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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Potential evaporation at eddy-covariance sites across the globe" *by* Wouter H. Maes et al.

Anonymous Referee #3

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âĂŃâĂŃâĂŃThe manuscript titled 'Potential evaporation at eddy-covariance sites across the globe' is a surprising piece of work to read through. The reasons are as follows:

(1) The title of the paper is inappropriate in my view. What the authors have done is they selected the events of unlimited soil moisture and/or high EF events and used a host of predictive Potential evaporation models to calculate the statistical errors of the models, based on which the appropriateness of the models are highlighted. The title of the paper should be 'Unstressed evaporation modeling at eddy covariance sites across the globe'.

(2) It is obvious that under unlimited soil moisture, radiation explained maximum variability in evaporation. Such results have been published in many literatures and not

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Discussion paper

new to the community. However, flagging it as potential evaporation is misleading. It should be seen as actual evaporation under unlimited soil moisture which is driven by radiation only. The authors should realize that potential evaporation is a notional term.? What happens in desert where high radiation load is accompanied by extremely high VPD? If we plot an image of global Ep distribution, we will see the deserts to have the maximum Ep values. Then how would one can pick potential evaporation events based on EF or soil moisture saturation. Although the authors have hinted (in Page 3, L15) that Ep is the potential evaporative demand, but finally inclined to wettest events instead of looking at the evaporative demand.

(3) The estimation rAH is extremely outdated and the no attempt is made to demonstrate how sensitive is the PM and Penman equation to rAH parameterization, which in my opinion should carry a section of results, instead of concluding biome specific PT is consistently better that any other models. Yes, when the evaporation is driven by radiation only, it is no wonder that PT will do a good job. However the calibration of PT was still needed to adjust the hidden VPD and rAH related variability in the 'alpha' parameter.

(4) How the residual ET error in PM and Penman was related to rAH? It is now becoming prominent to the ET community that rAH parameterizations are ambiguous and this needs to be resolved in surface energy balance modelling. Some recent studies have highlighted the importance of analytical estimation of aerodynamic condurtance to overcome the uncertainties in ET modelling, which authors are expected to be aware of.

(5) What about the feedback that rAH provides to the evaporation? Without considering those feedbacks, it would be unjustified to come to conclusion bout PM or Penman equations (as mentioned in section 4.3).

(6) Estimation of gc_ref is purely climatological and as a result the differences in gc_ref between the biomes are marginally significant.

(7) A Table of symbols and variables would be very helpful to the readers.

(8) section 4.3: A complete change of description is needed. The conclusion of Michel et al., 2016 and Ershadi et al., 2014 was an outcome of outdated conductance parameterization (despite they were published) and should not be used to as a justification in the discussion.

(9) section 4.3: It is important to highlight the fact that the conductances (both gAH and gc) in the PM equation provides feedback in evaporation that changes the aerodynamic vapor pressure and temperatures. This study used empirical gAH model to obtain evaporation estimates from PM and Penman. In addition the authors made an effort to show gC-VPD known curve to justify the results. In the present case, justification on why PM and Penman equation is complex should come from analysis of gAH and linking the model errors with empirical uncertainties in gAH.

(10) Also, the authots did not mention if they took care of the sky conditions. Ideally the study should select clear sky cases.

Finally, I would like to thank the authors for the honest effort to use large fluxnet dataset and untap the events of unstressed evaporation. But this should not be seen as potential evaporation. A detailed analysis of the role of gAH in PM, additional role of VPD in creating the differences in evaporation between PTb, PM, and Penman would make the study worthy of publication.

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