

REVIEW REPORT

Journal: Hydrol. Earth Syst. Sci.

Paper: HESS-2016-652

Title: Multivariate Statistical Modelling of Compound Events via Pair-Copula Constructions: Analysis of Floods in Ravenna

Author(s): Emanuele Bevacqua, Douglas Maraun, Ingrid Hobæk Haff, Martin Widmann, and Mathieu Vrac

GENERAL COMMENTS.

In my opinion, this paper represents a honest work dealing with a non-trivial problem. The Authors made a great effort to fix the model of interest, both at a physical and at a statistical level. I appreciated the fact that, beyond “advertising” the potential strengths of the model, the Authors also correctly mentioned (some of) its weaknesses, which might not be evident to unskilled practitioners.

In general, there does not exist a “perfect” model, especially if the problem is as difficult as the one investigated in this work. The approach suggested by the Authors is replete with model constraints and arbitrary assumptions, which are often (only) empirically justified. One may agree or disagree with the viewpoint and the *modus operandi* adopted by the Authors (and possibly suggest valuable alternatives), but overall I think that the procedures outlined in this paper look reasonable from a practical point of view, and the corresponding results could make sense (I also appreciated the discussion about uncertainties). Ultimately, I am in favor with having this work published, provided that the Authors fix the issues raised in the sequel.

A note about Bibliography. I was surprised that the following paper was not mentioned, since apparently it concerns the same Italian site (and about the same problem) investigated by the Authors: “Coastal flooding: A copula based approach for estimating the joint probability of water levels and waves” by Marinella Masina, Alberto Lamberti, and Renata Archetti; Coastal Engineering, Volume 97, March 2015, Pages 37-52. In addition, the reference to the 1997’s book by Joe on copulas should be updated to the 2014’s edition, and the reference to the 2007’s book by Salvadori et al. should be corrected (missing co-authors). Finally, since the Authors use R packages, suitable references should be given in the Bibliography (not only in the text, it is useless!): it is the only “reward” that smart colleagues developing R free software do receive, and without a significant amount of citations, their Institutions will not give them anymore the possibility to go on producing such a bulk of procedures. Please, always give proper credits to whom deserve credits.

Some further comments follow below.

SPECIFIC COMMENTS.

Page(s) 1, Title.

Referee. Usually, in international publications, if a site is mentioned in the title, then also the corresponding Country should be indicated: in turn, the Authors should write “Ravenna (Italy)”.

Page(s) 2, Line(s) 4–ff.

Authors. The impact of drought cannot be fully described by a single variable (e.g. Shiau et al. 2007)...

Referee. Here the Authors should also cite the seminal paper indicated below, where the usage of the Dynamic Return Period (i.e., the evolution of the joint RP along with the drought development) suggests mitigation strategies different from the univariate ones, traditionally used for assessing the risk (in agreement with some conclusions of the Authors).

C. De Michele, G. Salvadori, R. Vezzoli, and S. Pecora. “Multivariate assessment of droughts: frequency analysis and Dynamic Return Period”. *Water Resour. Res.*, 49(10):6985-6994, 2013.

Page(s) 2, Line(s) 10.

Referee. I am not sure that the adjective “systematic” is the proper one here (it could be deceiving). A systematic error “always goes in the same sense/direction”, whereas the differences between univariate and multivariate results may not. Please use another adjective.

Page(s) 6, Line(s) 8–ff.

Authors. The impact h of a CE can be formalized via an *impact-function*. . .

Referee. Essentially, this work adopts a multivariate Structural Approach, which has recently been well formalized in G. Salvadori, F. Durante, C. De Michele, M. Bernardi, and L. Petrella. “A multivariate Copula-based framework for dealing with Hazard Scenarios and Failure Probabilities”. *Water Resources Research*, 53:3701-3721, 2016. Furthermore, useful guidelines for dealing with a multivariate Structural Approach in coastal/offshore engineering are given in G. Salvadori, F. Durante, G. R. Tomasicchio, and F. D’Alessandro. “Practical guidelines for the multivariate assessment of the structural risk in coastal and off-shore engineering”. *Coastal Engineering*, 95:77-83, 2015. Actually, the structural approach discussed in these paper is practically the same as the one adopted by the Authors, but its mathematical/probabilistic foundation in terms of upper sets and suitable hazard scenarios is quite interesting and tickling, and may provide further (theoretical) support to the work of the Authors.

Page(s) 6, Line(s) 8–ff.

Authors. For instance, standard global and regional climate models do not simulate realistic runoff. . .

Referee. I am rather surprised by this sentence: could you please provide valuable references supporting such a strong claim?

Page(s) 7, Line(s) 16.

Referee. For the benefit of the unskilled readers and practitioners, the Authors should cite here some seminal books on copulas (e.g., Nelsen (2006), Salvadori et al. (2007), Joe (2014)), as well as some seminal papers like, e.g.,

C. Genest and A.C. Favre. “Everything you always wanted to know about copula modeling but were afraid to ask”. *J. Hydrol. Eng.*, 12(4):347-368, 2007.

G. Salvadori and C. De Michele. On the use of copulas in hydrology: theory and practice. *J. Hydrol. Eng.*, 12(4):369-380, 2007.

Page(s) 7, Line(s) 23.

Authors. . . . it is possible to construct a valid joint pdf.

Referee. Prudentially, I would re-phrase the sentence as “in general, it is possible to construct a valid joint pdf, provided that suitable constraints are satisfied”.

Page(s) 7, Line(s) 25.

Referee. Copulas do not “increase the number of available multivariate distributions”, they only make it easier to play with more and more multivariate distributions: please re-phrase the sentence.

Page(s) 8, Eq(s) 4–5.

Referee. Before the equations, I would write “if the following limit exists and is non-zero”.

Page(s) 19, Line(s) 13–14.

Authors. The accuracy of the estimated impact is very satisfactory. . .

Referee. Here, and throughout the paper, I would suggest to be more cautious about statements like the one reported above, especially given all the arbitrary assumptions/constraints introduced by the Authors, and the “visual” validations procedures. A sentence like “The accuracy of the estimated impact is empirically satisfactory. . .” may be more genuine.

Page(s) 21, Line(s) 18–ff.

Referee. Any way to show that the Simplifying Assumption (*simplified PCC*) does not affect (too much) the conclusions of this work?

Page(s) 23–ff, Appendix C.

Referee. There might be a lack of “objective” statistics here: diagnostic plots are often used instead of Goodness-of-Fit p-Values. Any way to get something better? I understand that computing p-Values using a Vine copula framework (even bootstrap ones) could be troublesome, but in general I do not like “visual” statistics (if not absolutely necessary or unavoidable).

Furthermore, the AIC is used to select the best Vine structure: as recently pointed out in the paper mentioned below, the AIC approach may not be a valuable solution when used for copulas. Instead, a cross-validation procedure (like, e.g., the one provided by the R package “copula” via the function “xvCopula”) could be a better choice.

Steffen Grønneberg and Nils Lid Hjort. “The copula information criteria”. *Scandinavian Journal of Statistics*, 41(2):436-459, 2014