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Hydrology and Earth System Sciences Discussions

Interactive comment on "Calibration of hydrological models using flow-duration curves" *by* I. K. Westerberg et al.

Anonymous Referee #2

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This paper presents an approach to the calibration of hydrological models using uncertain flow duration curves rather than a classical hydrograph. This offers opportunities in the case where data sets are non overlapping and stationarity may be assumed. This is shown in 2 case studies using 2 different models. The investigation is thorough in the sense that shifted time periods are used to evaluate FDC stability,

The paper in itself demonstrates the applicability of the method in 2 case study areas using 2 different models. The paper can be a valuable contribution, however I have some quite serious comments on the paper as well. These should be handled before considering publication in HESS:

1. The abstract refers to the method as 'new', however the method is not fully new to me, which is also outlined by dr. Sivapalan. In fact, as mentioned in the introduction, a number of authors have addressed the problem of non-overlapping time series and have exploited other information than revealed by overlapping least squares comparisons. In particular the use of 'signatures' should be noted in this respect (e.g. Vogel and Sankarasubramanian, 2003; and Yilmaz et al., 2008 presenting an overview of such signatures). The paper therefore presents an application of such earlier defined approaches, rather than a new approach. The authors should at least more clearly state which particular components of their application is new.

2. Furthermore, the choice of only using flow duration curves rejects potentially useful other information. In particular information about auto-correlation is neglected which constrains parameters related to time scales and routing.

3. The use of the uncertainty in information in flow series (be it FDC or other signatures) is also not entirely new. In fact, Winsemius et al. (2009) present such a calibration approach in a more generic way. A reference to this paper should be made.

I also have some detailed comments. These are given below:

p. 9474 l. 16-19. I do not quite understand why inverse-distance was chosen. If spatial correlation of rainfall is low (proven by low correlation between time series of nearby stations), is it not more sensible to maintain as much variability in the time series? This would plead for a more conservative interpolation approach such as nearest-neighbour.

p. 9478 l. 14-16. By summarizing all information into FDCs, the temporal auto-correlation of the hydrograph is lost. I would at least like to see this issue and its impact on parameter identifiability discussed in the last section. Again note that other authors have considered the use of auto-correlation (e.g. Montanari and Toth, 2007;

Winsemius et al., 2009)

p. 9478, l. 16-18. The triangular evaluation function: why was this function selected? Given the uncertainty, is it not more plausible to simply accept all sets within the evaluation points as equally likely?

p. 9479. – p. 9480. Eventually, I understood the selection of EP methods, but it would help to describe these in an equation. The second method: it seems to me that you can expect an unreasonably high density of EP points in the low regions of the flow regime. It seems to me that it is more objective to determine the EPs (and thus the weight of the evaluation on different parts of the flow regime) by the amount of samples rather than the amount of volume. Can you discuss this?

p. 9479, l. 22-23. I recommend removing references to a commercial package, unless it provides unique functionality.

p. 9489., l. 21. The approach cannot be presented as being fully new.

p. 9489., l. 4. How are input errors accounted for in this study? The uncertainty of precipitation has not been accounted for.

References

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