Response to comments by Anonymous Referee #2

Hey guys, we actually did this study back in 2012, presented at AGU in a session chaired by Martha Anderson, but we never ended up publishing it. No worries though– we snooze we lose! But, it would be nice if you wouldn't mind citing that presentation? It was led by a high-school student, so I think she'd be thrilled to be cited.
Palmer, C., Fisher, J.B., Mallick, K., Lee, J., 2012. The potential of potential evapotranspiration. American Geophysical Union, San Francisco, California, USA.
Also, we could send you our results and draft paper thus far, and you could draw inspiration and/or take whatever you want from it to help your paper out. We have a few other ways of looking at it. If so, you could add her as a co-author and make her

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.

1) *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).*

   Response: Reference to the CR was indeed absent in the previous version and we agree with the referee that it deserves to be incorporated in the paper. We will acknowledge the CR in the Introduction and Discussions section. We also agree that our definition of potential evaporation as "unstressed" evaporation closely matches the E_{p0} (well-watered surface) in the CR definition, and this will now be mentioned in the text as well. ("*Based on the above review, E_p is defined using the actual ecosystem evaporating at maximal rate as reference system, so E_p refers to the actual demand for water experienced by the ecosystem. This definition is similar to that of E_{p0} in the complementary relationship*.").

   However, we do not necessarily agree that the terminology should be changed in the manuscript. We realise that in the CR, the term 'potential evaporation' is used mainly for the 'real' potential evaporation (E_{pa} in our paper) and, to add confusion, not for the 'free' potential evaporation (E_{p0} or E_w). However, the term 'potential evaporation' is widely used by the climate community, often with the same meaning as we uptake here. Without denying the controversy surrounding the term, but acknowledging it, we understand we should be very precise on how we define potential evaporation in the paper. We believe that the inclusion of the CR will add more context to this definition and make things clearer for all readers.

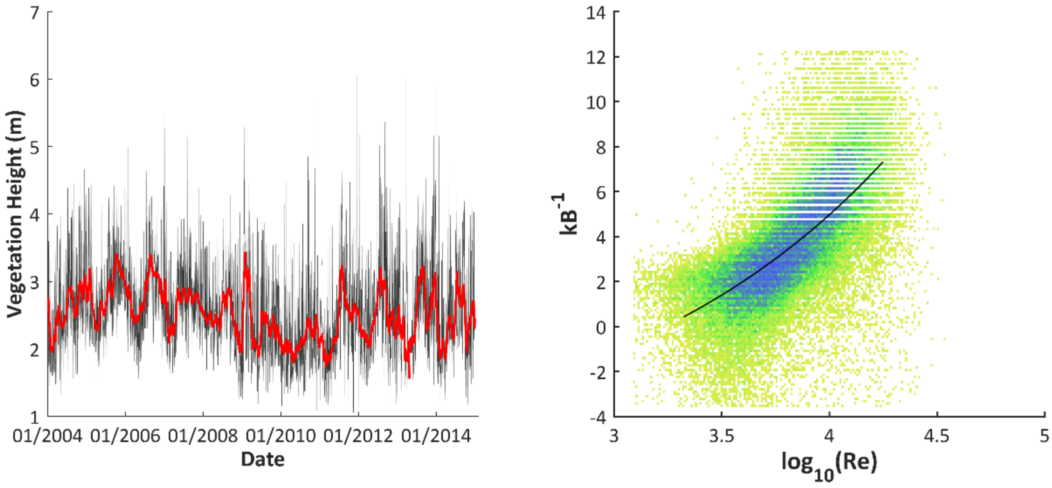
2) *The effect of scales has been discussed in page 2 (line 6) arguing that reference surface should not affect the meteorological condition, what about the effect of meteorological forcing on evaporation from that reference surface? Here is the place for discussion of feedbacks.*

   Response: We agree. In the next version, we will discuss potential feedback mechanisms more clearly. We will also incorporate this to the Discussion section, as suggested by Referee #1.

3) *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.

Response: - We believe that Pennypacker and Baldocchi (2016) did not use daily average friction velocity and drag coefficients, but averaged out height data from half-hourly to daily averages. This is also the approach we used, but we further smoothed the data using a moving window. The apparent difference between half-hourly and daily values in Fig. 2a is indeed misleading – the large majority of the half-hourly height estimates are close to the daily average – this would better be visualised with a density graph. Because we understand the reviewer's concern about this figure and agree that showing the half-hourly data in the format it was presented can be confusing, in the new version we will leave out the half-hourly dots.



Updated version of **Figure 2 (a) Vegetation height dynamics in time (grey dots: half-hourly measurements; dark grey lines: daily mean vegetation height; red line: 30-day moving average (i.e. the final vegetation height dataset). (b) Relation between the Stanton number (kB^{-1}) and the Reynolds number (Re). Both plots correspond to the woody savannah site of Santa Rita Mesquite (Arizona, USA).**

4) *The PT_r and PT_s are based on α_{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 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?*

Response: In Table 4, only correlations are considered,. Because the α_{PT} value here is a multiplying factor of $\frac{s\,(R_n-G)}{s+\gamma}$, the value of α_{PT} as such does not have an effect on the correlations. This is why PT_s and PT_B have the same correlation, as do MD_s and MD_B as well as Ou_S and Ou_B. On the other hand, the relatively high unbiased RMSE and bias PT_r and PT_s (see Tables 5 and 6) indicate that assuming a value of α_{PT}=1.26 introduces and overestimation.

5) *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].*

Response: We believe there is some confusion between a well-watered and a wet canopy. Indeed, as correctly observed by the referee, even for a well-watered canopy, $r_c > 0$. Only for a wet canopy surface (e.g. shortly after rainfall, …), r_c=0. This is also considered in traditional interception loss models (see e.g. Rutter et al., 1975, DOI: 10.2307/2401739).

6) *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.*

Response: We used the same threshold; this is now specified in the text. Note that a (half-)hourly mean of 5 Wm^{-2} is a very low light intensity, so even under very cloudy conditions, the use of SW_{in} does not hold the risk of excluding data in the middle of the day. TOA-atmosphere is almost always available for all sites (as it can be calculated directly).

7) *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.*

Response: Thank you, indeed. This will be corrected in the text.

8) *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?*

Response: The reviewer touches upon an assumption that needs to be made with any method extracting a subset of unstressed days from the time series of measured variables: that the dataset of each site contains at least a few dates in which unstressed conditions prevail. This is an assumption that is made for both the soil moisture and the energy balance criterion. However, there are not true arid sites in the database for which one could not assume that the conditions are unstressed for a few days per year. Even in the case of e.g. Au-TTE and Au-ASM (both very dry sites in the centre of Australia), there is a wet season clearly reflected in the observations. H

To further clarify, in the case of the soil moisture criterion, we divided the dataset into 20th percentiles of evaporation, and selected within all but the lowest percentile the days with 5% highest soil moisture. Specifically to avoid selecting days in which soil moisture is relatively high but still limiting, we added a second condition: the soil moisture of these days needed to be above 75% of the maximum soil moisture of the site, else, these days were removed from the unstressed dataset. This maximum soil moisture was defined as the 98th percentile of soil moisture (we took the 98th percentile, rather than the absolute measured maximum, to avoid influence from extreme values).

9) *Page 10, line 11: is there any specific reason/interpretation? Intuitively the wind speed would strongly affect turbulent transfer and, in turn, E_{unstr}.*

Response: Indeed, intuitively, while one would expect wind speed to have a strong effect on E_{unstr}, this is not shown in our study. However, this finding is quite consistent with the observations throughout our study: E_{unstr} is predominantly determined by radiation (R_n) (see left column of Table 3) and can best be estimated with the Milly and Dunne method, in which wind speed is not considered.

10) *Table 1: why you need to calculate r_{aH} for PT and MD?!*

Response: Thanks for pointing this out, this was indeed an error – we will correct Table 1. We noted a similar issue with RH/VPD, which will also be corrected.

11) *Is T_{eff} in Eq. (5) in degree of Celsius?*
Response: yes it is, see e.g. Pereira and Pruitt, 2004 (doi:10.1016/j.agwat.2003.11.003)