Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2017-9-RC1, 2017

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Bayesian joint inference of hydrological and generalized error models with the enforcement of Total Laws" *by* Mario R. Hernández-López and Félix Francés

Anonymous Referee #1

Received and published: 8 February 2017

The manuscript investigates residual error models for the calibration of conceptual hydrological models. The authors rightfully point out that the previous literature has identified failures in the joint calibration of hydrological and error model parameters under particular conditions, eg, for the GL approach (Schoups and Vrugt 2010) and for the WLS-AR1 approach (Evin et al 2013, 2014).

I generally found the study interesting as it explored an important aspect of hydrological model calibration using statistical techniques. The implications of the Total Variance Laws on uncertainty estimation in hydrological models is certainly worthy of research attention and this manuscript does provide some insights towards that aim. There are other interesting results, eg, some of the analyses around Fig 15 are instructive and

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visually well presented.

Unfortunately, there appear to be several major flaws in the study:

1. METHOD SELECTION

The aim of the study is to address the failure - via unstable/explosive prediction limits - of joint inference approaches. However, this failure has been reported only specifically when inferring the autocorrelation parameter rho - the manuscript notes this on lines 39-40 citing Evin et al 2014, but maybe overlooks that Evin 2014 show that if rho is fixed, there is no instability.

The manuscript then uses the SLS and WLS approaches as a major part of the analysis - even though neither of these methods have an autocorrelation parameter, let alone infer it! So WLS (and SLS), even if they are joint inference methods, do NOT suffer from the instability the study is trying to resolve, and this has already been known from the cited previous studies.

To demonstrate how the TVL approach removes the instability shown by Evin et al 2014, the WLS-AR1 approach from Evin et al should be clearly included in the analysis, which is the error model where the instability actually occurs.

2. CASE STUDY CATCHMENT

The catchment used in the manuscript to demonstrate its contribution is the French Broad River from the MOPEX database. This is a very strange choice of catchment for the given research objectives, because it is one of relatively few catchments where pretty much every residual error model has performed well, including the original GL approach of Schoups and Vrugt 2010, and the joint WLS-AR1 scheme of Evin et al 2013, 2014, as can be seen from those papers.

In this respect it should be clear that using such a case study catchment cannot provide supporting evidence of the conclusions.

If the authors wish to demonstrate they have "solved" the problems with the above error models, I think it should be obvious that the case study should *at least* use a catchment where the old models fail in the sense of producing clearly explosive prediction limits (the problem the study is trying to solve) and the new model shows significant improvement (the outcome the study is trying to achieve).

3. GENERAL SUPPORT FOR CONCLUSIONS

I struggle to see empirical evidence in support of the conclusions, which begin with "This paper has addressed the challenging problem of jointly estimate hydrological and error model parameters in a Bayesian framework, trying to solve some of the problems found in previous related researches, as in the second case study of Schoups and Vrugt (2010) as well as in Evin et al. (2014), among others".

As already mentioned above, neither the method nor catchment selection can support such conclusion.

But even if we consider Figure 15 which compares the reliability and resolution of the error models. The WLS error model that the manuscript claims to improve on is clearly amongst the best of the error models under consideration. It clearly has other deficiencies, such as lack of treatment of AR1, but this has already been remedied in the literature by including an AR1 term (Evin 2014).

So I struggle to see how the conclusions can refer to having addressed problems with this error model. There may be improvements related to the treatment of non-Gaussian errors by including skew and kurtosis in the GL error model, but this has already been shown by Schoups and Vrugt in 2010.

4. APPARENT TECHNICAL ERRORS

The discussion on pages 644-657 acknowledges the good performance of WLS to some extent, which helps. However this discussion does not appear in any way anchored to the previous theoretical presentation. To raise these points in the discussion,

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there has to be a corresponding background presentation of what is a "bad-posed" problem, how is it problematic and how to detect it. The way the manuscript reads at the moment, the study reached unexpected conclusions (which happens) but instead of re-thinking some of its key premises, it tries to "patch" it in the report using concepts that just haven't been properly introduced at that stage of the presentation.

Unsurprisingly, erroneous, or least confusing, statements appear to be introduced in this "patch". For example, Line 654 states that in the WLS error model, the same hydrological parameter "estimation" is inferred, as well as similar uncertainty bands obtained, using any multiple of (a,k)_ML,TL. Here (a,k) are parameters of the standard deviation of residual errors, sigma=a+k*Q and Q is the streamflow. I really struggle to see how this makes any sense - if we multiply (a,k) by some value c as suggested, sigma will increase by the same factor and the uncertainty bands will inflate accordingly. And the likelihood value will certainly be different. Perhaps this paragraph is missing some major extra detail, or maybe it is a wording issue, or maybe there is some other error/omission in the analysis or calculation, but as it stands it makes no sense. The text states "it can be demonstrated" - please do include the (mathematical) demonstration as it will clarify what you are trying to show.

5. THE PRESENTATION IS TOO DISORGANIZED

There is a reason why technical reports have a generally agreed standard set of sections, such as Intro, Theory, Methodology, Results, Discussion, etc. This allows the reader to easily find any details they are interested in. At the moment the specifics of the case study are scattered all across the paper and its very hard to ascertain exactly what experiments were undertaken and why. Please consolidate the presentation and explanation of the methodology in a single section, as this will avoid (likely) confusion on the part of reviewers and readers.

Articulating a more precise set of objectives early on, and justifying how the paper will prove these objectives have been achieved, would also help the reader navigate the paper.

In conclusion, even as I consider the theoretical investigations presented in the manuscript to show promise towards resolving the study objectives, the methodology is clearly inappropriate to demonstrate what the manuscript hopes to achieve. There are other important issues than need to be clarified and improved prior to resubmission.

For this reason I cannot recommend publication of this manuscript in its current form. I encourage the authors to apply their analysis in a thoughtfully designed and presented case study - if convincingly demonstrated, their ideas would provide a worthwhile contribution to the hydrological community.

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