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Discussions

Interactive comment on "A framework for likelihood functions of deterministic hydrological models" *by* Lorenz Ammann et al.

Anonymous Referee #2

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This is an interesting well-written paper that revisits some open problems with the statistical characterization of hydrological model residuals (differences between observed and simulated values) in the context of conceptual rainfall-runoff modeling. Specifically, it addresses the issue of accounting for autocorrelation of model residuals, which is known to be troublesome in e.g. semi-arid basins where performance of spatially lumped models often is sub-optimal. The paper shows that similar problems occur in humid basins when the temporal resolution increases from daily to hourly. A novel approach that uses different autocorrelation coefficients for dry and wet periods is shown to yield better probabilistic streamflow predictions compared to the common practice of using a constant autocorrelation coefficient.

Comments:

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Discussion paper

1. Title and contribution: the title is quite broadly formulated and doesn't really bring out the main novel contribution of the paper, i.e. improved autocorrelation modeling at sub-daily resolutions. In my opinion the proposed likelihood function framework is secondary to this: although it is different from previous approaches, its performance for constant autocorrelation is similar to previous approaches (at least qualitatively - a numerical comparison is not done in the paper), and the novel use of a variable autocorrelation coefficient could also readily be implemented with previous approaches. So it's not entirely clear what we gain from the new framework, even though I do find it quite elegant. If the main selling point is the new likelihood framework then more extensive comparisons (both theoretical and empirical) with existing approaches would be helpful. The proposed framework also has some (conceptual) issues, as discussed in the next point.

2. Section 2.1: the statistical model and corresponding likelihood is based on specifying the density of observed discharge Q conditioned on simulated discharge, Eq. 1. To avoid negative Q values, the density is truncated at zero by removing all probability mass for Q<0 and placing it at Q=0. This deviates from the usual truncation approach, which would scale the entire density by 1/(1-FQ(0)). In fact, the proposed approach results in strange bimodal looking densities with a peak at Q=0 and another at some Q>0; somehow I don't think this is an intuitive model that hydrologists would come up with based on prior knowledge (as suggested on page 5, line 11)! Another consequence of the chosen truncation is that the transformed variables eta in Eq. 2 are also truncated and not Gaussian. This is partially acknowledged on page 5 line 28, but I don't think it's correct that the lower tail of eta will be lighter: there simply will be no lower tail (truncation). Note that these issues could be remedied by adopting the usual truncation approach (scale the entire density) or by using a density with nonnegative support. It's not clear whether these truncation issues matter in practice, perhaps not for the humid basins studied here, but it may matter in drier basins with discharge close to zero.

3. Section 2.3, evaluation criteria: the reliability and precision metrics are counter-intuitive in that smaller values for these metrics indicate better performance. Unreliability and imprecision metrics? Another natural metric to consider is the maximum log likelihood value of each model (perhaps corrected with number of parameters, as in BIC).

4. Section 3.3, error models: the method of Fernandez and Steel (1998) to skew a symmetric density was also used by Schoups and Vrugt (2010), in their case to skew an exponential-power density. It may be appropriate to cite that paper here, especially if that's where you learned about the Fernandez and Steel method.

5. Table 2, page 14, line 1: the E1 model also truncates fQ at zero, which is another difference with maximizing NSE.

6. Figure 6: in the top-right plot for model E3, it's not clear that distributional assumptions for eta are satisfied; there are significant outliers in this plot, and the variance is not constant.

7. Conclusions: finding 5 (accounting for autocorrelation is good) seems to contradict finding 1 (accounting for autocorrelation can be bad); you may need to clarify/reformulate these a bit.

8. Conclusions: finding 3 states that errors in streamflow are expected to be less correlated during precipitation events than during dry weather. Is that always the case though? What about rainfall errors, these could lead to significant bias and correlated errors in simulated streamflow. Also, structural errors in the fast flow component of the model may be (much) larger than in the slow flow component. Perhaps a better, more general, justification for a nonstationary correlation model is to say that the error correlation structure can be expected to differ between wet and dry periods (for various reasons), and then let the data decide whether wet or dry has the larger autocorrelation coefficient.

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Edits:

- page 4, line 5, "Understanding...remains poorly understood": remove "understanding".

- Eq. 13, Nash-Sutcliffe formula: change Q to Qobs in the denominator

- page 21, line 12: "normality" has a typo

- figure 9, caption: left/right should be top/bottom

- page 27, line 28: likeli -> likely

- page 28, line 17: "appropriate" has a typo

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