

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

Anonymous Referee #2

Received and published: 13 June 2017

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 discussion, 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

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opinion the authors should focus on quality and innovation rather than applying existing techniques, and quantity of results and discussion.

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.

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.

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.

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.

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.

Can you clarify in page 9, lines 4-7 your justification for applying a different objective

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function for calibration (Eq. 2a, 2b, 2c) OBJ, to the functions used to evaluate predictive performance (Eq. 5) NSE and LNSE?

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 clarify.

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

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

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.

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”

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?

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.

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

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

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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.

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

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

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

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

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

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

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.

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Table 1: Typo: "Draiage"

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-138>, 2017.