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Interactive comment on "A comparison between parameter regionalization and model calibration with flow duration curves for prediction in ungauged catchments" *by* D. Kim et al.

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The manuscript "A comparison between parameter regionalization and model calibration with flow duration curves for prediction in ungauged catchments" compares parameter regionalization techniques with FDC-based model calibration. My specific comments are listed below;

1. A number of studies have already been conducted regarding the comparison parametric and non-parametric methods for the regionalization of FDCs. Some of the studies are listed below;

Ganora D, Claps P, Laio F, Viglione A. 2009. An approach to estimate non-

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parametric flow duration curves in ungauged basins. Water Resour Res. 45. doi:10.1029/2008WR007472

Qamar MU, Azmat M, Cheema MJM, Shahid MA, Khushnood RA, Ahmad S (2016) Model swapping: a comparative performance signature for the prediction of flow duration curves in ungauged basins. J Hydrol 541:1030–1041. http://dx.doi.org/10.1016/j.jhydrol.2016.08.012

The authors are advised to go through these studies in order to familiarize themselves with the latest developments in the field of PUB. They further have to defend how their study is different from the already executed comparative studies? Frankly, I don't see much innovation, here.

2. The authors used dataset from 2007-2015 for model calibration and validation phases. Such dataset is not enough to hunch the flow trends. In such case, the modeling technique can be considered suitable for a particular time phase but cannot be generalized due to inadequate data length. Since the results generated by the proposed model for the entire study area are tested by using a LOOCV procedure. One solution to increase the data length is to consider one station as ungauged, removing it from the whole database and estimating FDC for that station with the proposed approach.

3. Line 15 reads, "Though combining a temperature index snowmelt model with GR4J can be an alternative approach, it increases the number of parameters and thus model uncertainty". How can increase of parameters make the technique uncertain? Increasing parameters increase complexity but it always betters efficiency. A thorough explanation is needed for this claim.

4. The periods and range of streamflow data (2007-2015) and climatic data (1973-2015) are not overlapping? Will it not be problematic? Moreover how was the range of climatic data used in calibration and validation phases of the model?

5. The equitation (4) seems to be generated by multivariate regression analysis. The authors never explained its generation, which is inevitable. How effective was this rescaling?

6. Equation 6 should be eliminated as it is already discussed above (equation 2). More over the performances indices need to increased. I suggest including mean absolute error and root mean square error.

7. Why only five nearest neighbors were used? Why not, say, 8 or 10 or 12? There has to be a reason for that. The authors are suggested to go through the following paper and study Figure 9 in detail in which Samaniego and Kumar (2010) selected nearest neighbors by observing the error generated by different number of neighbors.

Samaniego, L., A. Bárdossy, and R. Kumar (2010), Streamflow prediction in ungauged catchments using copula-based dissimilarity measures, Water Resour. Res., 46, W02506, doi:10.1029/2008WR007695.

8. The authors never discussed the complications involved in the implementation of each method. The discussion section should also compare the simplicity of each method in terms of implementation.

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