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

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We thank Martin deKauwe 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

- "In particular, I was hoping to learn about differences in dry- down as a function of vegetation types, hydro-climate, frequency of droughts, etc?".

This point raises an important issue. We also considered a more granular analysis of underlying site-properties that could potentially explain the observed variability of obtained metrics. However, the limited sample size of this study did not allow for detailed stratifications of the data set. Nevertheless, in the revised manuscript, we provide a better presentation of how the results can be disentangled according to climate and vegetation types. To account for the small sample size, we now aggregated multiple climate types (tropical, mediterranean, temperate-humid) and vegetation types (short with grasslands and crops, mixed with savannas and tall for forests). However, we agree that it would be ideal to ultimately link the observed patterns to the physical properties of the plants rather than ecosystem-scale proxy variables. This is an important point that is now stressed in the discussion.

- "'We also explored [...] hydro-climatic properties of the sites' and I don't really see where they've done this?"

We agree that the previous wording failed to connect this statement to results presented later in the manuscript. Specifically, we refered to the mean seasonal WAI amplitude as indicator for regularly occurring water-limitation. We have clarified the manuscript accordingly.

- "Finally, I'm not clear why the authors only focus on ET? The paper frames the question around WUE and so they should also look at the evolution of GPP during a dry-down, shouldn't they? They could easily argue that GPP isn't directly observed and that is fine, but then I think changing the framing more clearly towards ET only, including removing "carbon" from the title, is warranted."

We agree that the previous version of the manuscript failed to convey a central part of how our analysis was conceived. As also remarked by Reviewers 2 & 3, there is a discrepancy between the stated goal of examing carbon–water coupling via water-use efficiency models and the fact that most of the analysis take transpiration as the target variable. Here, we do not assume that the measured gross primary productivity exhibits any less observational and processing uncertainties. In brief, using ET = f(GPP, x) instead of WUE = f(x) is merely a reformulation that focusses on how different WUE models affect the flux magnitudes of ET rather than the ratio WUE = GPP / ET. In the latter approach, small GPP and even smaller ET values can lead to very high WUE values and can in a least-squares regression bias the analysis towards time periods that should not receive as much weight. We have thus added an appropriate paragraph to the introduction.

- "The authors propose the need for two additional corrections, one related to radiation and the other soil water availability. I've commented on this below, it feels unnecessary (mechanistically) and a form of an artificial correction, but I'm happy to be corrected on this and keen to read a more thorough justification."

This comment engages a critical part of our analysis. For our level of analysis, we used a semi-empirical approach, the definition of which we also have explain more prominently in the revised manuscript. The approach is then primarily guided by empirical criteria such as goodness-of-fit measures, while aiming at effective model structures that can be related to physical processes at aggregated scales. In previous work, this approach was used by Boese et al. (2017) to identify a previously neglected driving effect of radiation on transpiration. As we also lay out in Fig. 1, the radiation-effect itself is beneficial to model performance both outside and inside dry-down events. Yet its inclusion exacerbates systematic model errors (Fig. 2), which in turn require correction. The chosen approach is thus primarily motivated by empirical performance of the models. Yet while we succeeded in remediating the model performance during dry-down events, the link to responsible mechanisms does indeed remain tenuous. In the revised manuscript, we discuss this problem in more depth.

- "A number of studies [...] have highlighted the need for a non-stomatal correction to GPP (which indirectly affects ET) in order to correctly capture observed responses. This isn't commented on here, but I note that the authors seem to be arguing the opposite, that is, there is a need for a more direct correction on ET but that GPP is fine."

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Discussion paper

This is a valuable idea to discuss. In the previous version of the manuscript, we did not assume any non-stomatal limitations of GPP during water-limitation. It is nevertheless important to consider to which degree our analysis, if implicitly, addressed this point. The model Zhou+SWL predicts ET as a function of both GPP and soil-water limitation. In our conceptualization, the +SWL term serves as a corrective for non-stomatal limitations of ET. Yet it would also be possible to see the term as correcting for any difference in how soil-water limitation affects ET vs. GPP. Nevertheless, this is an important complication that deserves more attention in the discussion.

- "I really think it is important that the authors document all their fitted terms, e.g. the terms in the supplementary, otherwise this study isn't reproducible."

Agreed. We added the values of the optimized parameters as table in the supplement.

- "What role does LAI, or more specifically, leaf turnover play in the modelling done here? Is it possible that some events see leaf area adjustments which could impact on ET fluxes?"

We agree that changes of LAI have been neglected until now. Especially for dry-down events in vegetation adapted to humid conditions, decreasing LAI due to drought stress has been observed (Anderson et al. 2015). For our purpose, we would expect any negative change in LAI to both affect ET and GPP negatively, as both fluxes depend on the effective surface area at which carbon uptake and water loss happen. It thus seems probable that changes in LAI would not manifest in changing WUE during drought.

SPECIFIC COMMENTS AND CRITICISM

Abstract

- "As written, I feel like it requires a fair amount of prior insight to follow [...]"

We edited the abstract to be more informative and easy to understand for readers unfamiliar with our approach.

Introduction

- Pg 1, line 20: it would be nice (but optional) to have a few physiological citations alongside the point about GPP decline with water limitations.

We have added appropriate citations at the respective location.

- Pg 2, line 18: the text about atmospheric and soil droughts co-occurring ... It reads as if there is an alternative? Surely, as far as the vegetation is concerned these two will always co-occur? If there is plenty of soil water, then even if there is a precipitation drought, it is not a drought for the vegetation. Am I missing something? I assume the point that is being made here is for the need to seperate out the response to VPD vs the response to soil water. I think this could be more clearly articulated here.

This was indeed the point and we have clarified the text accordingly.

Methods

- Pg 3, line 24 onwards: this text isn't clear enough - "namely the quantity does not necessarily reflect the water-stress actually experienced by the plants" - what specifically do the authors mean? Do they mean because these data are usually of limited depth, so do not fully reflect the root- zone?

Thank you for pointing this out. Yes, partially because of differences between rooting-depth and the depth of soil-water measurements. But also because the soil-water contents at specific depths would need to be weighted with the root water uptake which can differ substantially based on root architecture and physiology (Schneider et al. 2010).

- Pg 4, Eqn 1: What about groundwater? This deserves some mention here, if only to highlight it in the assumptions made.

This is correct, we now state that we make the assumption that this does not include groundwater access.

- Pg 4, line 20: Again ... the text about the Boese study and radiation requires further explanation. I suggest it is done once and then it could be referred to as done here. I need to read this paper, but my initial reaction is to query the statement. Why is radiation an important driver of transpiration, independent of GPP? And why Rg and not net radiation? This feels like a form of double counting here (radiation via PAR is a driver of GPP and Rnet is a driver of ET)? Clarifying this in the text would be worthwhile for the reader.

We agree that the manuscript assumed too much knowledge regarding the study of Boese et al. (2017). In that study, the authors identified that an additional radiation term was necessary to predict ET from GPP and VPD at the ecosystem-scale. Similar to the present study, this finding was thus an empirical one, justified by the performance of the models at multiple sites in cross-validation. Yet this finding can be connected to the theory of Jarvis and McNaughton (1986), in which one part of transpiration is driven by the gradient (imposed transpiration, in our case GPPÂůVPDˆ0.5) and the other is driven by the radiative energy input (equilibrium transpiration, in our case r * Rg). While preparing the analysis of the impact of radiation on WUE, we also considered Rnet. As the model performance was slightly higher for Rg and as both variables are temporally very strongly correlated for each particular site, we used Rg in that study. However, this is merely one possible explanation discussed in the preceding publication for what is an empirical pattern. We acknowledge that this needs to be clarified for readers not familiar with that work.

- On a related point - what about evidence of the need for a non-stomatal limitation of photosynthesis during drought? How do the authors suggest this factors into their analysis?

We addressed the closely related point regarding non-stomatal limitations of GPP above in the section "General Remarks".

- Pg 5, Line 1 onwards: "Both models"?? I assume the authors mean eqn 3 and 4? It isn't clear. I don't follow this text - the soil water availability would also have an effect on ET, the reduction in stomatal conductance due to drought would lead to reduced ET. The text as written makes it appear that this only affects GPP. They then propose an empirical correction on transpiration for declining soil water. I fail to see why this is necessary? The ET quantity reflects the soil water availability? I find this quite worrisome, as above with the Rg, this feels like a double correction that isn't warranted mechanistically.

We agree that the paragraph is unclear and can be misunderstood. In fact, we just wanted to state that while the models of eq. 3 & 4 (clarified in the revised version) do not contain an explicit variable for soil-water limitation, one can assume that any decrease of stomatal conductance would lead to reductions in GPP. As ET is here predicted from the variables on the right-hand side, any reduction of GPP induced by water-limitation would entail reductions in ET. The mentioned reductions introduced with the +SWL variants are necessary as Fig. 2 and especially Fig. 3 suggest that models with constant uWUE and r parameters fail to predict ET acurately over the course of dry-down events. More mechanistically, the introduction of the s factor in eq. 6 could be seen as fulfilling a function similar to g_1 attenuation of stomatal conductance models in response to water-limitation.

- Pg 5, eqn 5: where is q given by site? It needs to be shown to the reader.

This has been added to the supplement together with the other fitted parameters.

- "Short" vs "Tall" feels a pretty vague distinction. I think the tall category would have considerable variability and it would be more interesting to consider the results in the context of the actual heights rather than this arbitrary binary classification. I am aware that it is difficult to obtain these kinds of site characteristics, so the authors do not need to do this; however, I think it would be more interesting if they could.

In the updated version of the manuscript, we added a third category, "mixed", for savannah type ecosystems. This admittedly only partially resolves the problem that vegetation types are only crude proxies for the actual height of plants in ecosystems (which in turn can vary substantially for any given site). However, we also clarify that the stratification can reflect – through predominating growth forms – both differences in water-use strategies and rooting depths. Yet it has to be stated that these categories are at best imperfect proxies for variables that as of now are not at all or not consistently measured.

- Pg 8, Eqn 11: how does k vary between sites?

We apologize for the omission. For this analysis, we fixed k at 0.05 which is a reasonable expectation on a global scale (Teuling et al. 2006, also added the appropriate citation in the manuscript).

- How sensitive are the results from eqn 11 to the assumption of a WAI of 100 mm?

To address this point, 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).

Results

- does figure 2 need to be a figure? It strikes me that it could as easily be a table? It might be then preferable to give an example of a time-series between each model evaluation?

We think that Fig. 2 is useful as it visually represents the fundamental motivation of the study: Namely that both the Zhou and +Rg models fail to predict acurately during periods of water-limitation. However, we agree that the importance of this discrepancy has not been properly addressed in the manuscript itself. We would prefer this as a figure, as the variability inside the groups (95% CI intervals) can not be easily rendered in text. We further concur that time-series can be helpful to understand the model errors. While Fig. 3 averages the time-series of multiple sites, we added instructive examples of individual sites in the supplement.

- Pg 10, line 10: This point about the ET not declining fast enough, would fit with the narrative I presented earlier of the need for a non-stomatal limitation on GPP, which would also reduce gs and so ET. Of course this wouldn't work for this kind of empirical model. The correction (SWL) could be seen as effectively doing this, although I don't follow the justification for this approach.

This is an interesting point. In our model, the attenuating factor s could be seen as reflecting possible – process-agnostic – differences in the drought-sensitivity of GPP vs. ET. If, for example, GPP is additionally limited by non-stomatal factors during water-limitation, our model would be expected to underpredict ET (which is still mostly limited by stomatal conductance). If the +Rg+SWL model instead overestimates ET for longer events – while observed ET declines faster – it suggests that ET is more limited by non-stomatal factors when compared to GPP. It is however important to stress in the discussion that due to the empirical nature of the approach, observed patterns can only be tenuously be mapped back to particular processes.

- The interpretation of figure 8 seems a bit optimistic and at the very least should be justified ("significant association") with statistics.

Agreed. We referred to the confidence interval of the local polynomial regression used for smoothing. However, the same statement can be better supported with a linear model, which we use in the updated version of the manuscript.

- Figure 9 ... the difference in k is presented in terms of the "height" of the vegetation, whereas in my eyes it could as easily be interpreted as related to rooting depth and/or leaf area. I'd suggest that height as an explanatory of the difference in dry-down doesn't really have a mechanistic interpretation. At the very least the authors should outline what they think it is a proxy for, or state more clearly how height impacts on the rate of dry-down? Are they hypothesising it is via differences in roughness length?

The tall/short distinction can indeed be seen as an approximate indicator for both water-use strategies and mean rooting depths (see longer response above). We did not consider the distinction to be mediated by differences in roughness length.

Discussion

- Pg 15, Line 10: "Our analysis suggests an ecosystem scale soil- water availability effect on WUE that is statistically independent from VPD effects on the contraction of stomata" - This is a big claim, where is this supported in the data, it would be really helpful to link this to the results. Furthermore, the authors need to unpick this further. If it is independent of the response of gs to VPD, can they discuss the mechanisms they are invoking, presumably via the soil water. Why would it be invariant across ecosystems? This would argue against much of the emerging plant hydraulics literature, surely? Or have I simply misunderstood? I actually see they then link this to a hydraulic limitation related to height - which begs the need to be far more detailed in this analysis. In my eyes it is not sufficient to arbitrarily split the vegetation into small and tall and then to invoke a hydraulic explanation. The tall category could conceivably include a range of heights, do the authors know for certain it is largely made up of very tall trees? I am concerned this is pretty speculative to be honest.

This is a critical part of our discussion and we agree this needs to be discussed more carefully and linked better to the results. The statistical VPD-independence is connected to the observation that the Zhou-Model on its own cannot acurately predict the ET decline during dry-down events. This model integrates the mentioned effect of VPD on stomatal conductance (Zhou et al. 2015). As we demonstrate, this alone proves insufficient to explain ET decline during dry-down events (Fig. 2, 4). Yet even integrating the effect of soil-water limitation (Zhou+SWL) on uWUE (which is inversely proportional to g_1) did not provide substantial benefits to model performance (Fig. 4). Instead the complete reduction of stomatal and non-stomatal (r * Rg) transpiration components (+Rg+SWL) provided the highest performance of predicted ET. As the attenuating factor s is not exclusively reducing stomatal conductance in this model, it could be interpreted as sign of a process affecting both source of transpiration. A reduced stem hydraulic conductivity during water-limitation (Ladjal et al. 2005), could be responsible for this generalized decrease of transpiration. Nevertheless, our empirical approach at ecosystem-scale makes it difficult to pinpoint the mechanism responsible for the observed effects. In the discussion, we now make this clear and further highlight the importance of following up on the results with mechanistic studies in controlled settings.

Importantly, the reduction effect is certainly not invariant across ecosystems. As we show in Fig. 7, the effective reduction of ET varies notably between different ecosystems.

Additional References

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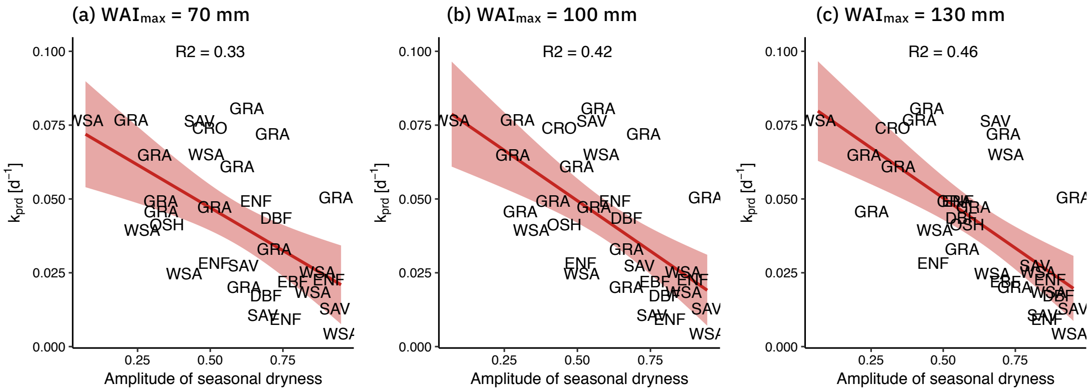

Fig.: Response of the relationship of k to the amplitude of seasonal dryness for three different values of WAI$_{max}$.

Fig. 1.

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Discussion paper

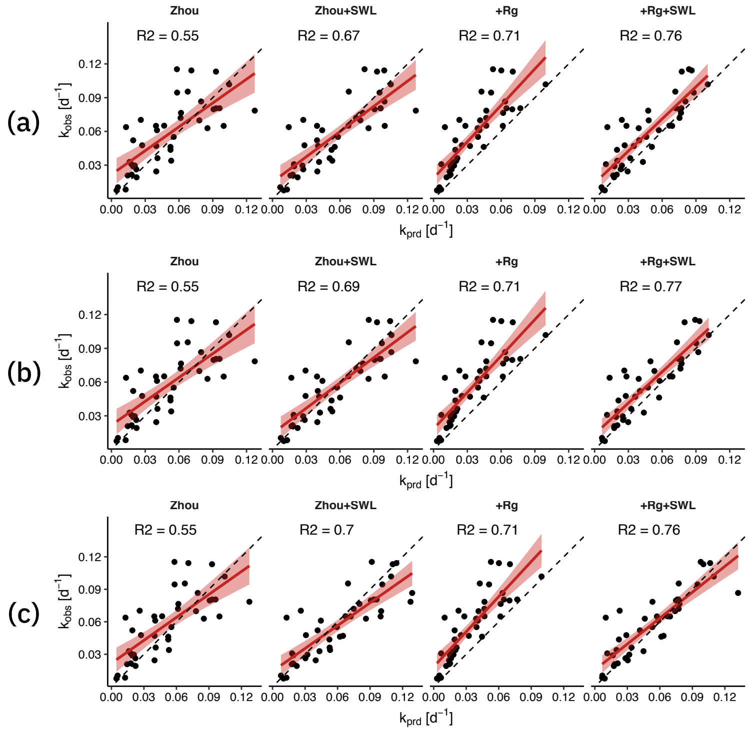

Fig.: Sensitivity of the comparison of predicted vs. observed k for three different calculations of S_{rem}. (a) Using the upper bound of the 95% confidence interval of the calculation of the initial S_{rem}, (b) the most likely value of the initial S_{rem}, as used in the manuscript, (c) using the lower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

Fig. 2.

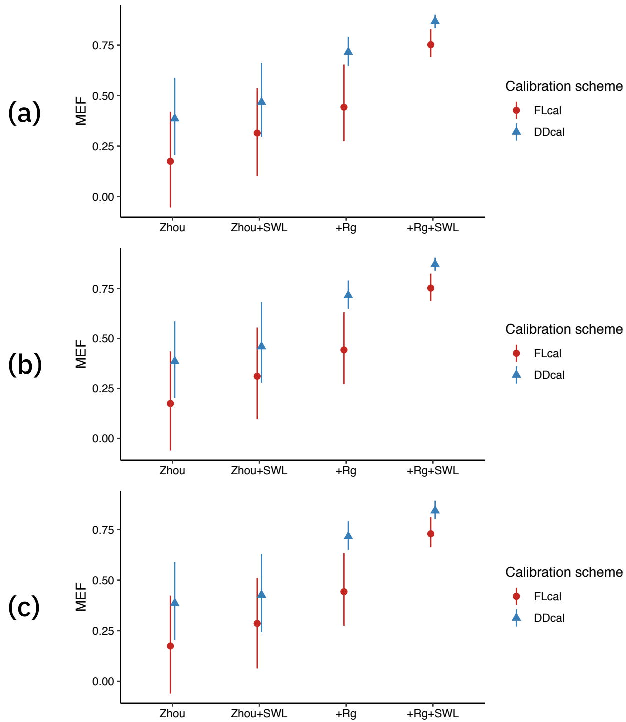

Fig.: Sensitivity of the comparison of model performances for three different calculations of S_{rem}. (a) Using the upper bound of the 95% confidence interval of the calculation of the initial S_{rem}, (b) the most likely value of the initial S_{rem}, as used in the manuscript, (c) using the lower bound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

Fig. 3.