

Author's Response to editor

General modifications

▼ Article Structure

The structure of the article was deemed as confusing by both referees. The general sentiment was that going back and forth. The outline is thus :

- Introduction
- Data → Description and sources of the different datasets as well as the primary preprocesses
 - Bathymetry
 - Water level records
 - Significant wave height
 - Storm surge identification
- Methods
 - Multivariate statistical theory using copulas
 - Maximum-Likelihood Method
 - Wave theory : from offshore to the critical velocity
 - Propagation
 - Overtopping Equations
 - Return period of soil erosion
 - Sensitivity analysis through Quasi-Monte-Carlo process
 - Uncertainty Parameters
 - Sobol indices
- Results
 - Return Periods Copula

- Computing the terminal velocity
- Sensitivity indices
- Return periods distribution
- Discussions
 - Results Validation
 - Good practices and dyke improvements
 - Limits of the study
- Conclusions
- Appendix

▼ **Novelty and clarity**

It was pointed, mainly in the abstract and introduction, that the article did not mention enough what gap in the research it aimed to fill. The introduction did mention the currently used methods and the last improved in the field but we did not highlight the limitations of these studies and how we wanted to improve on them

→ We added a few sentences in the abstract and the introduction to put in perspective what the study does and what kind of improvement it brings to the field (lines 10, 65 in the track-change file)

- The mentioned studies do not consider the behavior of the wave further than the overtopping point. We push the reasoning up to erosion damage.

▼ **Limitations and discussions**

Limitations

The limitations of the study were not mentioned enough in the newly added discussions section. We added a few paragraphs in this section explaining the shortcomings of the study as well as future potential improvements that could be done.

- The approach is aimed towards evaluating erosion damage on the landward slope. The scope is thus limited to this particular kind of damage and excludes other damage types as well as other potential damage locations.
- The study is currently made using a 1D bathymetry. Thus we make the approximation that the dyke is homogeneous along the shoreline, which is not the case in reality. An improvement would be to consider different profiles on the dyke

and use a numerical wave propagator such as TOMAWAC to obtain more precise results.

- We did not take into account many physical parameters such as tides as they are of a lesser influence in the Mediterranean sea.
- The scope of the study is too short to integrate climate change projections but long term studies would have to.

Discussions and results

The reviewer stated that the difference between the results associated with the 1st Sobol and the total Sobol indices should be better discussed, potentially with some physical interpretation. In the section where the return period distribution is presented, more should be said on its potential use.

→ We rewrote this part with more interpretation indicating the effect of the difference between the 1st order index and the total effect sobol index. This led to adding part 5.2 using the previous conclusions to deduce the most effective improvements that could be made to the dyke.

Underused return period distribution

It was considered that (fig. 11) was useful. However, we did not provide enough interpretation relating to the results shown in the figure.

→ We added paragraphs linking the median return period extracted from the distribution to the *in situ* records provided by the Salins du Midi company. Good correspondance was found, especially when taking the peak frequency as the representative mesure of the distribution. We then try to explain this difference.

▼ Data details

Data Type

The data used in the article raised a few questions. The data type was unclear (reanalysis ? modelled ?) and some sentences where imprecise.

→ We added sources and improved the newly formed data section to provide more information on the data sources, who maintains them and how they are generated with additional notes to the related institutions. The preprocesses involving the storm selection

has also been more thoroughly described with an additional figure (fig. 4). A map showing the sources' locations has also been added into the text (fig. 2).

Data uncertainty

There was a confusion in table 2 about the ranges of variation used for the global sensitivity analysis. The text led the reviewer to think that the references values in table 1 were generated as mean values from the ranges of variation while it is actually the other around. This confusion was due to a lack of details in the individual parameters descriptions and some misleading terms such as "mean values".

→ The individual parameters descriptions now contain a paragraph on how the range of variation was determined and how the reference value was defined.

→ The misleading terms have been rephrased with clearer terms.

Monte-Carlo sampling distributions

The reviewer pointed that we did not mention what distribution we used for the sampling of the parameters for the global sensitivity analysis.

→ We assumed that the use of the Sobol' sequence inducing a uniform distribution was self-explanatory but decided to add a sentence stating it for clarity.

▼ Bibliography and references

IPCC report in introduction

We referred to the last IPCC report in the introduction but did not take into account the uncertainty stated by the report. This has been corrected in the text.

Threshold source

The second reviewer noted that the 2 m/s threshold above which erosion starts seemed quite important but its origin was not mentioned, which weakens the rest of the results and their interpretation.

→ The results came from Peterka's formula which is simple but does not assume too much information on the characteristics of the soil. We added the source article as well as the formula to the text.

Copula selection

The reviewer pointed that we did not support with enough evidence the choice of the Gumbel copula as many other formulas could have been used instead and this choice was

not obvious. We should have therefore tested different copulas to decide which one was best.

→ Choosing a copula is itself a difficult process which requires time, effort and expertise that would in our opinion lead to a whole other article. Furthermore, there was enough evidence to suppose that the Gumbel copula was indeed one the best choice however it is true that the text did not provide enough proof using the literature. This paragraph has thus been rewritten and supporting references have been added to aid our case.

▼ **Technical and minor corrections**

- There were many typos in the text which have been corrected.
- Some figures were considered misplaced and have been positioned in a more fitting way.
- Some figures lacked units or had misleading captions. These have been corrected.
- The mentions of the equation and figures were not consistent throughout the text (using both '(Figure x)' and 'fig. x'). We have harmonized the text using only '(fig. x)' or '(eq. y)'
- Some sentences of the abstract were considered unclear and were rewritten to be more explicit on what the study is about and what results it brings.