

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

### **Anonymous Referee #1**

Received and published: 2 March 2018

#### General Comments

This is a landmark paper. The authors introduce a wealth of hard-won, empirical data into a longstanding debate over how best to parameterize potential evapotranspiration (PET). Their analysis is meticulous, thorough, and well documented. Their main finding (robustness of Milly-Dunne and Priestley-Taylor methods, relative to Penman-Monteith method) is convincing and will surprise many investigators.

#### Specific Comments

My most significant concern with this analysis was the use of evaporative fraction,  $LE/(LE+H)$ , in the characterization of stress. I wondered if this might somehow bias the analysis in favor of the Milly-Dunne method, since MD posits a constant value

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of  $LE/(LE+H)$ . For this reason, the sensitivity analysis using soil moisture as a stress criterion, and reaching similar conclusions, is a valuable part of the paper.

One other concern that might be allayed by a little more information is the use of “data corrected by energy balance closure.” For one who is not familiar with FLUXNET and might hesitate to dig into the Michel et al 2016 reference, could the authors say just a little more about how this method works, how big the typical adjustments are, and to what extent the correction method could potentially influence the findings?

If the authors mean to suggest (this is not entirely clear, and should be clarified) that the simple radiation-based methods should now be incorporated into climate models, then I disagree. Climate models use the same physics upon which Penman-Monteith is based, but it needs to be recognized that in such models the stomatal conductance is calculated dynamically in response to controlling environmental variables. (There is nothing wrong with the Penman-Monteith approach in principle; it’s just that it’s hard to apply observationally, since it is sensitive to variables that are hard to know with sufficient precision.) Furthermore, the value of stomatal conductance is crucial for the computation of land-atmosphere carbon exchange. I would agree, on the other hand, that the MD method beats the PM method hands-down for application in global “offline” analyses in which atmospheric feedbacks are not present. However, it can also be argued that analysis of climate-model outputs themselves is a better way to spend one’s time than doing offline analyses, which are sometimes amount to nothing better than attempts make a silk purse from a sow’s ear.

To address the question of whether or not PET can be calculated correctly from actual  $R_n-G$  when the system is stressed takes this otherwise solid empirical paper into the metaphysical realm. What is the meaning of PET in a stressed system? And how does one empirically test that meaning? If one considers the feedback to surface temperature and albedo, why not also the feedback to lower atmospheric conditions, such as humidity and temperature, leading to changes in downward longwave radiation? This passage, for me, detracts from the paper and might better be presented as a technical

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note elsewhere. Highlighting it in the Conclusion, at the expense of more concrete and surprising findings, seems not to be an ideal choice.

Could the authors compose a more fitting title? It is nice that they have provided estimates of PET all over the world, but the scientific value of the paper lies in its use of these data to test conceptual frameworks and related equations for quantifying PET.

#### Technical Corrections/Comments

P1L6 (i.e., Page 1, Line 6). "forecastING" P1,L14. "calibrated BY biome" (here and many places elsewhere through the paper) P2L39. "compared" P3L16. "atmospheric demand" seems an inappropriate phrase, given the dependence on surface properties. P5L14. "will be used, IN ADDITION TO a biome-specific" P7L5. "and WHERE u\*" P8L17. "if FEWER than" P8L18. "criteriON" P8L26-27. I don't think the authors mean "actual crop" here but rather "actual vegetation" P9L34. Seems more significant than "marginal" to me. P10L7. alpha\_RB typo? P10L17-18. Fig 3d rather than Fig 3c? P13L8. "SMOOTHs" P13L13. "relating to whether leaves" P13L17. "issues and would" P21. Thornthwaite is misspelled. Table 2. Use of color is a little distracting/unnecessary, and dark shades obscure text in first column. Why not use horizontal and vertical lines to serve same purpose? Table 2. I am not familiar with the a/b notation in the SUPERscripts (not subscripts). Is there a simple explanation so the reader doesn't need to search through a statistics book? Table 4,5,6. Again, reconsider use of color. Table 5,6. Could it be informative/helpful also to highlight the values that give the best results within the limits of the "standard" approach? Figure 1. The grey background is so dark that it reduces contrast with colored symbols. Could be lighter, or just put in coastlines. The symbols are very difficult to differentiate within a given color set. Figure 2. Label dates with full year, e.g., 2010. Figure 4. The symbols are very difficult to differentiate within a given color set. And really there is too much information on these plots, making it difficult to see the authors' main point. What about a 3x6 matrix of panels (some r-s-b spaces empty) with a single panel showing data from the six (or

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other number of) example sites? Figure 6. I don't understand the dotted, dashed, and solid lines. This plot overall is hard to read any might possibly be improved upon.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-682>, 2018.

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