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Discussions

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Interactive comment

Interactive comment on "Modelling dependence and coincidence of storm surges and high tide: Methodology and simplified case study in Le Havre (France)" *by* Amine Ben Daoued et al.

Anonymous Referee #4

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Review summary:

The main goal of the article, as claimed by its title, is to develop a methodology to model dependence and coincidence of storm surges and high tide, and to apply it to a case study in France. Although the objective is clear and laudable, the article proves to be disappointing as the initial goal is far from being reached.

The authors present and compare three methods to perform an Extreme Value Analysis (EVA) on sea levels on a single case study (Le Havre). One is based on a univariate direct approach (so called the reference method) as surge and tidal signals are not separated from each other but the total water level signal is instead considered as a

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single random variable - let's call it method 1. The remaining two methods are based on an indirect approach considering that the probability density function (PDF) of total water level can be modeled as a convolution of tide and surge PDFs. The difference between the two lies in the use of a suitable random variable for "surge": one method uses the skew storm surge (SSS) - let's name it method 2, and the other one uses the maximum storm surge (MSS) - let's name it method 3. Authors claim that the use of MSS is in fact the novelty of the article.

Overall, I think the paper should be substantially improved before getting published. In any case, I recommend not publishing it as is. I think the novelty of the research is low and the conducted analysis is poor. Particularly, the article does not provide any method to model dependence and coincidence of storm surges and high tides, despite its title. At most it allows to highlight the issue of combining storm surge and tidal signals in an indirect approach of EVA.

I detail my review below, separating major from minor comments.

Major comments:

Introduction/State of the art:

- Although the article mentions some key references that investigated the issue of combining tides and SSs (e.g. Tawn and Vassie (1989), Dixon and Tawn (1994), Haigh et al (2010), Kergadallan et al (2014)), it is not clear how the present work differs from or compares with others, for example what is not addressed in those studies that will be in the present work. The authors also could have cited Mazas et al (2014) "Applying POT methods to the Revised Joint Probability Method for determining extreme sea levels", Coast. Eng. 91, 140-150. This study is in line with what is done in the present work. Mazas et al (2014) compared several methods to determine extreme sea levels on a single case study (Brest) using convolution of the tide and surge density functions, but testing hourly vs skew surges and two methods for handling tide-surge interaction. They also compared results with a direct approach, just as authors did. I think the paper would benefit replacing the present work in this context and showing the novelty with respect to previous research.

- As the article focuses on extreme sea levels and indirect approach for EVA of sea levels, I think the entire introduction section should be revised to better document previous research in that domain (see for example the article of Batstone et al (2013)).

Methods:

- This section must be completed, as some basic information on EVA are not even mentioned. For instance, the authors do not describe the sampling method used in the analysis (either for SS or for total sea level marginals): do they use POT (as indicated in the results section line 187)? What extreme laws are used (Generalised Pareto Distribution or Generalised Extreme Value distribution?)? At least, the formula of the CDF should be provided, with appropriate definitions of parameters.

- I think that beginning of section 4 (results) from line 180 to line 195 should be included in the methods section.

- The method chosen by the authors for the indirect approach is a convolution of densities (tide and SS). But it is not clear to me if the tide density uses only high water values or the entire hourly time series. In addition, nothing is said about the derivation of tide density (which method is used? What is the duration of the sample used to derive the density?)

- Nothing is said either on the modelling of coincidence of storm surges and high tides in the methods section, although this is the title of the article.

Case study and data:

- Data characteristics (such as time step for the time series) should be given in the text (in addition to Table 1).

- The authors state that Le Havre is prone to marine and pluvial floods. In addition,

Table 1 relates characteristics of pluvial time series. Logically, I expected to see some compound events in the following with an appropriate method to tackle the issue. As pluvial data are not used in the present work, they should not be mentioned at all.

- There is a problem in the time span of tide gauge time series: 1971-2015 in the text VS 1938-2017 in Table 1.

Results:

- The authors write “the POT threshold selection process has been adapted to meet this criterion and the thresholds are, even though, checked regarding the stability graphs of the GPD parameters estimated with the maximum likelihood method.” To appreciate the quality of the fit and to justify their choices, the authors should provide some plots.

- As mentioned above, I am not sure if the convolution process uses only high water values for the tide density. If this is the case (it should be according to Figure 2), and since MSS is always greater than or equal to SSS, it is logical that return levels (RLs) of method 3 are always higher than those obtained with method 2. Method 3 is actually conservative as it selects the maximum value of instantaneous SS every 12 hours (or so). But without properly tackling the issue of temporal lag between tidal peaks and surge peaks, the results are probably overestimated. The authors should discuss this point.

- There is a problem in the presentation of results: Table 2 and Figure 4 are not consistent. If I trust Table 2, then the reference curve (method 1) is the middle one. This is consistent with the text of the article (line 233). But still, I find the behavior of the RL curves in Figure 4 odd especially at lower return periods. For instance, according to previous research (see e.g. Kergadallan et al, 2014 or Mazas et al, 2014), method 2 should provide higher return levels than method 1.

- The results section would be improved with plots of return levels of SS (for both SSS and MSS).

Discussion:

- The authors write in line 244 “A copula-based approach may be used to study the dependence of instantaneous SSs (or sea levels).” What exactly does that mean? Is it a dependence in time (to model autocorrelation)? Copula would be used to model time dependence of SS? To take into account time dependence of SS or sea levels, extremal index could be considered (see e.g. Batstone et al, 2013).

- The paragraph in lines 248-252 is exactly what we expect to be presented in the article. The authors then propose a method to tackle the issue of coincidence but they do not try it. However, this should be the core of the article.

- I have some doubts about the proposed method. Although Δs is a random variable, it is not an extreme variable. Expressed in hours, it is bounded between 0 and 12 (or -6 and 6) and can take any value within this interval. There is no tail of the distribution and I do not think extreme value theory can apply in that case. Thus, speaking of return level of Δs does not make sense. In fact, I would say a uniform distribution would be a good fit for Δs.

- The statement in lines 260-261 is wrong. A frequency analysis does not imply an extreme value analysis.

Minor comments:

- L11: Authors write that “Tide and extreme SS are considered as independent.” I think what authors mean is that in general, in most studies, tide and extreme SS are considered as independent. So this sentence should be modified as numerous studies have tried to tackle the issue of tide-surge dependence.

- L33: word to be deleted (in bold) : “The safety demonstration and protections and are. . .”

- L46-47: Probabilistic Flood Hazard Assessment. At least, the authors should mention the issue of multivariate return periods. Assessing flood hazard does not imply necessarily to compute the probabilities that one or more parameters are exceeded (see e.g. Salvadori et al (2011) "On the return period and design in a multivariate framework, Hydrol. Earth Sys. Sci., 15, 3293-3305).

- L51: "a river nuclear sites". Fragment unclear, consider revising.

- L53: spelling mistake (in bold) : "It is a common belief today that" . The probability of failure is not systematically the probability of exceeding an extreme event. This statement should be modified accordingly.

- L59 : "volume" does not seem appropriate for a river flood. I suggest to use the word "flow".

- L62: word is missing (in bold): "...marine flooding which is a combination of the tide (which can be predicted) with a SS." Defined like this, SS must also include the effect of waves (setup, runup). Since the effect of waves on total water level is not discussed nor mentioned in the article, this sentence needs rephrasing.

- L65: acronym SSS is not defined before.

- L71: Spelling mistake (in bold): "According to Salvadori and De Michele (2004)..."

- L80: Spelling mistake (in bold): Haigh et al (2010). Also the use of the word "recently" for a 10-year-old study is questionable.

- L87: I think a final point is missing after "distribution function of SSs".

- L91: reword (in bold): "GEV model is recommended"

- L92: the authors write "Based on the regional observations, the process of estimation of extreme water levels ..." Does that mean that this method (method 1) uses a regional frequency analysis ?

- L108: The authors write "Indeed, the SS is the main driver of coastal flood events". This is not true everywhere nor always. Coastal floods can occur from three main

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mechanisms: overflowing, overtopping, breaching. Impacts of waves on structures are sometimes crucial and the main driver of coastal flooding. The statement must be reworded.

- L111: The authors state again (also in the introductory section) that “tidal signals and SSs are independent”. This is not true, as shown in previous research (Idier et al, 2012; Batstone et al, 2013). The sentence must be reworded.

- L115-116 : the wording is awkward as extreme sea level is proposed as a variable to represent SS. This must be reworded.

- L124: Equation (2) is false: fZ(z) on the right hand side must be deleted.

- L126-127: I think there is a confusion here. The tide signal is clearly not a stationary stochastic process, but SS can be considered as so. As the authors write the opposite, they should clarify this point.

- L157-158: The sentence is not clear, I do not understand what is the variable of interest. Rewording should be considered.

- L174: Sentence is awkward and needs rephrasing.

- L193: Wording mistake (in bold): “and 1000-year sea level RLs”.

- L205-206: it seems that GPD is used to describe the tails of the distributions of SS. This does not seem consistent with statement in L91 where GEV is recommended. The authors should clarify this point.

- L210-226: I find this paragraph unclear, I do not see what the authors want to say. I suggest making it clearer.

- L231-232: I think there is a wording mistake (in bold): “The difference is high for high return periods.”

- L233: I think there is a wording mistake (in bold): “The difference is significant for lower return periods”

- L236-239: I do not understand the end of the paragraph. The authors should clarify their statement.

- L257: POT is not an fitting method, it is a sampling method.

- L262: The authors write “figure 4 shows that extreme sea level events tend to occur at the time of the high tide”. I do not see that in Figure 4. The authors should clarify their thought and better explain this result.

- L266-267: The end of section 5 is awkward and should be reworded. It seems that to overcome the problem of method 2, one just needs to follow Tawn and Vassie (1989). Then a question arises: why is method 3 necessary if method 2 limitation can be solved?

- L269-270: The first statement of the Conclusions section is a bit exaggerated. The authors should reword it.

- L277: I am not sure acronym ESL has been defined before.

- L281: spelling mistake (in bold): “Fitting results in terms of probability. . .”

- L290: word missing (in bold)?: “. . .around the high tide (high tide +/- 3 hours).

- References should be listed alphabetically and homogenized.

- Figure 2: SSS is defined as the difference between maximum observed minus predicted sea levels. Therefore, it is a discretized time series and not a continuous one as pictured in Fig 2.

- Overall, English could be improved.

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