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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 #2

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General comments:

This manuscript presents work undertaken to develop a general procedure for regional flood frequency estimation in Ireland with particular emphasis on the formation of homogeneous pooling groups. The work is heavily inspired by the methodology presented in the Flood Estimation Handbook (FEH) published by the Institute of Hydrology (1999). However, the researchers seems to have overlooked more recent developments of the FEH methodology as presented by Kjeldsen and Jones (2009) in which most aspects of the FEH pooling group methodology were revised. A key aspect of the revised FEH methodology is that the need for defining homogeneous pooling groups have been made redundant through incorporation of the between-site variation in the

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L-moment ratio directly into the underlying statistical model of the pooling group.

In general I think the manuscript does not read well – a bit like extracts from a larger report resulting in terms not appropriately defined to the extent necessary for a stand-alone manuscript. In many places the authors could be more helpful to the reader and provide more guidance and background to the analysis. Also, the introduction is a little light on references and acknowledgement of previous work into regional frequency analysis. A simple literature search should reveal many relevant previously published papers on this topic.

My own personal misgivings about the pooling methodology (in general) are that there are too many free parameters that must be fixed which make it difficult to ensure that an optimal procedure has been devised. For example, in this study the following choices has been made

+ Size of pooling group: 5T rule (adopted directly from FEH – no questions asked)

+ Weight of each site within pooling group: record-length weighted

+ Formation of pooling group: Distance measure based on lnAREA, lnSAAR and BFI-HOST (similar to FEH), with a set of weight assigned to each catchment descriptor based on trial-and-error.

It is worth noticing here that Kjeldsen and Jones (2009) revised all the above assumptions in the FEH and as a result developed a more efficient method. However, despite all that effort it turned out that a simple weighted least square linear regression model linking the at-site L-moment ratios and catchment descriptors produced a method for predicting L-moment ratios at ungauged sites which performed about as well as the more elaborate pooling procedure. I therefore suggest that the authors test how well a set of linear regression models compare to the method presented in this paper. Perhaps the authors could also compare their new method to the existing methods, for example the growth curves for Ireland published in the Flood Studies Report (FSR)

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published by NERC (1975).

In Section 2.1 a Monte Carlo simulation experiment is design to assist in the evaluation of the predictive power of different pooling group formation methods. But I am not sure why a simulation experiment is required as, to my mind, it seems an unnecessary complication. Why not simply report the RMSE and the BIAS based on the (squared) difference between at-site and pooled estimates, where the pooled estimates have been obtained as-if the site is ungauged? In my opinion the results of such an experiment would be much more transparent than the MC experiment described here. Also, there are some issues related to choice of at-site population values and the presence of intersite correlation between AM series that will have an influence on the simulated, and these effects are not discussed or even recognised. Consequently, I would suggest dropping the MC experiment in favour of a conceptually simpler cross-validation (leave-one-out) exercise.

It is not obvious why the authors want BFI to be included in the distance measure when they could not detect any improvements in predictive ability when including BFI. The only reason seems to be a belief that BFI should have some explanatory power. Consequently, as compromise solution has been found where BFI is included but not given much weight. Note here that the revised FEH procedure (Kjeldsen and Jones, 2009) came to the same conclusion that BFI does not add to the description of the between catchment variation in the high order L-moment ratios and, thus, was omitted from the revised similarity distance measure and replaced by FARL (lake and reservoir attenuation) and an index of upstream flood plain extent.

Section 3 describes another Monte Carlo simulation experiment – this time to examine the degree of homogeneity of the pooling groups. However, here I think the authors need to be much more helpful to the readers and start by telling them what the purpose of this investigation is rather than go straight to a point-by-point description of a simulation procedure and then leave it to an interested reader to try and understand the thinking behind this exercise.

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I am not confident that the test procedure described in Section 3 is entirely valid. Would it not be better to estimate the pooled L-CV as the record-length weighted average as this is a more efficient estimate of the mean than the unweighted average used in the paper? Then the standard deviation of t2R can be obtained by simulating from an assumed homogeneous pooling group (all sites have values of t2= t2R but different record length). Alternatively, simply derive the analytical standard deviation of a weighted mean and assume it is normally distributed.

Section 4 discusses reasons why individual pooling group might turn out to be heterogeneous and an exploratory plot is shown to illustrate how catchment descriptors vary within a particular pooling group. However, there is no real substance to this section. I think the authors should be more ambitious and include all their pooling groups into an investigation of reasons for heterogeneity. Perhaps they could define a measure of spread of catchment descriptor values within a pooling group and plot this against catchment descriptor values of subject sites (e.g. catchment area). This would enable a more systematic review of the impact of the subject site characteristics, and could potentially be used to inform a better definition of the similarity distance measure.

Minor comments:

Abstract: the first 10-11 lines do not belong in an abstract

Page 5102, 1st paragraph: Regionalisation is not necessarily equal to pooling analysis. There are several methods for regional frequency analysis of which the pooling method is but one.

Page 5102, line 14: the terms XT, QT and T needs better defining. These are key concepts for the study and the reader should be in no doubt what they represent.

Page 5102, line 17-18: Kjeldsen and Jones (2009) presented a version of the pooling group method which does not require homogeneous pooling groups to be formed.

Page 5104, line 3: (PUM) which is the squared difference between. . .

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Page 5104, line 5-7: why select only 4 catchment descriptors? Consider defining useful descriptors by defining relevant catchment descriptors through a linear regression model between high order L-moment ratios and catchment descriptors.

Page 5104, line 9-11: The reader has to know the FEH-terminology (QMED) for this sentence to make any sense.

Page 5104, Eq. (2): no explicit weights, Wk, are included in Eq. (2)

Page 5104, Eq. (3): perhaps replace d_ij with something else (t_ij?) to distinguish from similarity measure in Eq. (2).

Page 5110, line 14: What does 'special qualities' refer to?

References:

Kjeldsen, T. R. and Jones, D. A. (2009) A formal statistical model for pooled analysis of extreme floods. Hydrology Research, 40(5), 465-480, doi: 10.2166/nh.2009.055.

NERC (1975) Flood Studies Report, National Environment Research Council (NERC), London, UK.

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