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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "A nonstationary analysis for investigating the multiscale variability of extreme surges: case of the English Channel coasts" *by* Imen Turki et al.

Anonymous Referee #2

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The manuscript by Turki and co-authors addresses an important issue for the modeling of exceedance probability of extreme surges namely accounting for the dependence with climate patterns. The authors present an approach relying on wavelet analysis to investigate the correlation between the extreme surges and four climate oscillations (North Atlantic Oscillation, and Atlantic Multidecadal Oscillation, and the ones related to Sea-Level Pressure and Zonal Wind) at multiple time scales, ∼1.5-years, ∼2-4-years, and ∼5-8-years and 12-16-years. On this basis, they perform nonstationary extreme value analysis using the English Channel coasts as application cases and show the added-value for accounting for these multiscale processes when deriving the return periods.

Main comment

The manuscript is well organized and the presentation is clear. Yet, several aspects should be clarified and further elaborated before publication (state of the art, details of the implementation, discussion regarding the assumptions). Therefore, I recommend additional corrections by incorporating, if possible, the following recommendations.

Specific comments

1. State of the art.

Some key references about the link between extreme surges and climate variables should be added to the bibliography, namely: ** Marcos, M., Calafat, F.M., Berihuete, Á., Dangendorf, S., 2015. LongâĂŘterm variations in global sea level extremes. J. Geophys. Res. 120(12), 8115-8134. ** Marcos, M.; Woodworth, P.L., 2017. Spatiotemporal changes in extreme sea levels along the coasts of the North Atlantic and the Gulf of Mexico. J. Geophys. Res., 122(9), 7031-7048. ** Méndez, F. J., Menéndez, M., Luceño, A., Losada, I. J., 2007. Analyzing monthly extreme sea levels with a time-dependent GEV model. Journal of Atmospheric and Oceanic Technology 24(5), 894-911. ** Wahl, T., Chambers, D.P., 2015. Evidence for multidecadal variability in US extreme sea level records. J. Geophys. Res. 120, 1527–1544. ** Wahl, T.; Chambers, D.P., 2016. Climate controls multidecadal variability in US extreme sea level records. J. Geophys. Res. 121(2), 1274-1290.

My second concern relates to the differences of the present work with the recently published one, namely Turki et al. (2020). As far as I understood, the time scale 12-16-years and the British part of the Channel coasts were not tackled in this published work, but it would be useful to situate in more details the present study with respect to Turki et al. (2020), for instance in the introduction.

2. Details on the implementation.

The authors focus on extreme surges. To do so, the raw data of tide gauges should be pre-processed by accounting for the tide. Could the authors provide more details on how they proceeded? What type of tide data did they used?

Similarly, the authors used climate indices provided by the NCEP-NCAR Reanalysis. Could the authors provide the web link where they downloaded the data for the climate indices? Besides, the authors mentioned climate oscillations using SLP and Zonal Wind. Are they directly available from NCEP-NCAR Reanalysis or are they derived from a pre-processing using EOF analysis for instance?

3. Model selection in the non-stationnary Extreme Value Analysis (EVA).

Integrating the climate drivers as covariates in EVA is a good idea, but the selection of the 'most appropriate' model deserves more discussion.

3.1. Adequacy of GEV.

It is not clear to me whether extreme value distributions are applied to each spectral component. If so, I wonder whether these variables are 'extreme', and whether GEV distribution is appropriate. Could the authors comment on that?

3.2. Variable selection.

Table 2 is used to select the most appropriate climate variables to be integrated in the EVA. Though informative and useful to support discussion, my concern is that this selection is mainly based on a correlation analysis (Figure 7 and following ones), and I wonder why the authors did not perform a variable selection for the GEV model directly; for instance using AIC or alternative selection criteria. See a discussion by Wong (2018)

3.3 Model selection.

Furthermore, the results for Brest in Table 3 may raise some questions: - For scale $\sim$12-16 years, GEV0 does not seem to be the model that leads to the minimum AIC value (-1258 to be compared to -1980 for GEV1); - For scale $\sim$ 2-4-yr, the AIC values

fro GEV1-3 are very close, which make very hard to identify with high confidence the most appropriate model. The authors should comment on that. See also Burnham and Anderson (2004) for further details.

Reference: Wong, T. E. (2018). An integration and assessment of multiple covariates of nonstationary storm surge statistical behavior by Bayesian model averaging. Advances in Statistical Climatology, Meteorology and Oceanography, 4(1/2), 53-63. Burnham, K. P. and Anderson, D. R.: Multimodel inference: understanding AIC and BIC in model selection, Sociolog. Meth. Res., 60, 261–304, 2004.

4. Correlation.

The authors analyze the significance of the correlation through a visual inspection of the results provided by wavelet spectral analysis. In lines 339-341, the authors mentioned that they are using a Monte-Carlo-based approach to identify the most statistically significant correlation: could the authors provide more details on the implementation. Is it a bootstrap-based approach? How do they analyse the changes of the correlation at the Monte-Carlo iterations? Could the authors provide additional results about this significance assessment?

5. Typo.

Line 70: "investigates" should be "investigate" Line 467: "covariable" should be covariate

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