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Discussions

Interactive comment on "Geostatistical prediction of flow-duration curves" *by* A. Pugliese et al.

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The article "Geostatistical prediction of flow-duration curves" by Pugliese et al. proposes a method to evaluate the flow duration curve (FDC) in ungauged basins by adapting the interpolation skills provided by the Topological-Kriging (top-kriging). In the recent literature, this latter has been applied to reproduce single-valued flow statistics (e.g. flood flow quantiles for fixed return period, low-flow indexes) with good results, but it has never been used before to determine the complete FDC.

The paper is well written and the topic is of wide interest in the hydrological field. I believe it is suitable for publication in HESS after the improvement of some issues reported in the comments below.

Major comments

1. The Kriging procedure (i.e. the estimation of the weights λ) is applied, for each ungauged basin, to a number on neighbouring stations which is set equal to 6, as indicated on page 13067, after a preliminary analysis. Although this approach can improve the final outcomes, I think it is in contrast with what is claimed on page 13057 (from line 10), i.e. that the geostatistical approach allows to avoid the identification of homogeneous regions. Of course, strictly speaking, Kriging does not require the homogeneity of the region; however, this pre-selection introduces a subjective element and undermines the robustness of the original method.

   The point is that Kriging (as well as top-Kriging) automatically provides weights on the basis of the distance of the ungauged site to the donor stations (and according to the correlation structure). Thus, the weights are (automatically) greater for close donors and smaller for donor sites far away. Moreover, weights depend on the location of the ungauged site, so the weighting structure adapts for different ungauged basins.

   Under this perspective, I would like to see first an application where the whole dataset is considered. The variograms should also be reported.

2. The hypothesis that top-Kriging weights λ can be used to weight empirical FDCs is the core assumption of the method; it is a strong assumption, so I would try to verify it. I suggest one possible way to perform this task, but the authors are free to propose any other reliable method: i) consider one station among the whole dataset of N stations; ii) for that station compute the $N-1$ weights λ; iii) compute the $N-1$ δvalues between the empirical FDC of the selected station and the empirical FDCs of the remaining stations; iv) compare the λ values and δ values: large δ (dissimilar curves) should correspond to small λ (small weight) and viceversa; v) repeat points i)-iv) for each station.

Minor Comments

1. Due to the assumption reported in the previous comment the authors should specify, starting from the title, that the prediction of FDCs is somewhat an indirect product of the geostatistical framework. To do so, I suggest to change the title to "Geostatistical weighting scheme for prediction of flow-duration curve".

2. The point (i) at the top of page 13058, as well as other sentences in the manuscript, describes the TND as a characteristic of the whole curve. Actually, the flood-part of the curve (normalized discharge greater than 1) is not represented by the TND, so I would relax the statements regarding the *whole curve* by specifying that flood flows are not really accounted for by the TND.

3. Page 13060 line 10: the "non-decreasing" property of FDCs actually depends on the way the curve is represented. If ordered discharges are plotted against the non-exceedance probability, the curve is non-decreasing; otherwise (as in this paper), if the exceedance probability is used, the curve is non-increasing.

   I suggest to use "monotone relationship" to account for both the possible representations.

4. In page 13064, from line 14, the authors introduce an operational problem due to the different length of the period-of-record FDCs. The issue regards the lowest value dimensionless duration d which vary for different record lengths and thus affects the computation of the TND. The proposed solution is to fix a maximum d, which is equivalent to cut the right tail of the FDC at the specified d, in order to have the same limit for TND calculation for each FDC. I think this operation would be no longer necessary if the curves were previously resampled at a set of pre-imposed durations. A possible resampling set is $d_i = 1 - i/(N+1)$ with $N = 365$ ($i = 1 \dots 365$) if one refers to the equivalent number of days in a year, but it is not the only possible resampling.

   In fact, a resampling procedure has already been implemented by the authors in section 5, over 20 points equally spaced in the z space. Since this resampling procedure is essential to predict the FDC, it should be applied before the TND computation to keep the framework consistent.

Technical notes and misspellings

1. Page 13057: the text block of lines 13-16 has basically the same information as the lines 21-25. Please reformulate the paragraph to remove redundant information.

2. Parentheses around citations of equation numbers are often missing throughout the text (se for example P13059 L 14; P13060 L 17; P13064 L16; etc.) and should be added.

3. In plots showing FDCs in the frequency domain I would use"Exceedance frequenc" or "Dimensionless duration" rather than "Duration" which, instead, recall a dimensional time variable. The same correction should be done throughout the text, for instance before eq. (4) and on page 13064.

4. Missing punctuation after eq. (7).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 13053, 2013.