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Discussions

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

D. Dralle (Referee)

dralle@berkeley.edu

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REVIEW of the paper, "Probabilistic inference of ecohydrological parameters using observations from point to satellite scales" (Bassiouni et al.)

Submitted for possible publication in HESS

The authors pair in situ and remotely sensed soil moisture data with a Bayesian approach to infer parameters in a 1-d analytical model for soil moisture dynamics.

General Comments:

1) My primary concern is that the authors frequently claim "accurate" results, yet the

study does not include any comparison between predicted and measured soil moisture thresholds. I would say that the study is more accurately described as an exercise in Bayesian model calibration. The novelty of the study, in my opinion, lies in comparing parameters of calibrated PDFs across observation scales. This is a useful exercise, though it's not fully explored in the study. The authors only go so far as to say that "spatial heterogeneity" explains shifting parameter values across scales. The significance of the study would be greatly increased if the authors worked to address some of these scaling effects. A couple questions include: How transferrable are inferred parameter values between scales? How might the optimal form of the PDF change across scales if heterogeneity is the culprit? And, are there simple in silico exercises that could be performed to explore these questions? For example, if the authors generate spatially correlated fields of soil moisture parameters and solve the 1-d model at each point, can aggregation explain (even qualitatively) observed trends in the inferred parameters? What are the implications for applications in sparsely monitored areas, or for making useful predictions at a point using remotely sensed data?

2) While I appreciate the authors' thoroughness, the inclusion of 6 distinct models for soil moisture dynamics somewhat obscures the paper's results. What intuition does this degree of added complexity provide, other than "model performance increases when there are more parameters to tune"? Could some of these results be relegated to Supporting Information, keeping the two most illustrative models?

3) The authors assume steady-state conditions for application of the stochastic models. While this may be appropriate for MMS and ARM, soil moisture dynamics at the seasonally dry sites Tonzi Ranch and Metolius are highly non-stationary during the dry season study months April – September. One can see this in the bi-modality of the soil moisture PDFs in Figure 3. At the very least, it is important for the authors to address or test the effects of this non-stationarity on inferred parameter values. How might strong non-stationarity affect the interpretability of parameter inferences? Perhaps more appropriately, the authors could consider related models that can accom-

modate seasonally dry soil moisture dynamics. In particular, Dralle et al. (2016, doi: 10.1002/2015wr017813) develop a seasonal stochastic soil moisture model and apply the model at Tonzi Ranch. The calibrated parameter values in that study are exactly comparable to inferred values in the present study. Similarly, Viola et al. (2008, doi: 10.1029/2007WR006371) present a transient formulation of the same stochastic soil moisture model.

Specific Comments:

Page 1

Lines 8-9: What is a "hydrologically meaningful" scale?

Lines 9-10: Passive voice makes the sentence a little confusing; try, "we hypothesize that pdfs of soil saturation encode sufficient information. . ."

Line 12: When the authors refer to soil "saturation", do they mean "water content", or "moisture"? I associate the word "saturation" with a water content equal to porosity.

Line 28: Check spelling of reference.

Line 31: What are the "mean components of the soil water balance"?

Page 2

Line 17: Issues with citations

Page 3

Line 18: "interference"?

Page 4

Lines 1-2: Usage, "confront pdfs. . .to a commonly used analytical model"?

Page 6

Lines 3-4: I do not believe the model specifies that ET occurs at a constant rate Emax.

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Page 7

Line 12: Do Rawls (1982) list physical soil characteristics for these sites?

Page 8

Lines 9-10: It's not clear to me why values for Ew/Emax must be tested in a separate (not shown) calibration procedure. See General Comment (2). Page 12

Lines 6-7: My understanding of Emax is that it quantifies atmospheric moisture demand. Why should it scale with rooting depth? Typically, I've seen this value computed using Penman-Monteith e.g. Viola et al. (2008, doi: 10.1029/2007WR006371).

Page 13

Line 1: I would suggest that model performance at Tonzi and Metolius suffers primarily due to the stationarity assumption, which is likely not valid at these Mediterranean sites.

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