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Interactive comment on "Carbon–Water Flux Coupling Under Progressive Drought" *by* S. Boese et al.

Anonymous Referee #2

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The authors identified 47 dry-down periods in the Fluxnet database to study how evapotranspiration (ET) is affected by decreasing soil-moisture and if simple calibrated semi-empirical equations based on the concept of Water Use Efficiency (WUE) and an index of water availability (S_rem) are able to explain the reduction of ET with decreasing soil moisture. Results show that water availability exerts an important control on declining ET and this effect is different across Fluxnet sites and for Plant Functional Types characterized by short and tall vegetation or experiencing different seasonality of dry periods. Results indicate that only the combination of a WUE model with radiation and soil-water limitation provided very good predictions of ET during dry-down events (P 10 LL4-5, Figure 4) and remarks that soil water availability has an effect on ET and WUE independent of VPD (P 15. LL 10-11). While the addressed problem, i.e., ET changes with water availability, is not partially new or the findings particularly surprising, I definitely recognize the value of the quantification and in this regard the presented results are new. Especially, quantifying from observational evidence how ET changes during “clean” dry-down periods, as presented in the article is something important. Furthermore, the study is well written and presented. I have a couple of non-critical comments and then mostly specific comments.

The usefulness of the presented analysis is mostly at the diagnostic level since the presented metric is subjected to a local calibration, depends on the calibration period, and it is not evident how could be used beyond the description presented here. This is partially recognized in the final Section 4.3 (P. 18 LL 7-8) but I have the feeling that some of the statement about the utility of this metric (e.g., P 18 LL 14-17) could be overoptimistic due to the local calibration and strong variability across sites. This may be remarked.

The fact that after two decades of flux-tower data collection only 47 events satisfy the criteria imposed by the authors is a bit discouraging, and does not allow many generalization or comparative analyses, as stated by the authors themselves (P 17 LL 24-29), also due to the huge scatter in the results (Fig. 5 an 7). This scatter is probably due to observational uncertainties but also to behavior of the different ecosystems in response to specific dry-down events and to the definition of S_rem (see below). A few additional words on this problem could be added.

One interesting aspect of the work is to evaluate how much ET decreases because water stress affects GPP and how much is independent of GPP. This is partially shown in Figure 7 but despite the concept of WUE is used as motivation in the introduction of the article and in the Equation (3) to (7), all the figures and results show ET only. There is not a representation of how WUE (e.g., GPP/ET) varies with Srem based on observations. I guess this will provide an additional point of view, which is currently hidden in the analysis. A presentation of changes in WUE would also help to solve the apparently contradictory results according to which the ET attenuation is higher in sites with taller vegetation, but sites with shorter vegetation have a faster decline of ET (P 16 LL 2-5). I think this result can be only explained if WUE changes in a different way between short and tall vegetation in response to soil moisture decline.

I agree with the authors in not using soil moisture directly (not enough representative) but rather use some water balance proxy of it (P 3, LL 28-30). However, the main issue I have with the definition of Srem is that it cannot keep track of any precedent effect of water availability or soil water stress in the system, or in other words, the starting point of the dry-down is independent of the real initial condition and it just depends on the amount of ET occurring afterwards. This is partially recognized by the authors (P. 4 LL14), but I still would like to see some discussion of the potential implications in the discussion section.

Specific comments

P 1. LL 4. It is not very clear at the abstract level, what is meant with "semi-empirical water use efficiency models".

P 1 LL 18. I am not sure if Dominguez et al 2012 is the best reference here, I would search for articles with either a broader geographical perspective or with more focus on the sub-tropical climate that is mentioned.

P 2. LL 7. As a matter of fact, stomatal closure is occurring always at higher potentials than critical cavitation levels for xylem (Martn-StPaul et al 2017).

P. 2 LL 8. Increased leaf-temperature does not necessarily lead to a decrease in photosynthesis; it depends on the actual temperature and temperature-sensitivity of a given species.

P. 2. LL 15. Also the classic empirical models, not based on optimality, can reproduce stomatal conductance and WUE responses to VPD (e.g., Ball et al.,1987; Leuning 1995)

P. 2. LL 25. Why are you stating that ET and soil moisture are following a linear relation? Is this following the exponential decrease of ET with time? Then, very likely, the linearity is with some “proxy” values of soil moisture as S_rem and not with the actual soil moisture.

P. 3. LL 20-21. The way you compute dry-down event and especially the separation between daily ET limited by atmospheric demand and by soil moisture is crucial for the rest of the article as also shown by the sensitivity to the calibration period. Therefore, I would strongly encourage to move the Supp. Material 1 to the main text. It is not too long and it is important to have the full methodological explanation at this stage. I was very confused for many pages on “when” the exponential decrease was assumed to start, if immediately at the beginning of the selected period or after a few days.

P. 6 LL 24. Maybe just an impression but it is not very clear what “all models” refer to, an explicit reference to Eq (3), (4), (6) and (7) would be useful.

P. 8 LL 10-17. For how many steps the WAI_t variable is computed? Since the beginning is from the arbitrary 100 mm in order to extract the mean seasonal cycle of WAI, you need several years.

P. 8. LL 19. Given how WAI is computed, memory effects refer only to seasonal effects, since WAI is averaged.

P. 8. LL 25. I am not fully convinced by this definition; actually also the transpiration associated to GPP is linked to radiation even though indirectly. I would suggest to use a different wording and nomenclature for ETfract_t.

P. 9. LL 21. This is very much expected since they do not have any way of accounting for soil-water limitations.

P 13. LL 11-12. This result is a bit counterintuitive to me. At first glance, I would expect sites with short vegetation to have a higher ET attenuation than sites with taller vegetation, especially because sites with shorter vegetation have a faster decline of ET (P 14 LL 4-5). The two results seem in contradiction. How do you explain this?

Is because ET in shorter vegetation is more coupled to GPP than to the decrease associated to soil water availability and this reflects in a lower value of d?

P. 17. LL 30. I would tend to disagree with this statement. The results show eventually that we need more eddy-covariance measurements everywhere or other type of observations that could be used for similar purposes. Overall, semiarid regions are more resilient to decay of ET according to Fig. 10.

References

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