Hydrology and Earth System Sciences Discussions

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

M. A. Little et al.

Received and published: 13 January 2009

N.B. This file contains the replies for referee 3. Please note that this makes reference to comments in the other author comment file.

Referee 3 The general summary of the reviewer contains references to the comments by referee 1: please read the answer to referee 1's query.

(17) "The paper sets out to do both a subjective and objective classification of rainfall patterns over UK. The subjective classification is very scarcely described. I would suggests that the authors either develop that sections more, or lift it out of the paper all together and submit it as a study in its own."

This is basically the same comment as (7) above. We would refer the reviewer to the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

answer to that comment.

(18) "Also, I miss the practical implications if the classification scheme. I would suggest a case study on how exactly the classification is to be used in."

Page 3036, paragraph 2, describes the practical reasons for this study: to provide a manageable number of prototypical extreme spatial/seasonal rainfall patterns for flood risk modelling studies. We can describe in more detail how these clustered extreme events should be used in modelling studies in the HESS revision.

(19) "The language in the article is generally good, but it could be more concise and to the point. Sometimes the authors go into lengthy explanations about quite trivial matters, just to in the next section go very quickly through some more technical parts of the paper."

We can certainly attempt to make the level of detail more "even" in the HESS revision, although this comment doesn't really give us that much to go on in terms of exactly which parts the reviewer considers have too much detail, and which parts too little.

(20) "The authors do the link between extreme rainfall and floodings, and I would agree to that extent that it can cause flooding, but does not have to. Also, flooding could occur in less extreme rainfall events, for example after a long-time of intermediate rainfall."

We agree with this comment. Extreme daily rainfall is the main condition for severe flooding, even when prevailing conditions are wetter than normal, and without extreme rainfall flooding is very unlikely. Of course, no modelling study can be a perfect predictor of flooding: the classification scheme helps us to analyse the risks of flooding in a manageable way. We believe we have made this distinction clear from the title and throughout the paper, so perhaps a reiteration of this point is required in the HESS revision.

(21) "The authors do a literature review of existing methods/studies and draws on the work of Bardossy (1994). However, that reference is 14 years old, and he and his

colleagues have done a lot of research regarding the weather patterns classification, and I would suggest that the authors updated their reference list."

In answer to this, we would first refer the reviewer to the answers to comments (5) and (6) above. Secondly, there appears to be a contradiction in the reviewer's commentary here. In comment (27) below, the reviewer queries the relevance of our citations of circulation pattern classification work, but here, the reviewer urges us to cite "weather pattern classification" work. This seems contradictory: either circulation pattern studies are relevant (in which case our Lamb and GWL citations stand, and comment (27) below is erroneous), or they are not (and so the present comment (21) is in error). To us, considering the broad range of classification techniques in existence, the character of our classification scheme, with emphasis on spatial and seasonal layout, is much more akin to circulation pattern work (rather than rainfall amount classifications, but see the reply to comment (4) above).

(22) "The authors later states that the extreme rainfall is considered as above 63.5 mm or 50 mm (please refrain from using inches in an international journal), and I wonder how this number was derived?"

This is essentially the same as comment (11) above; we would refer the reviewer to the answer to that comment. We can of course use metric measures throughout, but the entire historical archive on which this study is based uses inches: for convenience of comparison it is useful to have these figures listed alongside the metric measures.

(23) "I agree with earlier reviewers that the first two paragraphs of section 3.1 should be deleted. The next two sections, describing the rainfall characteristics in UK can be shortened 8230;"

The reviewer here seems to be referring to comment (4) above. However, we did not interpret that comment as implying that we should remove a discussion of the prevailing synoptic patterns in UK rainfall; indeed this is an essential element in justifying the classification scheme and would severely compromise the sense of the paper if it were

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

removed (indeed, the reviewer appears to explicitly require these details, see comment 25 below).

(24) "8230; and I would like to see some references to earlier work in describing the precipitation pattern over UK."

We find the comment about making reference to earlier work somewhat odd: the cited work of Bleasdale et al. (1952) or Mill (1913) is very early work. Perhaps this comment requires some clarification?

(25) "3.2 This section is interesting, but it is not clear to me why five patterns were selected. I would like this section to be more explained."

Much of Section 3.1 to explaining this choice of patterns on meteorological grounds, and the whole of page 3040 motivates the choice in relation to earlier work on the same topic. We can of course increase the level of detail, but we run the risk of clashing with comment (23) above.

(26) "3.3 In the listing of existing objective classification schemes there has been substantial work done in recent years, and again I would suggest a more thorough background on this."

This is essentially the same as comments (5) and (6) above, and we would refer the reviewer to the answers to those comments.

(27) "On this note, however, I am not really sure how the weather classification work relates to this article? The authours mention Lamb8217;s weather types and Grosswetterlagen as subjective classifications. That is true, but they classify circulation patterns rather than precipitation. A certain pattern might have characteristic precipitation pattern, but it is not the same thing as a clustering of rainfall events."

This comment is apparently in contradiction with earlier comments. Please refer to the answer to comment (21).

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

(28) "The section on PCA can be omitted from the paper, since it is a well-known and often used technique."

We can shorten this description, but this needs to be balanced against the comments of the other referees that have requested more, not less, technical details.

(29) "Also, how large does the event have to be in real resolution to be considered an extreme event? It is enough for 1 station within the very coarse scale of 1 degree?" Surely, the number of stations over a threshold must be interesting in terms of extreme events. Why do you not consider this? If you want to look at extreme events there could be complex areal correlations that are not considered in this study. Why not use an equal-size grid such as the BNG instead if lat-long grid points?"

These are good points, but we feel these are already addressed in the paper, beginning with the last paragraph of page 3041, and continuing to the middle of page 3043. In particular, this query seems best answered by the text in the second paragraph of page 3043. Please also see the answer to comment (12) above. Our only additional comment here would be to report that we have indeed experimented with a variety of different simplification schemes, including clustering by the number of events as suggested by the reviewer, and this was found to be impractically unstable by comparison to the current scheme. There are good theoretical reasons why this should be the case: please read the answer to comment (9) above and our discussion beginning of page 3041.

(30) "4. Results and discussion This whole section is very much results and very little discussion. The paper is misbalanced, and a lot more need to go into the discussion part of the paper. What are the big differences between the classifications? How and when should they be used? Can they be verified using other meteorological variables?"

We agree with the reviewer that more discussion on these points would be helpful, and will address this in the HESS revision.

(31) "I would suggest figures 1-5 to e put in one figure."

We will of course experiment with this in the HESS revision, but it is important for us to retain the detail in these figures so this may not turn out to be that practical.

(32) "Also, what is the reason for classifying (a) as east coast and (e) as east coast? Table 2 suggests otherwise."

There seems to be a typo in this comment: perhaps the reviewer is asking the same query as in comment (15) above, in which case, please see the answer to that comment.

(33) "5. Summary and conclusion. This section is good, but much of it should go into chapter 4 since it is discussions. Try to keep the conclusion part very short and to the point."

This is a helpful suggestion; we will address this in the HESS revision. Also, this seems to be linked to comment (30) above; please read the response to that comment.

(34) "Here the authors explain a bit how the classifications should be used, but it is still not clear to me. How would the mapping be used in planning? Maybe an example would be useful."

This appears to be much the same as comment (18) above, please read the answer to that comment.

(35) "Also, what depicts a 8220;strong association8221; between the objective and subjective classification? A statistical test such as a contingency test could tell whether the two classifications are significantly different from each other or not."

This is indeed a good point; although we would initially refer the reviewer to the answer to comment (15) above which discusses which other information should be considered in assessing the strength of the association, in addition to the conditional probabilities in Table 2. We agree with the reviewer that the HESS revision may benefit from further

work in this direction.

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper