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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Diagnostic calibration of a hydrological model in an alpine area" *by* Z. He et al.

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This is a nice study breaking (partitioning) the hydrograph into several parts, each of them representing one (some) physical process(es) creating discharge. They iteratively calibrated one parameter at a time using the partition of hydrograph that is assumed to be represented by the parameter. I have some concerns/comments that I would like to share with authors:

1. I would see this study as a step-wise calibration rather than diagnostic calibration. In diagnostic calibration (diagnostic model evaluation, I would prefer to use), as introduced by Gupta et al. (2008) signatures of the system (data) are used instead of an ad hoc residual based likelihood (model evaluation) function. In this study a NSE measure

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was used for step-wise calibration of each parameter which doesn't correspond to the original term of "diagnostic model evaluation". Also when the term diagnostic is used, reader would expect to see it points out some kind of model/data error, while this study doesn't pin point which part of the model needs correction/modification.

2. Introduction doesn't connect to the body of paper. In the introduction section authors present a literature review of diagnostic model evaluation studies using several indices (signatures) of the watersheds and in the current study they just use NSE!

3. Recently a formal statistical framework for diagnostic model evaluation is introduced in the literature. Authors can include the following papers (amongst all) to give readers a better overview of diagnostic model evaluation literature: Olden, J. D. and Poff, N. L. (2003), Redundancy and the choice of hydrologic indices for characterizing streamflow regimes. River Res. Applic., 19: 101–121. doi: 10.1002/rra.700 Vrugt, J. A., and M. Sadegh (2013), Toward diagnostic model calibration and evaluation: Approximate Bayesian computation, Water Resour. Res., 49, 4335–4345, doi:10.1002/wrcr.20354. Sadegh, M. and Vrugt, J. A.: Bridging the gap between GLUE and formal statistical approaches: approximate Bayesian computation, Hydrol. Earth Syst. Sci., 17, 4831-4850, doi:10.5194/hess-17-4831-2013, 2013. Several step-wise CRR model calibration papers also exist in the literature than should be referred to in the paper.

4. Fig. 2: In some months like April and June, temperature estimated from equation 1 (based on temperature lapse rate) nicely follow the fluctuations of observed temperature while in others like February and November it fails to simulate the temperature dynamics. How do you explain this phenomenon?

5. What explains the significant temperature laps rate difference in different months (-0.36 to -0.86)? In the most basic form, this lapse rate is a constant number for the whole year.

6. Your objective function for estimating the lapse rate needs an "abs" function (absolute value), otherwise negative and positive residuals will cancel out. This might explain

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why we don't see a good fit to measured temperature in some months?

7. Suggestion: Fig 5a and 5b can be presented as subplots in one plot.

8. Months names in Fig 5a legend are not in order! Is it just a typo?

9. I expected to see all the curves in Fig 5a-b continue to a common elevation (∼5000), although might be horizontal at the end. Your study area does not change with month, just the melt area changes which can be represented by a horizontal line at higher elevations.

10. You should evaluate your model as a complete package for the evaluation period and don't partition the hydrograph into several constituents. Eventually, your model parts should work as a whole. Also in principle you don't know what type of process generates your surface runoff in the evaluation period, so it doesn't make sense to partition your hydrograph.

11. Page 1273, line 18-21: It is mentioned that results of this study is comparable to an automatic calibration method. If so, why do we need to partition the hydrograph? And what has been diagnosed in this study?

12. Table 5: for the evaluation period, we see a better performance for automatic calibration rather than step-wise calibration! How do you explain this? And why would a researcher leave automatic calibration for step-wise calibration?

13. Page 1274, lines 1&2: "number of criteria handled by an automatic calibration procedure should be lower than 5 : 1"! Number of evaluation criteria is not important, the amount of information that they extract from data is more important.

14. Page 1274, lines 9-12: It is mentioned that automatic calibration methods are sensitive to the calibration data period while step-wise calibration is not. Different calibration periods provide different events and might affect step-wise calibration in the same way it affects automatic calibration. Actually it does affect step-wise calibration as well. In the cross validation step (same page lines 19-21) it is shown that the value

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of parameter B changes from 0.2 to 0.8 due to different calibration events.

15. Page 1275, line 11-12: “the low performance of the model for extreme summer storm events indicated the inadequacy of rainfall measurement”. Cross validation shows that storm-runoff parameter (B) which controls the highflow to a high extent varies if the calibration period changes (0.2 to 0.8), so you can’t simply attribute the poor model performance to the lack of rainfall measurement for the extreme summer events!

16. Some mistakes in writing should be taken care of before publication including but not limited to: a. Page 1262, line 12: a similar procedure for temperature. . . → a similar procedure as temperature. . . b. Page 1262, line 26: downloaded from the website. . . → downloaded from the NASA website c. Page 1263, line 10: was combined → were combined d. Page 1265, line 8: annual mean → inter-annual mean e. Page 1266, line 12: indexes → indices f. Page 1271, line 15: years have → years with g. Page 1274, line 9: calibration data → calibration period

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