Dear Referee #1,

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 # 1

Yasser Hamdi

Comment	**Responses to comments**
General point 1: Too many details are sometimes given in points that are not further elaborated upon in the manuscript and on the opposite some critical information on the methods is missing. For example, the authors start the manuscript by discussing about nuclear power plant but this is not discussed further in the text other than they should not change the reference method. This seems to discredit the whole idea behind the need to compare and discuss different methods.	This is an interesting comment. The NPPs example is used as a motivation element. Yes, indeed, this work is done in a context of nuclear safety and review of the nuclear safety demonstration and protections. This was mentioned in the introduction section. It was also mentioned that the present work could be used to enrich safety verification approaches. It's also true that we don't aim to modify the reference method in the present work but attempt to propose other approaches, and simply confront all of them. This is now clearly indicated in Sect. 1, page 2, lines 40-42 "The present work could be used to enrich safety verification methods by proposing other approaches and confronting them to the reference method currently used in the guide"
General comment 2: In the introduction, the authors discuss at length different types of other hazards happening in coastal areas (pluvial, fluvial floods) but this is not further looked into in the paper. If I understood correctly, the present study is on extreme sea levels and therefore extensively discussing about pluvial and fluvial floods seems out of the scope in my opinion. Similarly, it was not clear to me why the authors present in Table 1 the rainfall datasets if this is not used in this study.	We agree that discussing other flooding sources was a bit exaggerated. A part of this discussion is now removed. Rainfall data characteristics are likewise removed from table 1.
There may be a general point to make that including statistical dependence is important to include when estimating (coastal) hazard but I am not sure why the authors put so much emphasis on this point if they don't themselves assess this statistical dependence in their selected case study. Throughout the paper, it is assumed that the tide and storm surge are independent but the authors never report on the validity of their assumption by reporting this statistical dependence. A good example of locations where this assumption might or might not be correct is given in Sterl, A., van den Brink, H., de Vries,	The comment is on matters of substance. Yes, indeed, it is always interesting to quantify the statistical dependence in a context of coastal flooding. In another work, we combined the storm surge with other flood phenomena (riverine flooding and/or local rainfall, etc.) and the correlation of the variables of interest was evaluated. The statistical dependence was measured with a Chi-plot technique and non-parametric estimators (the upper tail dependence, for instance). This allowed us to decide modelling the dependence structure of the two variables using the copula theory (when they are dependent) and to only consider the univariate CDF's in case of independence. Indeed, we did not aim in the present work to show details on how evaluating the dependence in extreme value context. Indeed, the general goal of the present paper is to characterize the hazard "coastal flooding" by combining the high-tide and extreme storm surges (SSSs & MSSs). A dependence analysis was conducted despite the fact that the study aims to use only the extreme values of these variables. Scatter graphs

H., Haarsma, R., and van Meijgaard, E.: An ensemble study of extreme storm surge related water levels in the North Sea in a changing climate, Ocean Sci., 5, 369–378, https://doi.org/10.5194/os-5-369-2009, 2009.	and the Spearman's Rho have been used to measure the statistical dependence between high-tide and extreme SSs. It was concluded that this dependence is weak and sufficiently low to consider the variables of interest dependents. The following sentence is now used: - In the Abstract (lines 11-12) : "Most existing studies are generally based on the assumption that high-tides and extreme SSs are independent." - In the Methods section (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." The discussion section (lines 291-293 and 302-308 with figure 7) has been changed to add a discussion on the dependence analysis. Another kind of dependence that caught our attention (but more important for the coincidence model) is the one between the high-tide and the other instantaneous storm surges around the high-tide (±6 hours). The Spearman's Rho was used as a measure of this statistical dependence (a further discussion section is now added to the paper).
At multiple points in the paper, the authors successively mention that dependence is not important but also that it could be important. These two statements, without further results or analysis, seem contradictory. For example page 3 – line 108-109: "Unlike to what is done very often in the literature, the question of dependency is not essential at all to combine phenomena in the present work. Indeed, as mentioned in the introductory section, tidal signals and SSs are independent." and later page 8 –line 283-284 "It has also been suggested that the questions of coincidence and dependency are essential for a combined tide and SS hazard analysis."	It was assumed in the present paper that the tide and storm surge are independent and a convolution model has been applied with a simple sum of them in the indirect method (with both, skew storm surges and instantaneous ones). I must admit that there is a contradiction here. The two sentences are now modified: Lines 145-147: "As it would be analyzed later in the discussion section, the dependency, in an extreme value context, is analyzed but not considered to combine the phenomena in the present work." The second sentence has been removed to the beginning of the conclusion section. "It has been suggested that the questions of combining tide and SSs is essential to better characterize the coastal flooding hazard." In addition, as suggested by one of the reviewers, the sentence "Tide and extreme SSs are considered as independent" in the abstract is now replaced by: "Most existing studies are generally based on the assumption that high-tides and extreme SSs are independent." (lines 11-12).
The authors state that the maximum storm surge (MSS) can happen randomly somewhere within the tidal cycle. Again as showed in Sterl et al. (2009), I would argue that this is not the case and that the timing of the maximum storm surge is often closely related to physical properties of the coastal system. If this temporal dependence is present, I believe that the suggested method is likely to overestimate extreme sea levels.	Thank you for this comment and for suggesting the possible explanation. Yes it was assumed that a maximum storm surge can happen randomly somewhere within the tidal cycle. We didn't analyse the relationship that can exist between the timing of the MSS and the physical properties of the coastal system. We however recognize that considering this interaction between the timing of the MSSs and the coastal system is difficult to conduct and further investigations are here necessary.
Table 2 and Figure 4 are not in line while I believe they should report the same values. When reading Table 2 for the 1000 year return period, one reads that MSS > ESL > SSS while when looking at Figure 4 the order is SSS > MSS > ESL. Based on my previous comment, I would suspect that the legend is	Right, the legend is not correct. It is now correctly labelled. The table has the number 3 and the figure has the number 6 now.

Figure 4 was incorrectly labelled and that the highest curve shows the method based on the convolution with MSS.	
In the discussion, the authors reflect on ways in which the possible dependence between the tide and storm surge and the timing between the latter could be included. The research presented here would greatly improve by actually doing these suggestions.	Very interesting idea. We agree that this will greatly improve the present research. We propose adding a "further discussion" section to take up this reflection (the way in which the possible dependence between tide, storm surge and the timing between them). We included in this new section the following paragraphs (page ??, lines ??-??): " **6. Further discussion** As show in Figure 6, RLs obtained with the joint MSS-tide method are always higher than those using SSS. This is consistent with the fact that the convolution process based on MSS uses only high water values for the tide density (as it selects the maximum value of instantaneous SSs every 12 hours) and since MSS is always greater than or equal to SSS. It is then logical to consider that the joint MSS-tide method is more conservative than the SSS based one. Figure 6 also shows that extreme sea level events at the right tail of the distribution (the middle curve) tend to occur at the time of the high tide, as expected. The results of this procedure confirm the general finding highlighted in the literature (Fortunato et al., 2016; High et al., 2016) that the return level estimations obtained with the convolution tide-SSS are not adapted up to a certain return period (100 years in the case of Le Havre). To overcome this problem, one can 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). On the other hand, the current practices and statistical approaches to characterize the coastal flooding hazard by estimating extreme storm surges and sea levels still have some weaknesses. Indeed, the combination of the tide and the storm surge do not take into account several scenarios in particular those with a time-lag where the tide and the storm surge could give likewise extreme sea levels. The choice of variables (high-tide, SSSs, MSS, etc.) would be a decisive step and an integral part of the logic behind the idea of combining the two phenomena. Interestingly, these variables could also include other explanatory variables such as the time-lag between the two phenomena (tide and SS). This time-lag would be an additional variable and it is defined as the difference of time of occurrence of the second variable with respect to the first (e.g. time between a maximum storm surge and a high-tide). **6.1 coincidence probability concept** Our interest to the probability of coincidence comes from our belief that a bias is introduced with the joint-MSS convolution because it does not take into account the time difference between the maximum instantaneous SS and the high tide. A probability of coincidence (i.e. the chance that a MSS occurs at the same time with high tide) can be used to better characterize the extreme sea levels using the MSS. In the present paper, we are only interested in the concept of the coincidence probability and the statistical dependence between MSS and tide at the moment of the high-tide and around it (±6 hours). An appropriate coincidence probability concept would then allow to better estimate the probabilities and thus reduce the bias and bring the RLs closer to those obtained by the reference method. Let Δ be the time-lag between the high-tide and the MSSs in each tide cycle. When considering coincidence, an additional hazard curve, associated to the variable Δ can be built. The time-lag variable Δ, which would allow us to compute a probability of coincidence, could be involved in a multivariate frequency analysis to consider the dependence structure between the variables. It is also interesting to note that the probability of coincidence would make it possible to conclude if the MSSs occur randomly in a tide cycle or not. The work must be performed for many coastal systems with different physical properties to conclude whether or not there is a systematic temporal

dependence, and whether or not the extreme sea levels are overestimated if this is indeed the case.

As shown in the right panel of figure 2 the MSS can occur randomly somewhere around the high tide M_n. The time difference between the MSS and the high tide is random as well. It is therefore quite legitimate to study it with a frequency analysis method. Then a coincidence probability concept can be drawn as follows:

- Extract an independent sample of Δ
- Fit this sample with the appropriate distribution function. "Indeed, Δs is expressed in hours and it is not an extreme variable, it is bounded between -6H 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 Δ.
- Use the desired probability to weight the probabilities of the MSSs, assuming that MSSs and Δ are independent. Many scenarios using many of these probabilities can be used in a probabilistic approach.

On the other hand and focusing on the statistical dependence, extreme SSs samples around the high tide (at the time Δ of the high tide) was extracted. The largest window (±6 hour) centered on the time of the high-tide was used and the statistical dependence was then studied. Table 5 shows the Spearman's Rho measuring the statistical dependence between storm surges and tide at the moment of the high-tide and around it (±3 hour). It can be easily concluded that the dependence between SSs and tides is very high around the time of high tide and it becomes weaker as delta increases. As mentioned in the previous section, the dependence structure that exists between the MSSs around the high tide could be modelled with copulas.

Table 5: Spearman's Rho calculated between high-tide and all the instantaneous surges in the tidal cycle

Δ	-6	…	-1	+1	+4	+5	+6
High-tide	0.29	…	0.85	0.77	0.44	0.33	0.30

6.2 The non-stationary context

It is noteworthy that the climate change in the past and working in a non-stationary context can greatly affect and invalidate the fit of the storm surge and sea level PDFs. Indeed, questions such as: what is the effect of potential trends and jumps in the sea water level time series? And should this affect the results and its confidence? are fair ones and perfectly justified. The non-stationary context is not covered by this paper because it moves us further away from the main objective which is the use and the confrontation of different methods for quantifying the exceedance probability of extreme sea levels. It could however be the object of another paper."

This paper would highly benefit from having more figures and analysis to make their point clear. For example, it would be interesting to see the studied time-series of Le Havre, examples of extreme events, an analysis of the dependence between the tide and the skew surge and/or and/or the MSS and/or the ESL events.	More figures are now added: - To the section case study: Figure 4. Studied time-series of Le Havre: (top) predicted and observed sea levels; (middle) SSSs data and (bottom) the MSSs. - To the discussion section**:** Figure 7. Analysis of the dependence between the tide and the SSSs, the MSSs and the ESL events.
The authors did not discuss nor report the effect of potential trends and jumps in the sea water level time series. They can greatly affect and invalidate the fit	Yes, indeed working in a non-stationary context can greatly affect and invalidate the fit of the storm surge and sea level PDFs. We didn't consider it in this work because we think that it moves us further away from the main objective of the paper. It could however be the object of another paper. The

of the pdf and are often present in such time series.	following paragraph is now added to the further discussion section (lines: 363-369) "It is also noteworthy that the climate change in the past and working in a non-stationary context can greatly affect and invalidate the fit of the storm surge and sea level PDFs. Indeed, questions such as: what is the effect of potential trends and jumps in the sea water level time series? What would happen with projected sea level rise? Is the estimated return period affected? Should this affect the results and its confidence? are fair ones and perfectly justified. The non-stationary context is not covered by this paper because it moves us further away from the main objective which is the use and the confrontation of different methods for quantifying the exceedance probability of extreme sea levels. It could however be the object of another paper."

Minor comments

Comment	**Response to reviewer**
The abstract would benefit from being more explicit: describe the three methods used and highlight some of the main differences (with numbers) and implications from these methods.	Two sentences are now added to the abstract (lines 17-22 and 24-26)
The extensive use of brackets makes the text at times hard to follow.	Fixed
At the beginning of the results section, the authors present the R packages they used. In my opinion, this should belong to the Methods section.	the R packages we used are now presented in the Methods section
Page 1 – line 11: "Tide and extreme SSs are considered as independent". Is this an assumption you made for this research or based on your results? If this is an assumption, then it seems contradictory to want to study the dependence but already assume that it is independent.	It is rather an assumption for the Havre based on results.
Page 1 – line 18: "It has also been suggested that the questions of coincidence and dependency are essential for a combined tide and SS hazard analysis." I would think that this is the question this paper is trying to answer.	This sentence is now removed and replaced by the following one in the abstract just before talking about the case study: Lines 21-22: "The question we are trying to answer in this paper is then the coincidence and dependency essential for a combined tide and SS hazard analysis."
Page 2 – line 53: "that the probability of failure (The probability of exceeding an extreme event)": Written in this way, it implies that the probability of failure is the equal to the exceedance probability and this is incorrect.	"(The probability of exceeding an extreme event)" is now removed from the sentence.
Page 2 – line 65: "SSS": At this point in the text, this acronym has not been defined yet.	Fixed
Page 2 – line 71: "Salvadori and De Mechele". Please correct this typo for "Salvadori and De Michele"	OK
Page3–line111:" On the other hand, it is commonly known today that the tidal signals can be predicted". Did the authors want to put the emphasis on the accuracy of the tidal predictions? Because the use of "today" implies that this is recent while this is actually known for some decades.	The word "today" is now removed.
Page 4 – line 124: I think there is a mistake in equation 2 because fz(z) appears on both side of the equation. If I understood correctly, it should only be on the left-hand side of the equation	Right. The equation is now fixed.

Page4–line38-39: “Indeed, a SSS occurring with a high tide is more likely to induce a high sea level than an instantaneous SS occurring with any other tide.” This statement is not clear to me. Can the authors elaborate to make their point?	This sentence is now simplified and replaced by the following one: Line 178: “Indeed, a SSS occurring with a high tide is likely to induce a high sea level”
Page 5 – line 150: “This feature makes the MSS a variable particularly useful for carrying out a PFHA exploring the entire tidal signal, not only the high tide ”. If my understanding of the method is correct, each MSS value per tidal cycle is paired with the high tide value within this tidal cycle. If the MSS does not occur randomly within the tidal period, I believe this might highly overestimate your extreme sea levels which may not be useful for PFHA.	Yes indeed, if the MSS does not occur randomly within the tidal period. As mentioned earlier in our response to a general comments, the probability of coincidence 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 allow us to be more conservative in the nuclear safety field. But it is not our objective to overestimate the extreme sea levels. The following sentence (added to the conclusion section in response to a comment of another reviewer) takes up this view of point: Lines 385-390: “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, which may not be useful for PFHA.”
Page 5 – line 157: “As it can also be noticed for this reference procedure, the variable of interest would be the maximum sea level between 2 high-tide values. ” Why do the authors mention “between 2 high-tide values”? Did you sample using a peaks over threshold method with some independence window criteria or using GEV?	We extract the max sea level in each tidal cycle and then we use these data as raw data to extract extreme values with a classic POT frequency model.
Page 6 – line 187: please mention the final threshold selected, the resulting number of peaks used to fit the distribution in each case and add in supplementary the supplementary graphs.	The following sentence (with a table and a figure showing the POT frequency model characteristics) is now added at the end of the first paragraph of section results. Lines 238-240: “The POT model characteristics (threshold and associated average number of events per year) are presented in Table 2. The stability graphs for threshold selection are presented in Figure 5”.
Page 6 – line 193: “storm surge RLs”: shouldn’t this be water level return levels?	Yes, it would be better. Changed.
Page 6 – line 197: “with the delta method”. Please briefly explain what is the delta method and add appropriate references. I believe this is important since the authors go on to compare the width of the confidence interval.	The following sentence, with the appropriate reference, is now added to the end of the paragraph before the last one of the section results. Lines 251-253: “It is interesting to note that the delta method (*Ver Hoef, 2012*) is a classic technique in statistics for computing confidence intervals for functions of maximum-likelihood estimates. The variance of RL estimates are calculated using an asymptotic approximation to the normal distribution.”

Page6–line218: “However, it should be noticed that extreme levels such as the MSSs may be only very weakly dependent.” Can the authors elaborate on this sentence? I don’t see why this would or would not be the case.	Because only one value per tidal cycle is extracted.
Page 7 – line 222: “This assumption is the most critical one since sea levels are highly non-stationary (due to the tide). ” Shouldn’t “tide” be replace with “storm surge” here?	Yes, indeed. Fixed.