

Interactive comment on “A strategy to overcome adverse effects of autoregressive updating of streamflow predictions” by M. Li et al.

B. Schaefli (Referee)

bettina.schaefli@epfl.ch

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This manuscript proposes a new method to correct forecasted streamflows based on the forecast error of the previous time step. The method represents a modification of the commonly applied autoregressive correction. The paper is well written, the method and the results concisely presented and discussed. However, the presented results did not convince me that the new method really outperforms the reference method; this might easily be improved by showing more details of the performed tests.

My suggestion for moderate revisions of this paper are:

General comment on used terms

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I would carefully revise the used wording to clearly distinguish between “forecast” (prediction of the system state at a given moment in time) from the more general “prediction”. At the moment, the two terms are used interchangeably, which might sometimes be misleading, especially because the discussed streamflow correction only applies to forecasting.

Intro

As far as I see, the Kavetski et al. 2003 reference does not discuss forecasting and thus also not updating procedures but parameter estimation. Please give here references for papers that actually use streamflow correction / updating in a forecast setting.

In the general discussion of streamflow prediction errors, you might want to add the recent reference by Pianosi, F., and Raso, L.: Dynamic modeling of predictive uncertainty by regression on absolute errors, Water Resources Research, 48, W03516, 10.1029/2011wr010603, 2012.

Method, section 2 and 5

- Eq. 2 as well as following eqs. does not show the involved parameters
- Eq. 4: which part of the equation does result in the “median value”? should be corrected;
- Reference for max. likelihood formulations in eq. 6 , 7?
- In general, I think the superscript notation is not nice to read, why not use two different variable names and subscripts for the parameters?
- I am not convinced by the current structure with section 5 presenting the new approach; instead of having an “idea-flow” paper structure (method - result 1 - new method - result 2), I would introduce the new method in section 3.
- Eq. 8: the same variable name is used for something new, to avoid, what is QM?

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- P. 6045 last line: word missing
- P. 6046 first paragraph: would be useful before eq. 8
- Likelihood formulation of the new approach?

Case study

- The GR4J model: do any specificities of the model influence the obtained results? (to be mentioned in results section?)
- P. 6041, line 21: “we then predict streamflow”: not clear here whether in prediction or in forecast mode
- The use of “simulation” and “prediction” is confusing; I recommend using the term “forecast” for simulations with forecasted rainfall and the term “simulation” in the other case

Results

- I suggest a new results section presenting all the results
- In any case, the results of the new method require a separate section (now part of section 5 presenting also the method)
- P. 6047, line 13: “notable better performance”: as far as I see, only 2 out of 4 cases show a slightly better performance; from fig. 7, the improvement of RAR over AR-raw is not evident
- The NSE results are aggregated, how do they look like for individual cross-validation experiments? Are the NSE samples really significantly better with RAR (different distributions with higher mean) or is this pure chance?

Discussion

I am not convinced that the paper shows that the new method leads to a more robust performance of the base hydrological model. This should be shown in a more

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convincing way by presenting some more detailed results of all the simulations.

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