Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-306-SC1, 2020

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Copulas for hydroclimatic applications – A practical note on common misconceptions and pitfalls" *by* Faranak Tootoonchi et al.

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Received and published: 27 July 2020

Introduction

This is a particularly interesting work that concerns a very active topic of research in the hydrological domain (and beyond). Below there are a few comments that I hope the Authors might find useful, aiming to improve the quality of the manuscript, as well as better highlight some common misconceptions and pitfalls that regard particularly the case of Gaussian copula.

Comments

1. L88-89. The Authors write: "Since the early 2000's, copula methods have been adopted in hydrological modeling, which was triggered by the study of Salvadori and De Michele (2010)." With above sentence in mind I would like to bring to the Authors attention the works of Favre et al. (2004) and Salvadori and De Michele (2004), which if I am not mistaken are the first applications of copulas in hydrological domain (chronologically preceding the one already mentioned in the manuscript).

2. L90-101. In this paragraph the Authors mention numerous works that have used the notion of copulas for the development of various methods in hydrological domain. In this extent I think that it is useful to mention that copulas have also been used for the generation of synthetic hydroclicmatic data, such as synthetic time series of rainfall, runoff, etc. (an important task required by many uncertainty-aware methods/models driven by stochastically-generated data). As in the case of random variables and multivariate distributions, also in this case copulas offer the necessary flexibility for modelling/simulation of non-Gaussian processes. For instance see the works of Lee and Salas (2011), Chen et al. (2015) and Hao and Singh (2013), as well as recent approaches in hydrological domain, based on the Gaussian copula (a construct related with the Nataf's joint distribution; see Lebrun and Dutfoy (2009), and references below for a discussion in a hydrological context) that allow the parsimonious simulation of multivariate stationary and cyclostationary processes with any marginal distribution and correlation structure (Kossieris et al., 2019; Tsoukalas et al., 2020, 2018a, 2018b) - also in a multi-scale/disaggregation context (Tsoukalas et al., 2019).

3. L204-205. With reference to Elliptical copula (i.e., the Gaussian and Student-t copula), the Authors write: "Later, Aas (2004) showed that the co-dependence structure for Elliptical copulas can be presented by linear (Pearson) correlation. Correspondingly, their copula parameter θ can either be estimated as being equal to the linear (Pearson) correlation or derived from Kendall' τ or Spearman's ðİŚĘ. For more details on the corresponding equations, we refer the readers to Aas (2004)." Indeed there are relationships that link the Pearson's correlation coefficient with Kendall's and Spearman's

rank-based correlation coefficients, yet as highlighted in Tsoukalas et al. (2018b), section 3.2.3, these are valid if and only if both the marginals, and the copula are Gaussian (see also references therein).

When the copula is Gaussian, and the marginals are not (which is typical in hydrology), these relationships are no longer valid. In fact, in such cases, the Pearson's correlation coefficient depends on the marginals; since it involves the first cross-product moment among the variables (i.e., it involves the term E[X1, X2]), while the Kendall's and Spearman's correlations do not (since they are rank-based measures of dependence). In the case of Gaussian copula and non-Gaussian marginal, there is a non-analytical relationship that links the Pearson's correlation coefficient in Gaussian (in the manuscript's notation, the Gaussian copula parameter θ) and target domain that has to be found by resolving of a double infinite integral. In particular, and with reference to hydrological domain, see Tsoukalas et al. (2020, 2019, 2018a, 2018b) and references therein.

In my view, the above are delicate, often neglected, points that concern the Gaussian copula, and therefore should be made clear in the manuscript, since they are both (very) common misconceptions/pitfalls that concerns the later (widely-used) copula.

4. L310-318. In this paragraph, as well as in other parts of the manuscript, the Authors discuss the debate between stationarity and non-stationarity. On this topic, and beyond the work of Lins and Cohn (2011), already cited in the manuscript, my suggestion to the Authors would be to review, (and cite if it is considered appropriate), the recent works of, Serinaldi et al. (2018), with emphasis on section 4.2, Koutsoyiannis and Montanari, (2007), (2015), Lins and Cohn (2011), Matalas (2012), and Montanari and Koutsoyiannis (2014). All these works discuss the importance of the assumption of stationarity, highlighting that it is an essential tool for inferencing from data (e.g., model fitting). See also the very interesting, note of Harry F. Lins[1], which concludes as follows:

Stationarity $\neq$ static

Non-stationarity $\neq$ change (or trend)

In my view, stationarity should not be viewed as a shortcoming, nor considered dead. It is recalled that non-stationarity implies non-ergodicity, which in turn makes inference from observed data impossible, unless of course the deterministic dynamics of the process (and hence potential change) are known; which in my understanding, is never the case in hydrological sciences.

Regards,

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PS. For convenience, please see the attached PDF file.

[1] http://www.wmo.int/pages/prog/hwrp/chy/chy14/documents/ms/Stationarity_and_Nonstati

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Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2020-306/hess-2020-306-SC1-supplement.pdf

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