of what we are looking for.

**R** : I think the wording “recommended” is here incorrect, also he found out that the “survival-Gumbel copula and Clayton” to keep its terms were the most suitable in both sites he studied.

**A** : Changed the word "recommended" to "pointed". We also use the survival copula in the study so I don't think we need to precise this when citing Orsel et al. We however changed the name to Gumbel copula (instead of Gumbel Hougaard) to preserve consistency.

**R** : “The use of the GEV fit (Coles, 2001) is still unclear to me. A GEV fit is commonly used on block maxima such as yearly maxima but it is really unclear how you applied the GEV here (on which datasets: the 30 years of hourly data on each independent variable?).

For me, more clarification is necessary here.” To me, this remark mentioned in the previous review has not been completely answered. We still do not understand clearly on which datasets in terms of “specific aspects” exactly you are applying the fitting on (how many events per year for instance). I think replacing the CDF plot with the PDF one (or putting both plots in parallel) can add clarity. A confidence interval could also be relevant to add I think.

**A** : We agree that there is definitely something wrong with the methodology. Upon further investigation, it appears that when using the peak selection method via the `find_peaks` method from the `scipy` package, the parameter `height` was low enough to not influence the peak selection process and we also used the “distance” parameter. This parameter is used to determine the distance between consecutive peaks, which actually corresponds to what we would do using a block maxima approach. Taking into account both facts, the non-influence of the threshold and the presence of the “distance” parameter in our code. We changed sections 4.1 and 2.4. Stating that we indeed use the block maxima approach and set the threshold to 0. This changed the shape of the copula significantly but it had no influence on the sensitivity analysis results as well as the Sobol’ indices.

**R** : Line 321 seems in contradiction with lines 326-327 (30 years of data / 20 years).

**A** : Corrected to 20 years

**R** : Figure 5: To be able to understand your datasets better (in parallel to my remark about section 2.4), I would suggest integrating your data points feeding the copula analysis as a scatter plot within your existing figure.

**A** : I don't understand this remark

**R** : Lines 405-406: This asymmetry might be different in another context/case study, I would suggest either deleting this sentence or rewriting it with more precision.

**A** : Sentence deleted.

**R** : Discussions : “Uncertainties linked to the methods used should be discussed.” To me, this remark mentioned in the previous review has not been answered in the “Author’s Response” document. Uncertainties linked to the copula return periods are introduced by the interdependence parameter but it is, to me, still unclear how you found values of the Variation Interval.

**A** : We added a paragraph in section 3.5.1 to explain our choice of interval of variation for  $\theta$ .

**R**: 5.3) The limitation section could in my opinion be extended. The proposed methodology inherently integrates some limitations that are here not mentioned (cf. previous comments). Also, sea-level data are analyzed for a rather long time period but your sentence (lines 472-474) mentioning a “non-stationary climate” is for me unclear. Studying trends is I think necessary in your case.

**A** : We added a paragraph stating the limitations due to data uncertainties. We also rewrote the last sentence to specify more clearly what we meant by dealing for a changing climate.

**Technical Corrections** : all corrected

**Additional note** : Following a family name change at the registry office, the name of one of the authors has been changed.