Dear Referee #4,

Thank you so much for reviewing our paper.

The manuscript will be, therefore, modified to consider your constructive comments. In the following, a point-by-point response to your comments will be presented.

Point-by-Point response / reviewer # 4

Yasser Hamdi

Major comments:

<table>
<tr><th>Comment 1- Introduction/State of the art</th><th>Our response</th></tr>
<tr><td>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.</td><td>This is an interesting comment.

• The work of Mazas et al. (2014) is now cited in the introduction section with a brief comparison to the present work.
• More details and references about the tide-surge dependence are now added to the introduction section.
• More details about the work performed by Kergadallan et al., (2014) and how it differs from the present work is now added to the introduction section.
• The fact that Idier et al. (2012) and Kergadallan et al., (2014) performed the work with skew surges (and not the MSSs) is a main point of difference with the present work. The following sentence was already in the introduction:
Lines 126-128: "This goal is in line with the recent literature (e.g. Idier et al., 2012, Kergadallan et al., 2014) challenging the use of the SSS and clearly demonstrates the importance of using the maximum instantaneous surge (MSS) instead."

We agree that adding more references would enrich the state of the art. These two paragraphs are now added to introductory section:

1. Lines 130-135: "Mazas et al., (2014) proposed a review of tide-surge interaction methods and applied a POT frequency model (with the GPD and Poisson distribution functions) to the family of JPM-type approaches for determining extreme sea level values in a single case study (Brest). The authors focused on the use of a mixture model for the surge component, which allows probabilities to be quantified for the entire range of sea level values, not just for the extreme ones, which is not the case here in the present paper."
2. Lines 64-81: "The problem of the surge-tide interactions has been addressed in the literature for many regions and with different approaches (Coles and Tawn, 2005; Gouldby et al., 2014; Pirazzoli, 2007; Idier et al., 2012; Idier et al., 2019). It was shown that tide–surge interactions can be relevant in several regions. The tide–surge interactions at the Bay of Bengal (corresponding to the effect of the tide on atmospheric surge and vice versa) were analyzed by Johns et al., (1985) and Krien et al., (2017). They showed that tide–surge interactions in shallow areas of this large deltaic zone are in the range ±0.6m occurred at a maximum of 1</td></tr>
</table>

	to 2 hours after low tide. Similar results were obtained by Johns et al. (1985), Antony and Unnikrishnan (2013) and more recently Hussain and Tajima (2017). Focusing on the English channel, Idier et al. (2012) used shallow water model to make surge computations with and without tide for two selected events (November 2007 North Sea and March 2008 Atlantic storms). The authors concluded that the instantaneous tide–surge interaction are significant in the eastern half of the English Channel, reaching values of 74 cm in the Dover Strait, which is about half of maximal storm surges induced by the same events. They also concluded that Skew surges are tide-dependent, with negligible values (less than 5 cm) over a large portion of the English Channel, but reaching several tens of centimeters in some locations such as the Isle of Wight and Dover Strait. More recently, Idier et al. (2019) have investigated the interactions between the sea level components (sea level rise, tides, storm surges, etc.) and the tide effect on atmospheric storm surges is among the main interactions investigated in their review. The authors stated that the studies, and other ones, converge to highlight that tide–surge interactions can produce tens of centimeters of water level at the coast."
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)).	As mentioned in the previous point, the introduction section has been revised and research in the combined tide-surge field and EVA are better documented. The following references are now used in the introduction section and added to the references list. • Antony, C. and Unnikrishnan A.S.: Observed characteristics of tide–surge interaction along the east coast of India and the head of Bay of Bengal. Estuar. Coast. Shelf. Sci. 131, 6–11. doi: 10.1016/j.ecss.2013.08.004, 2013. • Coles, S., Tawn, J.: Seasonal effects of extreme surges. Stoch Environ Res Ris Assess, 19, 417–427, doi: 10.1007/s00477-005-0008-3, 2005. • Gouldby, B., Mendez, F., Guanche, Y., Rueda, A. and Mínguez, R.: A methodology for deriving extreme nearshore sea conditions for structural design and flood risk analysis. Coastal Engineering. 88, 15–26. doi: 10.1016/j.coastaleng.2014.01.012, 2014. • Hussain M.A. and Tajima Y.: Numerical investigation of surge–tide interactions in the Bay of Bengal along the Bangladesh coast. Nat Hazards 86(2):669–694. Doi: 10.1007/s11069-016-2711-4, 2017. • Krien Y, Testut L, Islam AKMS, Bertin X, Durand F, Mayet C, Tazkia AR, Becker M, Calmant S, Papa F, Ballu V, Shum CK, Khan ZH Towards improved storm surge models in the northern Bay of Bengal. Cont. Shelf Res. 135, 58–73, doi: 10.1016/j.csr.2017.01.014, 2017. • Pirazzoli, P.A. and Tomasin, A.: Estimation of return periods for extreme sea levels: a simplified empirical correction of the joint probabilities method with examples from the French Atlantic coast and three ports in the southwest of the UK. Ocean Dynamics, 57(2), 91-107, 2007. • Idier D, Dumas F, Muller H Tide–surge interaction in the English channel. Nat Hazard Earth Sys, 12, 3709–3718, doi : 10.5194/nhess -12-3709-2012, 2012. • Idier, D., Bertin, X., Thompson, P. and Pickering, M.D.: Interactions Between Mean Sea Level, Tide, Surge, Waves and Flooding: Mechanisms and Contributions to Sea Level Variations at the Coast. Surv Geophys 40, 1603–1630, doi: 10.1007/s10712-019-09549-5, 2019.

	• Mazas, F., Kergadallan, X., Garat, P. and Hamm L.: Applying POT methods to the Revised Joint Probability Method for determining extreme sea levels. Coastal Engineering 91 140–150, 2014.
2. 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.	A sampling method sub-section is now added to the methods section (lines 198-206): **"2.4 The sampling method** The Peaks-Over-Threshold (POT) sampling method is used conduct the frequency analyses in the present work. Commonly considered as an alternative to the annual maxima method, the POT method models the peaks exceeding a relatively high threshold. The distribution of these peaks converge to the Generalized Pareto Distribution (GPD) theoretical distribution. In addition, the threshold leads to a sample more representative of extreme events. However, the threshold selection is subjective and an optimal threshold is difficult to obtain. Indeed, a too low threshold can introduce a bias in the estimation because some observations may not be extreme data and this violates the principle of the extreme value theory. On another hand, the use of a too high threshold reduces the sample size. " In addition, the section results contains now figures and more details about the frequency model settings (lines 236-240 with Table 2 and Figure 5).
I think that beginning of section 4 (results) from line 180 to line 195 should be included in the methods section.	Ok. It is now 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?)	All the tide density is used in the model but only the high tide is summed to SSSs and MSSs in order to calculate extreme sea levels. On the other hand, we used predicted tides already available for the Havre harbour, with the same duration of the sea level data set. Studied time-series of Le Havre (observed and predicted tide, SSSs and MSSs) are now better presented in the case study section (with plots).
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.	A further discussion section take up all these aspects is now added to the paper (lines 309-369).
3. 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).	As mentioned in table 1, the time step is one hour. The word "hourly" is now added in the case study section (line 227): "The 1971-2015 observed and predicted hourly sea levels … "
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.	It was a mistake. Pluvial data is now removed from the table 1.
There is a problem in the time span of tide gauge time series: 1971-2015 in the text VS 1938-2017 in Table 1.	You are right. The time span of tide gauge time series is now fixed.

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.	Stability plots for threshold selection are now presented in the results section and discussed (lines 236-240 with Table 2 and Figure 5).
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 method3 are always higher than those obtained with method 2. Method3 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.	Yes indeed, the approach using the MSS variable could overestimate the extreme levels if the MSSs does not occur randomly within the tidal period. The probability of coincidence (considering time lag between tidal and surge peaks) would make it possible to conclude if the MSSs occur randomly in a tide cycle or not and it must be tested for many coastal systems (with different physical properties). On the other hand, overestimating extremes, if it occurs, allows us to be more conservative in the nuclear safety field. But it is not our objective to overestimate the extreme sea levels. The following paragraph is now added to the discussion section (first paragraph): Lines 310-314: “As shown in Figure 6, RLs obtained with the joint MSS-method are always higher than those using SSS. This is consistent with fact that the convolution process based on MSS uses only high water va for the tide density (as it selects the maximum value of instantaneous every 12 hours) and since MSS is always greater than or equal to SSS. then logical to consider that the joint MSS-tide method is m conservative than the SSS based one..“ And in the conclusion as well: Lines 385-389: “Indeed, since MSS is always greater than or equal to SSS and since the convolution process using MSS selects the maximum value of instantaneous SSs every tidal cycle, the RLs are systematically higher when the joint MSS-tide method is used. But without properly tackling the probability of coincidence concept (i.e. the chance that a maximum SS occurs at the same time with high tide) concept and the issue of temporal lag between tidal peaks and surge peaks, the results will be probably always overestimated.”
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).	Yes indeed, there is a mistake in the legend. It is now fixed.
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	Here, we are rather talking about dependence between variables. The sentence is now changed to: Lines 297-298: “A copula-based approach may be used to consider this dependence.”

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.	A further discussion section presenting the coincidence between SSs and high tide is now added to the paper.
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 and6) and can take any value with in 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.	Very good issue! Yes indeed, non-extreme distributions could be more appropriate for the lag time variable. The following sentence is now added in the further discussion section. Lines 250-253: "Indeed, Δs is expressed in hours and it is not an extreme variable, it is bounded between -6 and 6H and can take any value with in this interval. There is then no tail of the distribution and the extreme value theory is not the appropriate framework to model this random variable. Thus, a uniform distribution would be a good fit for Δs." The RLs term is removed and the sentence is now changed to: Lines 354-355: "Use the desired probability to weight the probabilities of the MSSs, assuming that MSSs and Δs are independent. Many scenarios using many of Δs probabilities can be used in a probabilistic framework."
The statement in lines 260-261 is wrong. A frequency analysis does not imply an extreme value analysis.	Ok.

Minor comments:

Comment 1- Introduction/State of the art	**Our response**
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.	Ok. Done.
L33: word to be deleted (in bold): "The safety demonstration and protections and are..."	Ok. The word "are" is now deleted.
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).	Thank you for this comment. It is interesting. The following sentence is now added (but at a later paragraph in the introduction section). Lines 91-94: "As more than one explanatory variable are often used in a PFHA and in case these variables are dependent, the dependency structure must be modeled and a consistent theoretical framework must be introduced for the calculation of the return periods and design quantiles with multivariate analysis based on Copulas (e.g. Salvadori et al., 2011). Indeed,…" Also, the following reference is now added to the references list: "Salvadori, G., De Michele, C., and Durante, F.: On the return period and design in a multivariate framework, Hydrol. Earth Syst. Sci., 15, 3293–3305, https://doi.org/10.5194/hess-15-3293-2011, 2011."
L51: "a river nuclear sites". Fragment unclear, consider revising.	Ok. Replaced by: "… flood hazard for nuclear sites located alongside rivers…" (line 58).

L53: spelling mistake (in bold) : "It is a common belief today that" .	Ok. Corrected.
The probability of failure is not systematically the probability of exceeding an extreme event. This statement should be modified accordingly.	Changed.
L59 : "volume" does not seem appropriate for a river flood. I suggest to use the word "flow".	The sentence is already deleted as suggested by another reviewer.
L62: word is missing (in bold): "...marine flooding which is a combination of the tide (which can be predicted) with a SS."	Ok. Corrected.
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.	The following sentence is now added two sentences later: Line 86: "It should be noted that the effect of waves (runup and setup) on total water level is not discussed in the present paper."
L65: acronym SSS is not defined before.	Ok. It is now defined.
L71: Spelling mistake (in bold): "According to Salvadori and De Michele (2004)..."	Ok. Fixed.
L80: Spelling mistake (inbold): Haigh et al (2010). Also the use of the word "recently" for a 10-year-old study is questionable.	Yes, sorry about that. Fixed.
L87: I think a final point is missing after "distribution function of SSs".	Right! A final point is now added.
L91: reword (in bold): "GEV model is recommended"	Ok. Corrected.
L92: the authors write "Based on the regional observations, the process of estimation of extreme water levels..." Does that mean that this method (method1) uses a regional frequency analysis ?	No, here we talk about the FEMA study which recommend working in a regional scale… with regional frequency analysis. Otherwise, in reply to the question: all the methods use at-site observations.
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 mechanisms: overflowing, overtopping, breaching. Impacts of waves on structures are sometimes crucial and the main driver of coastal flooding. The statement must be reworded. -	We then suggest: "Indeed, the SS <u>is one of</u> the main driver of coastal flood events". (line 84)
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.	Replaced with: Lines 147-148: "Indeed, as mentioned in the introductory section and as it will be discussed later in this paper, extreme levels such as MSSs may be only very weakly dependent with high-tides."
L115-116 : the wording is awkward as extreme sea level is proposed as a variable to represent SS. This must be reworded.	The sentence is now changed to: Lines (152-153): "So the question that arises here is which variable of interest <u>can be used to better characterize coastal flooging</u>?"
L124: Equation (2) is false: fZ(z) on the right hand side must be deleted.	Ok. Corrected.
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.	You are right, there is a confusion here. The sentence is now changed to: Lines 164-166: "The hourly SS is often considered as a stationary stochastic process, since meteorological and seasonal effects give rise to series of SSs randomly

	distributed in time, but this is not the case of the hourly theoretical tide signals."
L157-158: The sentence is not clear, I do not understand what is the variable of interest. Rewording should be considered.	The sentence is now changed to: Lines (196-197): "The maximum sea level between 2 high-tide values is the variable of interest used for this reference procedure."
L174: Sentence is awkward and needs rephrasing.	Sentence changed to: Lines (229-230): "One of the most important features of Le Havre is the fact that it is subject to marine submersions and instabilities"
L193: Wording mistake (in bold): "and 1000-year sea level RLs".	OK. Corrected.
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.	The GEV was recommended by FEMA (2004)… but the GPD is used herein. It is now clarified in the Introduction section. (Line 120)
L210-226: I find this paragraph unclear, I do not see what the authors want to say. I suggest making it clearer.	The paragraph is now modified and we hope that it is clearer now.
L231-232: I think there is a wording mistake (in bold): "The difference is high for high return periods."	You are right. Corrected.
L233: I think there is a wording mistake (in bold): "The difference is significant for lower return periods"	Ok. Corrected.
L236-239: I do not understand the end of the paragraph. The authors should clarify their statement.	Ok. The statement is now modified and we hope it is clearer now.
L257: POT is not an fitting method, it is a sampling method.	Ok. But The sentence is already changed.
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.	The sentence is: "Furthermore, figure 4 shows that extreme sea level events (the right tail of the distribution: the middle curve) tend to occur at the time of the high tide, as expected." The paragraph is now removed to a further discussion section and the sentence is now replaced by: Lines 314-315: "As expected, figure 4 shows that ESL events at the right tail of the distribution, represented by the middle curve, tend to be close to high SSS RLs which are dominated by the high-tide."
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?	This is a good comment. The sentence is now changed to: Lines 318-320: "To overcome this problem, one can use the joint tide-MSS convolution method. Another solution is to use an empirical method to define the left tail of the distribution and an extreme values analysis for the right tail as stated by Tawn and Vassie (1989)."
L269-270: The first statement of the Conclusions section is a bit exaggerated. The authors should reword it.	The first sentence is now replaced by the following: Lines 371-372: "In the present paper, we provided a reasoning for the need, in a PFHA framework, to combine flood phenomena to better characterize coastal flooding hazard."

L277: I am not sure acronym ESL has been defined before.	It was defined in the introduction section. I also define it in the abstract.
L281: spelling mistake (in bold): “Fitting results in terms of probability...”	Ok.
L290: word missing (in bold)?: “...around the high tide (high tide +/- 3 hours).	Ok.
References should be listed alphabetically and homogenized.	Ok.
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.	The figure 2 is now changed.
Overall, English could be improved.	I hope English is now better.