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# *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 your constructive comment. Once again, we are thankful for your valuable time.

Anonymous Referee #2:

The work explores the predictive performance of application of a FDC in comparison with conventional hydrograph calibration and parameter regionalisation for gauged and ungauged catchments. While the manuscript has some interesting results and discus-



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sion, it is not clear to me from the text how the work is innovative and unique to the previous studies mentioned in the literature review and discussion. For this reason I suggest major review to lift the manuscript before the work is suitable for publication in HESS. To me the manuscript currently lacks focus in the sense that the key research gaps and innovation should stand out more clearly in the introduction and conclusion. In my opinion the authors should focus on quality and innovation rather than applying existing techniques, and quantity of results and discussion.

–> Although we applied existing techniques to predict runoff and FDCs, we believe the comparative evaluation in this study is meaningful to select an appropriate approach for ungauged catchments. There are a plethora of approaches to predict streamflow in ungauged catchments (e.g., rainfall-runoff modeling with parameter regionalization, model calibration against flow signatures, and direct FDC regionalization among many others). Which one is better is a practical and important question for modelers. It is a heavy burden for a modeler to apply all existing methods for ungauged catchments. This study provides a lesson that model calibration against a well-predicted FDC may not be comparable to a simple parameter transfer from neighboring gauged catchments. Thus, we can highlight that the loss of flow timing can significantly influence efficiency of rainfall-runoff modeling even in the case of ungauged catchments. It is no surprise in the fact that the FDC calibration with no flow timing information cannot provide a better model performance for gauged catchments. However, for ungauged catchments, we cannot assure if a parameter regionalization outperforms the calibration against a predicted FDC. This study implies that calibration against a well-predicted FDC may be less attractive than a simple parameter transfer. Despite the absence of flow timing information, FDC is regarded as comprehensive flow information reflecting catchment behaviors. Thus, one can hypothesize that behavioral parameter sets directly obtained from an empirical (or predicted) FDC may be better than a priori parameter sets. This study shows that a priori parameter sets gained from surrounding gauged catchments may be better. This is novelty of our comparative study. We believe a comparative study can provide a practical lesson as did Zhang et al. (2015),

although existing techniques are applied only.

Major comments: The innovation of this work compared to previous studies is not clear to me. Could the authors please state explicitly the innovation of their work compared to previous FDC regionalisation studies and existing methods? The specific research gap/s that the work is addressing should be more prominent in the introduction, and the innovations compared to previous studies need to be more prominent in the summary and conclusions section.

–> It is possible to improve the introduction to more clearly highlight the research question. Several FDC regionalization studies emphasized that a FDC can comprehensively reflect catchment behaviors. Thus, one may assume that direct calibration against a regional FDC is a solution to overcoming drawbacks in parameter regionalization. Our study argues that it is not true in South Korea. We can improve the introduction with the given references.

Could the authors also please describe in detail how you improve on your previous 2016 submission to HESS that uses the same 45 South Korean catchments and has a similar goal: “Kim et al. A comparison between parameter regionalization and model calibration with flow duration curves for prediction in ungauged catchments”. Reading the comments from the reviewers on the previous submission there are some points that have not been fully addressed in this submission.

–> Here, we briefly summarize how we considered the comment given by the previous review process. We believe the comprehensive comments were considered in the revision generally. For example, actual constraining with flow signatures, and replacing the objective function, evaluating low and high flows are main revisions that considered the comments. If necessary, we will recheck the comments again.

–> The referee 1 mainly argued that our study had limited contribution to prediction in ungauged basins because of existing FDC methods for runoff prediction. However, the objective of our study was not to provide a new FDC-based runoff prediction, but a

comparative evaluation between existing methods. Hence, we disagreed. The referee 1 also argued that it is no surprise with low performance of the FDC calibration. However, we cannot assure it in case of ungauged catchments. We disagreed. The small number of gauged catchments was pointed out; however, we cannot do anything to improve it because it is a given condition. 45 is not a great number, but some parameter regionalization studies used even smaller samples. The reviewer 1 argued that the objective function of NSE is not practical because of its emphasis on high flows. We replaced the objective function with one proposed by Zhang et al. (2015). And, we considered all catchments for regionalization instead of only using high performance catchments. Other minor comments were considered as well.

–> The referee 2 recommended to soften conclusions that PROXreg is better than the other. If the revised version still needs it, we will tone down again. Use of multiple criteria was recommended as well, thus we used NSE and LNSE together in revision. Some minor suggestions for title, tables, and context were given together. We added new figures and tables. The manuscript is retitled. Because the definition of "orthogonal" was missed in the revision, we will add it.

–> The referee 3 provided very constructive comments, asking first "why not parameter regionalization gained from observed FDCs?" We did not consider this comment because it is beyond the objective of this paper. We did not intend to propose a new parameter regionalization method and its performance evaluation. The primary research question of this study is that "Do parameters directly identified by predicted FDCs outperform a conventional parameter regionalization?" Regionalization of parameters gained against observed FDCs is expected to have low performance due to the loss of flow timing. Regionalizing those parameters may lead to higher uncertainty for ungauged catchments. The referee 3 also suggested including uncertainty evaluation for both approaches for ungauged catchments. Although it indirectly shows uncertainty for ungauged catchments, the uncertainty evaluation added in the revision provides a lesson that uncertainty of the FDC calibration would be two-times of that in

the hydrograph calibration for gauged catchments. Referee3 also argued that there is no evidence that the rising limb density can complement the FDC. Hence, we provided actual calibration results conditioned by the rising limb density. With some minor comments, it was asked to provide more specific examples using flow signatures in runoff modeling. So, we improved the introduction with more literatures.

I suggest adding either “ungauged” or “regionalisation “ to the title of the manuscript to make the title more descriptive of the work undertaken in the manuscript.

–> We agree. We will imply it in the title.

Minor comments: In the future please line number the manuscript continuously e.g. 1-999 rather than by each page, this will aid the review process.

–> For convenience, we will add line number continuously.

The first paragraph of Section 3 introduces the GR4J model, and I see no logical progression to Section 3.1. I recommend an opening paragraph describing the structure of the methodology and turning your current paragraph into a new Section e.g. “3.1 Hydrological model (GR4J)”. Furthermore I suggest a second section e.g. “3.2. Flow duration curve (FDC)” for consistency and to ensure reproducibility of your work.

–> We can consider this comment to improve readability.

Can you clarify in page 9, lines 4-7 your justification for applying a different objective function for calibration (Eq. 2a, 2b, 2c) OBJ, to the functions used to evaluate predictive performance (Eq. 5) NSE and LNSE?

–> We will clearly show the objective function and the evaluation criteria to prevent confusion between them. As known, NSE and LNSE are metrics evaluating reproducibility of high and low flows respectively.

Page 10, Line 12 I disagree that the term NSE was used “directly” for calibration, rather I understand that you used a combination of the NSE and the WBE in OBJ. Please





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Discussion paper

clarify.

–> We will remove the term “directly”. Since NSE is still used in the objective function, optimization can be toward high flows. WBE is to reduce bias, not to regenerate low flows. No metrics regarding low flows are included in the objective function.

Figure 3: I suggest adding headings “GR4J”, and “FDC” to the top panels to ease interpretation.

–> We will add the headings in the figures to give prompt indications. However, all simulations were from GR4J in this study. We will use other appropriate headings.

Figure 4: If these are 1:1 plots then I suggest adding a 1:1 line to the panels to ease interpretation.

–> They are not 1:1 plots. They display the relationship between input-output consistency and model performance.

Figure 5. Where is the difference between the first and second column of panels described in the caption or figure? I suggest adding headings to describe the difference in a similar manner to my recommendation for Figure 3.

–> Left and right panels are presented in same scales. We will add headings and tick labels to provide prompt indication.

Could you please provide a more professional title (i.e. remove the phrase “performs good”) to Subsection 5.2? e.g. “performs well”, or a new title “Suitability of the FDC calibration for prediction of low flows”

–> We will consider “Suitability of the FDC calibration for prediction of low flows” as a new title of the section. Thanks for the good suggestion.

In Figure 10a it is very difficult to see the difference between observed and modelled FDCs. If this result is presented then could the authors provide an inset zoom to allow the reader to see the difference between the FDCs for the highest flows?

–> This figure indicates higher variance loss in direct flow than in baseflow when using the FDC calibration. We will improve readability.

Please proof read future submissions in greater detail, see some notes below. Typos and clarifications: Abstract line 11: ". . .Monte-Carlo framework. . ." is a bit vague given the complexity of your calibration (e.g. initial use of the SCE) please be more descriptive.

–> We will provide a clearer explanation about the methodology in the abstract.

Page 1, Line 2: Should we not have an "and"?

–> For convenience, we will add it later.

Page 2, Line 9: Should "has" be replaced with "is"?

–> We consider either "gaining" or "has increasing".

Page 2, Line 15: In the papers that you refer to in the previous sentence (i.e. Beven 2006), the term used is "equifinality" rather than "equi-finality". As this is a widely used term in the field of hydrological modelling I think that this consistency is important. Furthermore, the paper referenced (Oudin, 2008) does not refer to the term "equifinality", and so I feel that you may wish to choose a reference that better reflects the implication of the sentence.

–> We will use "equifinality" consistently. Oudin et al. (2008) did not use the term "equifinality" literally; however, they pointed out that "most models have been shown to have no unique set of parameters to define the best model fit to the flow response of a catchment" (in paragraph 3). In the context, we could find equifinality is an important uncertainty source when extrapolating parameters to ungauged catchments. Thus, we cited it.

Page 4, Line 3: Please clarify what you mean by "orthogonal" here

–> "orthogonal" here means something that can complement FDC. We used the term in

Hrachowitz et al. (2013). We will clearly define it, or use a more appropriate expression.

Page 4, Line 13: Why have you used the term “simply”? I suggest removing it.

–> We will remove it.

Page 4, Line 18: “Characterized”, previously you have used UK English rather than US English, e.g. Page 4, Line 7 “regionalisation“. Another e.g. Figure 1 caption “regionalization”. Another Page 8, Line 25: “regionalization”. Another example when you refer to Figure 2 you use “schematized”, but in the Figure 2 caption you use “schematised”. Please be consistent throughout the paper.

–> We will have consistency in English. We will globally review the expressions.

Page 4, Line 32: typo “Mistry”, should be “Ministry”

–> We will check typos globally.

Page 7, Line 25: Please choose an alternative wording to: “and thus of consistency”, e.g. “and therefore are consistent”

–> “and therefore are consistent” is better. We will revise it.

Page 8, line 10: “50 parameter sets” I recommend adding “. . .from the Monte-Carlo. . .” to remind the reader what you are referring to here.

–> Maybe it is in page 9. We will add it as recommended.

Page 10, Paragraph starting with line 22. Please clarify what correlation coefficient you are referring to. I.e. Pearson correlation.

–> It is the Pearson correlation coefficient. We will clearly show it.

Page 16, line 15. I am not sure if the word “Obviously” is necessary here. How is this future work more “obvious” than the other limitations that you have discussed above? I suggest removing it.

–> We agree. We will remove it as suggested.

Table 1: Typo: “Draiage”

–> We will correct typos globally.

References

Hrachowitz, M. et al.: A decade of Predictions in Ungauged Basins (PUB) - A review. Hydrolog. Sci. J., 58, 1198–1255. Doi:10.1080/02626667.2013.803183, 2013.

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Zhang, Y., Vaze, J., Chiew, F. H. S., and Li, M.: Comparing flow duration curve and rainfall-runoff modelling for predicting daily runoff in ungauged catchments, J. Hydrol., 525, 72-86, 2015.

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