1  We thank the reviewer for their constructive comments and we address their various

2  concerns below. Referee comments are highlighted in bold, with our response below in

3  each case. We note that we made two earlier responses to the reviewer during revision,

4  this response now incorporates the key points of those interactions to make things easier

5  for the editor.

6

7  **Prompted by recent observations from chamber measurements of a decoupling**

8  **be- tween photosynthesis and transpiration at high temperatures, De Kauwe and**

9  **col- leagues examine eddy covariance flux data to see whether such decoupling can**

10  **be observed at the ecosystem scale. To my mind, this manuscript suffers from**

11  **several important inadequacies, and requires major revision before it would be**

12  **acceptable for publication. Anticipating that some of my criticisms will be viewed**

13  **as controversial, I will nonetheless lay them all out, so that the editor can**

14  **determine which (if any) deserve to be taken into consideration:**

15  **1. Both Tier-1 FLUXNET2015 data and OzFlux data suffer doubts regarding their**

16  **validity due to their persistent failure to demonstrate conformity with the**

17  **principle of energy conservation (i.e., to close the surface energy budget).**

18  **Although it might be going too far to say that it is inappropriate to download and**

19  **analyze such data as the authors have done, neither do I think it is correct for this**

20  **issue to be neglected entirely. Specifically, I am not aware that anyone has looked**

21  **at the effect of heat waves on the energy balance closure, but this would certainly**

22  **seem to be germane to the scientific questions that the authors are posing in the**

23  **context of dataset validity. Also, although the FLUXNET2015 database includes a**

24  **GPP variable, this is not measured by flux towers and the procedure from which**

25  **it is inferred is of dubious validity during conditions of extreme heat stress. Given**

26  **that the authors are attempting to tease out subtle temperature dependencies of**

27  **GPP (which is not measured directly) and LE (which fails energy conservation**

28  **checks), it seems inappropriate to me that such issues are not mentioned at all in**

29  **this paper.**

30  We appreciate the Reviewers concerns on this issue.

31

32  We note in response to their statement about GPP that on page 6 of our original

33  submission that we stated: *"Our analysis also relies on GPP which is not directly*

*observed but is instead modelled using assumptions related to the extrapolation of night-time respiration (ER) and measured net ecosystem exchange. It is debatable whether these assumptions hold at very high temperatures, and examining these modelled GPP estimates at high temperatures warrants further investigation particularly as researchers leverage these data to explore the responses of the vegetation to temperature extremes*."

In our revised discussion we have more fully addressed this concern: "*Our approach relies on GPP which is not directly observed but is instead modelled using assumptions related to the extrapolation of night-time respiration and measured net ecosystem exchange. It is debatable whether these assumptions hold at very high temperatures, and examining these modelled GPP estimates at high temperatures warrants further investigation, particularly as researchers leverage these data to explore the responses of the vegetation to temperature extremes. Eddy-covariance data are also known to have issues closing the energy balance (see Wohlfahrt et al. 2009, for a detailed discussion), which may introduce errors into the LE flux. For the seven Australian flux sites that make up the majority of our analysis, we calculated the ratio of the sum of latent and sensible heat fluxes to the sum of the net radiation and ground heat flux, finding on average a ~17% imbalance in the ratio (minimum=30%; maximum=7%). Importantly however, we did not find any difference in this imbalance in heatwave vs. non- heatwave days. Despite these limitations, FLUXNET eddy covariance flux measurements still present our best ecosystem-scale estimates of vegetation responses to heat extremes and have been widely analysed to address these types of questions (Ciais et al. 2005; Teuling et al. 2010; Wolf et al. 2013; von Buttlar et al. 2018; Flach et al. 2018).*"

**2. The paper draws no concrete conclusions, partly I think because the organisation of the manuscript is below standard.**

We would disagree with this interpretation. We draw no concrete conclusions because the data do not allow us to do so. In our paper we tested whether a photosynthetic decoupling mechanism identified in whole-tree chamber experiments (e.g. Drake et al. 2018, Global Change Biology), as well as other leaf-level experiments, was present at the ecosystem scale. As our results demonstrate, outside of the experimental

environment, it is difficult to isolate such a mechanism. We did not find strong support for the original experimental result. However, absence of evidence is not evidence of absence and, given the caveats attached to the data, more concrete conclusions would be unwarranted. Instead, we discussed the need for new field-based studies to tackle this issue further. Although we are unable to draw concrete conclusions, we nonetheless believe the analysis is worth publishing as this is the first study to test for photosynthetic decoupling at an ecosystem scale and as such, discuss the associated uncertainties. Our revised Discussion section also includes a route forward section, which may help satisfy the reviewer on the merit of the study.

**The paper contains about 1 page of introduction, 1.5 pages of methods, and 2.5 pages of "Results and discussion" to which will be added five figures and a table. This last section makes for difficult reading, in part because the authors appear to make little effort to distinguish between the facts and their inter- pretations thereof. Furthermore, the paper contains no equations whatsoever, despite the fact that the authors plot a variable (the product of GPP and the square root of the vapour pressure deficit) whose grouping cannot be justified (see comment number 3 below). All of these structural shortcomings make it particularly difficult for the reader to extract and evaluate the underlying message of the manuscript. I believe that the paper would be much better organised with a classical structure of 1. Introduction 2. Methods 3. Results 4. Discussion & 5. Conclusions.**

We have now reorganised our manuscript as the reviewer suggested, adding an improved Methods and new Discussion and Conclusion sections.

**3. According to the abstract, an important aspect of the paper addresses "the role of vapour pressure deficit" (D). The authors describe this in terms of the "theoretical expectation of the effect of D on $g_s$" (page 3, line 27), citing previous works in this regard. Although not explicitly appearing in this manuscript, the "equation" underlying this idea is eq. (7) from the 2011 paper by Medlyn et al., which is demonstrably in- correct. One of the major contributions to science of Joseph Fourier is the criterion of "dimensional homogeneity", which states that only quantities with the same dimen- sion can be compared, equated, added or**

**subtracted. An obvious example would be the ridiculous statement that one**
**kilometer is greater than one second. At the risk of sounding harsh, I must point**
**out that equation (7) of the Medlyn et al. (2011) paper is equally absurd, and**
**should not be considered as a "theoretical expectation". This ab- surdity seems to**
**me to be a likely explanation for the fact that no units are included on the abscissa**
**of Figure 5 of the De Kauwe et al manuscript, defined by a combination of**
**variables (again: the product of GPP and the square root of the vapour pressure**
**deficit; since it would be fitting for such a group of variables to be defined and**
**assigned a symbol, I will call it Beta). The units of Beta would necessarily include**
**the square root of a pressure unit such as mb or Pa (equivalent to the square root**
**of a kg m-1 s-2). My guess is that the unpleasantness of such a unit caused it to be**
**excluded in the axis label. I would argue that Beta should be rejected altogether**
**based on the powerful tool of dimensional analysis, which invalidates eq. (7) from**
**the 2011 Medlyn et al. paper.**

We have now clearly explained the theory that supports our analysis: "*As temperature*
*increases, vapour pressure deficit (D) also increases, which will drive an increase in*
*LE unless there is stomatal closure, but this effect is unrelated to the decoupling*
*mechanism we seek to find. To disentangle the potentially contributing role of D, we*
*also explored these data based on the theoretical expectation (Lloyd et al. 1991;*
*Medlyn et al. 2011; Zhou et al. 2014) that transpiration (E) is approximately*
*proportional to GPP $\times D^{0.5}$ (g C $kPa^{0.5}$ $m^{-2}$ $d^{-1}$; Eqn. 7). This expectation is based the*
*idea of optimal stomatal behaviour proposed by Cowan and Farquhar (1977) that*
*stomata should be regulated so as to maximise photosynthetic carbon gain less the cost*
*of transpiration. Medlyn et al. (2011) derived the optimal stomatal behaviour as:*

$$G_s = 1.6\left(1 + \frac{g_1}{\sqrt{D}}\right)\frac{A}{C_a} \tag{1}$$

*where $G_s$ is canopy stomatal conductance to $CO_2$ (mol $m^{-2}$ $s^{-1}$), A is the net assimilation*
*rate (μmol $m^{-2}$ $s^{-1}$), $C_a$ is the ambient atmospheric $CO_2$ concentration (μmol $mol^{-1}$), D*
*is the vapour pressure deficit (kPa), the parameter $g_1$ ($kPa^{0.5}$) is a fitted parameter*
*representing the sensitivity of the conductance to the assimilation rate and the factor*
*1.6 is the ratio of diffusivity of water to $CO_2$ in air. Assuming that transpiration is*
*largely controlled by conductance, this relationship can be rearranged to show that*
*water-use efficiency (A/E) is approximately proportional to $1/\sqrt{D}$. This dependence has*
*been remarked by many authors (e.g. Lloyd et al. 1991, Katul et al. 2009). Based on*

*this dependence, Zhou et al. (2014, 2015) proposed an "underlying water-use*
*efficiency" (uWUE) for eddy covariance data:*

$$uWUE \approx \frac{GPP\sqrt{D}}{E} \qquad (2)$$

*Zhou et al. (2014) argued that the $D^{0.5}$ term provided a better linear relationship*
*between GPP and E. Thus, to probe the effect of D, we focused on heatwaves (i.e.*
*approach 2) and plotted LE expressed as evapotranspiration (mm day$^{-1}$), as a function*
*of GPP$\times D^{0.5}$."*

Both of our earlier responses to reviewer argued that there was in fact no problem
with units, rather our original submission was simply not clear enough. We hope that
our revised text will now satisfy the reviewer that there are no further issues. We refer
the editor to earlier responses on this issue.

We have also added the requested units to the figure labels.

**4. The ordinates of figures 3 and 4 are labelled with "density", a variable that**
**normally would have units such as kg m-3. Rather, I believe that what the authors**
**have plotted is a frequency of occurrence, which is a fractional, non-dimensional**
**quantity that requires no units. However, since the values in figure 3 go well above**
**unity, I suspect that they should be described in terms of percent (%). In any event,**
**I think this needs to be clarified.**

The plot is correct, and the confusion here relates to the normalisation of densities in
the kernel density estimate. This is essentially the difference between probability mass
functions (discrete variable) and probability density functions (continuous), the former
no longer integrates to 1. We have now added "Probability density" to the figure label
and added an interpretation sentence to each of the figure captions.

**5. (This final comment may be viewed by the editor as excessively ego-centric on**
**the part of the reviewer. Nonetheless I feel obligated to point it out.) I have applied**
**the laws of physics to demonstrate that the paradigm underlying the definition of**
**the "stomatal conductance" is fundamentally incorrect (Kowalski, Atmos. Chem.**

**Phys., 17, 8177–8187, 2017), and furthermore to \*predict\* a decoupling of transpiration and**

**photosynthesis at high temperatures. The long-standing paradigm in ecophysiology presupposes all transport through stomata to be diffusive in nature, whereas my analy- sis, based on conservation of linear momentum, shows that non-diffusive transport also occurs in the form of "stomatal jets". In brief, because the exchange of water vapour dominates surface exchange of all gases, the evaporation rate defines a flow velocity away from the evaporating surface and consequent transport of all gases away from the evaporating surface. For the particular case of water vapour, the analysis shows that the specific humidity represents the fraction of water vapour transport that is non- diffusive. Students of thermodynamics know that, for a saturated environment such as that supposed by ecophysiologists within a stomatal cavity, the specific humidity increases nearly exponentially as a function of temperature. Thus, at extreme temper- atures the role of non-diffusive transport becomes non-negligible and a decoupling is expected between exchanges of water vapour (whose egress is aided by non-diffusive transport) and carbon dioxide (whose ingress is opposed by the outgoing Stefan flow). At the extreme limit of the boiling point, the vapour pressure inside the stomatal cavity would equal the total air pressure, meaning that (1) water vapour would be the lone gas inside the stomatal cavity, therefore (2) no diffusion could occur, and all transport would be non-diffusive (i.e., a specific humidity of 100%), and therefore (3) no photosynthesis would be possible (with no CO2 present). Since my analysis is soundly based on the laws of physics and satisfactorily explains the decoupling between photosynthesis and transpiration at high temperatures, I believe that the authors should take it into account when exploring this "previously overlooked vegetation-atmosphere feedback that may in fact dampen, rather than amplify, heat extremes". However, I hardly think it is my place to insist that other scientists cite my papers, and so must leave judgement of this matter to the editor.**

We thank the reviewer for their insight on this issue. Despite our back and forth discussion on this topic, we still maintain that that in order to argue for a paradigm shift ("*paradigm underlying the definition of the "stomatal conductance" is fundamentally incorrect*"), a certain weight of evidence, including measurements, will be required.

195    We further thank the reviewer for spelling out the hypothesis regarding the effect of

196    temperature presented in their paper. Their hypothesis is that WUE should decline as

197    temperature increases because of the change in specific humidity with temperature.

198    This hypothesis is actually consistent with our baseline theoretical expectation that E

199    is proportional to GPP $\times$ D$^{0.5}$ where D increases with temperature. The hypothesis

200    does not predict the divergence from proportionality under temperature conditions

201    that we are interested in, and hence we maintain that it is not directly relevant to the

202    work presented here. However, if the editor feels we should refer to this work, we will

203    of course abide by their decision here.