

## ***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.***

**Anonymous Referee #2**

Received and published: 28 November 2016

The paper “A comparison between parameter regionalization and model calibration with flow duration curves for prediction in ungauged catchments” by Kim D., Jung I. and Chun, J. A. shows a comparative assessment of two methods for predicting daily streamflow series in ungauged catchments employing on the one hand the parameter regionalization of a rainfall runoff model, on the other hand the calibration of the same model using predicted flow duration curves. Even if the authors do not introduce any novel technique or method, the topic is of a wide interest in the hydrological field, thus I believe it might be suitable for publication in HESS after some minor improvements, which in my view the author might consider to take into account. Also, the paper is well written and is rather complete in all its sections.

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### **Minor comments**

1. The authors rely often on to NSE efficiencies to assess the reliability of each model, and they conclude that, given the good results obtained with  $PROX_{reg}$ , which is the regionalization of the parameters of the rainfall runoff model, that this model is preferable to the other given its capacity to reproduce the true hydrograph in time. However, this conclusion seems to strongly contrast with other performance indices they deliver, e.g. VEQ, RQP, IBF, so I would soften a little the conclusions and would let the judgment be more flexible.
2. NSE is the most used performance index in literature and I agree with using it, but recently it has been criticized its capacity to understand how a model produce good result with low flows, while it emphasizes the capacity to understand high flows, so that NSE could end to be a biased index (the authors also recall this behavior at P11 L8). I wonder how the final performances vary by adopting LNSE, which is the same as NSE but taking the natural logs of streamflows. I assume that the lesser influence of the low flow regime prediction into NSE might somehow introduce a distortion of final judgment. In case the results are substantially different I would recommend the authors to make an effort in discussing these results as well.
3. I would suggest to change the title including the word “streamflow” somewhere. In this way, the topic is clearer. Perhaps “A comparison between parameter regionalization and model calibration with flow duration curves for prediction of streamflows in ungauged catchments” ?
4. Table 1 reports for each model two columns in which the author say whether or not NSE efficiencies are greater than 0.6 or 0.8 respectively. I think that would be of more interest for the reader to see all the values for each catchment as well as the cut-off at 0.6 or 0.8. For instance, they can report efficiencies for each

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catchments and let the numbers above the cutoff in bold face. Reporting “Y” or “N” only might result uninformative and, at the very end, useless.

5. I would recommend to extend paragraphs 3.3.1 and 3.3.2 to introduce some more details of the two proposed approaches. Furthermore, I would move those two paragraphs before the evaluation indices adopted.

#### **Technical notes and misspellings**

1. P3 L7. “Siberian high pressure”, perhaps is “low pressure”.
2. P8 L19. Please add the word “between” in between the words “coefficients” and “CPI”.
3. P10 L3. Please remove the article “the” between the words “values” and “between”.
4. P10 L4. “Based on the high [...]” is perhaps “Based on the highest [...]”?
5. I have not completely understood what the author mean with “orthogonal” referring to streamflow signatures, please consider to add some more details arguing what does this word mean for them into the context of the sentence.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-487, 2016.