

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1  
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## **Comment on nhess-2021-50**

Anonymous Referee #1

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Referee comment on "Regional analysis of multivariate compound coastal flooding potential around Europe and environs: sensitivity analysis and spatial patterns" by Paula Camus et al., Nat. Hazards Earth Syst. Sci. Discuss.,  
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Review of the paper "Regional analysis of multivariate compound flooding potential: sensitivity analysis and spatial patterns" submitted to NHESS by Camus et al.

This is a well-written manuscript that analyses the compound flooding potential in Europe considering 4 drivers: precipitation, river discharge, storm surge and waves. To my knowledge, it is the first study that analyses these 4 drivers altogether at European scale, therefore providing meaningful insight about compound flooding potential. This study also provides a useful sensitivity assessment to different choices that can be made when analysing compound events, such as sampling method, time window, etc. Lastly, they derive a severity index that combines the effects of all drivers into one index to explain potential coastal flooding, which is later analyzed with self organizing maps and kmeans cluster analysis to identify hotspots of potential flooding.

I recommend its publication after addressing the comments below (minor revision).

Conceptual aspects:

Line 17 "with homegeneous forcing" (also in line 97). I think the term "homogeneous" is not correct here. Modern reanalysis are subject to temporal inhomogeneities due to increasing amount and type of assimilated data over time. I suggest to

replace it by "with the same forcing" or "with coherent forcing", I would also remove the "temporally consistent" part in line 105.

Line 74-83. It should also be mentioned that, if information about the impact is available, another option is conditional sampling on the impact variable, ie analyse the behaviour of the drivers/predictors when the impact is extreme.

Line 180: "ignoring the astronomical tidal component of sea level, as it is deterministic". I am not sure this is a valid argument. It is true that astronomical tides are deterministic but the timing in which large tide might occur respective of, for example, large storm surges is not. I suggest removing the deterministic part or change it to "which is deterministic" or similar.

Line 185-186 I am a bit confused about these pairs. I thought that waves were characterized in terms of SW (simplified wave contribution to water level), and WL (wave set up including the effect of  $T_p$  + surge). First, why do you consider surge together with wave set up when is calculated considering  $T_p$ , but then wave set up and surge are separate when using the simpler formula? Then why WL does not appear as part of the pair of driver combinations analysed? Maybe I got something wrong but I suggest clarifying this in the text.

Line 204-205: I suggest listing threshold as the first parameter introduced in POT. Then I would keep the other factor more general, such as "criteria used to select independent storms" (it is not always just one time window between peaks, alternative or complementary times can be used to determine if two storms can be considered independent, such as the time below the threshold between storms)

Section 4.1.1 What is the time window used to assess the sampling sensitivity?

Figure 3. As noted by the authors, it is expected that the number of co-occurring events are larger for POT6 than POT3, and POT3 than AM, as the number of total events differs for each sampling method. I suggest to show a normalized number of co-occurring events, which might provide more interesting information.

Line 447. Why is W multiplied by 0.2 if afterwards is normalized?

Section 5. I acknowledge the authors provide an extended discussion of many of the results. However, I would discuss a bit more the results obtained, such as why correlation with AM is larger than with POT. Could it be because the sample is shorter? Also, why the correlation is lower when using a wave set up formulation that includes  $T_p$ ? Influence of remote swells that are not correlated with local storms?

Formal aspects:

Line 48: add space ; and space between Couasnon and Ridder citations.

Line 53: This paragraph is quite long. I suggest dividing it in two, for example just before "High-dimensional systems can be modelled using..." in line 60.

Figure 1 is hard to see (specially panel a). I suggest making the figure larger.

Line 182: I suggest adding in the manuscript the exact formula used to calculate wave set up.

Figure 2. Figure is too small, and I also suggest making the legends smaller so there is not overlap with the histograms bars.

Figure 6. I suggest to clarifying the difference of what is substracted from what. Perhaps add subpanels titles.

Figure 7. Isn't there overlapping of red and blue colours when the join occurences (expressed as circle size) are similar for both combinations of pairs?

Section 4.2.2. I believe Figure S8 should be Figure 8 (line 455, 457 and 462)

Line 606: remove "found" after Coausnon et al. (2020)