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Biogeosciences Discussions

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

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We thank Referee #2 for his valuable feedback on the submitted manuscript. Below, we address general remarks and important specific remarks that required a response and describe how we incorporate these in the revised manuscript. In addition we carefully considered all specific comments related to spelling, clarity and references and integrated them into the revised manuscript where appropriate.

GENERAL REMARKS

- "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."

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We agree that the discussion of our results requires a better differentiation between the diagnostic insights we could provide and its applications in future work. Due to the limitation of this metric (specifically the local calibration and its recursive, ET-dependent character), any future work should first try to link it with directly measurable variables. If representations of such variables reflecting soil-water content can be derived from mechanistic land surface models, this could provide an additional step to verify the presented patterns in ET–GPP coupling. We have changed the discussion accordingly.

- "[...] 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."

The limited sample size of the study and vast variability in sampled climate, plant and ecosystem types does in fact pose a substantial challenge for obtaining generalizable understanding with a small sample of sites. We have added this to the section discussing limitations of our approach.

- "There is not a representation of how WUE (e.g., GPP/ET) varies with Srem based on observations."

We concur that such a representation could be helpful to understand to what degree GPP–ET coupling holds during the periods of interest. We propose for this purpose to further not only show the covariation of Srem with WUE but also uWUE as proposed by Zhou et al. (2014 & 2015), which already accounts for the dependence of water-vapor diffusion and stomatal conductance on VPD.

- "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." \

This is an important point worth discussing more extensively in the manuscript. The Srem metric and its analysis is certainly limited to the approximately exponentially decaying ET of dry-down events. However, antecedent conditions can be reflected in this metric. Consider an ecosystem that experienced intermittent periods of water-limitation that did not qualify as dry-down events according to our definition. After a given last, weak precipitation event, we might see a longer period without any rain-fall in which ET starts following an exponential dry-down decay. Even though we identify only the latter part as dry-down event, plants in the ecosystem are already in drought stress at the beginning of the event. Yet this lower water availability would then also manifest in the reduced ET at the beginning of the event and subsequently a smaller integral of ET used to obtain Srem. Any normalization of the variable for the sites (p4 l10–11) will of course prevent a possible interpretation of q values between sites (as Srem_max can no longer be compared across sites).

We further verified the robustness of our results by using two additional calculations of Srem. In these, we used the lower and upper 95% confidence intervals of the parameters (ET_0 and k) used in the exponential model to obtain a higher and lower variant of Srem. The discrepancy of the two Srem calculations therefore incorporates uncertainties about how the exponential fit could capture the ET decline despite unknown initial conditions and missing values in the time series. We attached this comparison as figure below. Nevertheless, antecedent conditions might well be responsible for deviations of the highly idealized behavior of the models we employed. As such, we haven given this limitation more prominence in the discussion.

SPECIFIC COMMENTS

- "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)."

We have amended the sentence in question.

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

We agree that this statement was too generalized. We have corrected this in the revised manuscript.

- "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."

For our analysis, we assumed that the rate of supply-limited ET depended linearly on the water available for root water uptake (Teuling et al. 2006) as in a one-storage water balance model. Thus: ET $\sim$ k * Srem. We agree that even this simplification only holds for the plant-available water and not for the total soil moisture. We have corrected the sentence accordingly!

- "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."

This is correct. We used 115 years from CRUNCEP reanalysis to obtain mean seasonal WAI amplitudes. Further, we reran the analysis with three different values of WAI_max (as now referred to in the manuscript): 70, 100 and 130 mm. The corresponding plots with labels of the IGBP vegetation classes are attached below. The results suggest that there is indeed some sensitivity of our results, yet all levels show a significant correlation between k and the seasonal amplitude of dryness (higher correlation for lower WAI_max).

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

We have clarified this in the description of the metric.

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

This is an important point. The two models indeed do not contain explicit variables reflecting the soil-water status of the ecosystems. Yet indirectly, observed reductions of GPP, even with a constant underlying water-use efficiency uWUE, could reflect plant responses to soil-water scarcity. Yet we agree that this wasn't phrased well enough and have amended the sentence accordingly.

- "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?"

Thank you for noting this crucial point. We believe that the two observations do not have to be seen as standing in contradiction. As you mention, in low vegetation types (in our case dominated by grasslands), rapidly declining GPP seems to be largely sufficient to predict the also quickly diminishing ET. For tall vegetation types (dominated by trees), more gradual, possibly hydraulic limitations could lead to a shallower decline of ET, while a deeper root zone can sustain ET for comparatively longer periods.

- "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."

We concur that this statement does not follow from our results in its current form. What is primarily needed is more focus on regions prone to intermittent water-scarcity. For semi-arid regions, it is better to see the remaining scatter rather than than mere amplitudes as indication for more eddy-covariance measurements. We have clarified this in the discussion.

Additional References

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Teuling, A. J., Seneviratne, S. I., Williams, C., & Troch, P. A. (2006). Observed timescales of evapotranspiration response to soil moisture. Geophysical Research Letters, 33(23).

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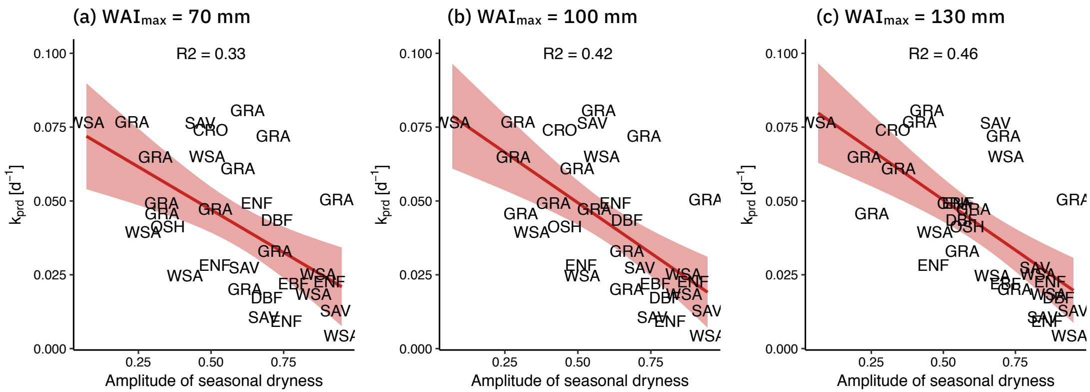

Fig.: Response of the relationship of k to the amplitude of seasonal dryness for three different values of WAI$_{max}$.

Fig. 1.

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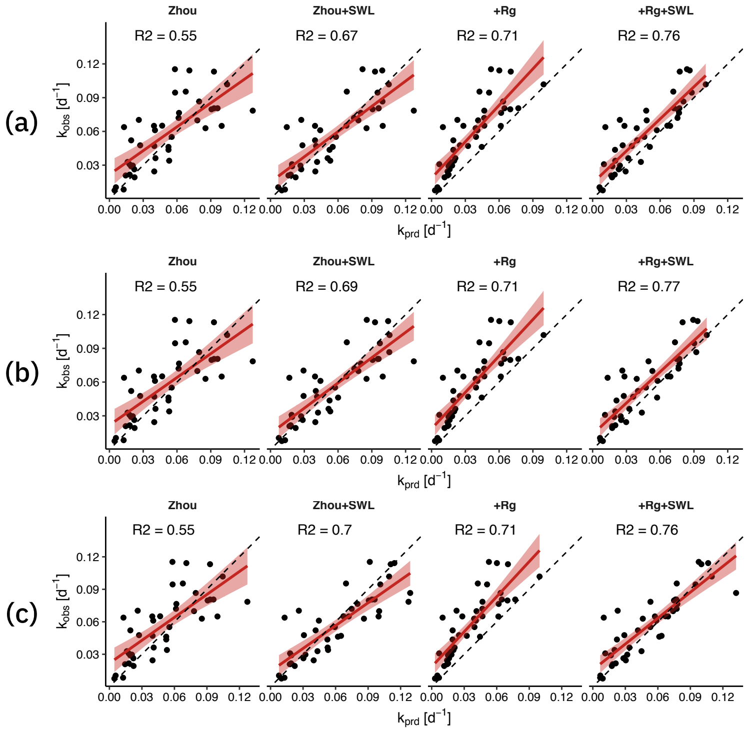

Fig.: Sensitivity of the comparison of predicted vs. observed k for three different calculations of S_{rem}. (a) Using the upper bound of the 95% confidence interval of the calculation of the initial S_{rem}, (b) the most likely value of the initial S_{rem}, as used in the manuscript, (c) using the lower bound of the 95% confidence interval.

Fig. 2.

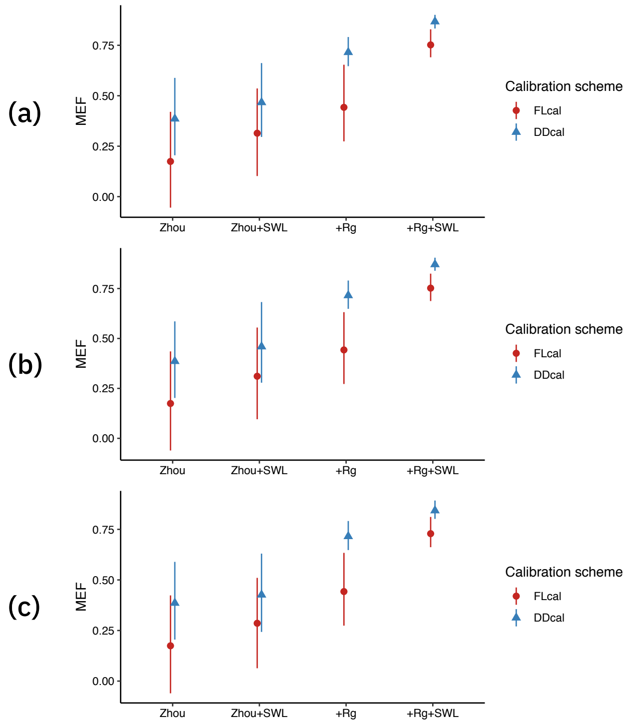

Fig.: Sensitivity of the comparison of model performances for three different calculations of S_{rem}. (a) Using the upper bound of the 95% confidence interval of the calculation of the initial S_{rem}, (b) the most likely value of the initial S_{rem}, as used in the manuscript, (c) using the lower bound of the 95% confidence interval.

Fig. 3.