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Discussions

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

Anonymous Referee #3

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The manuscript by Massmann, Gentine and Lin explores how (evapo)transpiration rate responds to changes in vapor pressure deficit (VPD), employing a combination of model developments and data. The topic is interesting and important, with implications for different disciplines. Nevertheless, I have some fundamental concerns regarding the theoretical developments presented in the manuscript and how to best present the results.

Methodology:

Even though not reported in the manuscript, the optimization model parameter g_1 is a function of the marginal water use efficiency λ (see e.g. Medlyn 2011 Global Change Biology). Neglecting such functional dependence has at least two main consequences regarding the theoretical developments and hence the results.

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Discussion paper

1) The authors justify neglecting the role of soil moisture by stating that uWUE has been shown to be relatively constant. I disagree with this conclusion. λ is known to be changing with soil water availability (Manzoni et al 2011 Functional Ecology; Zhou et al 2014 Agricultural and Forest Meteorology). As such, I consider the results discussed in the manuscript problematic, not only at extremely low soil water contents, also in the light of complex (and differing) distributions of soil moisture in the different PFTs (at least two sites exhibit a bimodal distribution; Figure 7). I thus wonder if it would be more robust to consider the soil moisture variability in the theoretical development, albeit in a simplified manner. This would greatly enhance the impact of this work and usability of the theoretical developments, because there are no doubts that soil water availability affects ET rate and, as such, may mediate its response to VPD. Furthermore, it may even reduce the 'uncertainty' σ, by actually accounting for the mechanisms at play. A simpler (but less powerful) approach would be to restrict all (or most of) the analyses and discussions to well-watered conditions, but this would result in number of observations being reduced and changing significantly from ecosystem to ecosystem.

2) It is assumed that the uncertainty parameter σ modulates uWUE, but not g_1 (or any other parameter). I understand the idea of focusing the uncertainty analysis on the most uncertain parameter, but, given the strong relation between uWUE and g_1, I question this approach. Moreover, given that both uWUE and g_1 are affected by soil water availability, considering the uncertainty of one of the two only, is problematic. This undermines the conclusion that any systematic bias stemming from neglecting the effects of the soil water content should be 'absorbed' by σ and, implicitly, that the proposed method does not work only in the cases where a relation between σ and soil water content emerges.

More in general, the method section would benefit from a number of clarifications.

- Several of the symbols are not defined at their first appearance and/or do not appear in the table (e.g., R_{net}, g_0); some symbols have two meanings (e.g., T, which is used both as temperature and transpiration rate); and some others have the same meaning despite being different (e.g., R_{air} and R). Particular care should be posed in defining λ, because this symbol has been used to denote both $\frac{\partial E}{\partial A}$ and $\frac{\partial A}{\partial E}$, depending on the publication.

- As noted above, the dependence of g_1 on λ should be clarified, or else a reader not familiar with Medlyn et al (2011) work is left wondering what the relation between g_1 and uWUE is (in its present form, the manuscript just hints at a possible relation – P7, L7; but the relation is strong, as both are functions of λ and Γ).

- It remained unclear to me why the authors chose to present also the leaf level stomatal conductance, when, in the end, they then use a canopy scale stomatal conductance. The latter is derived from the leaf level stomatal conductance, but such derivation has been published elsewhere.

- A range of observed g_a are considered (e.g., in Fig. 4). I think it is worth mentioning how the aerodynamic conductance was determined. This affects the validity of the conclusion that wind conditions play a secondary role (P14, L20).

- The FluxNet data are used to determine all the terms in Eq. 7, directly or via fitting, as stated on P7; L13. Later on, apparently the same data are used to determine $\sigma (Eq. 9)$. I think it would be best to clarify how these two uses of data 'co-exist'. I suppose uWUE is first determined, assuming $\sigma = 1$; and then, for the obtained uWUE, σ is determined.

Presentation of results:

The authors discuss in detail the sign and value of $\frac{\partial ET}{\partial VPD}$. This makes sense, given the aim of the work, but I think that the distinction in scaling term and sign term is more confusing than clarifying: the 'sign' term affects also the magnitude of $\frac{\partial ET}{\partial VPD}$. I wonder if it would be cleaner to really focus on the sign of the derivative (i.e., Eq. 13 and the bottom part of Figure 3) and then discuss the overall magnitude of the derivative, without distinguishing between the two terms. This would also reduce the number and complexity of figures. Alternatively, the authors could try to interpret the terms in $\frac{\partial ET}{\partial VPD}$

as largely ‘plant driven’ and ‘environment driven’. This however poses the question of to which extent g_a is determined by the environmental conditions (chiefly wind speed) vs. plant/canopy features (chiefly, canopy height).

More in general, the results presented in this manuscript are many and it is not easy to see the logical connection among the different aspects discussed. The take home messages would emerge more clearly, should the presentation of results be streamlined. Thus, steps should be taken to simplify the presentation of results. For example, Table 4 presents the ‘bulk statistics’ of the observed $\frac{\partial ET}{\partial VPD}$ and their match with the theory and then Figure 5 somehow re-iterate the conclusion, but now breaking down the data. Also, Section 3.7 comes a bit as a surprise and, in a certain way, it would fit better in the Methods.

Finally, some of the conclusions are not fully and quantitatively supported. Examples are P16, L20 where reference is made to ‘a bit more variability’ (no quantification of ‘a bit’), or the use of the expression ‘leading order behavior’.

Minor comments

Introduction: The introduction would benefit from a more thorough review of what is known about transpiration response to VPD. Examples are Oren et al 1999 (Plant Cell and Environment) and, more recently and based on FluxNet data, Novick et al. 2016 (Nature Climate Change), also discussing the effects of soil water content. Furthermore, the discussion on plant strategies reported here feels a bit oversimplified. While clearly it is beyond the scope of this work to discuss plant-plant interactions and competition for water or other strategies like CAM photosynthesis, the current text implicitly suggests these other factors do not exist. For example, the ‘ultimate’ adaptation to exploit times of low VPD is CAM photosynthesis, where stomata are (mostly) opened during the night. Also, lack of soil moisture conservation not being a sensible strategy is most likely correct when considering a uniform stand (or isolated vegetation), but competition for resources may affect what a sensible strategy is. In this respect, one additional feature of crops is that they are (generally) planted in an even aged monoculture, while other ecosystems may be characterized by mixed species/ages.

P 9: The first part of the Results and discussion appears to belong to the Methods, entirely or at least Eq. 11 and 12.

P13, L32: the effect of temperature is also direct, not just through Clausius-Clapeyron.

P16, L15: ‘between’ should be removed (or the sentence revised)

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