Review of revised manuscript nhess-2020-80

The authors have fairly improved their manuscript with a better structure, more references and appreciable rephrasing. The methodology used for obtaining a trivariate distribution is clearer, and the result could be quite useful for coastal engineers in particular when wave period plays a role in addition to wave height and sea level.

However, I think that some points could be better explained, while I remain quite disturbed by one choice of the authors.

As regards the possible clarifications:

- Section 2, l.76: the return period T_AND is introduced, but not defined. The associated probability of joint exceedance should be mentioned. Similarly, this probability could be explicated in section 2.4, l 177.
- the description of the constitution of the sample from the time series is confused, in particular l. 88 to 102. First, present the time series (besides, you mention the measurement network CANDHIS for wave data, while later you use a series from the numerical database Anemoc). Then, explain and justify how and why you build the event sample of high tide values, including the recommendation by Kergadallan. Last, describe the modelling of the marginal distributions (empirical + exponential).
- section 4.2: for the construction of the trivariate copula, make clearer that you use the fully nested hierarchical copula method, and not the first approach discussed in section 2.3.2
- section 2.5: the method you propose for assessing the sample dependence refers only to lower tail dependence. Furthermore, other methods exist such as the chi-plot proposed by Fisher and Switzer (1985, 2001), used in coastal analyses by Mazas (2017) for instance.

My biggest concern is related to the assessment of the sample dependence, and its consequences on the choice of the copula. You assess the lower tail dependence of your two samples, and find a moderate one. This is enough for you to justify the use of Clayton and survival Gumbel copulas. But you do not show that the samples have no (or negligible, or even smaller) upper tail dependence! because you are interested in extreme values (large H, T and S), I still think you focus on the wrong tail. At the very least, you should justify that lower tail dependence is more important than upper tail dependence for your analysis.

Last, I think that physical comments could be made from time to time. For instance, you should note that the threshold o 1 m used for filtering wave height in section 3.1 (Figure 2 and l. 264-266) excludes the swells, and leaves only a very homogeneous population of pure wind waves. This will change a lot of things for the assessment of H/T dependence. Similarly, I would comment the fact that you find better results in section 4.2 when you begin by fitting a copula to wave height and wave period, before nesting it with sea level. Indeed, on the one hand you have two parameters (height and period) describing a single physical phenomenon (sea state) and on the other hand a different physical phenomenon (sea level). See for instance the classification of multivariate analyses proposed by Mazas (2017, 2019). I think that it is not by chance that you get the result, in particular because your sea states are pure wind waves.

One final small remark: indicate in Table 1 that Ali-Mikhail-Haq will be noted AMH in what follows.