

Interactive comment on “Assessment of the indirect calibration of a rainfall-runoff model for ungauged catchments in Flanders” by N. De Vleeschouwer and V. R. N. Pauwels

B. Schaefli (Referee)

bettina.schaefli@epfl.ch

Received and published: 14 February 2013

This paper analyzes the performance of different model calibration strategies for a range of Belgian catchments. The studied calibration strategies address situations of what they call the spatial or temporal gauging divergence, i.e. situations where there are no discharge observations for the simulation period and location at hand. Such situations can be addressed by indirect calibration where data is obtained either from a different but similar location or from a different time period (in this case, spectral calibration is required). As far as I know, spectral calibration has not been proposed before for regionalization, i.e. for indirect calibration on neighboring and scaled discharge.

C79

The spectral calibration approach proposed in the paper can be considered as a so-called "informal" calibration procedure": the performance measure used for calibration is not based on some formal assumptions about the modelling errors (e.g. McMillan and Clark, 2009). This should be made clear to my view. In fact, the direct calibration in the time domain used the root-mean-square-error, which implicitly assumes a normal error distribution (Kavetski et al., 2006). What is the implicit assumption about the error in the proposed spectral calibration criteria? It should be at least mentioned that the two calibration criteria are not equivalent from this perspective. This should include a discussion of possible effects of the heuristic choice of the spectral criteria and anticipated impacts on the calibration results (the time domain calibration criteria is designed to reduce bias and Nash, what about the spectral domain criteria?). With this respect I also recommend re-thinking the "overfitting" hypothesis advanced in the paper and conclusion.

The paper is well written and organized, it nicely discusses some limitations of the proposed approach (p. 112, line 10, p. 119, line 10) and the topic is highly relevant to HESS. I thus recommend publication in HESS but I have a number of suggestions to improve the manuscript.

Comment on terminology

I really like the new term of "spatial or temporal gauging divergence" even if this term is already in use in genetics and even if it is not necessarily self-explanatory. For the definition of these terms, I recommend, however, the more widely used term of "concomitant observations" rather than "contemporaneous". Furthermore, I recommend attributing the idea of indirect calibration to Montanari and Toth (2007) or to adequate references therein. At the moment it looks like this was an ancient concept known to all hydrologists. To my view, this term is only used so far for spectral calibration in case of temporal divergence. For spatial divergence, the term regionalization is used in the literature.

C80

I also recommend making a consistent use of the "spectral terminology". I recommend using consistently "power density spectrum" and "estimates of the power density spectrum (PDS)" instead of terms as spectral density or spectral density estimates (which could refer to a probability density function).

Furthermore, it should be made clearer in the paper that all computed "power density spectra" are necessarily estimates of the true property of the natural process. At the moment it reads as if the approximation comes only from the fact that a certain number of lags are retained (p.e 108, line 1).

Detailed comments

- *introduction*: RR-models = ?

- the *literature review* should refer to some key publications in the field of model calibration on regionalized discharge signatures or similar regionalization approaches (e.g. Bardossy, 2007) (Viglione et al., 2013); there should also be a reference for direct calibration on scaled discharge from neighboring catchments; if this has not been done before, this should be mentioned

- *methods*: the fact that the PDS at frequency zero is a measure of the mean of the process at hand should be introduced in the theory section, this is not obvious to many readers (especially also because in time series analysis the mean is often subtracted before completing this kind of analysis); this is important to understand the separation into $k=0$ and $k>0$ in figs. 3 and 4. I would also recommend to explicitly discuss the difference between your approach and the Whittle likelihood introduced to hydrology by Montanari and Toth (which does not include the zero frequency and does not estimate the mean). With this respect, I would also explicitly discuss the relation between equation 3 and the more commonly known autocorrelation function. An earlier explicit discussion of the above would also contribute to understand the sentence (p. 110) "The drainage area is an important indicator of the discharge magnitude and thus the spectral density magnitude."

C81

- p. 113: line 7: assumption of stationarity rather than of periodicity

- p. 114, line 9: it is not clear here why repeated calibration experiments (optimisation algorithm not presented so far)

- p. 116: line 10: are the evaluation criteria influenced by the period length? the Nash value certainly is

- overall, the *Nash values seem extremely low*; this is certainly due to the hourly time step but the reader cannot judge what it means from a hydrograph perspective; I recommend plotting at least one simulation and observed time series; perhaps also mention what the Nash values would be if aggregated to daily time step

- p. 117: are you sure that overfitting is the reason (see also conclusion)? you measure the bias of something which has not been calibrated such as to reduce the bias (contrary to the time domain)

- p. 112, line 5: NDIB should read NDIR.

- p. 121: "rather low" instead of rather lower

- *conclusion*: a bit a strange end, what is a "certain discharge signature"?

- figure 7: I would recommend presenting all criteria on the same ordinal scale, i.e. use 1-R and 1-NS to have only criteria to be minimized; otherwise very difficult to read

- mathematical notations: E for "expected value" not in italic; eq. 11: S_{don} is also an estimated spectrum

References

Bardossy, A.: Calibration of hydrological model parameters for ungauged catchments, *Hydrology and Earth System Sciences*, 11, 703–710, 10.5194/hess-11-703-2007, 2007.

Kavetski, D., Kuczera, G., and Franks, S. W.: Bayesian analysis of input uncer-

C82

tainty in hydrological modeling: 1. Theory, *Water Resources Research*, 42, W03407, 10.1029/2005WR004368, 2006.

McMillan, H., and Clark, M.: Rainfall-runoff model calibration using informal likelihood measures within a Markov chain Monte Carlo sampling scheme, *Water Resources Research*, 45, n/a-n/a, 10.1029/2008wr007288, 2009.

Montanari, A., and Toth, E.: Calibration of hydrological models in the spectral domain: an opportunity for ungauged basins?, *Water Resources Research*, 43, W05434, 10.1029/2006WR005184, 2007.

Viglione, A., Parajka, J., Rogger, M., Salinas, J. L., Laaha, G., Sivapalan, M., and Blöschl, G.: Comparative assessment of predictions in ungauged basins ndash; Part 3: Runoff signatures in Austria, *Hydrol. Earth Syst. Sci. Discuss.*, 10, 449-485, 10.5194/hessd-10-449-2013, 2013.

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 10, 103, 2013.