Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-437-RC3, 2016

Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Feasibility analysis of using inverse modeling for estimating field-scale evapotranspiration in maize and soybean fields from soil water content monitoring networks" *by* Foad Foolad et al.

Anonymous Referee #3

Received and published: 25 September 2016

Authors estimated field scale evapotranspiration (ET) by calibrating a 1D unsaturated zone model (HYDRUS-1D) using soil water content measurements, and compared simulated ET with observed ET from an eddy covariance tower. The HYDRUS-1D soil hydraulic parameters were calibrated using daily soil water content measurements from four theta monitoring probes at multiple depths and one cosmic ray neutron probe. While this is an interesting study, the novelty of the current study is not clear. Based on presented results, large differences exist between simulated ET and eddy covariance data and results of soil moisture simulations are not entirely satisfactory given the negative NSE during calibration and small coefficient of determination for soil moisture

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simulations at certain depths. In particular, authors have not discussed the implications of their results and what can be done to improve model estimation. While the focus of the inverse modelling was on soil hydraulic parameters estimation, the study can benefit from a detailed model sensitivity experiment to soil hydraulic and root growth function model parameters. I suggest authors to perform a detailed uncertainty estimation approach to identify the sources of errors (model input, parameters, or model structure) in ET and soil water content estimates. This can help to identify why the model did not perform well in some cases and how authors can improve their results.

1. Introduction, the rational and implications of the current study are not entirely clear. I suggest authors outline the main objectives of their study and discuss how their results advance our understanding of ET estimation using unsaturated zone models. It is not clear whether authors try to develop a benchmark for soil moisture or ET estimation or how their soil hydraulic parameter estimation can help parametrize hyper-resolution land surface models? These are the ideas that are discussed in the Introduction but their links with the current study are not clear.

2. Section 2.2.1. It seems authors have used a different growth root model compared to the HYDRUS-1D root growth model for annual vegetation. Have authors performed any experiments to assess how the results of the two root growth models compare?

3. Section 2.2.1. It will be very useful if authors can report Kc parameters and root growth model parameters as they can impact the results of ET estimation.

4. Section 2.2.2. Additional details regarding the inverse modelling algorithm and an objective function that is used for parameter estimation are required.

5. Section 2.2.2. Line 206- Can authors provide further details about initial soil hydraulic parameters that they used in the modelling experiment? Did they use soil hydraulic parameters based on soil texture class information? Similarly, authors used the same parameter bounds for model calibration for all soil texture classes. It will be useful if authors can incorporate the soil texture information to define priors and initial

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parameter values.

6. Section 2.2.2. Why homogeneous soil type was used for simulating water content for the Cosmos-Ray neutron probe while for the Theta probes variability in vertical hydraulic conductivity is considered?

7. Why the spin-up period is varied between the inverse modelling approach and the forward model? What criteria authors used to define model spin-up?

8. Table 2-Why negative NSE is obtained during calibration period particularly in deeper soil layers? Even R2 values are pretty small for a VZM model that is calibrated to observations. Can authors describe the reasons for this mismatch? Similarly results of soil moisture simulation are not satisfactory for the CRNP calibration based on Table 3.

9. Authors indicate that inverse modelling based on CRNP data is most useful during the periods that soil evaporation is dominant. Can authors further explain why that is the case? One would expect that CRNP should provide better estimate of ET as its footprint is likely to overlap the EC tower footprint.

10. Section 3.2. Authors relate variability in performance of the model in ET simulation to variability in soil texture. However, one important information that is missing is vegetation type at the location of the probes and the EC tower footprint scale. Perhaps, authors should combine ET estimates from multiple probes to estimate ET at a field scale.

11. It will be useful if authors can provide information about deep drainage from model simulations at multiple locations.

Minor comments: Figures 1 and 2 can be combined in one Figure.

Line 166- Extinction

Line 238-Please revise the Figure number to 7.

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Figure 5- Can authors describe the reason for large differences between the spatially averaged TP and CRNP by the end of year 2014?

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