Thank you very much for an excellent and very thoughtful review. Addressing your comments will greatly improve the manuscript. We agree with your comments, and think the manuscript could use substantial re-framing and rewording to clarify how we are answering our research question, and how the answer may vary with climate and environmental factors.

Just a quick general comment before addressing your specific comments. By including a PFT-focused analysis we did not fully communicate the major goal and scope of our project: we are trying to characterize the response of ET to VPD, with all other environment variables held fixed. To accomplish this, we need to formulate an explicit function of ET in terms of environmental variables and parameters, where any parameters can be approximated as constant with regards to some [arbitrary] VPD perturbation scenario. We will elaborate more below on why we used PFT-focused scenarios for much of our analysis. However, the goal of our manuscript was much more simple and fundamental: at a given place or time, if you introduce a perturbation to VPD, what is the immediate ET response (e.g. positive or negative)? Answering this question does not require the much stronger assertion that any parameters must be invariant within a given PFT.

1 Compounding uncertainties

We agree that the uncertainties are large both within a PFT and across PFTs. Looking back, we believe that some of our language communicating *Lin et al.* (2018), *Medlyn et al.* (2017), and *Zhou et al.* (2015)'s results was misleading, and this was exacerbated by our focus on PFT-analysis. We will remove that language from the manuscript, and add language to better communicate our results as reflections of the considerable within-PFT uncertainty.

We think comparing the rankings of given PFT values for g_1 and uWUE can be misleading, given the magnitude of the uncertainties involved. Most of our calculated values for uWUE are within one standard deviation of *Zhou et al.* (2015)'s results, but these deviations can result in some changes in the ordering of PFTs from high to low uWUE. The bigger problem in our eyes is that we made a mistake with some language suggesting within intra-PFT variability is less than inter-PFT variability, but clearly this is not the case. We will remove this language. This misinterpretation should not have been in the manuscript and actually contradicts other manuscript content; for example, we included *Zhou et al.* (2015)'s results on uWUE in Table 2 explicitly to be transparent about the within-PFT uncertainties. g_1 values also exhibit considerable uncertainty, and again, while the relative rankings of these values may change, they are all within observed ranges in *Medlyn et al.* (2017). We did not include these in Table 2 because *Medlyn et al.* (2017) does not provide the numerical values; however, we think it would be useful to provide estimates of these ranges from *Medlyn et al.* (2017)'s figures in our Table 2.

Regarding how to interpret g_1 and uWUE: we think the best way is with established physical relations from *Medlyn et al.* (2011) (Equation 3 in the manuscript) and *Zhou et al.* (2014) (Pg. 7, line 10 in the manuscript), with the knowledge that there is uncertainty involved. The quantities in these relationships all have some intrinsic physical meaning, but can vary substantially within a PFT. Again, we need to alter our language to better reflect this intra-PFT variability.

However, *Zhou et al.* (2014) did establish a constant uWUE approximation as a good approximation for capturing the relationship between GPP, ET and VPD at a given place and time, so it is still a very useful and robust approximation for answering our research question (see discussion in the introduction of this comment). Additionally, a constant g_1 approximation is used in many earth system models, so while it introduces uncertainty, it also makes our framework useful for interpreting modeled vegetation response in ESMs and GCMs.

We also agree with the comment that the best way to think of this is probabilistically. This is the most robust approach to dealing with approximations - we introduce randomness to our variables and parameters to account for all of the physics that are not explicitly accounted for, as well as observational uncertainty. However, as far as we know we still have not developed a general, robust, and efficient arithmetic for random variables. We could try and adapt a Bayesian model to this problem, but given the large amount of arithmetic involved, fully incorporating a Bayesian representation to every variable in the analysis would be a very hard problem, and a significant research project in its own right. We think a good compromise is to add language directing the reader to interpret our results more probabilistically, which we have already presented probabilistically in the figures. For example, in Figure 5 the range of values in each plot represents a range of possible responses in the sign term. Within this figure, it's worth noting that the intra-PFT variability is greater than the inter-PFT variability, which is consistent with some of the previous results you highlight. Again, we need to add language highlighting this, and its consistency with previous results. We included this variability and uncertainty explicitly to be transparent. A more difficult question is how much of this variability is due to observational and model error, and how much of it is due to climate and plant physiological variability (see Section 3.5).

2 Attribution to physiological responses

We do not want our analysis to be interpreted as an assertion that g_1 and uWUE are attributes of PFT only. We expect them to vary both within a PFT and across a PFT. Our primary goal in using g_1 and uWUE was to develop an explicit expression of ET as a function of environmental variables, and use this to assess the response to a change in VPD. We focused our analysis using PFTs because this is how it was framed in previous studies in the field, and specifically the studies used in our derivation (*Zhou et al.*, 2014, 2015; *Medlyn et al.*, 2017). We were originally thinking the PFT-focused analysis could be useful, especially given that climate models generally hold plant parameters fixed with respect to PFT, so long as we were transparent about the large uncertainties and problems with this approach (see Figure 5).

We agree with you that stating that any quantity is fixed within a PFT is hard to believe. Phenotypic variation and adaptation within a given *species* can be considerable (i.e. effecting λ, g_1, and uWUE), so it would be hard to say that anything would be constant within a PFT made up of many different diverse species. Both phenotypic variation as well as the species distribution and dominant PFT at a given location will all be strongly optimized in response to climate. In this sense, the distribution and evolution of local climate is a strong control on the local structure and physiology of a given ecosystem.

What we need to communicate better is that a given ecosystem's state at a place or time controls its response to a VPD perturbation. We are making an approximation that we can parameterize the effect of the ecosystem's state on VPD response with g_1 and uWUE. These quantities can also vary due to soil moisture condition; the approximation we need to answer our research question is that they are fixed with respect to a VPD perturbation. In this sense, we do not view the two statements: "plants that are evolved to bred to prioritize primary production over water conservation (e.g., crops) exhibit a higher likelihood of atmospheric demand-driven response" and "ecosystem types are responding in this way because they have, on the whole, been subjected to less soil water limitation (due to the non-negligible effects of irrigation?)" as mutually exclusive. In fact, we view them as consistent with a view that crops and their physiology (parameterized by g_1 and uWUE) exist at a given time and place because they have not been subjected to soil water limitation, and they have a given response to VPD because of their physiology (which is a direct effect of the environment). In this way, climate and land surface state are causes of the VPD response both directly and through their effect on plant physiology (parameterized by g_1 and uWUE).

When writing this manuscript, there was definitely some internal tension and debate about how to best frame the analysis and results. We could either focus on PFT-oriented results as previous literature has done (e.g. holding plant physiology fixed within a given PFT), or allow plant physiology terms to vary through time and space and look at the distribution of ET response to VPD (see Section 3.5). After re-examining the manuscript both after some time away from the problem and in light of your comments we think a strong argument could be made that we made the wrong choice with respect to this focus. It may have made more sense to focus our analysis on the ET response more generally across space and time as ecosystem-scale plant physiology varies in response to climate and soil moisture.

3 Next Steps

In order to improve the manuscript and our communication of the answer to the question "When does VPD drive or reduce ET?", we see a few potential paths that we will consider between now and the final response after the discussion period. If you (or anyone else) has any opinions or comments in the meantime, we would appreciate the insight and feedback.

- **Option 1:** We include the discussion presented in this review and response, and include language explicitly stating that the purpose of the PFT analysis is to provide connections to other PFT-constant analyses and models (e.g. ESMs and GCMs). We will add extensive language on the uncertainty and weaknesses of this approach, and reframe our conclusions to reflect his uncertainty.
- **Option 2:** We instead alter our analysis to look at how VPD response

varies with climate and general plant physiological variability, instead of focusing only on PFT analysis that poorly captures all of the observed variability in ET response. Sections 3.1 - 3.2 would be replaced by analysis directly relating ET response to general climate and plant physiological terms, rather than PFT-mean analysis. For example, the "scaling term" analysis would be presented in terms of generic changes in plant height and temperature, and the "sign term" analysis would be framed by generic changes to g_1 and uWUE, as informed by previous literature.

- **Option 3:** This is the most extreme option in terms of modifying the manuscript. We significantly alter the manuscript and instead focus on just the general shape of the ET-VPD curve (with environmental variables held fixed). This was not discussed in the review, but one of our most noteworthy results is on the general shape of the ET-VPD curve being concave up, given an assumption of a square-root VPD dependence of ET. This result is independent of any assumptions of uWUE and g_1, and to our knowledge it is first derived curve of ecosystem response to VPD (see Section 3.7). It also highlights the importance of discerning the exact exponent of VPD dependence, as this alters the fundamental nature and shape of the curve. Essentially, the manuscript would become just our motivation and derivation, followed by Section 3.7 exploring the consequences of the derivation for the shape of the VPD curve. This alteration of the manuscript would likely result in more of a technical note-type paper, and we are hesitant to do this because we think there is still a lot of useful information obtained by tying our results to real-world scenarios (as in Options 1 and 2 above).

Here we present some final minor comments and concerns on the technical details of our proposed changes to the manuscript, which are relevant to representing uncertainty and spatiotemporal variability in uWUE *and* g_1. In the original manuscript we used a single σ term to represent this variability. We did this because changes in uWUE and g_1 induce a very similar change in the ET solution, which made solving for independent σ_{uWUE} and σ_{g1} terms intractable at a given time and place, and representing variability in both g_1 and uWUE difficult. We decided to hold g_1 fixed within a PFT for a two reasons: 1) ESMs and GCMs generally hold g_1 fixed, and 2) letting uWUE vary seemed more appropriate to represent specifically soil water variations on stomatal conductance, as uWUE modifies stomatal conductance analogously to how soil moisture factors modify a maximum stomatal conductance in land surface models (e.g. it is a multiplicative factor on the entire stomatal conductance term). Because uWUE and g_1 induce similar changes in ET, at least qualitatively we think that having a single σ can represent some of the variability in both $uWUE$ and g_1. However, formally there is no explicit variability in the g_1 term. The discussion in this review rightly points out that based on previous results we do expect some variability in g_1 as well. So, an unresolved question is how important is it to the analysis and its interpretation to include an explicit g_1 variability term in addition to the existing σ term. We are including these comments to hopefully stimulate some discussion on the importance of explicitly representing g_1 variability, given the difficulties of doing so within our framework. However, we could imagine some timescale based approaches where we might be able to account for both the g_1 and uWUE variability, for example by making assumptions over what time scale each quantity is fixed, and fitting based on that (e.g. g_1 is fixed for a given season, and uWUE is fixed for a given day). This approach might allow a tractable solution, and also could help us filter model error and observational noise from "true" plant physiological and climatic variability in uWUE and g_1. The cost of all of this is a significant increase in the analysis and content of the paper, as well as some increased opacity to the methods. Comments are welcome.

Thank you again for the thoughtful review. We hope it stimulates further discussion.

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