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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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Dear anonymous referee:

We thank for your comprehensive review on our manuscript and greatly appreciate your valuable time and contribution. We generally agree to your comments and recommendations, and want to improve our manuscript for publication in HESS. We agreed that our manuscript needs to be restructured for clearly showing scientific contribution. In our opinion, it would be better to address comparison between hydrograph-based and FDC-based calibrations at gauged catchments first, and then move to the ungauged case (i.e. parameter regionalization and calibration with regional FDCs). To evidently suggest orthogonal flow signatures, it would be better to provide actual results of FDC-

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based calibrations in combination with the three signatures (i.e. runoff ration, baseflow index, rising limb density) at gauged catchments. In revision, we will provide in-depth discussion on performance and uncertainty in simulated flows.

However, we still want to focus on our main research objective to compare parameter regionalization and calibration with regionalized FDCs for ungauged catchments. Although you proposed regionalization of parameters fitted to empirical FDCs, the parameter sets calibrated to FDCs (i.e. only flow magnitudes) are likely to have more uncertainty than those fitted to hydrographs (i.e. flow timing and magnitudes). Hence, we expected that regionalization of parameters from empirical FDCs would be more uncertain than conventional regionalization approaches too. On the other hand, it is difficult to answer whether calibration with regionalized FDCs has less performance than parameter regionalization or vice versa. Through this comparison, we want to provide novel information for selection of methods for predictions in ungauged catchments.

From our knowledge, daily runoff prediction in ungauged catchments has been barely studied in South Korea in comparative ways. Thus, our manuscript will be also beneficial to expand spatial coverage of previous regionalization studies. Considering your comments and recommendations, we want to restructure our manuscript as follows:

(1) We will include more literatures about prediction in ungauged catchments in the introduction as you requested. Additional literature will be about FDC regionalization and signature-based calibration methods. We will also introduce the decade-long project of the IAHS in Prediction in Ungauged Basins (Blöschl et al., 2013; Hrachowitz et al., 2013) for providing comprehensive knowledge about rainfall-runoff modeling in ungauged catchments.

(2) We will restructure the manuscript from gauged to ungauged catchments. First, we will show results and discussion about predictive performance and uncertainty at gauged catchment of both hydrograph- and FDC-based calibrations. Then, we will

move to compare and discuss the parameter regionalization and calibration with regional FDCs.

(3) The part of signature reproducibility will be replaced with actual FDC-based calibration in combination with three flow signatures at gauged catchments. We still expect the rising limb density (RLD) would be orthogonal because it is information of flow timing that FDCs do not have. Actual results will be evidence of our conclusions.

Once again, we thank for your contribution, and please find our response as per your comment below.

1. In this manuscript two regionalization approaches are compared: the first approach regionalizes parameter sets calibrated with the hydrograph, and the second approach regionalizes normalized FDCs that are used for the calibration of a runoff model. Although I like this second approach it seems to be rather different from the first one. I wonder why the authors did not apply an approach that is closer to the first one such as calibrating the model using the FDC and regionalizing these calibrated parameter sets. Using the suggested approach would make the results more comparable because the uncertainty sources are more similar (e.g. uncertainty due to top-kriging would be eliminated).

-> Although two approaches appear to be similar, uncertainty sources involved in them are very different. The proximity-based regionalization is to transfer parameters fitted to more informative data (i.e. both flow timing and amount), but there is no calibration process for ungauged catchments. On the other hand, fitting parameters to regionalized FDCs has a calibration process for ungauged catchments but with less informative data (i.e. only statistical flow amounts). The main research question of this manuscript is: which one is better between (1) no calibration for target ungauged catchments but more informative calibration in gauged catchments and (2) direct parameter calibration for ungauged catchments but with less informative data? It is difficult to answer if regionalized parameter sets have greater uncertainty than calibrated parameters with less informative data or vice versa. When embarking on this study, we believed that this question is more meaningful than regionalization of parameter sets from empirical FDCs. In our sense, it was likely that regionalizing parameters fitted to both flow magnitude and timing would be more reliable and thus of a better predictive skill than regionalizing ones fitted to flow magnitudes alone. For clearly showing the objective of this study, we would include a figure that schematizes two approaches. And, we will explain our research objectives more clearly in the introduction.

2. The manuscript would benefit from a detailed discussion on the sources and the influence of uncertainties related to the different regionalization approaches. They are crucial for the interpretation of the results.

-> We will do it in revision. It would better to comparatively discuss uncertainty associated in calibration against runoff and empirical FDCs. We will include a quantitative comparison between two approaches at gauged catchments. Then, we will move to ungauged catchments for providing in-depth discussion about uncertainty in parameter regionalization and calibration against regionalized FDCs.

3. Besides the hydrograph and the FDC also runoff ratio, baseflow index and rising limb density of the ungauged catchments are evaluated. The authors state several times that the calibration of the runoff model against the regionalized FDC and the rising limb density simultaneously would improve the prediction in ungauged catchments. However, I see no strong evidence for this statement based on Fig. 9. I also don't understand why the rising limb density is regarded as being orthogonal to the FDC. I recommend to weaken these statements or to provide good evidence for it. Furthermore, no information is provided on how the rising limb density could be derived for the ungauged catchment. Would you also regionalize it?

-> We wanted to introduce flow signatures that can potentially enhance the FDC-based calibration. In Fig. 9c, PROX_reg has much less medians and heights in box plots than RFDC_cal. It confirms that RFDC_cal has a shortcoming to reproduce the average time to peak in runoff time series. Because the rising limb density (RLD) itself is a flow signature indicating flow timing, we suggested a calibration against FDC (flow magnitude) plus RLD (flow timing) would enhance the signature-based calibration. However, we agree that this inference is not evident. In revision, we will test this hypothesis at gauged catchments and will provide the results. We expect better predictive performance from addition of RLD in calibration. However, regionalization of RLD is another important research topic. We believe it would be better to address this in a separated paper. The top-kriging method can also be a candidate method for regionalization of RLD. However, there is no guarantee that the geographical interpolation will show similar performance to the FDC regionalization. RLD could be more sensitive to physical properties of catchments than FDC regionalization. Since the calibration against empirical FDC plus RLD at gauged catchments can be a potential evidence for ungauged catchment, we want to state regionalization of RLD as a further topic.

4. It could be interesting if you actually tried to constrain the runoff model by the FDC and the rising limb density (or any other suitable runoff signature). -kriging is used for the regionalization of the normalized FDCs. Is this approach really a well-established method as you mention? How many studies have used this approach? Is this approach suitable for FDCs and the density of the gauging stations in your study area? Can you give good reasons for not using ordinary kriging?

-> As replied to comment 3, we want to provide actual calibration results against empirical FDC plus other signatures in revision. The geographical method is recently proposed by Pugliese et al. (2014), thus it has not been frequently adopted in previous studies. However, its performance in the original study was 0.914 and 0.922 in terms of NSE and Log NSE between observed and predicted quantile flows in 18 Italian catchments, and the geostatistical method outperformed other two conventional regionalization methods. In Pugliese et al. (2016), this method was also compared with a regression-based method for 182 catchments in southeastern U.S., and found good predictive performance in both low and high flow estimates. Top-kriging can consider topological features of watersheds while ordinary kriging cannot. In top-kriging, nested catchments have more weights for interpolation than adjacent catchments. For spatial interpolation of functional behaviors of catchments, top-kriging seems to be better than the ordinary kriging. Based on this information, we adopted this method that has a great merit to non-parametrically preserve features in FDC continuum.

5. The results from the two regionalization approaches are presented as separate numbers (performance value) or separate boxplots that are next to each other (Fig. 7-9) which is inconvenient for their direct comparison. I would recommend to improve the presentation of the results by using the parameter regionalization approach as a benchmark to which the second approach (calibration with regionalized FDC) is compared. E.g. take the difference between the Nash-Sutcliffe efficiency of approach one and two for each catchment.

-> We will provide better presentations for clear understanding. In revision, we will use a combined objective function between NSE and log VE for calibration as in Zhang et al. (2015) such that we can have a balance between high and low flows in parameter calibration. We will also apply this to the FDC-based calibration for consistency. Then, we will take differences for each catchment as advised. We agree that it will improve readability.

6. The snowmelt model used for the calculation of snow accumulation and ablation needs more explanation, especially because snowmelt models based on energy balance usually are data intensive. From shortly reading the publication from Walter et al. (2005), I don't have the impression that this physics based snowmelt model is simple, as you call it. I agree that the snowmelt model doesn't have parameters that are calibrated, however it has various parameters that have to be estimated (e.g. cloud cover, albedo, windspeed, etc.). It would be worth to discuss whether there is really less uncertainty involved than when using e.g. a degree-day method for the simulation of snow accumulation and ablation.

-> Although the snowmelt model of Walter et al. (2005) is not very simple, it requires temperatures and precipitation only. In our sense, it is difficult to say that the snowmelt model is data-intensive as is a typical physics-based snowmelt model. As you commented, physical parameters (e.g., albedo, transmissivity, cloud cover, etc.) are necessary for physical snowmelt modeling. Hence, Walter et al. (2005) mainly addressed how to estimate them only with precipitation and maximum and minimum temperatures. We will more clearly address uncertainty sources of the snowmelt model in revision. However, snowmelt is of minor influences on streamflow regimes in South Korea because summer season rainfall is dominant liquid forcing to catchments. Our intention was to reduce bias from no snow component in GR4J snowmelt, not to better simulate the snowmelt process. For justification, we will provide ratios of highest SWEs to annual precipitation for each catchment. The reason why we did not combine a temperature index (i.e. degree-day) with GR4J is to avoid interaction between the temperature index and GR4J parameters. It can worsen the equi-finality problem. We still want to maintain the parsimonious structure of GR4J. We agree that it is necessary to answer if the addition of the degree-day actually causes higher uncertainty than the physical snowmelt model. However, we believe that it is not meaningful in the case that most information for parameter calibration is in summer season hydrographs. It would be better to regard the physical snowmelt modeling as one choice for considering snow component with no additional parameters.

Moderate comments: P1 L16-18: I would remove the sentence about the rising limb density and instead add some information about the results of the FDC prediction in the ungauged catchments, because that was one focus of your study.

-> We agreed. We will improve the abstract.

P2 L1: The study from Seibert and Beven (2009) did not use any regionalization in their analysis. This paper is not the right citation here. Please make sure that you cite properly.

-> It would be a mistake. We will check if all references are properly cited again.

P2 L7: When writing about regionalized flow signatures it would be worth to include the study of Yadav et al. (2007) and Hingray et al. (2010) at this point. âĂć Yadav, M., Wagener, T., and Gupta, H. V.: Regionalization of constraints on expected watershed response behavior for improved predictions in ungauged basins, Adv. Water Resour., 30, 1756– 1774, doi:10.1016/j.advwatres.2007.01.005, 2007. âĂć Hingray, B., Schaefli, B., Mezghani, A., and Hamdi, Y.: Signature-based model calibration for hydrological prediction in mesoscale Alpine catchments, Hydrolog. Sci.

-> We agreed. They are informative references. We will cite them.

P2 L9: You mention that flow signatures have been frequently applied for model calibration. Please give some more examples including runoff ratio, baseflow index and rising limb density.

-> We will add more studies on signature-based model calibrations in the introduction.

P2 L16: Please give more examples of studies that regionalize FDCs and explain how they do it. This is important because the regionalization of FDCs is a core method of your study.

-> Yes. We will more comprehensively introduce studies on regional FDCs (e.g., Shu and Ouarda, 2012)

P2 L19-26: The information in the first sentence contradicts your subsequent paragraph.

-> We will globally recheck and improve all sentences.

P3 L4-11: Please cite where your information about this paragraph comes from. Is the information of this paragraph for South Korea in general or does it only relate to the study catchments?

-> It is general description of climatic and geophysical characteristics of South Korea.

South Korea is not a large country, thus can be common features of the study catchments. We will provide references from the Korean Meteorological Administration for this paragraph.

P3 L13-L17: Where is the data of the 29 catchments with high quality from? Please add this reference and also the reference for the inflow data of the multi-purpose dams to the reference list at the end of the manuscript.

-> We will provide the reference from the Ministry of Land, Transport and Maritime Affairs of the Korean government.

P3: Chapter on description of study area and data: where is the evaporation data from that you need as input for the runoff model? Do you need elevation data for the runoff model? Do you have any information about geology, vegetation etc. because you mention this as possible reason for poor top-kriging performance (P9 L15).

-> Evaporation is estimated from temperature data using the simple temperature-based model proposed by Oudin et al. (2005). Oudin et al. (2005) concluded that the scientific potential evaporation model was not good for daily runoff modeling with GR4J. They proposed a temperature-based model for GR4J together with the evaluation of numerous ET models. We will include this description in the methodology section. And, no elevation data are necessary for rainfall-runoff modeling. We will provide average slope, vegetation, urban areas of each catchment as recommended.

P4 L3-8: I recommend to add a short description of the structure of GR4J, information about the required input data and its resolution as well as information about the use of elevation bands.

-> We will add the description and information as advised.

P6 L24: Why do you evaluate the hydrograph with Nash-Sutcliffe and volume error, but the FDC only with Nash-Sutcliffe?

-> We will redo parameter calibrations with a criterion balanced between high and low flows, e.g. a combination of NSE and log VE in Zhang et al. (2015), in revision. We will also apply this to FDC-based calibration for consistency. Then, we will separately evaluate performances with other measures (e.g. NSE, Log NSE, Pearson r and RMSE).

P6 L28: Give an explanation why you selected runoff ratio, baseflow index and rising limb density as signatures. Why three signatures?

-> They were regarded as major flow signatures in catchment classification (Sawicz et al., 2011). The runoff ratio explains average portion of precipitation that is discharged. Thus, it explains water holding capacity and evaporation loss of catchments. The baseflow index explains the portion of slow flows in hydrographs, and thus quick flows can also be evaluated together. Rising limb density shows how fast the catchment response is. Although there are more flow signatures (e.g., spectral density in hydrographs), we assumed the three signatures explain climatic, soil, and topographic characteristics of catchments. Snow day ratio is also an important signature, but we only focused on catchment response to liquid forcing, which is important in South Korea. We will add this point in the manuscript.

P7 L22: Please explain why you use 5 donor catchments and not 3 or 7.

-> It is for consistency with the TND interpolation using the top-kriging. We found five nearby catchments were best for interpolating TNDs. It is achieved from iterative calculations and explained in page 9. In revision, we explain n=5 in the section of FDC regionalization. We wanted to have consistency in the number of donor sets for both approaches for ungauged catchments. In addition, from results in a comparative study of Oudin et al. (2008), we were indicated that adding donor catchments would worsen predictive skills of proximity-based regionalization when using GR4J (see figure 6 in Oudin et al., 2008).

P8 L9: Again, why do you use 5 parameter sets and not 3 or 7? Does it make sense to give weights to these 5 parameter sets given the uncertainty related to them?

-> We preliminarily tested an average-sized catchment by plotting the number of parameter sets vs. NSEQ. We will add this explanation in revision. In fact, the weights did not sensitively affect our LOOCV because five parameter sets showed similar NSEFDC values for most catchments. We just hypothesized weighting parameter sets with higher NSEFDC would be better. As you commented, it would not be meaningful under given uncertainty. We will consider simple averaging in revision. P8 L31: Why do you use NSE(Q) 0.6 as threshold?

-> Oudin et al. (2008) discussed that low predictive performance at donor catchments clearly affected performance of ungauged catchments with GR4J. They used 0.7 of NSE for screening out catchments with low performance. In our study, 0.7 of NSE was too high to have adequate proximity between gauged and ungauged catchments. So, we reduced it to 0.6. However, in revision, we want to include all 44 catchments for regionalization irrespective of predictive performance in order to fully consider uncertainty sources in parameter regionalization.

P9 L4-10: This paragraph belongs to the methods section and should not be in the results. Do I understand correctly that 5 donors were used for the regionalization of the FDC? If yes, how do you get a total weight of 1? If no, please write this sentence more clearly.

-> We will move this sentence to the methodology section. It means that five donors were used for estimation of TNDs, which is an area between mean annual flow and below-mean flows in an FDC. The weights for estimating TNDs were used for regionalization of the FDCs too. Thus, the number of donor catchments for the TND interpolation is same as that for FDC regionalization. The sum of weights for TND interpolation is constrained as one when solving the ordinary kriging linear system, which is a part of top-kriging. Please find this information in Pugliese et al. (2014) or Skøien et al. (2006). We will explain this more clearly.

P9 L29: Why a threshold of NSE(FDC) of 0.8 is used? Is it necessary to reduce the number of catchments used in model calibration by the additional constraint of NSE(FDC)? Wouldn't it be better to keep as many catchments as possible?

-> Because we only considered catchments with high performance in parameter regionalization, we need to apply the screening for calibration with regional FDCs. We agree that it would be better to consider all catchments irrespective of predictive performance. We will use all catchments in revision.

P9 L31: Figure 5b – how do you explain the scatter in the low flow?

-> This is because the plot is in log-scale. Thus, residuals in low flow appear significant. This plot is similar to Figure 8 in Pugliese et al. (2014). In our opinion, important metric is the NSE in the figure. For clarification, we will provide Log NSE together.

P12 L10: Discussion: The manuscript would strongly benefit from a deeper and more extensive discussion of the results with other studies. Many statements appear in the discussion for which it is not clear where they are taken from - so please cite other studies properly (e.g. first sentence in discussion). There is no chapter in the discussion about the prediction of the hydrograph in ungauged catchments.

-> We will provide references accordingly. As replied, we will provide in-depth discussion about hydrograph prediction in gauged and ungauged catchments when restructuring the manuscript.

P14 L1: Summary and conclusions: I recommend to shorten this chapter. Point 1 (L9-13) is in my opinion no key finding of the study, point 2 (L14-17) is more an assumption than a result and point 5 (L27-30) is also rather a hypothesis than a result and should be formulated as possible further steps.

-> We agreed. In revision, we will summarize and results accordingly. And, the evidence of additional signatures will be provided as mentioned.

P15: Please add the information on data availability and author contribution.

-> We add the data availability and author contributions as requested.

P20 L1: If you want to show the parameter ranges used by Perrin et al. (2003), you should also argue why you use different ones. Since there are only 4 parameters the information could also be added in the text.

-> We will revise the part as requested.

P24: Figure 4 – please add labels to the FDC-plot. Why did you select catchment 15 which is within the 50

-> We will revise the plots accordingly. It was a just random selection. We will do this more meaningfully in revision.

P26: Figure 6 – The authors often mention that the calibration with the regionalized FDC results in hydrographs with poor timing. Such timing issues are not obvious in the plots of Fig. 6. Thus, I would recommend to show time periods or catchments where timing really is a challenge.

-> We will redraw all plots in revision.

P27-29: Figures 7, 8 and 9: These plots all look very similar to me and I recommend to condense or reduce the information of these plots. In my opinion it is not necessary to show the calibration values, I would rather focus on validation efficiency because that's the tougher criteria. I also think it's not ideal to compare the regionalization approaches in this way and I recommend to use the concept of benchmarks for comparison: e.g. make the difference between the benchmark strategy (RFDC_cal) and the PROX_reg strategy. The use of benchmarks results in one single value which can easily be interpreted: e.g. positive values mean that PROX_reg is better than RFDC_cal.

-> As replied earlier. We will provide actual calibration results with an FDC and additional flow signatures. As recommended, we will take differences for improving readability.

P29: Figure 9: I like the idea of evaluating further signatures and using them as additional constraints in model calibration. However, this plot does not give enough evidence for the conclusions drawn. To show that RLD and RFDC_cal are uncorrelated different methods are needed. I am also not convinced that the additional use of RLD would improve model calibration with the regionalized FDC more than RQP.

-> We will provide actual calibration results with an FDC and additional flow signatures at gauged catchments.

Minor comments: Please use the HESS guidelines for all abbreviations, so that all are done in a similar style as e.g. the abbreviation of the baseflow index. Please also write Figure 9 and not Fig. 9 when you refer to it at the beginning of a sentence.

-> We will globally recheck all abbreviations and expressions.

P1 L1: I would adapt the title: ". . .and model calibration with regionalized flow-duration

-> We will rethink about the title in revision. The proposed the title will also be considered.

P1 L12: Shouldn't it say "Leave-one-out cross validation"?

-> Some studies used the term "cross-evaluation". If LOOCV is more familiar to readers, we will change it.

P3 L1: Why do you consider the selected signatures as "major signatures"?

-> Our response to this question is provided above.

P3 L11: Please provide numbers for the percentage of precipitation falling as snow.

-> We will provide them as requested.

P5 L25: Where does this equation for calculating MAP* come from? Why do you need the constant?

-> It is also provided by Pugliese et al. (2014). MAP* is just a multiplication of the mean annual precipitation and the drainage area. Pugliese et al. (2014) was regarded MAP* as the mean annual flow for ungauged catchments. The constant is for a unit conversion from (mm yr-1 km2) to (m3 s-1).

P7 L14: I recommend to integrate this whole chapter in chapter 3.1, because it is about regionalization and not about evaluation.

-> We will restructure the manuscript, and consider this comment in revision.

P7 L19: This information is already in the introduction and is not needed in the methods part. Furthermore, you cite different studies here than in the introduction.

-> We will combine this part with the introduction.

P7 L25: I recommend to integrate this whole chapter in chapter 3.2, because it is about regionalization and not about evaluation.

-> We will restructure the manuscript, and consider this comment in revision.

P8 L2: I don't think that the regionalized FDC is used as objective function. It is rather used as reference value against which model simulations are evaluated.

-> We agreed. We will check expressions in the sentence.

P8 L25: Can you say what the CPI was for these catchments that were poorly modelled?

-> The CPI values will be provided in revision.

P10 L8: What about the efficiencies of catchment 13?

-> In revision, more comprehensive discussion will be provided. As replied, we need to recalibrate with a balanced criterion. Hydrograph simulations will be newly provided with improved discussions.

P10 L19: Please introduce the abbreviations such as PROX_reg earlier in the manuscript, because e.g. Fig. 5b already uses abbreviations.

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-> We will consider this comment in revision.

P19 L1: Table caption should be adapted because NSE(Q) and NSE(FDC) are not catchment properties.

-> We will revise the caption as well.

P21: Figure caption - it's the catchments that are labeled in the center and not the numbers. Also skip ". . .for GR4J model and FDC regionalization"

-> We will revise the caption.

P23: Figure 3a – I agree that it is important to know that the model is able to simulate runoff in most catchments. However I don't think that a boxplot is needed for that. The median and the range of the model performance in calibration and validation could also just be mentioned in the text.

-> We agreed. We will remove it and comment on it in the text.

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