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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Trivariate copula to design coastal structures" *by* Olivier Orcel et al.

Anonymous Referee #2

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First, I wish to apologize for the delay in transmitting the review, but a lockdown at home with a baby around is not the most favourable environment for such a work... Having taking note of the first review, written by somebody that is much more expert than I am in the theory (and application) of multivariate extremes, I will simply complement his review with additional comments.

This paper aims at replacing the so-called Defra method currently practiced in northern France by an approach relying upon the use of bivariate and trivariate copulas. Some statistical tools that are presented are quite useful and could indeed improve the current practice. However, in addition to the comments posted by the first reviewer, I believe that some significant improvements are requested as regards sampling and event definition, the choice of the copulas, the analysis of tail dependence and the signification of the return period.

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Discussion paper

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First, the so-called Defra method should not be presented as “state of the art”, in particular for the simplified version proposed by Kergadallan with the dependence factor. If this is “current practice”, it should be specified “where” and “by who”.

A crucial point is the sampling, and hence the event definition. The choice of the values at high tide certainly has its justification if the final purpose is wave overtopping or coastal flooding. However, this is not the only one. It does not consider extreme sea states or surges occurring around low tide, even though it may be valuable information. For instance, Kergadallan (2015) recommends selecting the maximum Hs value within a time window centred on the time of high water. Furthermore, it yields quite a large sample (706 events per year) and low to moderate values may be overweighed in the sample. A threshold on Hs may be applied to reduce sample size. Last, the sample should be made of independent and identically distributed (i.i.d.) tuples. Is the independence assumption valid when two tuples per day are selected? Is there only one wave population, or in other words is the extreme behaviour of waves similar for storms from the west or from the north-east? The topic of event definition in such a context (waves / level in coastal areas) is discussed by Hawkes (2002) and Mazas (2017, 2019), among others.

As regards tail dependence, the authors rightly present both the lower and upper tail dependences, and the fact that copulas with the same structure of dependence as the sample of observations. But surprisingly, they focus on the lower tail dependence only for the choice of the copula. Because they find a (weak) lower tail dependence, they choose copulas that will fit best... the least interesting part of the sample! Why not assessing the upper tail dependence, and possibly include extreme value copulas (a special case of Archimax copulas) such as Gumbel-Hougaard, Galambos or Hüsler-Reiss copulas? See for instance Mazas and Hamm (2017) for an application of these copulas for Hs / surge modelling.

Another concern is the return period, a topic intimately lonked to sampling / event definition. First, the return period of “source phenomena” such as Hs / sea level is a very

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different thing than the return period of “response phenomena”, as discussed among many others by Hawkes et al. (2002) or Mazas (2019). Therefore, when writing in the introduction (l. 26-27) that “the design of coastal structures is based in particular on the return periods of wave overtopping or of armour damage”, the authors should acknowledge that they do not address the return period of such phenomena in the paper. Second, there are several definitions of return period (that is a yearly probability of exceedance) in the bivariate case, let alone the trivariate one: see in particular Serinaldi (2015) and Haselsteiner et al. (2017) who detail the different types of environmental contours with respect to the definition of the return period (i.e. the definition of the bivariate probability to consider). In this paper, the authors consider the joint exceedance probability and the associated contours, which is of course quite a relevant choice; however, it should be recalled that this is not the only one possible.

I believe that, once all these points (along with the ones presented by the other reviewer) are clarified, the interest of a trivariate modelling will appear much more plainly.

Specific comments:

- l. 43, “incompatibility problem”: maybe a very short explanation of what it means would help

- l. 56: to be accurate, the random variables are “Hs (resp. T, S) at high tide” (see discussion on sampling and event definition).

- l. 63-65: a short description of the mixture model would be welcome

- section 2.3: explain in which context the Defra method is “current practice”

- l. 92: please specify that FD=25 corresponds to “weak dependence”

- section 3.1: change the title of the section

- l. 312-313: the value of FD=20 is lower than the minimal value of FD=25 recommended by Kergadallan

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- Figure 6 really needs some improvement, I have not understood it

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