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

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Boese et al. use 47 dry-down events from 31 FLUXNET sites to explore the response of WUE to declining water availability. The authors focus on the response of ET in particular and propose a parameterisation to help improve predictions during drought. I think this is a great experimental approach from which we can learn a lot about how the vegetation responds to declining water availability. However, as currently written this manuscript simply isn't clear enough on a number of small but nonetheless, important points. I suspect many of these could be quickly clarified which I think will help improve the manuscript.

In addition, I did have a few more critical points, specifically:

- With the results, I was hoping to gain a deeper insight into how the land surface responses during drought. 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 feels like an opportunity missed with these data? I think the authors might consider a more nuanced presentation of their findings, but that is of course up to them. Although I will note they stated: "Furthermore, we also explored whether the lengths of included dry-down events depended on hydro-climatic properties of the sites" and I don't really see where they've done this? - 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 leads me to ask about non-stomatal limitations? A number of studies (Keenan et al., 2009; Egea et al., 2011; Flexas et al., 2012; Zhou et al., 2013) 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. This could be a worthwhile discussion point? - 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? Again, I imagine hard to show or not show, but it could be discussed as it may be relevant. - 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. - 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.

* Egea, G., Verhoef, A., and Vidale, P. L.: Towards an improved and more flexible representation of water stress in coupled photosynthesis–stomatal conductance models,

Agr. Forest Meteorol., 151, 1370–1384, 2011. * Flexas, J., Barbour, M. M., Brendel, O., Cabrera, H. M., CarriquiÌĄ, M., DiÌĄaz-Espejo, A., Douthe, C., Dreyer, E., Ferrio, J. P., Gago, J., GalleÌĄ, A., GalmeÌĄs, J., Kodama, N., Medrano, H., Niinemets, UÌĹ., Peguero-Pina, J. J., Pou, A., Ribas-CarboÌĄ, M., TomaÌĄs, M., Tosens, T., and Warren, C. R.: Mesophyll diffusion conductance to CO2: an unappreciated central player in photosynthesis, Plant Sci., 193, 70–84, 2012. * Keenan, T., GarciÌĄa, R., Friend, A. D., Zaehle, S., Gracia, C., and Sabate, S.: Improved understanding of drought controls on sea- sonal variation in Mediterranean forest canopy CO2 and wa- ter fluxes through combined in situ measurements and ecosys- tem modelling, Biogeosciences, 6, 1423–1444, doi:10.5194/bg- 6-1423-2009, 2009. * Zhou, S., Duursma, R. A., Medlyn, B. E., Kelly, J. W., and Prentice, I. C.: How should we model plant responses to drought? An analysis of stomatal and non-stomatal responses to water stress, Agr. Forest Meteorol., 182–183, 204–214, 2013.

Abstract

- As written, I feel like it requires a fair amount of prior insight to follow, e.g. "current semi-empirical water-use efficiency models" - could the authors give an example? "with a previously discovered additive radiation term" - zero context; "20–33% of the observed decline in ET was due to the previously unconsidered" - previously, where? - "in junction" -> "in conjunction"

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. - Pg 1, line 22: do you mean "tenuous"? This text doesn't make sense to me, sorry. - Pg 2, line 14: "On leaf-scale" -> "At the leaf-scale"? Also, "can accurately predict" - crucially, under well-watered conditions ... - Pg 2, line 17: "uWUE" hasn't been defined, you need to shift underlying back a bit in the sentence. It is also worth explaining how this differs from WUE described above. - 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. - Pg 2, line 20-21: in what context? In a coupled model, a change in radiation due to stomatal closure would be an emergent feedback. I assume this is in terms of an empirical model? I think the authors needs to further explain this point as it isn't self-evident. Similarly -> "Yet these water-use efficiency models" - which WUE models? I've no idea what the authors are referring to here/

Methods

- Suggest renaming "2 Detection of Dry-Down Events & Structure of the Analysis" to methods? - 2.1: it is important to note for the reader that GPP, is flux-derived and not a direct observation -> "Observations of gross primary productivity (GPP)..." - Pg 3, line 20-21: I feel this information should be in the main text and not the supplementary? It seems core for the reader. In the supplementary "I" presumably should become "we". "significant negative trend" - statistically? Can the authors also add the fitted terms, a, b and k to their table 1 in the supplementary. - 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? - Pg 4, line 3: to what depth are these "available soil-water" data? My reading of the text is that the authors aren't using any soil water data but instead inferring it but the text is confusing to be honest. It would be worth clarifying. - Pg 4, Eqn 1: What about groundwater? This deserves some mention here, if only to highlight it in the assumptions made. - 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. - 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? - 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. - Pg 5, eqn 5: where is q given by site? It needs to be shown to the reader. - "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. - Pg 8, Eqn 11: how does k vary between sites? - How sensitive are the results from eqn 11 to the assumption of a WAI of 100 mm?

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

Discussion

- Pg 15, Line 9: "Established WUE models" - which? Please add some citations, this is very speculative. - 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. - Pg 18, Line 12: Again, *which* "previously developed ecosystem-level water-use efficiency models" - this phrasing or similar is frequently used throughout the manuscript. Please clarify what models and support with citations you are discussing. I'm 99.9% sure you don't mean LSMs but it is very unclear.

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Discussion paper

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