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Hydrology and Earth System Sciences Discussions

Interactive comment on "Examination of homogeneity of selected Irish pooling groups" *by* S. Das and C. Cunnane

Anonymous Referee #1

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The paper presents a procedure to pool sites together for regional flood frequency analysis and to assess the homogeneity of the pooled groups. The only novelty in this paper is represented by the homogeneity criteria. It is a sort of graphical version of the Hosking and Wallis' H1 heterogeneity measure in which confidence bounds for the regional L-CV are defined and the number of local L-CVs lying outside these bounds is used as heterogeneity measure. I have concerns on how the confidence bounds are formed. It is assumed that the parent distribution of the variable of interest (annual flood peak) is a 2-parameter GEV distribution with the third parameter selected a-priori. This is a much more strong assumption than the kappa distribution used in the Hosking and Wallis heterogeneity measures and appears to me quite subjective. Also, the authors suggest to consider as heterogeneous the groups with more than two values of sample

L-CV falling outside the confidence limits, without taking into account the number of sites forming the groups. I would have reasoned in terms of percentages of values of local L-CV falling outside the 95% confidence limits and I would have expected it to be around 5% for homogeneous regions, in the ideal case of correct evaluation of the confidence bounds. Other points as the formation of the pooling groups (Section 2) and the investigation of the causes of heterogeneity (Section 4) do not differ from what suggested in other works as the FEH (1999) or Catellarin et. al (2001), among those cited by the authors. In general, I did not like the presentation of the methods, which is sometimes confusing and incomplete (see comments below). Even if the work has its own value as an illustration of the methods used for flood frequency regionalisation in Ireland, I deem it is not appropriate for the special issue "Advances in statistical hydrology" because of the lack of novelty. More detailed comments follow.

Suggestion: the abstract, specially the first part of it, looks like an introduction and could be shortened. I would avoid the citations in the abstract, if possible.

Page 5102, lines 9-10: I would not say that "heterogeneity among the sites in a group is mainly due to variations in the sample L-CVs" but that "heterogeneity among the sites in a group can be detected looking at the variability of the sample L-CVs" or something like that.

Page 5102, lines 23-24: I don't understand the sentence on the population distribution. Does it mean that the expected variability of t2 is not modelled assuming a kappa distribution? Please be more clear here.

Page 5104, line 10: what's Qmed? I guess it is the index flood (the median). If so, why should the same variables be used to identify the ROI groups that relate to the growth curve?

Page 5104, eq. 2: is this the equation proposed for Qmed by the FSU (2009)? If yes, I deem it is not necessary to state it here. If not, please clarify how is it obtained.

Page 5104, lines 14-15: What does "the information about the flood distribution contained in the pooling groups" mean?

Page 5105, lines 14-15: Eq. 3 does not return t2 and t3 as output. The procedure of estimating the local "population" values of L-CV and L-CA (pages 5104-5105) is not properly explained. Maybe it would help to add the equations used to estimate those values (eq. 3 just refers to the ROI pooling procedure). Which weights are used to obtain the average t2 and t3? Are they related to sample length?

Page 5106-5107, lines 17-22/1-4: looking at table 2, the natural choice would be lnAREA alone, which gives the same RMSE (if not better) than lnAREA+lnSAAR or lnAREA+lnSAAR+BFI. It is said that "there is an intuitive attraction in also representing some descriptor of catchment response even at the cost of a small apparent loss in effectiveness". What is this intuitive attraction? Please extend this point, which is relevant. It represents an expert-judgement decision on top of the statistical method, which is indeed always required in practical applications.

Page 5107, lines 5-10: the trial and error procedure to find the weights for equation (1) is of interest. Maybe some more space should be devoted to describe it, even qualitatively.

Pages 5107-5108, point 3: the confidence intervals for t2R are drawn from a Gumbel distribution (or a GEV, but with what selected shape parameter?). Why is it necessary to assume that the sample t2 are normally distributed? Couldn't the empirical distribution obtained from the simulations be used instead? More importantly, at page 5102, line 11, it was stressed that one assumption of the Hosking and Wallis heterogeneity measures was the parent kappa distribution. Here 2 parameter distributions are used instead, which is a much more strict assumption. Please comment on that.

Page 5108, point 4: is the number of t2 outside the confidence bounds (m) a proper measure? It depends on the number of sites forming the group (say N). In general, one would expect 5% of the values to be outside the 95% confidence bounds, i.e.,

m/N=0.05. I would rather use m/N as a measure for the homogeneity criteria.

Page 5108, line 14: these k are those used to produce Table 3, while for figure 3 just k=0 is displayed, isn't it?

Page 5108, point 2: what does this point 2 imply? If a left skewed parent distribution is assumed, then the groups appear more homogeneous?

Page 5109, point 3: are the 27 sites named here also listed in table 3?

Page 5109, point 5: here the "range of t2" is analysed in relation to H2. Does this range correspond to the 95% confidence bounds? Please define it more clearly.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 5099, 2010.