

# Review report on “Impact of spatio-temporal dependence on the frequency of precipitation extremes: Negligible or neglected?”

by Demetris Koutsoyiannis

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Recommendation: Major revision

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**Reviewer's assertion:** It is my opinion that a shift from anonymous to eponymous (signed) reviewing would help the scientific community to be more cooperative, democratic, equitable, ethical, productive and responsible. Therefore, it is my choice, consistent with my aesthetic attitude, to sign my reviews. Furthermore, I believe that the current trend in the review system to seek credit for anonymous transactions (by asking recognition for anonymous reviews through Web of Science, a practice also encouraged by journals) is problematic on ethical and aesthetic grounds. Only eponymous transactions can deserve recognition.

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**Reviewer's clarification:** The references included in this review have the same meaning that references have in scientific documents. In brief, they justify the reviewer's statements and provide links where further details can be found. They are not meant to be suggestions for the author to include them in the paper in review.

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1. I welcome this paper, which starts as “*Most of the methods reported in handbooks of applied statistics have been developed under the assumption of independence, distributional identity, and well-behaving bell-shaped/exponential distributions*” and intends to show the problems these assumptions cause in real-world hydrological applications, and to suggest remedies. I appreciate the investigation of temporal and spatial dependence as causes of spurious trends. I have devoted a book on these issues (Koutsoyiannis, 2023) as I believe they are important and not well known in the hydrological community. Hence, in principle I favour publication of the paper.
2. The paper makes several good points on epistemological grounds, related to concepts not well known or forgotten: the fact that statistical estimates (and diagrams) always rely on an underlying model; the importance of ergodicity for inference; the attention drawing on exploratory data analysis, including graphical diagnostics and theoretical considerations; the clarification of the meaning of stationarity and nonstationarity, which are not properties of the observed time series, but assumptions of the models we devise to describe physical processes.
3. At the same time, the paper tries to debunk some common misunderstandings, such as that dependence has limited effect on statistical inference and that statistical tests, typically devised for independent samples, are serious means of

inference for hydroclimatic processes. This may be acceptable for processes with short-range dependence but not for ones with long-range dependence (Cohn and Lins, 2005; Koutsoyiannis, 2023).

4. The paper is a result of a large amount of work and effort, based on analyses of a huge data set and using newly developed methodologies.
5. I particularly liked the graph in Fig. 8 (and similar figures in the Supplement). If I understand it well, there is not even one point with statistically significant trend. I also like the next figures that show that dependence alone can explain the behaviours observed, while independence would result in false trends.
6. A question not discussed in the paper is if trend identification has some usefulness or not. Let us assume that we have correctly (whatever this might mean) identified statistically significant trends based on past data. If our focus is on the past, what is the usefulness of a trend, once we have a better description by the data themselves? And if the focus is on the future, what is the value of such trend identification? Will a trend identified in the past continue in the future, or will it bend? Does the trend have any predictive value? By the way, this issue has been investigated by Iliopoulou and Koutsoyiannis (2020; see also the motto in that paper), but I would be interested to know the author's opinion on this. The current trend in detecting pointless trends continues to dominate in literature, and it would be useful and beneficial to the community if the paper discussed this issue.
7. Since the paper contains a sound epistemological part, it would be suitable to give a definition of a trend on a scientific (not colloquial) basis. I believe this notion, is more unclear than is popular and lacks a proper definition. I am interested to see what the author thinks about this issue.
8. On the negative side, the paper is difficult to read—and review. While the epistemological parts of the paper and the related questions are well discussed and clear, the technical parts are difficult to assimilate. Near the end of the paper, the author states that he offered an “*alternative point of view*”. However, this is not clarified and is spread in many different sections without coherence. At least a summary of the approach proposed would be helpful and would improve the paper.
9. The structure of the paper is not optimal. Its style is not didactic, and, at places, it is too technical and unclear. The paper needs a substantial overhaul to make an attractive narrative—otherwise I think it would not be read.
10. A major negative issue is that the paper is aligned with another paper by Farris et al. (2021). It even attempts to present the methodology of that previous paper (section 3.1). In this respect, it does not look as an independent paper but a discussion on another paper. Yet it is not a formal discussion, as the paper by Farris et al. (2021) was published in another journal. I think the present paper reflects a sound work that could justify publication, but it needs reworking to be presented as a stand-alone paper.
11. It appears that the Hurst-Kolmogorov (HK) behaviour (long-term persistence; long-range dependence) is not present in the main paper but only in the Supplement. It has been shown (Iliopoulou and Koutsoyiannis, 2019) that

subsetting of a time series (using thresholds or block maxima) distorts the dependence and may hide that behaviour, but this does not mean its influence disappears. The main statistic used in the paper appears to be the lag one autocorrelation ( $\rho_1$ ), but this does not capture the HK behaviour, so I doubt if it is appropriate. I think this would be useful to discuss in the paper.

12. Overall, it would be a pity if this work and the important points it makes were not published. On the other hand, its current form needs substantial improvement, before the paper can be publishable. I am sorry that I am not more specific in my comments, but, as I said, I had difficulties to read the paper.

## References

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