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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Comparative evaluation of rainfall-runoff modelling against flow duration curves in semi-humid catchments" *by* Daeha Kim et al.

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We greatly appreciate your valuable efforts to review our manuscript. Following are specific responses as per comment.

Comments from Anonymous Referee #1: This study evaluates the predictive performance of a rainfall-runoff model when it is calibrated against flow duration curve (FDC), and compares the results with those obtained with conventional hydrograph-based approaches. Authors focus on 45 gauged catchments in South Korea and derive FDCs and streamflow indices using regionalization. Their results show that even though FDC calibration yields promising performance in predicting low flows, it could generally lead

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to noticeably weaker performance and higher uncertainty in streamflow predictions (in comparison to hydrograph-focused calibration), potentially due to the absence of flow timing. In ungauged catchments, their results demonstrate that the proximity-based parameter regionalization (i.e., not using FDC) performs better than the calibration against regional FDCs estimated by a geostatistical method. I have found this study valid from the scientific and presentation quality, however, I have a number of major issues with its scientific contributions, which I am elaborating on in this review. Overall, I recommend re-submission after major revisions. Major comments: The first objective in this study, as stated on page 4 lines 8-10, is to evaluate predictive performance of the hydrograph calibration and the FDC calibration as well as their uncertainty for gauged catchments. I think this idea has been addressed extensively in the literature (some of which are cited in the present manuscript), and therefore, it does not need any further examination. The fact that this study finds FDC-based calibration less promising than hydrograph-based approach (as stated on page 11 lines 13-15) is not of a big surprise, e.g., due to different challenges in FDC estimation and that timing is not handled by FDC, as authors point out in the manuscript as well. Probably, what is more worth studying is how FDC can help to reduce equi-finality. As a result, I suggest that authors remove the first part of the study, or consider FDC as an additional criteria in model calibration and show how its use would improve parameter identifiability (e.g., posterior ranges) and reduce uncertainty (e.g., uncertainty ratio of hydrograph+FDC to only hydrograph).

–> We agree that low performance of the FDC calibration is not a surprise for gauged catchments with continuous hydrographs. However, we think it is necessary to show uncertainty from equifinality in the FDC calibration is double of that in the hydrograph calibration. It may provide information that doubled equifinality can produce much higher errors than transferring parameter sets.

Authors claim that FDC calibration performs promising for low flow prediction. I would argue that FDC-based approach performs only better than hydrograph-based ap-

proach, not good overall. Looking at figure 9, I see that there are several large deviations between simulated and observed BFI (up to 90%) which means that FDC-based method is not that reliable.

–> We disagree. It was difficult for us to conclude that BFI reproducibility of the FDC calibration is worse than hydrograph calibration. In figure 9(b), the median of the FDC calibration is less than hydrograph calibration. Its 3rd quartile is much smaller than that of hydrograph calibration. RFDC_cal and PROG_reg also showed similar performance to reproduce BFI.

The reason why it performs better than hydrograph-based approach is that the latter only focuses on high-flows as the Nash metric is biased on large values. So, this claim is of a sort of concern to me.

–> The FDC calibration used the same objective function in Eq. 2(a). If NSE exaggerated high-flow reproducibility in the hydrograph calibration, the FDC calibration should be in the case (i.e., high flow quantiles should be emphasized too). Nonetheless, the FDC calibration showed reproducibility comparable to the hydrograph calibration in low flows.

My other major issue is with how authors set the experiments related to streamflow predictions in ungauged catchments. They first mention three classes of parameter regionalisation in lines 26-30 on page 8, but then mention that they chose the proximity based approach due to its simplicity. I think, given than the first part of the paper can be removed according to my view, authors should focus more on this part and compare different regionalization approaches.

–> Although comparing between regionalization methods is a meaningful topic, it has been studied widely. For example, Oudin et al. (2008) and Parajka et al. (2013) provided a lesson that the high performance of proximity-based calibration. The proximity-based regionalization was attractive under modeling conditions in Korean catchments based on their comprehensive evaluations. On the other hand, it may be difficult to

find a comparison between a FDC calibration and a regionalization. This study shows that a simple parameter transfer from gauged catchments outperform a local calibration against well-predicted FDC. This lesson can be practically meaningful, because regionalization of flow signatures (e.g., FDCs) requires additional efforts.

Also, why not considering the proximity-based transfer of FDCs from donor catchments as an additional approach? Then, a potential topic for the paper can be "comparative evaluation of different regionalization approaches for model calibration in ungauged catchment".

–> This is an outside topic of this study. We can consider it for future studies. For example, we can answer a research question "Can simply transferred FDCs be comparable to empirical or regional FDCs in rainfall-runoff modeling?" It is a good suggestion, but beyond our topic to compare between a local calibration against a regional FDC and a parameter regionalization.

Page 7 line 15 says that "Synthetic runoff time series were generated by GR4J for the same 45 catchments by treating each catchment as ungauged.

–> Nothing is requested. This sentence is to explain how to evaluate runoff simulation for ungauged catchment. If necessary, we will review the sentence again.

Introduction needs to be shorter. Objectives are stated after 6 very long paragraphs in the introduction section. Moreover, discussions sub-sections are too long. I think authors can make them briefer, but still transfer the message to readers.

–> We can consider this comment in revision to make the manuscript concise to have better readability.

Minor comments (for improving manuscript quality):

I suggest continuous line numbering in the next version of the manuscript.

–> For convenience, we will add the line numbers continuously.

Page 3, line 34: I suggest that a little explanation is provided here about the proximity-based approach. It is not clear up to this point what that approach actually is. Authors provide a brief description on page 7 line 17. Also, I suggest removing “in truth”

–> We will shortly add the description in the sentence. We will remove “in truth”.

Also related to the description of proximity-based approach, section 3.3.2 is not fully understandable. I suggest rewording the paragraph so that the approach is explained in a clearer way. Moreover, please explain at the beginning of this section that when you talk about parameters in the proximity-based approach, you actually mean the parameters of the hydrologic model. Because one can also estimate the parameters of a parametric FDC using this approach.

–> We will review this section again and will concisely restate the methodology with more readability.

Page 9 line 1: what do you mean by “synchronizing” donor catchments?

–> It simply means that we used same donor catchments for the regional FDC and the parameter regionalization. We will reword it.

Page 4 line 3: define “orthogonal”

–> We adopted the term of “orthogonal” from Hrachowitz et al. (2013). “orthogonal” means something that can complement FDCs. We will clearly define it, or use a more appropriate expression.

Please explain why Monte Carlo is used for parameter estimation, whereas SCE has been used by authors in one of the catchments. I believe that there is the possibility of quantifying uncertainty bounds using the solutions sampled by SCE.

–> Using SCE, it was difficult to find convergence when calibrating against FDCs because of high equifinality. Thus, we used a similar method in Weterberg et al. (2011, 2014) that proposed a FDC-based calibration. The Monte-Carlo framework was better

for us to flexibly use for all calibrations in this study.

Page 12 line 26-28: the sentence is not understandable. Please reword.

–> We will rewrite it. We just mentioned that errors in regional FDCs are not a great concern based on high performance of the geostatistical method.

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