

Interactive comment on “Bayesian objective classification of extreme UK daily rainfall for flood risk applications” by M. A. Little et al.

Anonymous Referee #1

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General comments

The manuscript "Bayesian objective classification of extreme UK daily rainfall for flood risk applications" introduces new subjective and objective classification schemes for UK extreme rainfall. The paper is an interesting contribution within the scope of HESS. However, I would fully support the criticism raised by the other referee concerning the method used for clustering, and, together with my following comments, I would recommend a major revision.

Specific comments

1. I am slightly surprised about the foci of the authors. In fact, I would suggest to delete

certain parts, but to strongly expand other parts.

1.1. Parts of Section 3 are somewhat missing the point. The whole paragraphs from the evapotranspiration to the Bergeron-Findeisen theory would be fine in any textbook about atmospheric sciences, but it is not relevant in this context. Please delete this. On the other hand, either in this Section 3, or maybe even better in the introduction, the authors should discuss the state of the art of research on UK precipitation extremes, e.g., Osborn et al, IJOC, 2000, Fowler and Kilsby, IJOC, 2003, Moberg and Jones, IJOC, 2005, Maraun et al, IJOC, 2008. So please delete the mentioned paragraphs and add a discussion of relevant literature.

1.2. The section about the subjective classification is a bit too short, or, one could say, subjective. There are many open questions:

- why do the authors introduce this subjective scheme, when there are others available?
- what makes this scheme better, distinct? -does it perform better? Is it evaluated? -
- what are the criteria for the selection? And how do they enter?

Without such a discussion, the authors show only 5 nice examples (Figs. 1-5) without justifying the new scheme nor evaluating it. I would urge the authors to expand on this issue, otherwise I would ask to delete the whole subjective scheme. My personal preference would be to delete the whole part about the subjective scheme and publish it in a separate paper (then of course, the objective scheme would have to be compared against an existing scheme).

1.3. The authors spend too little time on explaining their methods. They should keep in mind that not all HESS readers might be familiar with cluster algorithms, likelihoods, and Bayesian information criteria. So just writing down the maximum likelihood estimator (and shortly explaining the concept of likelihood) for the cluster variance given a chosen number of clusters, and then introducing the BIC in its general form and the specific form for this case, would help to understand the approach. Some minor comments on this will follow below. Following the recommendations of the other referee,

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this part will have to be rewritten, and the authors should take my comment as a further recommendation.

2. I have got some points related to the slightly overblown language of the paper. I will discuss two points in particular.

2.1. I am wondering, if the point, that the set of possible layouts of extreme rainfall events is exceedingly large, but only a small number of such events have been sampled, is really a manifestation of the curse of dimensionality. The actual idea behind this term is that hypervolumes increase exponentially with dimension. For data analysis, this means the following: When in a one-dimensional setting, N data points are enough for an accurate estimate, you would need N^2 in a two dimensional setting, N^3 in a three dimensional and so on. In the case described here, this does not apply. It is true that there is an infinite number of possible (spatial) shapes of extreme events, and only a small number has been observed. But by attempting to reduce the dimensionality (which is the key point of the paper), the authors imply that the high dimensionality is just a matter of climate noise, and that the effective dimensionality of the system is much lower. This is just the case in any, e.g., one dimensional system, where you assume that y is a function of x plus some noise - but this is trivial and not an example of the curse of dimensionality. Therefore, I would ask the authors to rather delete this bit.

2.2. A similar case is the authors reductio ad absurdum. It is a trivial case in statistical modelling that one can describe N data points best with an N parameter model - to the cost of losing all predictive power. Please keep simple things simple.

3. How do the authors derive the threshold of 50mm? This is neither linked to quantiles nor to hydrological relevance. It is also not clear if one gauge is representative of the whole grid cell, especially for small scale events (thunderstorms) and small scale orography (e.g. Scottish Highlands). The authors should spend some words on motivating this approach.

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4. I am not very much convinced by the verification of the objective scheme. First of all, it is based on an unverified subjective scheme (see above), so the verification has not much value. Second, it is very subjective, as the authors themselves state. I am in particular concerned by the "Depression" and "East coast" types: According to the observations used by the authors, cluster (e) should be linked to the Depression, and cluster (a) to the East coast type. However, the authors simply swap the two based on the "spatial layout" (page 3046). These two cases are quite clear from the conditional probabilities, so simply swapping them is quite critical. This either suggests that either the cluster algorithm produces unphysical spatial patterns, or that the subjective scheme produces erroneous results. In either case this needs more investigation by the authors. This also suggests a more modest conclusion of the authors, who state that "the objective scheme can be readily interpreted in terms of known meteorological mechanisms" (page 3048).

Minor comments

page 3034 The sentence "Too little water..." sounds a bit odd.

page 3037 The authors should explicitly state that the 257 rainfall events are extreme events

page 3043 The authors should explain that $M=132$ stems actually from 11×12 . This whole paragraph could actually be rendered more precisely and shorter at the same time.

page 2044 B. I. CriteriON, not Criteria.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 3033, 2008.

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