Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-675-RC1, 2017


Hydrology and Earth System Sciences
Discussions



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

**M. Zhang (Referee)**

minghuiz@berkeley.edu

Received and published: 15 December 2017

I. General comments

Thank you for the opportunity to review the paper "Probabilistic inference of ecohydrological parameters using observations from point to satellite scales". This work introduces a Bayesian inference technique that estimates four ecohydrological parameters from empirical soil moisture pdfs. The paper's novelty lies in the application of this technique beyond the point scale. In the method, the four ecohydrological parameters, which encompass soil water holding thresholds and evapotranspiration, were related to soil moisture observations through Laio et al. (2001)'s analytical formula. The authors then pose questions about the spatial scale, data availability, and model complexities that are appropriate for such an estimation method, and provide concise answers: estimates are most robust at the satellite scale; the method is accurate with as few as 75 random daily observations; and a specific group of parameters (sw, s*, Emax, Ew = 0.05Emax) can be inferred with highest accuracy. In my opinion, this paper, with major revisions, will have important implications in hydrological modeling. Below are my scientific comments, requests for clarification, and technical corrections.

II. Major comments

1. Applicability of the method

I appreciated the paper's use of sensitivity tests to define the method's applicability in a range of data availability levels, spatial scales, rooting depths, and model complexities. However, I think there's room for another, broader view of method applicability. The conclusions about method applicability were (naturally) only applied in cases where the simulation converges. It would be important to also define the conditions under which the method does (or does not) behave well. On page 1 lines 15-16, the authors wrote that "parameter estimates were most constrained for scales and locations at which soil water dynamics are more sensitive to the fitted ecohydrological parameters of interest". Am I correct in concluding that the method does not converge when soil moisture is NOT sensitive to the ecohydrological parameters of interest?

I recommend that the authors address the conditions under which the method fails to converge. They have briefly mentioned the effect of dry vs. wet climates, but I would like to see a discussion on the effects of soil and vegetation type as well.

2. Choice of estimated parameters

On page 3 line 3, the authors state that the method focuses on estimating "vegetation controls on soil water dynamics". Within this broad category of parameters, four were chosen specifically: sw, s*, Ew, and Emax. The authors should elucidate their choice





Printer-friendly version

Discussion paper

of parameters in two ways.

First, there should be a brief explanation of why four was chosen as the maximum number of parameters. If it was out of concern for equifinality, a formal analysis should be included.

Second, I was surprised to see that the rooting depth Z was not among the estimated parameters. From my point of view, Z could be estimated in the same manner as the four chosen parameters and significantly affects the soil moisture pdf. Porporato's work indicates that the volume of storage in the rooting zone is a key determinant of the pdf shape, so there is an a priori reason to expect that Z is an important parameter. In Section 4.2, the authors mentioned that the four estimated parameters aren't very sensitive to the value of Z, but I'm not convinced that Figure 5 supports this conclusion. I strongly suggest a practical or theoretical explanation about why Z was not chosen as an estimated parameter.

III. Minor comments

Section 2.2.1: Model definition

In my opinion, ignoring interception is questionable given the differences in forest type (and especially the presence of deciduous forest in some sites). I recommend a defense of the decision to ignore interception in the soil moisture model.

Using a date range of April to September might introduce nonstationary behavior in climate parameters as the seasons progress from spring to autumn. I suggest a discussion of the impact of (1) nonstationary Emax within this period due to vegetation growth, particularly leaf out and LAI changes in the deciduous forest sites; and (2) any large changes in rainfall occurrence in summer-dry climates on the method's accuracy.

Section 2.2.2: Climate, soil and vegetation parameter characterization

On page 7 lines 17-18, the authors provided a reasonable explanation for why sfc, sh and Ks don't significantly affect soil moisture pdf. It would be nice, though not crucial, to support this claim using either a sensitivity analysis or with reference to existing analytical studies from Laio et al., (2001).

Section 2.3.1: Application of the Bayes theorem

The authors have assumed uninformed prior knowledge of each of the soil balance parameters while applying Bayes theorem. However, the soil type, climate, and primary forms of vegetation are known at each site, and soil threshold parameters may be estimated from pedotransfer functions. Therefore it seems that an informed prior for each of the four parameters was in fact possible. I suggest exploring the influence of including informed priors on the results and, based on this exploration, defend or reject the decision to use an uninformed prior.

Section 4: Results and Discussion

Several times over the course of this section, the authors mentioned that “acceptable results” were obtained in the various sensitivity tests. The authors should define what is meant by “acceptable” earlier on.

The Kolmogorov-Smirnov statistic is subject to bias and therefore a problematic way to compare pdfs. I recommend exploring measures that compare pdf quantiles, as was done in Muller et al. (2014).

In addition to comparing pdfs, I recommend validating values of the individual estimated parameters. For example, estimations of Emax should be compared to Emax calculated from the Hargreaves equation, and estimates of s* and sw should be compared to results from pedotransfer functions.

Section 4.1: Level of model complexity

Based on Figure 4, it looks like certain location-parameter pairs are very sensitive to model complexity, whereas others are not. I recommend that the authors further explore and explain this sensitivity.

Section 5: Conclusions

I suggest including proposed next steps to improve this method, or planned applications using this method.

Figures

Figure 1: In general, satellite scale soil moisture seems to fluctuate much more than that of footprint scale under dry climate conditions. The caption should include a comment on why this is so, and on the implications of this on performance at the satellite scale.

Figure 4: In the caption, explain why are there error bars associated with only some data points.

Figure 5: In the caption, explain the abrupt changes and “dangling” data points around soil depths of 400m and 600mm for the point and footprint scale plots, respectively.

Figures 4 to 6: please add a legend showing that each of the different colors represents a different location.

IV. Technical corrections

Page 1 line 13: be more specific about what is meant by “footprint” scale.

Page 1 line 25: “back to the atmosphere”

Page 2 line 29: “space-borne”

Page 6 line 9: “commonly used in soil water balance”

Page 9 line 20: the run was discarded”

Page 9 line 21: “more than 10 run samples”

The paper skips directly from section 2 to section 4.

Figure 3 caption: “empirical versus modeled”

Reference

Muller, M.F, D. N. Dralle, and S. E. Thompson (2014), Analytical model for flow duration curves in seasonally dry climates, Water Resour. Res., 50, 5510-5531, doi: 10.1002/2014WR015301.

---

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