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

Response to Minghui Zhang (Referee #1)

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.*

Thank you for your thorough review and constructive suggestions. We have provided responses and preliminary corrections below.

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.

Thank you for this suggestion. We agree that this is an interesting aspect of this study and will amend the results and discussion section to elaborate on the interpretation of convergences. The convergence and inference uncertainty obtained through the Bayesian approach provides insight on (1) whether the data is consistent with the model form used: whether the model is not complex enough or too complex and equifinality arises and (2) whether the assumptions necessary in the model are met by the data: whether the data spans an appropriate range of values or whether the data meets the stationarity assumption. There was no evidence of effects of soil and vegetation type on the accuracy and convergence of the results. We generally concluded that the best results are obtained when model assumptions are appropriately met and therefore the empirical pdf is shaped by the parameters defined in the analytical model. We expect that estimated soil saturation thresholds (s_w and s^*) will have greater certainty if the empirical soil saturation pdf is most defined around those values and greater uncertainty if there are relatively fewer soil saturations values observed around the thresholds. Thus s_w may be more certain for drier sites and s^* may be more certain at wet sites. If the range of observed values is not representative of the soil moisture pdf because it is truncated by missing observations or affected by noise in the data, parameter estimates may have biases.

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 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.

Thank you for this comment. Practical reasons determined the choice of the four parameters that were estimated. Among all the parameters necessary to compute the analytical soil saturation pdf in Equation 2, four (s_w, s^*, E_w, and E_{max}) are not directly observable and generally not reliably estimated using available data and existing methods. The other parameters including rainfall characteristics (λ and α) and physical soil parameters (s_{fc}, s_h, K_s, and b) were characterized based on readily available data and established methods explained in section 2.2.2.

Z was not included as a parameter to be estimated because it is most appropriate for Z to be equal to the measurement depth associated with each scale. Our analysis shows that estimates of sw and s* are not very sensitive to the depth Z assumed in the model inversion (Emax scales as expected with Z). This is important if the sensing depth is not precisely known or is variable in time and space, which is the case for the cosmos and satellite measurements. We have previously tested the model inversion including Z as a parameter to be estimated. We found in this case, a decreased the number of MH-MCMC runs that converge without significantly increasing goodness of fit because there is equifinality between pairs of Z and E_{max}.

We will remove the sensitivity test related to soil depth because it is not useful to determine whether estimates of s_w and s* derived from surface soil moisture measurements are relevant to deeper soil depths and does not provide information on the homogeneity assumption. We will amend the methods section to clarify the choice of setting Z to the measurement depth

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 defence of the decision to ignore interception in the soil moisture model.

We agree that interception is an important component of the soil water balance at forested sites. In this analysis we decided to apply the simplest form of the soil water balance model that would be consistent with the empirical soil saturation pdfs and did not include interception. Results for forested sites were acceptable and did not indicate that the level of model complexity needed to be increased by including interception. The proposed methods can be modified for other studies in which it is important to include interception as a known or unknown parameter (the code associated with this analysis that will be also published included interception as a parameter, here set to 0). Errors due to ignoring interception at the forested sites in this study may have been absorbed in other estimated parameters such as E_{max} or compensated by uncertainties in observed rainfall characteristics. We added the following sentences of section 2.2.1 to clarify this point.

For simplification, we assume that the rainfall applied is equal to the amount reaching the ground surface and do not account for rainfall intercepted by vegetation. Interception may be a significant component of the soil water balance at forested sites and may need to be accounted for in other studies.

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.

We acknowledge that the date range may not be optimal for stationary behaviour in climate parameters at each site. Nevertheless, we decided to select concurrent and consistent time periods for all data sources and sites. Results therefore revealed which sites had poorer goodness of fit statistics and for which the steady-state solution for the analytical soil saturation pdf may not have been most appropriate because of seasonality in the selected period. We will amend the discussion to argue this point. (see responses to other RCs).

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).

Ok this reference will be added here. We also added to Table 1 minimum and maximum observed soil saturation values (April-September, 2012) for comparison with soil saturation threshold estimates.

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.

We acknowledge that some information about the soil type, climate, and primary forms of vegetation are

known at each site. We have taken advantage of this knowledge in defining the parameters that were not estimated in the model (see Table 1) and defining the bounds of parameters to estimate (Eq 3) therefore better constraiingn the estimated parameters and avoiding equifinality. The added complexity of informed prior knowledge was unecessary. Our goal was to develop a method with the minimum level of complexity in order for it to be applied at any location using easily available data, which is particularly important for the satellite scale analysis. If pedotransfers are not well defined and inconsistent with the soil moisture data they unnecessary uncertainty would introduced in the methods.

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.

We will revise section 2.3.2. to explicitly state the evaluation goals and metrics used.

Optimal analytical soil saturation pdfs are evaluated by the following criteria.

(1) The Bayesian inversion converges and the Gelman-Rubin diagnostic approaches 1 for each estimated parameter (<1.1).
(2) There is goodness of fit between the optimum analytical pdf derived from the mean parameter estimates and the empirical pdfs derived from observations using the Kolmogorov-Smirnov (KS) statistic and the quantile level Nash-Sutcliffe efficiency (NSE) (Müller et al., 2016).
(3) Posterior distributions of parameter estimates are physically plausible and have low coefficients of variations.

We will also revised the Results section to specify the criteria values that are described and discussed as acceptable.

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).

This is a good suggestion. We agree that the KS test has disadvantages. We have reported it because was the most strict in quantifying divergence between the analytical and empirical pdfs. In contrast, the NSE values were almost always greater than 0.95 and were less useful. We will report both the KS and NSE in the revision.

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.*

We agree that it would be useful to validate estimated parameters with other estimates. However, estimated ecohydrological parameters that are generally not directly measured. This is also argued in Miller et al., 2007). It is therefore challenging to compare estimated parameter values to site-specific observations and determine their accuracy because these are not directly available. We can relate estimated parameters to calibration efforts of comparable parameters from previous studies.

E_{max} is not exactly the atmospheric moisture demand, it is a fraction of the atmospheric moisture demand that can be withdrawn from the soil layer considered. E_{max} can be equal to the atmospheric moisture demand approximated by potential evapotranspiration (PET) if the full soil column or rooting depth is considered. In this study we cannot assume that E_{max} = PET because only the surface soil moisture is sensed. It was not meaningful to compare s* and sw to estimates from pedotransfer functions because these functions are highly non-linear and not specifically calibrated for data used at each site/scale.

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.

OK, we will add a comments related to the differences in uncertainty for certain sites in Figure 4.

Section 5: Conclusions
I suggest including proposed next steps to improve this method, or planned applications using this method.

OK, we will add the following sentences to the conclusions

This study provided a method to estimate ecohydrological characteristics that are not directly observable, and for which established estimation methods are not available. This study only used available datasets from sensor networks and global satellite products and methods can therefore be applied to a large range of sites or to full global datasets to improve understanding of spatial patterns in ecohydrological parameters relevant for local and global water cycle analyses..

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.

Thank you for noticing this. We are not aware of references that analysed causes of higher noise in the satellite-scale soil moisture observations during dry periods. We are not able to make any clear interpretations of this pattern based on the short observation period and limited sites presented in this study. Data indicates that the noise in the satellite-scale soil moisture observations does not significantly affect the mean of the observed soil moisture but may have increased the kurtosis of the empirical pdfs. We will report the mean, standard deviation and kurtosis of empirical soil moisture pdfs in Table 1. Overall, we do not expect our methods to be affected by the noise in the satellite data at the selected locations. This is an illustration of the advantage of analysing pdfs versus time series (mentioned in our introduction) to estimate ecohydrological parameters from satellite soil moisture data. Often areas with highly uncertain satellite soil moisture observations are masked out data products and should not be an issue. Future studies should always assess data quality related to this potential problem

We will revise the following sentence in section 2.1 Data Analysed:

Soil saturation and rainfall data at each scale and for each site during the selected analysis period are presented in Fig. 1 and summary statistics are reported in Table 1. The difference in data quality between data sources and sites is not expected to significantly affect empirical soil saturation pdfs and resulting parameter estimates in this study.

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

Error bars are not visible if the standard deviation is smaller than the plot marker, we will add this statement in the legend. Error bars were different for the different figures because not every figure took into account the same soil depths (Z). The revised figures will only show results using Z equal to the sensing depths for each scale. Error bars representing the standard deviations of the estimated parameters will be consistent for all revised figures and different from figures in the previous draft.

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.

Because the revision will remove the sensitivity test related to soil depth, this figure will also be removed.

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

Figures 4 to 6 have a legend with each location's name, we will add the title 'Site name' to the legend to increase clarity.

IV. Technical corrections

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

The footprint scale is specifically defined in Section 2.1.

Page 1 line 25: "back to the atmosphere"

OK this will be corrected

Page 2 line 29: "space-borne"

OK this will be corrected

Page 6 line 9: "commonly used in soil water balance"

OK this will be corrected

Page 9 line 20: the run was discarded"

OK this will be corrected

Page 9 line 21: "more than 10 run samples"

OK this will be corrected

The paper skips directly from section 2 to section 4.

OK this will be corrected

Figure 3 caption: "empirical versus modelled"

OK this will be corrected

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.