Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-184-RC2, 2018

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Explorative Analysis of Long Time Series of Very High Resolution Spatial Rainfall" *by* E. Dybro Thomassen et al.

Anonymous Referee #2

Received and published: 22 May 2018

Explorative Analysis of Long Time Series of Very High Resolution Spatial Rainfall by E. D. Thomassen, H. J. D. Sørup, M. Scheibel, T. Einfalt and K. Arnbjerg-Nielsen

Recommendation: Reject

I must admit, reviewing this paper was rather challenging. On one hand, the writing is quite good. The structure is clear and the different methods are described and applied in a satisfactory way. The authors seem to know what they are doing. There are no obvious flaws, inconsistencies or statistical fallacies. Still, I strongly advise against the publication of such work (see major comments below). The most important reason for doing so is the fact that the entire paper seems to be a mindless application of different statistical analysis techniques without any clear objective, insight or practical benefit. As the title suggests, the approach is mostly explorative in nature. And while descriptive

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studies and exploratory data analysis are important steps in the scientific process, they aren't sufficient on their own without proper context. There are an infinite number of techniques and analyses that you can apply to your data, and an infinite number of features that can be extracted. But what do you actually expect to learn from these analyses? And how do you think this will be useful to design better stochastic weather generators? Maybe the authors already have some good ideas about that. But as long as these are not clearly formulated in the paper, I see no compelling reason to publish this research.

Main reasons for rejecting this paper:

A) Lack of novelty & usefulness: Most of the results and numbers presented in this paper are not interesting or useful. They are just vanity metrics, used to fill tables and give the illusion of hard work. But just because you can extract a lot of information from your data does not mean that this information is useful or actionable. Sometimes, fact-collecting yields nothing more than a collection of facts; no revelation follows. As a result, the paper does not really contribute to the general advancement of our knowledge about extreme rain events. It also does not contain any technical novelty, algorithm or new method that could be applicable to other studies. Therefore, I strongly encourage the authors to think more about what question(s) they actually want to address in this study. The statistical analysis techniques should be selected based on their ability to answer these questions, and not just to fill tables with numbers.

B) No real conclusions: Because of the lack of a real scientific question underlying this work, the conclusions are extremely limited. They can be summarized as follows: (1) A bunch of statistical techniques were applied to analyze features of extreme rain events. Some features are correlated to each other, but not all methods agree on which are the most important ones. (2) Small and large-scale rainfall extremes are not produced by the same physical mechanisms and their statistical properties differ (which has been known for decades). (3) The sampling strategies and event selection method matter a lot, but we still don't know how to properly do this.

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C) Questionable link to the design of weather generators: According to the authors, this paper presents the “preliminary steps prior to setting up a weather generator with similar properties as high-resolution radar rainfall data” and “a first step into the direction of good practises to find and analyse single storm events”. I strongly disagree with that statement. Actually, I think this study is quite the opposite of good scientific practice. Good practice means you only compute what you really need to improve your understanding and modeling capabilities. This can be guided by a-priori knowledge about how the system behaves or about what end-users need. The paper does none of that, nor does it explain how new weather generators capable of reproducing all the 16 features considered in this study could be designed. Clearly, there is value in trying to use techniques like PCA and cluster analysis to figure out which features are the most important and which can be dropped. But how exactly this is connected and useful for the design of weather generators remains unclear.

Minor Comments:

- Equation 2: What does U represent here?

- Section 3.5.2, p.8, lines 11-17: Maybe you could shorten this section. There are plenty of good references for explaining how clustering works.

- Section 4.2.3: “The spatial variation of extreme events is indicated by calculating the similarity of choice of events by grid cells close to the chosen grid cell.” This sentence is not clear. Please reformulate.

- The small size of the study area (38 x 48 km) means that many storm scales and structures will not be properly resolved. Organized systems of thunderstorms can extend several hundreds of kilometers in size. Their properties are thus likely to be misrepresented in the analyses.

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