Thank you for a very thoughtful review; as in our response to RC1, we feel very lucky to have received such quality reviews.

We read through your major and minor comments, and do not anticipate issues reconciling your minor comments for the final response after the discussion period. We will focus our response on your major comments, with the goal of opening the door to further discussion.

Also please note our response to RC1, as some of the discussion there is relevant to this response; particularly discussion on PFT-scale analysis and variability of g_1 and uWUE (RC1 Response).

1 Major comment (i)

One issue with analyses attempting to diagnose the ET response to VPD with pure data (e.g. using binned boxplots, as you suggest), is that ET varies strongly with both aerodynamic conductance and radiation. Simply binning by VPD or by site includes too many confounding factors to really diagnose what the attributable response is to VPD. This issue is really what motivated our analysis and makes it unique; by building an analytical framework based on confirmed results in the literature (uWUE, Medlyn model, Penman-Monteith [PM]), we are able to formally deduce what the actual ET response is, with other environmental factors held fixed. This "other environmental factors held fixed" component is, in our mind, nearly impossible to deduce from pure data, because the environment is always changing and direct analogues are either impossible to find or result in too small of a sample size to form a meaningful conclusion. However, while we cannot assess our formulation of the derivative with respect to VPD, we can assess the actual model of ET, which includes all approximations we introduced (specifically uWUE). As a part of our analysis test suite we generated a figure of this comparison and reproduce it here (Figure 1). Apologies this is not a publication quality figure, but we wanted to get this response out sooner rather than later to stimulate discussion, especially as we are already behind because of the AGU Fall Meeting. This comparison is fair in the sense that by using uWUE we introduce an extra free parameter, so in the original PM model we introduce an extra free parameter modifying g_s and fit it also for each given PFT. The introduction of the uWUE approximation does degrade the quality of the ET model relative to the original PM model. However, we expect this, as using uWUE is a simplification over using GPP directly,

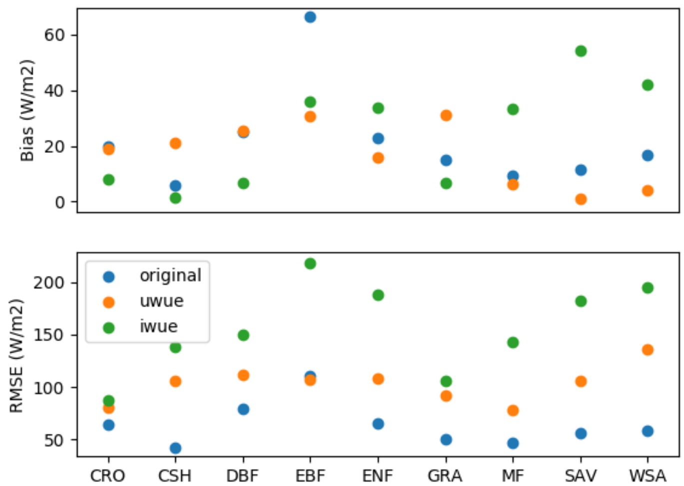

Figure 1: Mean bias (absolute value) and root mean squared error (RMSE) for three different ET estimates, relative to FLUXNET-2015 observations: original (original Penman-Monteith formulation, e.g. Equation 1 in the manuscript, with Equation 4 providing g_s); uWUE (Equation 8 in the manuscript); iWUE (as in Equation 8, but if we had used iWUE to remove GPP dependence rather than uWUE).

and may break down in extreme, limiting cases like when VPD, ET or GPP go to 0. Additionally, we expect errors introduced by assuming that uWUE is fixed within a given PFT. These errors may be reduced if we account for spatiotemporal variation in uWUE, as discussed further in our response to RC1. As a purpose-built model for assessing the ET response, we think the uWUE approach is justified. Essentially we are saying that to the degree we trust arithmetic, uWUE, Penman-Monteith, and the Medlyn stomatal conductance model, our evaluation of the derivative is robust.

However, the discussion so far assumes that the variables in our model (Equation 8) do not change directly with VPD and we are free to evaluate the derivative. You bring up a very good example that we did not consider: a change in VPD will induce a change in net radiation through surface temperature that PM does not consider. We will definitely add language addressing this oversight. Ideally, we would incorporate that effect directly in Equation 8. We briefly tried to do this in time for this response, but difficulties deriving a diagnostic equation for temperature sabotaged the effort, and we do not want to delay the response further. We will continue to work on this for the final response phase. In the meantime, we think we can make some logic-based arguments in the interest of discussion as to what the effect would be. In the case where the aerodynamic and physiological ET response (which the manuscript does consider) increases with VPD, we would expect a decrease in surface temperature. This would induce an increase in net radiation, as OLR and ground heat flux decrease. This increase in net radiation would induce a further increase in ET response. Granted, the net radiation perturbation would damp the negative temperature response to the aerodynamic/physiological terms, but all of the signs of the change would stay the same. The converse is true for a decrease in ET with respect to VPD: surface temperature increases, OLR and ground heat flux increase, net radiation decreases, ET decreases. In this way, we believe the effects of VPD on radiation do not change the nature of the sign of the response, but could amplify the magnitude. Comments on this are encouraged, and we will work to include a more quantitative and rigorous analysis in the final response. Sometimes it is easy to trick oneself with these types of qualitative arguments, particularly with complicated systems, so hopefully we have not presented faulty logic in our rush to make this response public.

2 Major comment (ii)

Thank you for having an open mind about the concave up ET-VPD curve result and providing clear explanations of concerns about this result based on literature. Given the apparent controversy, we will definitely add more language connecting previous results, particularly at the leaf scale, with the presented results. Here we will informally elaborate in the interest of discussion.

One of the primary differences between ecosystem scale assessments of ET (specifically using Penman Monteith) and experimental and modeling results of leaf scale response to VPD is that by taking an energy balance approach Penman Monteith consistently accounts for the effects of the energy cost of evaporating water from a surface (e.g., changes in $e_{sat}(T)$). To demonstrate this, compare Figure 2 and Figure 3 ("Original Penman Monteith" Curve). In Figure 2 we apply a conceptual leaf scale approach to calculate ecosystem ET without the energy balance consideration introduced by PM. In this figure, the curve shows a concave downward shape, in direct analogy to leaf scale T. Once the effects of evaporation's thermodynamics are included in the energy balance, the curve's shape is no longer concave downward (Figure 3, "Original Penman Monteith" curve). How these conceptual results translate to leaf scale experiments depends on the experimental setup (e.g., *Rawson et al.*, 1977; *Turner et al.*, 1984; *Mott and Peak*, 2013); however generally chamber-based leaf scale experiments do not preserve the energy balance relationships we expect for a surface in a natural environment (sometimes intentionally by design). Additionally, we would like to note that a concave up result is not necessarily inconsistent with the statement "transpiration at the leaf scale is mostly positive" with VPD. Especially for plants in well watered environments like crops, the manuscript's ecosystem scale shape (Figure 3) would not be out of place on the figures in *Rawson et al.* (1977) and *Mott* (2007) for the range of VPD considered (Figure 3 compared to Fig. 2 in *Rawson et al.* (1977), also Figure 7 in *Mott* (2007)). However, the change in relative magnitudes are not similar, and this effect can be traced partially to the effect of decreasing surface $e_{sat}(T)$ in response to increasing evapotranspiration. While not directly considered here (or in the manuscript) we also think it is important to consider how plant physiologic response in the natural environment could vary from plants grown in well-watered conditions in experiments. It is not inconceivable that phenotypic variability and adaptation in response to water limitation could result in plants with different responses to those grown in a lab. We presented crops in our figures because they are the most likely to well watered and analogous with laboratory results; however for other PFTs the concavity of the VPD-ET curves is more pronounced at higher VPD as compared to Figure 3.

The previous paragraph discussed the change in frame of reference induced by using PM (and energy balance) to assess ecosystem scale ET rather than directly applying leaf-scale logic from controlled experiments. However, our uWUE-based PM framework additionally accounts for changes in photosynthesis induced by VPD (Figure 3). Discussion in *Damour et al.* (2010) ("The issue of co-regulation of g_s and A_{net}") provides some interesting background both on the general weaknesses of current stomatal theory, and specifically on the possible importance of resolving the effects of water stress on A_{net} in stomatal conductance. Given that the current state of the art in physically-derived theory has not converged on the proper way to account for changes in A_{net} and g_s (*Damour et al.*, 2010), we believe using the validated

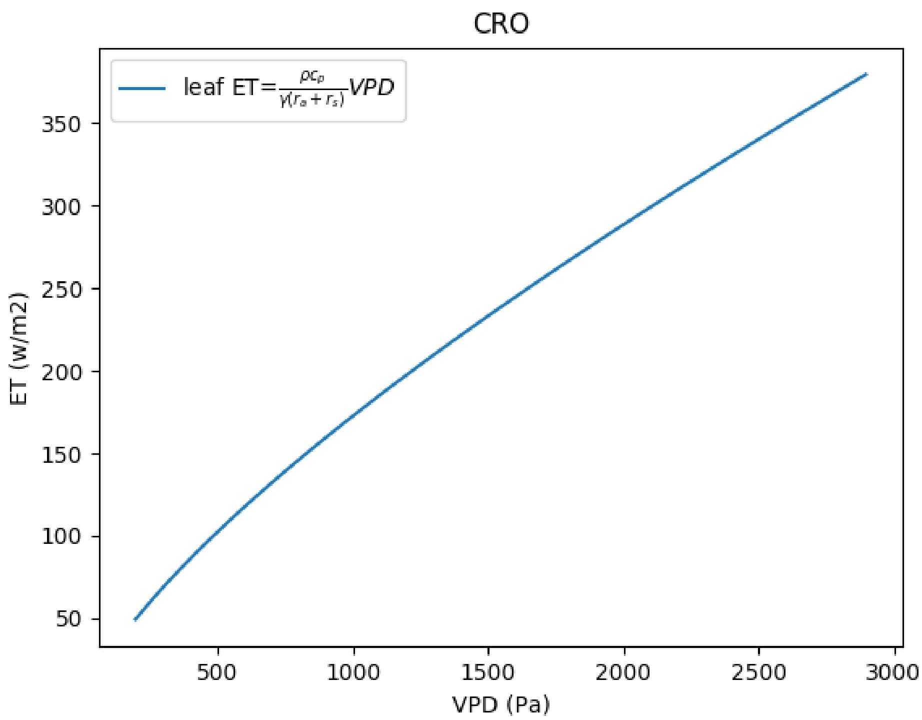

Figure 2: The conceptual relationship between ET with VPD using a model analogous to leaf scale models and experiments, evaluated with the study's median ecosystem scale resistances for the crop PFT. This estimation of ET is by definition not physically representative, but the figure is intended as a conceptual description of how leaf-scale theory's ET-VPD relationship deviates from energy balance ecosystem theory (Fig. 3). This figure is demonstrative, and not intended to be of publication quality.

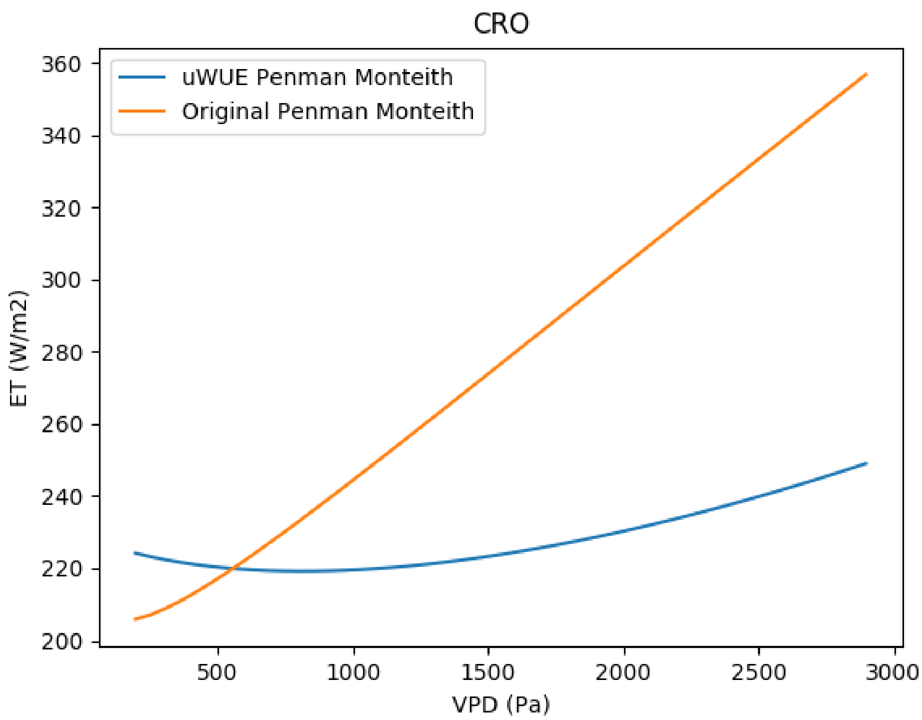

Figure 3: The conceptual relationship between ET and VPD using the uWUE derived version of Penman Monteith (Equation 8 in the manuscript) and the original Penman Monteith formulation (Equation 1 in the manuscript), evaluated at the study's median conditions for the crop PFT. uWUE Penman Monteith includes the effects of VPD on photosynthesis. This figure is demonstrative, and not intended to be of publication quality.

uWUE empirical results (e.g. Figure 3, *Zhou et al.*, 2014) is a good approach to develop an estimate of ET response to VPD accounting for changes in photosynthesis. This method of using an empirical result in the absence of a complete physical theory has been used often in science (e.g. turbulence) to increase our knowledge about a given system's behavior, and we think it is well motivated here. However, it is important to add language on how introducing this empiricism could alter interpretation of our results. In particular, while uWUE was developed using leaf scale photosynthesis theory, it was validated with observations of a fully coupled land-atmosphere system. It captures a relatively consistent observed relationship between GPP, ET, and VPD as they vary. There is a chance that some of this relationship could be due to feedbacks between the land and the atmosphere, even though the relationship was derived by leaf scale theory without feedbacks (e.g., if the assumptions behind the uWUE theory introduced "real" errors that were compensated for by feedbacks or ecosystem-scale processes in the observations). Were this the case, then we introduced a conceptual impurity to the statement "we are examining the one way response of ET to VPD." But, given the theoretical motivations for uWUE we think the portion of the relationship related to land-atmosphere feedbacks are likely small, and regardless we argue that this approach is still an improvement over ignoring the documented effect of water stress on net photosynthesis.

Given the continued gaps in our ability to represent stomatal behavior (*Damour et al.*, 2010), particularly for ecosystems in natural environments, we think it is hard to rule out our results. In the end, it comes down to the degree to which we trust our three tools: the Medlyn model, Penman-Monteith, and uWUE. To us these offered the most compete analytical approach to the problem at present, and are supported in the literature. However, it's important to emphasize that we are not saying this is certainly correct, and to do so would be irresponsible given the continued development of stomatal theory and how it translates to the ecosystem scale. What is important is to recognize that something as fundamental as the shape of the ET-VPD curve is still very much an area of open research, and sensitive to the representation of plant physiology (of which there are many possibilities) in a given model or framework. We thank the reviewer for having an open mind and giving us a chance to elaborate; others in the community have rejected this result off-hand, which reinforces for us the importance of demonstrating current uncertainty in VPD-ET relationships.

Just a quick final note:

We think that we miscommunicated with our (mis)use of "plant physiology" throughout the manuscript, and will alter our language in the final response phase to be more precise. Many times when we refer to "plant physiology" we mean any effect (direct or indirect) of an ecosystem response, e.g. any behavior that would not be observed over a wet or bare soil surface.

3 Major comment (iii)

Yes, through our manipulation of PM we have replaced stomatal conductance with two parameters (g_1 and uWUE). However, given that any physically reasonable representation of stomatal conductance will include more parameters (usually at least some sort of VPD-related parameter [g_1], and a model for photosynthesis with more parameters), we think we are actually reducing the number of unknowns in our model for ET. We agree with you that in reality g_1 and uWUE are spatially variable, and we elaborate in detail in our response to Reviewer 1 (RC1 Response) on why and how it might be better to frame our results more in terms of this variability. Also, see our comments in Section 1 on the effect of VPD on R_n and the suitability of a binning approach to VPD.

We also agree with you that uWUE includes a representation of ecosystem response. However, we would argue that this response dependence of stomatal conductance is not introduced by uWUE, but is actually an issue that exists inherently in any stomatal conductance theory that includes a photosynthesis term: photosynthesis depends on the physiological response and environmental conditions. Using uWUE is our (approximate) solution to this problem, and it works because we are able to algebraically manipulate PM to isolate ET after introducing uWUE. We do recognize that using uWUE introduces some uncertainty, but as previously discussed in this response we believe it is a useful tool for representing the photosynthesis dependence of stomatal conductance in an analytical framework, with the specific goal of examining VPD sensitivity.

4 Major comment (iv)

We agree on both counts: use of "leading order" is misguided and confusing in this context. Also, better separation of theory and empirical results would help communication/understanding.

Thank you again for such a thoughtful review. Including these insights will really improve the manuscript in the final response phase. In our response to Reviewer 1 (RC1 Response) we also laid out some alternatives to our approach for the final review phase not explicitly discussed here. If you (or anyone else) has any opinions on that please weigh in.

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