Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-639-RC2, 2020

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "A time-varying parameter estimation approach using split-sample calibration based on dynamic programming" *by* Xiaojing Zhang and Pan Liu

Anonymous Referee #2

Received and published: 30 August 2020

1. OVERALL RECOMMENDATION

The manuscript addresses the important topic of calibration of rainfall-runoff model parameters, and presents results obtained on two different catchments with two models. Even if the introduction includes relevant references and the methods are well presented, the paper lacks important discussions on the rainfall-runoff model performances, observed time series quality, attribution of observed/simulated changes, consideration of only two catchments, and several obtained results are over-interpreted. Finally, several figures and tables must be significantly improved. Therefore, I think the manuscript requires major revision before publication.

2. GENERAL COMMENTS

The tables 6 to 8 might be presented as figures to be more easily interpreted. Figure 5, 7 and 10 are very difficult to read, and must be significantly improved.

Line 28 to 29: several studies highlighted the difficulty of conceptual rainfall-runoff models in the context of climate change impact studies.

Line 35 to 36: the terms "constants" and "stable" must be defined: constant/stable in space and/or in time?

Line 43 to 44: in this context, it may be needed to define what is called "climate conditions".

Line 122: the terms "behavioural" must be clearly defined or not used in this context.

Line 137, line 150 and Table 1 and 2: please presents parameter units.

Section 2.2: the need to reduce the number of Xinanjian free parameters using a sensitivity analysis must be investigated more deeply in the paper. In the current version of the paper, this model is considered with different number of free parameters depending on the modeling experiments. Why not calibrating the 15 free parameters of this rainfall-runoff model for all experiments?

Section 2.3.1: one of the main hypotheses of this paper is the important "fluctuations" of the model parameter values over adjacent sub-periods, hypothesis that is not justified by the literature review, and that is not illustrated with the obtained results. This point must be discussed more deeply in the paper.

Evaluation criteria: Why only use the NSE criterion as only evaluation criteria, and no other criteria, such as KGE and its components? NSE appears to be non-discriminating between considered calibration methods. Using other calibration criteria - looking at different time step and/or different error characteristics such as bias on the highest streamflow values – might be interesting in this context.

Section 3.2 (Wuding river basin), lines 364 to 369: the changes of the studied catchment characteristics seem to be decisive for the interpretation of the results obtained on this watershed. Nevertheless, no quantitative results / analysis of these changes are given in the paper: what is the percentage of the catchment that has been afforested? What are the number and the capacity of the built reservoirs? When are they built? Finally, an important point not discussed in the paper is the stationary and the quality of the precipitation and streamflow time series studied and used for the model calibration. This point is crucial in this context and need to be discussed.

Section 3.3 (Xun River basin): same remarks as the Wuding river basin: what about potential changes on this basin? Are precipitation and streamflow time series of good quality?

Section 4.1: the seasonal signal of the parameter values (cf. figures 6 and 8) must be more significantly discussed in the paper.

Line 456 to 458: this conclusion must be significantly moderated: the “SSC-DP” calibration method is by definition better to select more continuous parameter values.

Section 4.2: this data analysis is crucial in this context. It might be relevant to present it in the data section. Moreover, this analysis must be significantly improved: what about potential errors (random or systematic) in the observed precipitation and streamflow series? What about potential break in the streamflow series due to rating curve changes? What is the statistical significance of this analysis? The analysis of only one catchment requires to look carefully the studied time series in the context of attribution of changes. The relative bad performance of the rainfall-runoff model on this catchment (NSE=0.41) must be discussed. In particular, the systematic streamflow underestimation for the different calibration methods must be discussed.

Line 492 to 495: the “unreasonable model states” between sub-periods might be illustrated in the paper.

Line 500 to 501: this conclusion must be moderated, since results have been obtained on one catchment only.

Line 511 to 514: this attribution analysis must be moderated (see previous remarks on attribution analysis).

Line 520 to 522: again, what about potential error in the rating curve in this context?

Line 526 to 539: why not presenting a Figure such as Figure 10 to illustrate rainfall-runoff simulations on this catchment?

Line 531 to 532: is this out-performance significant?

Line 534 to 539 and line 637 to 648: again, these attribution conclusions must be moderated, because they are drawn from only two basins, without any investigation of potential systematic errors in the observed time series.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-639, 2019.