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Hydrology and Earth System Sciences Discussions

Interactive comment on "Comparison of catchment grouping methods for flow duration curve estimation at ungauged sites in France" *by* E. Sauquet and C. Catalogne

Anonymous Referee #1

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General comments

The paper reports a regional procedure for flow duration curve (FDC) estimation at ungauged catchments based on three different classification methods (approaches to create homogeneous regions). The topic is of wide interest and the paper can provide interesting advances in practical as well as scientific applications concerning the FDCs; however, some major issues should be addressed before publication, while minor corrections can make the paper clearer.

Specific comments

ABSTRACT. On line 2, it is reported that one of the aims of the study is the quantification of the uncertainty associated to the estimated FDC. In my opinion, this point has not been addressed in the paper. For instance, in the last figures the performances in terms of prediction errors are reported for each classification model, but there is not any evaluation of uncertainty in ungauged sites (e.g. with confidence bands).

P3236, itemized list. On lines 5-7 the authors introduce the idea of a parsimonious model to represent dimensionless FDCs, and then list some advantages of this choice.

- lines 8-9. I do not understand the meaning and the usefulness of this sentence. In particular, it is not clear the double-reference to the long-term mean (once as 'qa' and then as 'index value').

  I can try to guess an alternative sentence like 'the use of a parsimonious dimensionless FDC can provide an easy way to reconstruct the FDC, provided an estimate of the index flow'. Please, reformulate the sentence.

- lines 10-11. The adoption of a model of FDC with few parameters certainly simplifies the regional analysis, during both the calibration and the application phases. However, I think that the 'steps of the regional procedure' represent a more general concept; e.g. the steps can be: the computation of hydrological parameters, the (possible) definition of homogeneous regions, the choice of a function relating hydrological variables to basin characteristics, ...

  The use of few parameters reduces only the effort of each 'regionalization step', but not the number of steps, so I would write 'It reduces the computational effort...' and I would add that 'a few parameters can be easier to interpret'.

- lines 12-15. The sentence states that the dimensionless FDC (through its shape parameters) provides a synthesis of the characteristic response of the catchment

to rainfall. In general, however, the low-flow part of the FDC is not directly affected by rainfall as, in many cases, low-flows are mainly driven by the groundwater release and the geology of the basin (e.g. Smakhtin, 2001). This point needs to be reviewed.

P3241, EQ (5). I have different concerns about the equation:

- The index p should refer to the percentiles used for the FDC discretization (see for example line 16 on page 3241 and line 20 on page 3240), so $p = 1, ..., M$ is incorrect. I would introduce a new index (e.g. m) in order to define $m = 1, ..., M$.
- The summation should become

$$\sum_{m=1}^{M} \alpha_m(i)\beta_m(p)$$

  where β is the m-th orthogonal function and α is the weight function which depends on the location of the site i. This notation is also congruent with the references provided by the authors (e.g. Hisdal and Tveito, 1991). α_j and β_i on lines 8-9 need to be corrected accordingly.
- The term $\overline{\log(Q(i))}$ is defined only on page 3242, line 1, while this should be done just after eq. (5). Moreover, the authors should explain how it should be interpreted.

  For example, Hisdal and Tveito (1991) and Braud and Obled (1991) do not use this parameter, while Holmström (1963) considers it a large-scale spatial average, independent from the measurement site (instead, in eq. (5) it is a function of the site i).

  In any case, I suggest to change the notation, because $\overline{\log(Q(i))}$ looks like too similar to the log of the index-flow $\log(\overline{Q(i)})$.

P3241-3242, L22-27. In this case study, FDCs have a large variety of shapes, as can be deduced from fig. 4. Due to this variability, some further information need to be reported to evaluate the fitting skills of the EOF method. In particular, I would report:

- a plot of the shape function β_1 obtained for the case study;
- a plot showing the variability of the empirical weights α_i;
- a plot showing how the modeled FDC changes as the parameter α_1 (and/or $\overline{\log(Q(i))}$) changes, in order to represent how much 'flexible' the fitting is.

SECT 4 and 5. The authors defined fixed-region classifications (through the RT and CCA methods) based on two hydrological indices: IC and SR. I agree with them that IC and SR are useful because they have a physical meaning; however, a 'reference classification', directly based on the regional variables (α_1 and $\overline{\log(Q(i))}$), should also be used in the regionalization and the results compared with the previous ones.

SECT 5 and 6. In general, it is not clear if any statistical test for homogeneity has been done to 'validate' each region. This point need to be discussed in the paper.

Minor comments

P 3236, L2. The term 'identical' is a bit strong. I would prefer something like 'equivalent' or 'statistically identical'.

P3237, L14. For the purpose of streamflow regionalization (prediction in ungauged basins), the classification must be done in a space based on some catchment characteristics (morphological, climatic, geological,...) which can be determined

independently of the streamflow data. The catchments characteristics are also know as 'descriptors', so that this space is often called 'descriptors space'.
The authors use the term 'hydrological space' to refer to the descriptors space (or space of auxiliary variables); this may be misleading because the descriptors space cannot contain variables related to the streamflow. In this context, I would change 'hydrological space' to 'descriptors space'. The same correction should be done on line 1 where the 'hydrological neighbourhood' is cited.

P3238, L27. In the presented case study, only basins without significant human influence have been selected. Which are the criteria used to define the presence/absence of a significant anthropic effect?

P3240, L20. Please, specify what is intended with 'standardized': zero mean and unit variance or just 'dimensionless' (divided by the mean value) like in P3236 L6?

P3242, L3. I would delete 'uncertainty'.

SECT 2. The number of suitable stream gauge used in the study is very large. Did the authors consider the effect of nested catchments?

SECT 5.1.3. The indices i and p used in this section are in contrast with those used elsewhere in the paper. Other indices will avoid misunderstandings.

P3250, L3. Multivariate should be Multiple.

P3250 EQ.(8)-(11). Please define the coefficients λ.

P3250 L19-21. The authors found one suitable model (regression) for each region. It is interesting to report:

- the most significative descriptors selected;
- if they considered completely different regressions for each region, or a unique model (i.e. the same set of descriptors) with coefficients changing from a region to another.

Technical corrections

P3247, L7. 'Mahalanobis' instead of 'Mahanalobis'.

P3251 L 2. Correct 'Sect. 4.5'.

References

Braud, I. and Obled, C.: On the use of Empirical Orthogonal Function (EOF) analysis in the simulation of random fields, Stoch. Hydrol. Hydraul., 5, 125-134, 1991.

Hisdal, H. and Tveito, O. E.: Generation of runoff series at ungauged locations using Empirical Orthogonal Functions in combination with kriging, Stoch. Hydrol. Hydraul., 6, 255-269, 1991.

Holmströ, I.:On a method for parametric representation of the state of the atmosphere,Tellus, 15, 127-149, 1963.

Smakhtin, V. U.: Low flow hydrology: a review, J. Hydrol., 240, 147-186, 2001.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 3233, 2011.