Review of HESS manuscript # HESS_2017_682

Title: Potential evaporation at eddy-covariance sites across the globe

This is a well-organized and comprehensive manuscript that uses the broad FLUXNET datasets to assess some important and well-known methods for estimation of potential evaporation. The findings here could provide a basis for hydrological and climatological analysis and modeling. Despite merits of the work, certain aspects need to be clarified to improve its impact and avoid confusion of readers.

- Although the authors have nicely reviewed different definitions and ambiguities relevant to "potential evaporation", there is no discussion in the context of Complementary Relationship (CR). Based on the CR, the potential evaporation serves as a dynamic measure of evaporative demand reflecting the land-atmosphere coupling as land dries; hence, what is provided in this work as the potential evaporation is more consistent with definition of wet surface evaporation or a reference evaporation where water availability is not limited (for example, see Brutsaert [2005], Kahler and Brutsaert [2006], and Aminzadeh, Or and Roderick [2016]). I am not also sure what should be the exact definition for potential evaporation, but still prefer to call what you considered as "reference evaporation" or "unstressed evaporation" rather than "potential evaporation" (somehow reflected in the last lines of section 2.4).

- The effect of scales has been discussed in page 2 (line 6) arguing that reference surface should not affect the meteorological condition, what about the effect of meteorological forcing on evaporation from that reference surface? Here is the place for discussion of feedbacks.

- Figure 2a: what is the reason for difference between dots and dark gray line? I understand they are calculated based on Eq. (9), but such difference between half-hourly and daily values is not intuitive! Looking at section 3.1 in Pennypacker and Baldocchi [2016], the daily VH is calculated from daily average friction velocity and drag coefficient and not aggregation of half-hourly VH values obtained from half-hourly database.

- The PT_r and PT_s are based on $\alpha_{PT} = 1.26$. Based on data in Table 4, we see there is a good performance for both (especially PT_s) regardless of the vegetation type. Considering that $\alpha_{PT} = 1.26$ was obtained from measurements (basically) over water bodies (e.g., oceans), and noting that energy partitioning over a water body is quite different with land surfaces, what is the reason for such nice performance here?

- Page 4, line 5: I doubt even for a well-watered canopy r_c=0; this is nicely shown in Plate 1 of Baldocchi et el. [1997].
- Based on the criterion described in section 2.2 for aggregating sub-daily measurements, it is not clear what happened for cloudy days when surface shortwave incoming radiation is used instead if radiation at top of atmosphere.
- The discussion in page 2, line 17 is not consistent; I think the lower skin temperature yields a higher net radiation (less outgoing longwave radiation); please check.
- Although the main analysis of unstressed days is based on the energy criterion, the definition of unstressed days based on soil moisture is a bit questionable as an unstressed day is recognized based on 98th percentile of measurements in each site; what if a site has always very low water moisture levels?
- Page 10, line 11: is there any specific reason/interpretation? Intuitively the wind speed would strongly affect turbulent transfer and, in turn, E_{unstr}.
- Table 1: why you need to calculate r_{aH} for PT and MD?!
- Is T_{eff} in Eq. (5) in degree of Celsius?