We thank the reviewer for their constructive comments and we address their various concerns below. Referee comments are highlighted in bold, with our response below in each case.

Prompted by recent observations from chamber measurements of a decoupling be- tween photosynthesis and transpiration at high temperatures, De Kauwe and col- leagues examine eddy covariance flux data to see whether such decoupling can be observed at the ecosystem scale. To my mind, this manuscript suffers from several important inadequacies, and requires major revision before it would be acceptable for publication. Anticipating that some of my criticisms will be viewed as controversial, I will nonetheless lay them all out, so that the editor can determine which (if any) deserve to be taken into consideration:

1. Both Tier-1 FLUXNET2015 data and OzFlux data suffer doubts regarding their validity due to their persistent failure to demonstrate conformity with the principle of energy conservation (i.e., to close the surface energy budget). Although it might be going too far to say that it is inappropriate to download and analyze such data as the authors have done, neither do I think it is correct for this issue to be neglected entirely. Specifically, I am not aware that anyone has looked at the effect of heat waves on the energy balance closure, but this would certainly seem to be germane to the scientific questions that the authors are posing in the context of dataset validity. Also, although the FLUXNET2015 database includes a GPP variable, this is not measured by flux towers and the procedure from which it is inferred is of dubious validity during conditions of extreme heat stress. Given that the authors are attempting to tease out subtle temperature dependencies of GPP (which is not measured directly) and LE (which fails energy conservation checks), it seems inappropriate to me that such issues are not mentioned at all in this paper.

We appreciate the Reviewers concerns on this issue.

We note in response to their statement about GPP that on page 6 of our 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 estimate estimates at high temperatures warrants further investigation particular as researchers leverage these data to explore the responses of the vegetation to temperature extremes*."

In revision, we will add a caveats section to our new discussion section (see next response) where we will discuss issues related to the GPP data and the energy balance closure issue in relation to the latent heat flux. Furthermore, despite caveats, eddy covariance data represent one of our key constraints on the carbon, energy and water cycles and are regularly used to probe ecosystem responses to extremes (e.g. von Buttlar, et al. 2018: Impacts of droughts and extreme-temperature events on gross primary production and ecosystem respiration: a systematic assessment across ecosystems and climate zones, Biogeosciences, 15, 1293-1318).

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. 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) was present at the ecosystem scale. As our results demonstrate, outside of the experimental environmental, it is difficult to isolate such a mechanism. In so far as we can draw conclusions from the FLUXNET data, we did not find strong support for the original experimental result. However, absence of evidence is not evidence of absence and as result, to be more concrete with our conclusions given some the caveats of the data felt unwarranted. As a result, we discussed the need for new field-based studies to tackle this issue further.

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 are happy to reorganise our manuscript as suggested by the reviewer and this will allow us to tackle the issue they highlighted in their first comment.

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 will add the equation underlying the analysis; the equation is given in the corrigendum to the Medlyn et al. (2011) paper, as well as many publications since, and is as follows:

$$g_s \approx 1.6(1 + \frac{g_1}{\sqrt{D}})\frac{A}{C_a}$$

Where g_s is stomatal conductance (mol m^{-2} s^{-1}), A is the net assimilation rate (μmol m^{-2} s^{-1}), C_a is the CO_2 concentration (μmol mol^{-1}), D is the vapour pressure deficit (kPa) and the parameter g_1 (kPa$^{0.5}$) is a fitted parameter representing the sensitivity of the conductance to the assimilation rate. A full derivation for this equation is provided by Medlyn et al. (2011). It is unclear why the reviewer thinks it is "absurd" – the equation is dimensionally correct. We agree that one should not equate different dimensions, but it is perfectly sensible to relate different dimensions: an equation may relate degrees of temperature to metres gained in elevation, for example.

Regarding the Figures: as explained in detail in the paper by Medlyn et al. (2011), it is not possible to visualise this non-linear relationship directly, but a useful approximation that allows the relationship to be visualised is to ignore the "1+" term and plot g_s vs $A/(C_a\sqrt{D})$. The slope of this relationship is then related to the parameter g_1. This visualisation approach is taken here but expressed in terms of transpiration. We can add further explanation of this visualisation approach to the text.

We did not include units in a similar way to other authors who have expressed water use efficiency in this fashion (e.g. Zhou, S., B. Yu, Y. Huang, and G. Wang (2014), The effect of vapor pressure deficit on water use efficiency at the subdaily time scale, Geophys. Res. Lett., 41, 5005–5013, doi: 10.1002/2014GL060741.). We are happy to include units on the axis of the revised figure. In addition, we will also add to the revised methods a fuller explanation for where this equation comes from.

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 will clarify this point in our revision.

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 CO_2 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. However, we think 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.

We will of course abide by the editor's decision here, but our feeling is that it would not really be appropriate to add any text regarding this work, given its relative newness and the fact that the paper referred to does not make explicit predictions for behaviour under heatwave conditions, nor even with rising temperatures. We instead would encourage the reviewer to develop their theory to make a prediction for the relative size of decoupling under heatwave conditions and test this against our published data (Drake et al. 2018, both the data and code to repeat the analysis are freely available). It might provide some empirical support for this novel and untested idea.