Review for manuscript "Copulas for hydroclimatic applications – A practical note on common misconceptions and pitfalls"

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Summary

The study by Tootoonchi et al. lists and discusses pitfalls related to the multivariate analysis of precipitation and temperature using copulas. To identify pitfalls, they look at a sample of published manuscripts analyzing precipitation (P) and temperature (T) within a copula framework. To illustrate them, they fit different copulas to daily July precipitation and temperature for a case study in Sweden. They finally present a 'decision support framework' for the application of copulas to precipitation and temperature datasets consisting of the following steps: (1) determination of spatio-temporal scale, (2) computation of correlation between variables, (3) testing for stationarity, (4) testing for autocorrelation, (5) testing for ties, and (6) copula fitting and evaluation.

General comments

I agree with the authors that copulas are often used in the fields of hydrology and climatology and that there is a lot of room for improvement in when and how they are applied. In my point of few, the most common 'pitfalls' are that (1) the nature of dependence is often not studied before starting to test various copulas; (2) the dependence structure is often reduced to correlation, (3) no proper goodness-of-fit tests are applied to reject inappropriate copulas. While the authors detect several other pitfalls related to P-T analyses, these important pitfalls are not addressed. By looking at the literature I get the feeling that the authors are not very familiar with the basic statistical copula literature [*Nelsen*, 2006; *Joe*, 2015], which may have prevented them from coming up with a comprehensive list. While I am in favor of a piece addressing such pitfalls, I rather see this as a technical note, a review, or a commentary than an independent piece of research because it does in my point of view not present novel concepts, ideas, tools or data. In addition, I think that such a manuscript should properly review and cite the statistical copula literature and acknowledge previous practical guides for copula application in hydrology e.g. by [*Genest and Favre*, 2007]. In addition, it should not create the wrong sense that dependence=correlation because dependence is a wider term including other dependence properties such as symmetry or tail dependence [*Joe*, 2015]. Even though I do not see this manuscript as a paper in the scope of HESS, I provide some suggestions of how to improve it because I think it could be published as a review/commentary in another hydro-meteorological journal after major modifications.

Specific comments

Title: I think that the title is too general. The study only reviews manuscripts related to P-T copula analyses and some of the pitfalls described are very specific to that pair of variables (e.g. ties, zero values). I would rephrase it to something like: 'Copulas for joint precipitation-temperature studies – a practical note on common misconceptions and pitfalls'.

Abstract: goal, methods, and outcome of study are clearly described. I would probably summarize the different pitfalls identified to summarize the conclusions.

Introduction: The study content is generally well introduced. I would personally jump into P-T analyses a bit more directly by removing the first paragraph because it rises the expectation that different hydro-climatic variables are addressed in the manuscript, which is not the case. Instead, I would extend the section on where previous studies looking at hydro-climatic variables are introduced (l. 89-92) because I think this short section does not do the existing literature justice. I would in particular better introduce the study by [*Genest and Favre*, 2007] that describes 'the various steps involved in investigating the dependence between two random variables and in modeling it using copulas' and illustrate these steps on a hydrological example. I would also highlight what exactly is the benefit of your study compared to this previous one, which had a very similar goal.

Step by step copulas: This section in my opinion needs a more solid theoretical/statistical basis. Proper citations to the statistical literature should be provided for all equations and statements. All variables should be introduced properly and used consistently. I would furthermore expect a discussion of the following points:

(1) Two-dimensional copulas are not popular in hydro-climatology because people are necessarily interested in only two variables but rather because they are easier to apply and visualize than higher dimensional copulas.

(2) I would add a short section about when to use empirical rather than theoretical copulas and vice versa.

(3) I would mention that copulas model the form and intensity of dependence between variables. The form can be represented by the choice of the copula function while the copula parameter describes the intensity.

(4) I would introduce the notion of dependence and clearly state that correlation only describes one particular part of a dependence structure which also comprises tail dependence [*Poulin et al.*, 2007] or symmetry characteristics. I would recommend having a look at Chapter 2 in [*Joe*, 2015]. These additional characteristics are very important for choosing a suitable copula form.

(5) The particular copulas introduced seem a bit random. Why are extreme value copulas not introduced? They are very important when looking at joint P-T extremes. At least, it should be specified how you determined 'the five most widely used copulas' (l.143).

(6) Some Archimedean copulas have more than one parameter (l.166) and Archimedean copulas have the disadvantage that the same degree of dependence is assumed for all pairs of variables.

(7) Elliptical copulas have the advantage that they can handle the same degree of dependence for different variables pairs but they have symmetric dependence structures which may be a disadvantage in some cases [*Favre et al.*, 2018].

(8) I would treat parameter estimation methods separately and mention why maximum likelihood in some cases can be computationally very expensive and may be replaced by pseudo-maximum likelihood estimation (l. 194) [*Han and De Oliveira*, 2019].

(9) I think equation 16 is wrong. Where does it come from?

(10) A goodness-of-fit test never 'accepts' a hypothesis but rather 'rejects' it. 'non-rejection' does not imply 'acceptance' (l. 238).

(11) It is new to me that NSE and RMSE can be used as copula evaluation metrics (l. 242-245). NSE is used to evaluate time series rather than distributions. I do not see the link to the

dependence structure (except that correlation is evaluated as part of NSE) and neither is the statement underlined by a reference.

Common issues, misconceptions and pitfalls: I would move the methods description (l. 248-264) to some methods section. I would also describe how the 'six aspects' investigated (l.255-264) were determined. As mentioned in my general remarks, I would also look at whether authors characterized the nature of dependence (e.g. by looking at rank scatterplots or by computing different dependence metrics including tail dependence) and I would look at whether they performed a proper goodness-of-fit test [*Genest et al.*, 2009]. Furthermore, I would suggest to illustrate the different concepts on your case study example in a separate section called 'Application'.

Spatio-temporal scale: It remains unclear to me why exactly this matters unless you wanted to model spatial dependencies. I would introduce the case study in the newly created methods section as suggested above (l. 278-280).

Correlation: I would call this section 'Dependence' and discuss dependence aspects going beyond correlation as assuming dependence=correlation is a pitfall in itself (see also my earlier comments). How should correlation be independent of the selected sample? (l. 288-289). By 'generate copula co-dependence structure' do you mean 'fitting a copula structure'?

Stationarity of correlation: I would not say that the detection of non-stationarity per se precludes a copula analysis. However, it requires the use of a proper non-stationary model [e.g. *Ahn and Palmer*, 2016]. I do not see the value of the resampling experiment (l. 319-327). Why should this be useful to detect non-stationarity? Why not just test how mean and variance change over time?

Autocorrelation: 'time series are dependent on a delayed copy of themselves'?

Correlation of data with the same rank: use the term 'ties'. I would remove subsection 3.5.1 because there is just one subsection at that hierarchy level.

Selecting suitable copula families: I would remove the NSE and RMSE part (l. 399-400).

Decision support framework for applying copulas: would add 'to jointly model P and T' because some of the points are very specific (particularly the one on ties). I would re-order the different steps and put pre-treatment steps such as removal of autocorrelation (2), testing for non-stationarity (3) and ties (4) before dependence assessment (2) and copula fitting (6). I would also include two additional steps: (x) visual inspection of dependence structure and (x) goodness-of-fit testing. My new suggested order is the following: (1) scale (if this is even important), (2) removal of autocorrelation, (3) removal of ties, (4) testing for non-stationarity, (5) visual dependence assessment, (6) computation of dependence metrics, (7) copula fitting, (8) goodness-of-fit tests. Maybe you could even have two main parts called (A) pre-treatment and (B) copula analysis.

Concluding remarks: I would discuss which parts of the decision framework are transferable to other variable pairs and which ones are specific to P-T analyses.

Structure and language: The manuscript generally has a nice flow and would profit from some editing.

References: Some additions from the statistical literature required as specified above.

Figures: In general, I would recommend the use of subplot labels (a, b, c) to facilitate referencing.
Figure 2: What do these turquoise bars on the left and lower part of the figure to the right mean?
Figure 3: would remove the grey borders in the figures to the right (point clouds).
Figure 4: would use distinct colors in the different subplots (different shades of turquoise are used

for lakes).

Figure 5: As mentioned above, I do not see the value of the analysis presented in 5b.

Figure 6: remove random black borders and increase legend (one should be enough).

Figure 7: Would recommend to add isolines to the scatterplots in 6b.

Figure 8: Would recommend to restructure figure according to the steps order suggested above.

Minor points

- I am less familiar with the term 'co-dependence' than 'interdependence'. Evtl. reword? (e.g. l. 12).
- I. 11-13. I would restructure the sentence and start with the subject 'Several multivariate analysis approaches have….. to account for precipitation….
- L. 55: I would talk about 'joint' instead of 'compound'.
- The use of commas could be improved, e.g. l. 70 'At the annual resolution,…' or l. 73: 'However,…'
- L. 136: 'can be' instead of 'needs to be'
- L.139: 'provide' instead of 'provides'

References used in this review

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