

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

X. Feng (Referee)

xue.feng@berkeley.edu

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The manuscript titled “Probabilistic inference of ecohydrological parameters using observations from point to satellite scales” by Bassiouni et al. adopts a Bayesian inference approach to estimate parameters from a parsimonious soil moisture model based on readily available data (soil texture, rainfall, soil moisture) at the point, footprint, and satellite scales. This is a worthwhile exercise and paves the way for the evaluation of the utility of soil moisture data from satellite products. I recommend its publication contingent on clarification on a few issues.

1. A key assumption embedded in the use of this approach requires that the time

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series of soil moisture capture the whole range of realizable values. This is required to disentangle cases where soil moisture values cannot be observed due to physical constraints (e.g., imposed by saturation thresholds – the point of this study) versus heuristic constraints (e.g., we simply have not measured it under sufficiently wet or dry conditions). Please include this caveat and discuss practical considerations in overcoming this issue.

2. Relatedly, the study concludes that “model inference at wetter sites ... is more successful than at dry sites” because known rainfall parameters have been used to constrain the model at wetter sites, where it is hypothesized to play a stronger role in determining the soil moisture pdf. I think this is true, but does not capture the whole story. The “drier” sites used in this study (Tonzi Ranch and Metolius) are also located in Mediterranean climates where substantial seasonal variations in soil moisture can occur between early summer (April/May) and late summer (Sept), which span the period of study. This is apparent from inspection of Figure 1, where soil moisture undergoes an initial rapid decay in Tonzi and Metolius.

As such, I suspect that this assumption of steady state may impact the following statement which I found very interesting (Page 11, line 15): “sw was more important in the analytical equation for soil saturation pdfs and soil water loss equations than s^* .” If the time series span a transient period that eventually converge toward a dry state, then the shape of the soil moisture pdf would be less defined around s^* because there would be relatively fewer soil moisture values near s^* than near sw. In that case, sw would naturally become a more important parameter because the shape of the soil moisture pdf would be more defined around sw, but this would be purely an artifact of the relative data availability around sw and s^* . To test this issue, I think it might be useful to divide the time series into more distinct periods of “wet,” “transition,” or “dry” and use those periods to explicitly estimate the relevant parameters sfc, s^* , and sw.

And a tangential note on Page 6, line 22 “this framework was derived under the assumption of steady state, wherein parameters are constant for a given period of time.”

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Constant parameter values are not sufficient criteria for achieving steady state – as it can also result in a transient period based on initial conditions. Please be careful with this terminology.

3. The role of rooting depth. While the model-data fit was not greatly affected by different rooting depths, the resulting values for E_{max} certainly was. Thus, the authors were able to demonstrate equifinality of results by using E_{max} to compensate for changes in Z . If the goal is to ultimately estimate meaningful values of vegetation and hydrological thresholds from data, is model-data fit a sufficient metric for evaluation of this approach? My own take away from this part of the study was that rooting depth can in fact be a very sensitive parameter due to the large amount of change in E_{max} required to achieve similar fit with data. Perhaps a more useful way of tackling this question would be to include Z as another model parameter and evaluate the site and climate conditions under which its impacts would be limited.

4. A few definitions: Page 1, line 14: “parameter uncertainties” – how are these defined? Page 11, line 9: “the most successful parameter estimations were obtained... with 97, 94, 85 percent converging results” – how are these percentages defined (via GR diagnostics?) and what is the significance of the different levels of convergence? I couldn’t find a reference in the text. Minor point: section 4 (results and discussion) should actually be section 3.

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