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Interactive comment on "Water stress induced breakdown of carbon-water relations: indicators from diurnal FLUXNET patterns" *by* Jacob A. Nelson et al.

Anonymous Referee #3

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Overall, I am supportive of the goals of this study. I agree that the asymmetry of the diurnal cycle of ET, and the correlation between GPP and ET, likely contain meaningful information about ecosystem response to drought. I also applaud the author's efforts to link these metrics to insights informed by mechanistic theory.

The analysis presented here is very broad (i.e. results are synthesized across many sites, and often many PFTs). Cross-site syntheses like this are invaluable for understanding broad patterns in vegetation functioning; however, the objectives of this study would benefit from a more in-depth analysis of the results at a least few sites, preferably sites where there exist independent estimates of plant water relations during pe-

riods of hydrologic stress (i.e. from gas exchange, sap flux, isotope analysis of tree cores, etc). This sort of analysis would give the readers confidence that the C_ET and DWCI metrics are really reflecting stomatal and non-stomatal limitations to transpiration and carbon uptake, and aren't unduly contaminated by the many sources of uncertainty in using eddy covariance measurements to infer GPP and water use efficiency (e.g. Knauer et al. 2017). Or, to put it differently the new metrics introduced in this manuscript merit some "proof of concept" before they are applied broadly.

I also had a few concerns about the presentation and interpretation of the water use efficiency theory. First, the authors attributed the afternoon decline in ET to "hydraulic limitation" driven specifically by challenges of moving liquid water from roots to the leaves as soil dries (e.g. Lines 24-25). While I agree that hydraulics are an important control on stomatal functioning, stomates may also close directly in response to rising VPD even if soil moisture is unchanged (as discussed at length in the stomatal optimization literature), and the mechanisms responsible for the VPD response are still not yet clear. Thus, it may be more appropriate to describe the afternoon decline in ET as simply "stomatal limitations."

Next, the function ET=i*GPP*sqrt (VPD), proposed by Zhou et al. (2015), is referred to in this manuscript as the "Katul" model; presumably this nomenclature originates from the theory presented in Katul et al. (2010), which presents arguments leading to the equation:

ET = GPP x sqrt(VPD) / sqrt(1.6*lambda*ca)

The parameter lambda is the so-called "marginal water use efficiency" and ca is atmospheric CO2 concentration. This is similar to the Eq. 8 in the present manuscript:

ET = i x GPP x sqrt(VPD)

If i = 1/ sqrt(1.6*lambda*ca)

I appreciate that the authors have attributed the model to Katul et al. (2010), who presented the theory on which the equation is based. However, before the authors attribute the model's "inability to make accurate predictions" to "be a result of a failure of their underlying assumptions," (Page 9, lines 10-11), care should be taken to make sure the underlying assumptions are properly stated and considered. The Katul et al. (2010) result relies on an assumption of Rubisco-limited photosynthesis (which generates a linear A-Ci curve)…this assumption is not likely to hold in dense forests where understory vegetation is often light-limited. Second, and perhaps more importantly, stomatal optimization theory assumes that the parameter lambda should hold constant over timescales of hours, but varies over longer timescales (days to weeks) as other slowly-evolving boundary conditions change (Manzoni et al. 2013, Palmroth et al. 2013). So in that regard, I disagree with the author's assessment on Page 8, Line 5, that the "Katul" model "makes the assumption that the WUE is constant if corrected by the effect of VPD." The potential for lambda to vary may also help to explain the tendency of the Katul (and other) models to underestimate WUE during dry conditions (i.e. Figure 5), especially if the 'i' parameter is determined using observations from well-watered conditions.

Finally, when using the shape of the diurnal pattern of ET to infer stomatal limitation, I wondered why the authors focused only on the shift in the peak, and not the overall degree of asymmetery between morning and afternoon periods (for example, if the ET data from hours 0-12 are reflected about the solar noon axis, that is the area between the reflected and actual ET data).

A few other comments include:

Page 2, lines 15-24. The presentation of the iWUE models would benefit from a more general explanation of what each of the three metrics actually describes and/or is sensitive too. E.g. WUE is useful for understanding broad patterns of ecosystem water use and carbon uptake, but is sensitive to non-biological drivers (e.g. VPD). The iWUE attempts to correct for the direct effect of VPD on transpiration, and is thus a more biologically relevant metric. The uWUE further attempts to correct for stomatal clo-

sure under high VPD, and therefore may be more closely linked to the "non-stomatal" limitations to gas exchange during drought.

Page 4, Line 17: "the use of EC measured diurnal patterns of carbon, water, and energy fluxes to derive clues on ecosystem drought responses at a daily resolution could prove valuable, if nothing less than a benchmark to test current hypotheses." As far as rationale for the work goes, I found this to be rather weak. Perhaps the authors could give specific examples of hypotheses that could be tested with these metrics.

Page 9, line 20: "sites under water stress tended to have C_ET < -0.50." How do the authors know that the sites were under water stress? This gets back to my original point about validating the metrics against independent observations of plant function.

Figure 3: The text is small and hard to read.

Page 13, Lines 15-25: I found this discussion of the links between C_ET and isohydricity to be highly speculative, notably because isohydricity tends to describe plant response to declining soil moisture, yet the afternoon stomatal closure may be largely caused by increasing VPD.

References: Katul, G, Manzoni, S., Palmroth, S., Oren, R. 2010. A stomatal optimization theory to describe the effects of atmospheric CO2 on leaf photosynthesis and transpiration. Annals of Botany, 105, 431-442.

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