Replies to Reviewer #2's comments on "Partitioning the uncertainty contributions of dependent offshore forcing conditions in the probabilistic assessment of future coastal flooding at a macrotidal site ". (nhess-2021-271)

We would like to thank Reviewer #2 for the constructive comments. We agree with most of the suggestions and, therefore, we have modified the manuscript to take on board their comments. We recall the reviews and we reply to each of the comments in turn.

Reviewer #2:

The paper "Partitioning the uncertainty contributions of dependent offshore forcing conditions in the probabilistic assessment of future coastal flooding at a macrotidal site" by Rohmer et al. presents an analysis of future flooding probability at Gâvres (France).
Lot of work have been put in the paper, which introduces several results that are exhaustively explained and discussed (perhaps even too much! see below). The research topic matches well the scope of the journal and the analysis presented seem to be correct.
We thank Reviewer #2 for his/her positive analysis.

However, there some flaws that make the manuscript hard to follow and some aspect must be clarified. Therefore, I believe that the paper should undergo a major revision before it is accepted for publication. See below for a list of comments:
We agree with this comment. Below Reviewer #2 will find the description of the different modifications that we made; in particular to clarify the structure as well as our assumptions.

The abstract is too long. If I remember correctly, NHESS recommends that it does not exceed 200 words. Perhaps you could shorten the paragraph starting at line 16; in fact, there is no need to provide so many details on the SLR contribution, given that you also introduce other variables further on the paper. Analogously, you could shorten the title (which is also rather long), removing information that are given in the text, e.g., the terms "uncertainty", "dependent", "at a macrotidal site" could be left off (but this is just a suggestion).
Thank you for this suggestion. We have now shortened the abstract (total number of words = 200) as follows:

"Getting a deep insight into the role of coastal flooding drivers is of high interest for the planning of adaptation strategies for future climate conditions. Using global sensitivity analysis, we aim to measure the contributions of the offshore forcing conditions (wave/wind characteristics, still water level and sea level rise (*SLR*) projected up to 2200) to the occurrence of a flooding event at Gâvres town on the French Atlantic coast in a macrotidal environment. This procedure faces, however, two major difficulties, namely (1) the high computational time costs of the hydrodynamic numerical simulations; (2) the statistical dependence between the forcing conditions. By applying a Monte-Carlo-based approach combined with multivariate extreme value analysis, our study proposes a procedure to overcome these two difficulties by calculating sensitivity measures dedicated to dependent input variables (named Shapley effects) using Gaussian process (GP) metamodels. On this basis, our results show the increasing influence of *SLR* over time, and a small to moderate contribution of wave/wind characteristics, or even negligible importance in the very long term. These results were discussed in relation to our modelling choices, in particular the climate change scenario, as well as the uncertainties of the estimation procedure (Monte Carlo sampling and GP error)."

We also propose a new title: "Partitioning the contributions of dependent offshore forcing conditions in the probabilistic assessment of future coastal flooding".

We choose to keep the term "dependent" because we believe that it is one major novelty of our work and we would like to emphasize this aspect.

Why did you not account for river discharge? Isn't it relevant? You should at least provide some information on the mean discharge and explain why it is not considered in the analysis.
Thank you for this suggestion. The study is focused on the drivers of meteo-oceanic origin that participate to the occurrence of marine flooding. Yet, we acknowledge that other drivers exist and could also play a key role in the compound flooding like river discharge (in particular with the proximity of the Blavet river[1] on the study area) or rainfall. This is now underlined in Sect. 5.2 dedicated to discussing the limitations of our work as follows:

"Regarding the physical drivers of flooding, the analysis was focused on marine flooding by considering the joint effects of wave-wind-sea level, but additional processes are also expected to play a role in driving the compound flooding, like river discharge (in particular with the proximity of the Blavet river[2] on the study area) or rainfall. Including additional drivers is made here feasible by the flexibility of Heffernan and Tawn (2004)'s approach for analysing high dimensional extremes. This was shown in particular by Jane et al. (2020), who also highlighted the value of copula-based approaches, such as Vine copula. An avenue for future research could include the comparison of different approaches for multivariate extreme value analysis, i.e. a type of modelling uncertainty on top of the uncertainties in the parametrization and in the threshold selection of these techniques (e.g. Northrop et al., 2017)".

Added references:

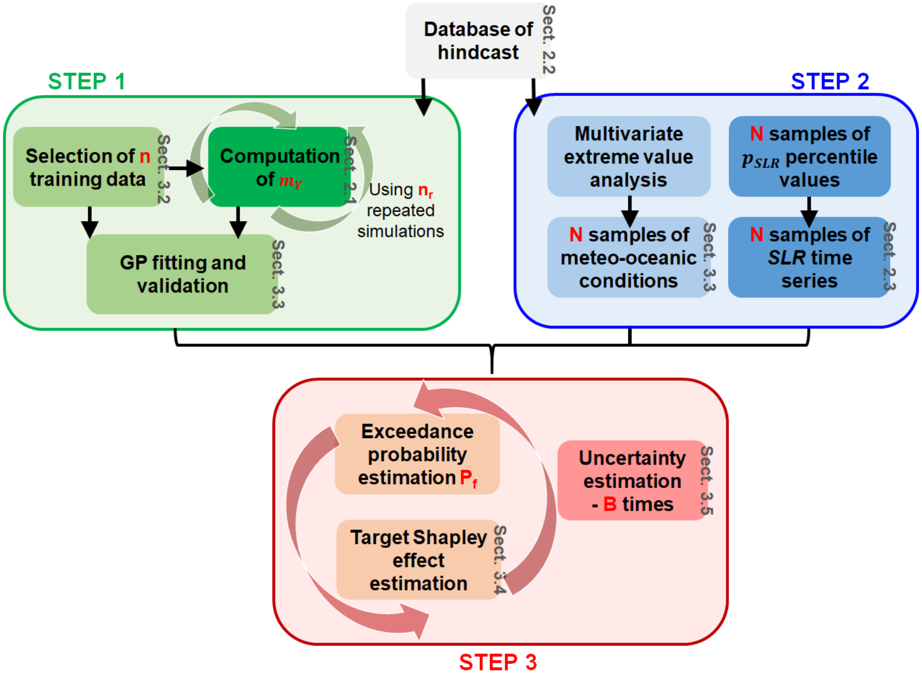
Jane, R.; Cadavid, L.; Obeysekera, J.; and Wahl, T.: Multivariate statistical modelling of the drivers of compound flood events in south Florida. Natural Hazards and Earth System Sciences, 20(10), 2681-2699, 2020.
Northrop, P. J., Attalides, N., & Jonathan, P.: Cross-validatory extreme value threshold selection and uncertainty with application to ocean storm severity. Journal of the Royal Statistical Society: Series C (Applied Statistics), 66(1), 93-120, 2017.

I believe that Sect. 2 and 3 should be reorganized, as right now it is hard to understand what has been done. The data should be introduced in a dedicated section, introducing the study area and describing the hindcast, current sea level, projected sea levels.
Next, the workflow should be detailed in a separated "Method" section, presenting first the hydrodynamic model and its validation (at least introduce appropriate references); next, the GP Metamodel should be described along with the selection of the events used to validate it against the hydrodynamic model. Then you can introduce the steps needed to force the validated GP Metamodel and analyze the results, that is Sect. 3.2 and 3.4. Finally, you could wrap up the section with a summary merged with what is now Sect. 3.5. Please make sure that all the

[1] See Blavet gauge measurements (in French), https://www.vigicrues.gouv.fr/niv3-station.php?CdEntVigiCru=8&CdStationHydro=J571211004&GrdSerie=H&ZoomInitial=3
[2] See Blavet gauge measurements (in French), https://www.vigicrues.gouv.fr/niv3-station.php?CdEntVigiCru=8&CdStationHydro=J571211004&GrdSerie=H&ZoomInitial=3

different bootstraps are clearly explained in the Methods section, i.e., the N realizations of the forcing conditions, the B repetitions to compute the Shapley effects, the 20 repetitions to mimic the variability of waves. In the current form of the paper, it is quite hard to understand the methodology.

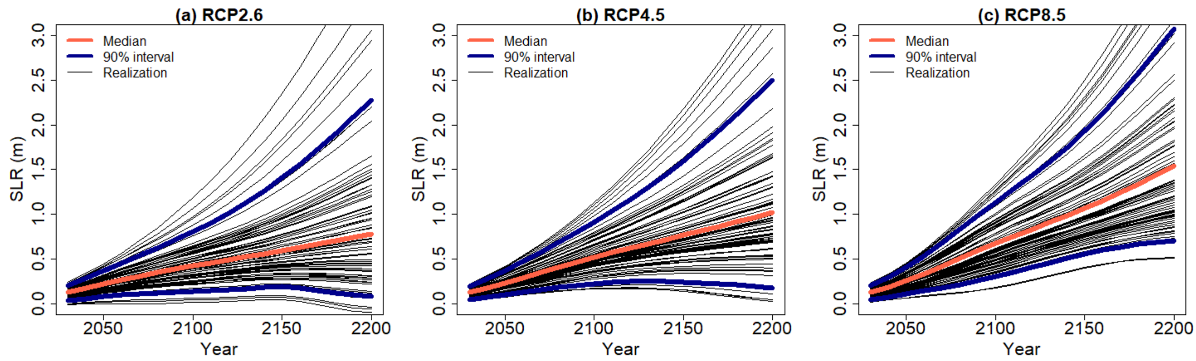
Thank you for this suggestion which is in agreement with the comments of the two other reviewers. We have rewritten the methodological section in this sense (by moving any methodological description of Sect. 4 in Sect. 3). In this view, we have also rewritten the data description in Sect 2. Sect. 3.1 has been rewritten to better describe the different steps as well as the links between the sections. Finally, a flowchart (new Figure 5) has been added to clarify the different steps.



New Figure 5: Flowchart of the procedure. The sections describing the methods/data are indicated in grey next to the boxes.

Please fix the legend in Fig. 3. Lines of the 90% CI should be dashed if I understand correctly. Also, in panel a) it seems that the upper bound lies outside of the realization ensemble (it cannot be).

Thank you for noticing this problem. It has been corrected. From Reviewer #2's comment, we understand that there is some confusion in the interpretation of Fig. 4 (old Fig. 3). The percentiles depicted in color in Fig. 4 are actually not calculated from the random samples but they are directly provided by Kopp et al. (2014): this is now better highlighted in the caption and this is also specified in the main text of Sect. 2. Furthermore to avoid any additional confusion, we have increased the number of random samples to better cover the space.



New Figure 4: Future projection of regional *SLR* for 3 different RCP scenarios. The red line indicates the median, and the blue lines indicate the bounds of the 90% confidence interval provided by Kopp et al. (2014). The different black lines correspond to a subset of 75 randomly generated time series using the procedure described in Sect. 3.1.

Are you sure the offshore variables are dependent? In other words, is there the need to use a different equation rather than Eq. 5? Looking at the scatter plots of Fig. 4 I cannot appreciate any significant correlation pattern. Please provide some measure of the dependence between the time-series of different forcing.

We agree with this comment. Indications of the evidence of the strong dependence have been added in Sect. 2.2 when presenting the offshore forcing conditions and in Sect. 4.2 when presenting the randomly generated samples. Full details of this dependence analysis have been provided in Supplementary Materials A. See below.

First, we provide the matrix of pairwise correlation coefficients (considering two types of correlation, i.e. Pearson and Kendall) calculated for the database of hindcasts (Sect. 2.2) used to perform the multivariate extreme value analysis. This clearly shows some dependencies.

Figure A1. Pairwise correlation coefficients for the offshore forcing conditions (Pearson – left, Kendall - right)

Second, we provide insights in the extremal dependence, i.e. the dependence when the considered variables take large values. To do so, we focus on the empirical estimates of $\bar{\chi}$ of summary statistics as defined by Coles et al. (1999) defined as follows:

$$\bar{\chi} = \lim_{u \to 1}\left(\frac{2\log(P(U>u))}{\log(P(U>u \cap V>u))} - 1\right) \quad \text{(A1)}$$

where U, V are the two different forcing conditions, u is the quantile level. This indicator is used to screen locations where extremal dependence between both variables is exhibited: this is indicated where the $\bar{\chi}$ tends to 1.0 for very large quantile level u of both variables. The evaluation of $\bar{\chi}$ on the hindcast database shows that this remains below 1.0 in the limiting case hence indicating asymptotic independence. In this class of extremal dependence, $\bar{\chi}$ further provides a measure of the strength of dependence. Table A1 shows that this strength reaches non negligible values (>0). The same analysis was conducted using the randomly generated samples (Table A2) and shows that the extremal dependence is well reproduced. We note that the differences are larger for (Hs,U) but these can be considered satisfactory given the relatively large width of the confidence intervals (values in brackets).

Table A1. $\bar{\chi}$ **value for the hindcast database. Values in brackets correspond to the bounds of the 95% confidence interval**

	SWL	*Hs*
Hs	0.28 (0.06, 0.50)	
U	0.28 (0.06, 0.50)	0.46 (0.20, 0.70)

Table A2. $\bar{\chi}$ **value for the random generated samples. Values in brackets correspond to the bounds of the 95% confidence interval**

	SWL	*Hs*
Hs	0.35 (0.10, 0.60)	
U	0.25 (0.03, 0.48)	0.70 (0.40, 1.00)

Finally, we analyze in Table A3 in more details the values of the (a,b)-parameters (as defined in Eq. 2) of the dependence model. Note that the values should be read column-wise, which means that the value of the first column corresponds to the (a,b)- parameters when *SWL* is used as the conditional variable in Eq. 2. As discussed by Heffernan and Tawn (2004), their semi-parametric model allows to cover different types of extremal dependence with the following general rules:

For $2 \leq j \leq d$,

- When $a_j=1$ and $b_j=0$, the variables (X_1, X_j) are asymptotically dependent;
- When $a_j<1$, the variables (X_1, X_j) are asymptotically independent.

In this latter case,

- When $0<a_j<1$ or $a_j=0$ and and $b_j>0$, means positive dependence;
- When $a_j=b_j=0$, means near independence.

Table A3 clearly shows a non-negligible positive strength of dependence in the class of asymptotic independence (as indicated by $0<a_j<1$ and and $b_j>0$).

Table A3. (a,b)-parameters of the dependence model.

	SWL	Hs	U
SWL		(0.348, 0.296)	(0.338, 0.352)
Hs	(0.413, 0.327)		(0.662, 0.481)
U	(0.167, 0.337)	(0.738, 0.359)	

Added reference

Coles, S., J. Heffernan, J. Tawn: Dependence Measures for Extreme Value Analyses. Extremes 2:4, 339–365, 1999.

It is unclear to me why and how SWL and SLR contributions are split. Please clarify how you group the contributions of water levels between the two effects in the Methods section.

This is now clarified in Sect. 2 as follows: "The analysis is conducted for future climate conditions by considering future still water level as $SWL_f(t) = SLR^{\mathrm{RCP}}(t) + SWL$ where $SLR^{\mathrm{RCP}}(t)$ is the value of sea level change in the future (relative to a given reference date) for a given RCP scenario, and SWL is the present day still water level expressed with respect to the mean sea level of the considered reference date".

Fourteen plots are a lot, make sure the total size does not exceed the journal recommendations while guaranteeing appropriate resolution of the figures. For example you could put Figures 9, 10, 12, 14 in the Supplement and summarize the related results in a single section. This way results would be more directly interpreted and their importance would be better framed.
We agree with this comment. We have placed Figs.9,10,12,13 in Supplementary Materials E and have rewritten Sect 5.1 by focusing on the summary plot, new Fig. 9 (old Fig. 14).

Please review the English grammar. I am not a native speaker but I found quite a few typos here and there.
We thank Reviewer #2 for his/her careful reading. We have now double checked the grammar.

Orleans,
March 28th, 2022
J. Rohmer[1] on behalf of the co-authors

[1] BRGM, 3 av. C. Guillemin - 45060 Orléans Cedex 2 – France