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Hydrology and Earth System Sciences
Discussions

Interactive comment on "When does vapor pressure deficit drive or reduce evapotranspiration?" *by* Adam Massmann et al.

Anonymous Referee #2

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The authors present a study of how evapotranspiration respond to vapor pressure deficit (VPD) at the ecosystem scale by deriving an analytical equation based on Penman-Monteith equation and an empirical model of stomatal conductance (Eq. 7) and by analyzing flux tower observations in 66 sites of the Fluxnet-2015 dataset. Theoretical and empirical results suggest that ET mostly decreases as VPD increases below a given VPD threshold, this threshold is at quite high VPD values except for crops. The authors attribute this result to a dominant role of plant physiological controls on ET as VPD rises.

The question raised by the authors is important for various fields as ecohydrology, land-atmosphere interactions, biogeoscience and the analysis is interesting and controversial. The postulated concave down relation between ET and VPD is generally

counterintuitive and I think need to be confirmed better with more direct results. This is indeed contrary to our leaf level understanding (and observations) of transpiration response to VPD (see my detailed explanation with reference to literature below, Comment 2). While I am intrigued by the article, I have a number of major and minor comments that hopefully can be solved to support the findings of the study.

(i) Many figures are related to the functional form derived theoretically by the authors (Eq. 7, Eq. 10), however the basic result (e.g., ET vs VPD) is never directly presented in any plot. I would like to see a plot with ET boxplot for different bins of VPDs for different sites (e.g, the partial derivative of ET with respect to VPD mentioned by the authors PP 3, LL 12). This would be important also to understand the uncertainties and potential problems associated with Eq. 10, see for instance my remark on the net-radiation dependence on VPD in the minor comments. Maybe it is obvious and the fit is perfect by construction but it is not very clear to me how Eq. 7 and 10 are fitting the raw data, the Figure 2 with σ value is not sufficient to understand this aspect.

(ii) I think the introduction of the article should contain a discussion of what is known about transpiration response at leaf-scale. We know very well that stomatal conductance is reduced in response to VPD (e.g., Oren et al 1999; Damour et al 2010) and many observational studies suggest that transpiration increases with a concave downward response (e.g., Rawson et al 1977; Turner et al 1984; Mott and Peak 2013). Some observations show a reduction of transpiration at high VPD (e.g., Farquhar 1978) the so-called "feedforward response" of stomatal but this behavior was mostly dismissed as an artifact of measurements rather than a true behavior (e.g., Franks et al 1997). Therefore we are left with the fact that transpiration-VPD relation at the leaf scale is mostly positive and surely concave downward. Now there could be several reasons why ecosystem scale response of ET to VPD may be different from leaf-scale, e.g., effects soil moisture limitations, land-atmospheric feedbacks between transpiration and VPD. The presented approach is looking only at one-way response of ET to VPD without accounting for land-atmospheric feedbacks (for instance during dry soil conditions)

P 23 LL 5-6. This has limitations as discussed by the author themselves but also gives too much weight on the plant physiological control. One thing is to attribute the overall result to plant physiological response (e.g., PP 1 LL 8), another is to attribute the observed response to feedbacks and controls acting at the ecosystem scale and not at the leaf-level. Currently, the article is attributing the observed response (concave upward, PP 23, L13) to plant physiology, which is at odd with what we know at the leaf-level. This needs to be clarified in the manuscript and a very convincing explanation needs to be provided, otherwise my current feeling is that other controls are incorrectly attributed to plant physiology. At least for me, it is not “easy” to see the mechanism leading to a change in curvature from leaf-scale to the ecosystem scale.

(iii) In one hand, there is beauty in the newly derived Eq. (7) since should substitute the unknown gs of Penman-Monteith equation with two variables g1 and uWUE, which are theoretically better known (e.g., Table 1, PP 8 LL 26-27). However, this also comes with a risk, because if g1 and uWUE are spatially variable as they could be (especially g1 according to the original publications), we are passing from one unknown to two unknowns. Plus, one variable is representing a plant control (g1) and the other one is somehow representing the response (uWUE), so I am afraid there is a mixing of concepts in the same equation. The author are very confident that their equation captures the relationship between ET, GPP and VPD with the fit of σ* uWUE (P 19, LL 10-12), but the imposed lack of variability of uWUE and g1 and also the assumption on Rn being unaffected by VPD must be mostly trusted. Therefore, I would be more careful in the argumentation and as wrote before I would like to see how ET and VPD are actually related using a binning approach on VPD.

(iv) I think sometime there is an abuse of the “leading-order term/behavior” (PP 16 LL 18, P 18 LL 9, P 18 LL 29), which in mathematical function has a specific meaning but it is unclear how it is used in the context of this article. I would also suggest to separate better the results related to the theoretical derivation (up to Section 3.3 pretty much) from the ones based on empirical data (afterwards)

Minor Comments

PP 1 LL 17. I would not necessary talk of “plant stress”. A drop of leaf water potential reduces stomatal conductance but this does not necessarily mean “stress”.

PP 1 LL 22-24. Please see also Roderick et al 2014.

P 2. LL 17 . . . could “also” cause a decrease in the likelihood of precipitation

P. 2. LL 33-35. I would tend to disagree, there are many old articles starting from the ones of G. Farquhar (see for instance references in the main comments) that describe humidity (VPD) role on stomatal response. The basic physiological knowledge was established since quite some time. However, I agree that much more uncertainty exists on the ecosystem scale response.

P. 3. LL 2. See also Katul et al 2010

P. 3. LL 9. I would not refer to those as “novel tools”.

P. 5 LL 4. It could be worth mentioning that “g1” is the water use efficiency parameter in the “optimal stomatal conductance model”.

P.6 LL 4. I guess you are referring to Eq. (4) and not (3) here.

P. 7. LL 14. I would state already at this stage that g1 is derived for each PFT from Medlyn et al 2017 as in Table 2 and uWUE baseline values from Zhou et al 2015.

P. 7. Eq. (10). Please note that in such a derivation the indirect role of VPD in modifying surface temperature and therefore net radiation (Rn) is not accounted for. So technically speaking Eq. (10) is incomplete. This needs to be stated and justified.

P. 10. LL 2 and LL 5. The direct link between ET decreasing with VPD and “physiological controls” is not fully justified as I am discussing in the main comments.

P. 10. Equation (12). There is a “σ” missing or is intentional? If yes, please explain.

P. 10. LL 11. Also g1 could vary with soil moisture. As a matter of fact, the water use efficiency parameter of optimality models has been shown to vary with soil moisture (Manzoni et al 2013).

P. 10 Equation (15). Why σ is removed from Eq. (13)?

P. 10 LL 29-30. As a matter of fact, WUE is much lower in semi-arid sites than in wet environments because of their lower productivity (e.g., Beer et al 2009). WUE and uWUE are diagnostic variables. It is “g1”, which represents a physiological control that should be lower (more water use efficient) in water limited ecosystems and higher where there is plenty of water, as you write in Page 11 LL 7-9. I think the distinction on the role of those two variables (parameters of your model) should be better framed.

P. 11. LL 14-17. uWUE is much more constant than g1 in Table 2, therefore most of these effects should be attributed to variability in g1.

P 13. LL 7-9. In these patterns there could be a significant contribution of water availability with crops that are irrigated and maintain high ET in dry (high VPD conditions) while shrubs are mostly water limited at high VPD levels.

Figure 4. I would suggest modifying the plot and having ga in the x-axis and different temperatures plotted with various lines.

P. 16. LL 16. Maybe I am missing something obvious but observations are also plotted based on the same Equation (10) allowing only variability in σ to fit better the data. However, in such a case the functional form is partially prescribed except if σ departs significantly. Is there not the risk of some circular reasoning?

P. 16. LL 14. “nearly exactly” is a bit exaggerated, I think.

P 18. LL 32. The issue with low soil-moisture can emerge even when soil-moisture is not extremely low, because of the atmospheric feedbacks that increase VPD.

P.21. LL 1. There are many cases even before getting to extremely dry soil that land-surface could feed back on humidity and VPD (e.g., Rigden and Salvucci 2015) I am

not sure the assumption of constant uWUE and g1 in time and space really works for the majority of the conditions.

P. 21. LL 2-3. I think this is much more relevant than only for extreme conditions and could actually affect the observed behavior much more than currently stated.

P 23 LL 6. Where the 30% number is coming from? I think it is actually quite challenging estimating from observations when soil moisture is limiting evapotranspiration.

P 23. LL 16-21. I think this part should belong more to introduction than results, please see also my major comments on the shape of ET – VPD.

P 24. LL 24. Which exponent are you referring to here?

P 26. LL 2-4. Yes, this is true but at least an overall representation of how ET changes with VPD binning VPD in order to average variability for various conditions should be provided in such an article.

P 27. Appendix A. It would be nice to provide additional information for the Fluxnet sites relevant to this article as VPD, net radiation, temperature, wind speed, and latent heat for the analyzed period during the growing season, number of hours retained for each site for the analysis, etc.

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