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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Effectiveness of a regional model calibrated to different parts of a flow regime in regionalisation" *by* H. S. Kim

Anonymous Referee #1

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Review of ms no. hess-2015-196 "Effectiveness of a regional model calibrated to different parts of a flow regime in regionalization" by H. S. Kim

In this manuscript, the author proposes a refined calibration approach to reduce the parametric uncertainty of the IHACRES rainfall–runoff model. The approach follows by calibrating the model separately to different parts of flow regime in 11 Korean river basins. Following this the author establishes potential linkages between catchment characteristics and the calibrated parameters to form regional models, concluding that "the regional models from the refined calibration approach clearly enhanced the hydrological behavior . . .". While the presented work clearly falls within the scope of this journal, there are some serious concerns (mentioned below) and unfortunately, I can't recommend the publication of this manuscript in its current form.

Lack of novelty and advancement from the previous works: After reading the authors previous two papers published in the HP Journal (Kim and Lee; 2014a,b), I do not see any novelty in here presented current work. Both the seasonal calibration and regionalization of the IHACRES model parameters were already detailed in the previous study. Besides, there are many (significant) overlap between the authors previous and the current work - some figures are almost same. What is the point of presenting them again and again? One can just refer them to past publications. I, however, do not have any idea on how much overlap is allowed in the HESS journal considering that the work is already published previously in another journal (HP).

Throughout the manuscript the author makes the case of model inadequacy to capture the hydrological effects of non-stationary catchment response dynamics under different climate conditions so to propose a refined calibration approach in which the model separately to different parts of flow regime (specifically to wet and dry periods). In context of the presented work, I have a fundamental concern on the usages of words like non-stationary catchment response and climate conditions. What are different climatic conditions used in this study? The seasonal course of the wet and dry periods of flow regimes is the part of hydro-climatic conditions, governed mainly by the seasonal precipitation and other forcing variables patterns. Also it is not clear to me what does the author mean by the non-stationary catchment response? Is the seasonal pattern of discharge time series classified as non-stationary response? If such is the case then the daily discharge time series which are highly variable could also be regarded as non-stationary catchment response. I would ask the author carefully consider these words that meant to be used for climatic time scale behavior and not for seasonal or daily time scale responses.

Another major shortcoming of this paper is that the author does not consider evaluating the calibrated model parameters to the independent validation period. The whole available time series is used for the model calibration so how do we judge the model performance outside of the calibration period? It is a standard practice in hydrological

modeling to at-least retains a year of data outside of calibration period to evaluate the model performance.

The first line of the abstract stating the objective of this study as "to reduce the parameter uncertainty . . .", but there is no uncertainty analysis conducted in this study. I could not find any advancedMonte-Carlo or GLUE or Bayesian type of parameter sampling analysis in this manuscript. Besides there is also an equifinality issue in hydrologic modeling studies (i.e. there are many parameter sets that can provide acceptable results). How did you choose one among many optimal parameter sets for the regionalization?

P7059: L7: The author should enhance the literature review by considering more recent work on hydrologic model parameter regionalization studies, for example, by Yadav et al 2007 (doi:10.1016/j.advwatres.2007.01.005), Pokrel et al 2008 (doi:10.1029/2007WR006615), Oudin et al 2008 (doi:10.1029/2007WR006240.), Samaniego et al 2010 (doi: 10.1029/2008WR007327), Kumar et al 2013 (doi:10.1029/2012WR012195) as also discussed in detail in the recent book by Blöschl, G.: Rainfall-runoff modeling of ungauged catchments, in: Encyclopedia of Hydrological Sciences, edited by: Anderson, M. G., John Wiley & Sons, Chichester, 2061–2080, 200

P7061: L7: The author states "The objective of this study was to reduce the parameter uncertainty in regionalization studies .... without adding additional parameters or modification of the model structure" is not true. In your refined calibration approach you need to estimate two set of (same) model parameters – one for the wet period and one for the dry period – so essentially you have two times more the unknown parameters. Isn't the case?

P7062: L24: As stated if all basins show a high degree of similarity then what do we learn from having so many catchments whose response to precipitation is same? Despite this the author should consider increasing the sample size (i.e. the number of

study basins) to get more robust results from the regionalization analysis. Only eleven river basins is not enough – and it may be that you are catching more local behavior than regional ones as is the goal of regionalization studies.

P7066: L: 10: What is NIRE and how does this method work? Please provide detail on this?

P7070: L: 14: Why such a selective approach of verifying the regional models is used? What is unique about the selected two basins for verification, and why not to use the established Jacknife type of resampling approach for the verification purpose?

The presented Figure 3 is not discussed - either consider removing it or discuss it in the manuscript. Besides I find Figures 2 and 3 presenting as separate figure redundant (suggestion merge them together). Figure 7 is redundant considering that all information is given in Table 2. Likewise the information provided in Table 3 can be easily assimilated into Table 1, and then we do not need Table 3 as such.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 7057, 2015.