Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-675-RC2, 2017

Hydrology and Earth System Sciences Discussions

HESSD

Interactive comment

Interactive comment on "Probabilistic inference of ecohydrological parameters using observations from point to satellite scales" *by* Maoya Bassiouni et al.

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Received and published: 20 December 2017

The authors use soil moisture observations in a Bayesian inversion procedure to estimate vegetation-related drivers of soil moisture dynamics in the root zone, as modeled by a simple model of soil moisture distribution. The authors apply the approach to a diverse sample of study regions where soil moisture and climate observations are available at different scales. The presented research is important and innovative in that it investigates the potential for recent remote sensing approaches that monitor spatially aggregated soil moisture to estimate eco-hydrologic parameters that are very challenging to observe in-situ, even in well instrumented basins. The research also

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bridges the gap between different observation scales, which has potentially interesting implications in poorly gauged regions. While I recommend the paper for publication in HESS, I would also like to raise a few comments/questions that could possibly help the authors during the revision of their paper.

Major comments 1. The authors appear to use the same sample of soil moisture observations to calibrate (via Bayesian Inversion) and validate (KS tests and Fig 3) the approach, which instictively raised red flags on a first read. After reflecting, it became clear (well, to me at least) that the purpose of the exercise was to show that Bayesian inversion can be use to estimate vegetation-related drivers of soil moisture using soil moisture time series, conditionnal on the assumed pdf model being an accurate description of soil moisture dynamics. In that case, the research design would be appropriate because the posterior CV portrays estimation uncertainties and the goodness-of-fit shows that the soil moisture model is, indeed, appropriate. Consequently, the purpose of the goodness-of-fit test appears to be to evaluate the functional form of the pdf, not the estimated parameter values, so it is fine to use the same dataset to calibrate parameters and evaluate outcomes. Please clarify the distinct function of these two metrics as appropriate.

2. I am having issues with the way you use KS tests to evaluate pdf fits. First off, if I am not mistaken, the null hypothesis of a ks test is that the two tested distributions are identical. If so, the p-value could be interpreted as the probability of obtaining a ks-distance at least as large as the one that would be obtained if the two samples were taken from the same distribution. This is loosely equivalent to the probability of falsely rejecting the null. In other words, a p-value of 5% would mean that one has a 95% chance of being right when stating that the two distributions are different, which is quite a low standard when assessing goodness of fit. Significance levels don't tell anything about type II errors, which is what I would think we are ultimately after when evaluating goodness of fits. More importantly, the KS satistic does not follow the kolmogorov distribution (i.e. estimated p-values are wrong) if the same sample of data is used to

calibrate the cdf model and construct the empirical cdf to which it is compared. In my opinion, however, a formal test is not necessary to make your point here (see point 1). The graphs in Fig 3 are sufficient to make the point that the laio model reproduces the shape of the observed empirical histogram. You can then use a distance measure to monitor fits in the sensitivity analysis. The KS-distance is probably not the most appropriate measure for that though, as it only considers the largest distance between the cdfs âĂŤ global distance metrics like the Cramer Van Mises statistic or quantile-level nash sutcliffe efficiency, Muller 2016), or information based criteria (e.g, AIC, Ceola 2010) are useful alternatives to consider.

3. Your sensitivity analysis on soil depth (Section 4.2.) convinces me that the value assumed for Z in eqn 2 has little effect on the modeled soil moisture dynamics. This is of course important, but without actually measuring whole column soil moisture, I fail to see how you test the homogeneity assumption (i.e. that near surface soil moisture observations can be used to estimate whole-column characteristics). Please elaborate.

4. I would find it interesting to elaborate on the interpretation of convergence in the context of Bayesian inversion. You mention (I think) that MCMC runs do not converge if insufficient information is available in the empirical p(s) to determine the considered model parameters. I would find it interesting to elaborate on when (and why) these non converging runs arise, perhaps in your discussion on data availability (section 4.3).

5. Finally, I would find it useful for get a sense of how parameters estimated using SM observations taken at a certain scale are valid at different scales. This would have interesting implications, for instance in terms of using satellite remote sensing SM observations to estimate smaller scale SM dynamics in ungauged regions. You discuss this point a little in the paper, but it would be interesting to substantiate your arguments with some analysis. For instance you could run a goodness of fit analysis between modeled SM distributions using params estimated at one scale to empirical SM pdfs observed at another scale.

Minor comments

p3. I would find it useful if you could comment on the advantages of using the Bayesian inversion approach you propose vs more "standard" frequentist approaches such as maximal likelihood, which is the go-to approach I would take to fit a "low dimensional" (4 params) closed form analytical pdf.

p7 l.18. To illustrate your claim, it would be useful if you could present statistics on the frequency of s in each zone of the pdf (in eqn 2) using your best estimation of s* and sw at each site.

p7. Please describe your procedure to compute empirical pdf's from time series observation. If you use kernels to estimate density functions, please specify and justify the chosen shape and bandwith.

p9 l.20: 'discarded'

p10: section 3 is missing

p11. It would be useful to summarize the results (model, scale, posterior CV, goodness of fit distance) for the different cases of the sensitivity analysis in a table.

p12 l.27. "Consistent" has a very specific statistical meaning (asymptotically unbiased), please rephrase if necessary

p13 l 18: "versus"

p 14 l3. Please elaborate on how you could disentangle confounding effects of scale and observation depths. The way I understand it, your analysis in Section 4.2 shows that the results are insensitive to the assumed root-zone depth, not the actual depth, which appears to be unknown (see point 3 above).

Fig 5: you state that the Kolmogorov statistic is significant with a 95% confidence levels. Does that mean that the statistic is significantly different from zero? If so, I would interpret that as having a 5% chance of being wrong if I state that the two compared

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distributions are different (see my point on KS tests above), which I don't think is the point you intended to make.

References

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