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Interactive Comment

Interactive comment on "Inverse modelling of in situ soil water dynamics: accounting for heteroscedastic, autocorrelated, and non-Gaussian distributed residuals" *by* B. Scharnagl et al.

Anonymous Referee #1

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This paper presents and applies an inverse methodology to estimate soil hydraulic parameters from a time-series of soil moisture content measurements and a Richards-based soil water flow model. The main contribution is focus on a correct probabilistic description of the model residuals, namely accounting for their temporal correlation and their heteroscedastic and non-Gaussian nature. I enjoyed reading this paper, it is well written, and can make a meaningful contribution to inverse modeling in soil hydrology by promoting a probabilistic approach with due attention to the underlying assumptions.

1. My main comment relates to the modified first-order autoregressive or AR(1) model that the authors propose here (Eq. 20). This model relates the standardized residual at time i to the standardized residuals at all other times (via the third term on the right in Eq. 20). I don't think you can still call this an AR(1) model, since you lost the first-order Markov property by introducing that third term: there are no conditional independencies in this model. The likelihood becomes a product of factors, each depending on all residuals, as opposed to just the current and previous residual as for Likelihood 2 in Eq. 19. I suspect that the likelihood is now an unnormalized function of the data (not a normalized distribution). Also why does the expression in Eq. 14 still apply in this new model? It would be good to provide a clarification and discussion of these issues.

2. The reason for introducing the new AR(1) model (Likelihood 3) is that the original AR(1) model (Likelihood 2) leads to biased estimates, which the authors claim is revealed by Eq. 13. However, Eq. 13 shows that when the expected value of the standardized residual at time i is zero (as it is by definition), so will be the expected value of the decorrelated residual at time i. In other words, I fail to see the bias in Eq. 13.

3. The bad results with Likelihood 2 appear to stem from the very high value for correlation coefficient phi obtained with Likelihood 2 (phi is almost 1). The AR(1) model indeed becomes unstable in that case (magnitude of residuals in Eq. 14 becomes infinitely large). However, have you tried limiting phi to lower values? For example, you could try to fix phi = 0.9, or some other value far enough from 1. Wouldn't you then remove most of the correlation and still have a stable result?

4. Schoups and Vrugt (WRR, 2010) also presented a model that accounts for autocorrelation, heteroscedasticity, and non-normality of residuals. They developed it in the context of rainfall-runoff modeling, but in principle their approach could also be applied to soil hydrology. It would be helpful to briefly point out how your approach builds on and differs from theirs.

5. Section 2.5: some of the parameters were transformed (log-transformation). In that

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case you would in principle need to include additional terms in the prior (related to the Jacobian of the transformation), however this is not documented here.

6. The authors report problems with convergence and acceptance rates of the MCMC algorithm when using the AR(1) model. I wonder if these problems are also related to the fact that phi -> 1 in this application. For example, do you still encounter these problems when setting phi = 0.9 in Likelihood 2?

7. The quantile-quantile plot in Figure 6 shows systematic deviations from the straight line at the lower quantiles. It would be good to discuss the meaning of this in the text.

8. Looking at Fig. 7, I wonder if your parameter prior is actually too strong, as the posterior tends to sit at the edges of the prior. Have you tried relaxing the prior to see if it resolves some of the systematic deviations between your model and the observations?

9. I suggest also making plots of the posteriors of the likelihood parameters (phi, sigma, etc).

10. The notation in Eq. 15, 16 and 19 is confusing: you should really be conditioning these distributions on the old observation y_{i-1}.

11. Do you have any independent measurements of the soil hydraulic properties at the site to check your inferred results in Fig.8 against?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 2155, 2015.