**Response to reviews**

for Drake *et al*. “*The partitioning of gross primary production for Eucalyptus tereticornis trees exposed to experimental warming and drought*”

We received three positive reviews of our manuscript. We are grateful that our colleagues saw value in our work, and we are happy to incorporate their suggestions in an effort to improve our manuscript.

In the text below, we respond to all reviewer comments. Reviewer comments are numbered and bolded; our responses follow in plain text. We made many alterations to the manuscript to reflect the excellent advice provided by the reviewers. The line numbers quoted in our response reflect those of the document showing our edits in “track changes”. We thank the reviewers for their suggestions and constructive criticism.

**Editor:**

**Thank you for submitting your manuscript to New Phytologist. Three referees have provided comments that you may find below this message. Based on these comments and my own look at the manuscript, the decision is accept pending minor revision. My largest concern, which is echoed by some referees, is the rather small drought treatment that you imposed and the use of such small plants. I think if you are very forthright in the revision, including in the title, abstract, results, and discussion, about these limitations, then this could be publishable. Due to these limitations, please also strongly highlight what is novel about your paper, so that it may garner more attention and value to society. Finally, please also address the other valuable comments that the referee's raised. Please include a cover letter with your revision that describes how you have addressed the referee concerns within the revised manuscript. I must mention that to have all three referees provide such positive reviews on a manuscript is rare, so congratulations on a nice product.**

Dear Dr. McDowell,

Thank you for your comments and your summary of the reviewer’s primary concerns. We address these concerns in detail below. In short, we addressed the size and drought issues in the title, abstract, methods, results, and discussion sections. We removed the word “drought” from the title and abstract to address this recurring concern. We address the issue of whether the trees were droughted in detail in response to comment #1 of reviewer #2. We highlighted the novelty of our study at the beginning and the end of the discussion section.

**Referee #1:**

**A warming and drying experiment was performed on Eucalyptus seedlings using an existing experimental system of whole-tree chambers. Warming-induced increases in growth and respiration were stronger above- than belowground. Drought reduced C gain and growth, without changing this partitioning. This a good, very well written manuscript, with fine analyses.**

We thank the reviewer for his or her complimentary summary of our work.

**My only concerns are about (1) the power of the experiment, with observations on seedlings for a period <1 year,**

This reviewer and the editor raised this issue of tree size and experimental duration, suggesting that the experiment was limited because it was done on very small trees for a short time. We suggest that this reflects a slight misunderstanding of the tree size and growth during this experiment. First, the warming experiment lasted 15 months, which is slightly longer than 1 year. Second, yes, the trees began the experiment as small seedlings that were ~40-cm-tall and 2.5-mm-diameter. But these trees grew exceptionally quickly, and were >60-mm-diameter and ~900-cm-tall at the end of the experiment; this was shown directly in Figure 1. A tree of 6-cm-diameter and 9-meter height is much too large to be considered a “seedling”. We note that the United States Forest Services classifies “seedlings” as trees < 1 inch in diameter and “saplings” as trees from 1-4.9 inches in diameter (<https://www.nrs.fs.fed.us/fia/data-tools/state-reports/glossary/default.asp>). We are happy to reiterate tree size and experimental duration in the manuscript, but we do not think that this “seedling” criticism is warranted in this case. These trees were an order of magnitude larger in mass when they were harvested than in other warming experiments with “seedlings” (e.g., Reich *et al.*, 2016).

We edited the title, abstract, methods, and results sections to emphasize the tree size and experimental duration.

**and (2) the impact and novelty of the results, with relatively small treatment effects. Overall, this study demonstrates the high level of plasticity and sustainability of the studied species, which has been observed in other Eucalyptus species.**

There are elements of this manuscript that are confirmatory. However, there are also elements of this manuscript that are novel. We directly quantified the allocation of gross primary production (GPP) for relatively large trees growing in ecologically meaningful field conditions, while also implementing a factorial warming and drought experiment. Our combination of detailed and continuous growth and CO2 flux measurements is also novel. We also note that the warming effect did significantly affect allocation. We think that the modest nature of the drought effect on allocation is actually quite interesting and informative to this field.

**Below are a few comments on issues that came up while reading:**

**1. In the title, ‘saplings’ or ‘young trees’ should be used instead of ‘trees’.**

Done. We chose to specify these as “young *Eucalyptus tereticornis* trees”, as suggested. For the reasons above, we think that using “seedling” in the title would give the reader the wrong impression regarding the size of these trees.

**2. Similarly, the abstract should report the age or size of the young trees, as well as the span of the experiment.**

Done.

**3. Throughout the manuscript, the use of ‘W’ for warming is a bit confusing with ‘W’ for wet (vs. dry here). Earlier studies used ‘H’ for heating, alternatively.**

We have consistently been using “warming” in this and our many other papers regarding temperature experiments, and we would like to continue using this convention. We have addressed this reviewer’s concerns by altering the word choice for the drought component of this work. We now use the words “ambient” and “warmed” to denote the temperature treatments, and “control” and “dry” to denote the water treatments. These treatments no longer share a beginning letter, which addresses this concern.

**4. Are the definitions used for growth respiration and maintenance respiration rates mutually exclusive? This should be discussed, as a C balance approach would not permit this.**

The conceptual separation of autotrophic respiration into growth and maintenance components is a relatively old idea, attributed to classic works from the 1970s (reviewed by Amthor, 2000). This approach is actually used fairly frequently in ecosystem C studies; for example most ecosystem models of C cycling utilize this approach directly (reviewed by Medlyn *et al.*, 2016). However it is generally not possible to measure ecosystem respiration via an approach like eddy-covariance and then partition that flux into growth and maintenance components. We added a sentence to address this comment to the methods (~line 270).

**5. Comparison of means across treatments are needed, e.g. as letters in bar graphs in Figs 2, 3, and 6.**

This was done in the text directly. We reported the statistical significance of each main and interactive effect.

**6. In Fig. 3, it would be nice to see SEs for individual tree compartments.**

We attempted to implement this suggestion, but this resulted in a messy graph that was challenging to interpret. The vast majority of these components do not differ between treatments

**7. In Fig. 4, consider adding labels for soil depth values directly on the panels.**

Done.

**Referee #2:**

**In this paper the authors look at how temperature and water stress impact carbon allocation and growth and find that warming increases aboveground allocation. Droughting the trees reduced carbon but did not affect partitioning. The authors hypothesize that this lack of a partitioning effect is due to the fact that the trees tapped deep water (as is evident from the relatively high predawn leaf water potentials in the drought trees). I found this study to be interesting and well written, and I think the topic of allocation is extremely important to the fields of both plant physiology and Earth system science.**

We thank the reviewer for his or her summary of our work.

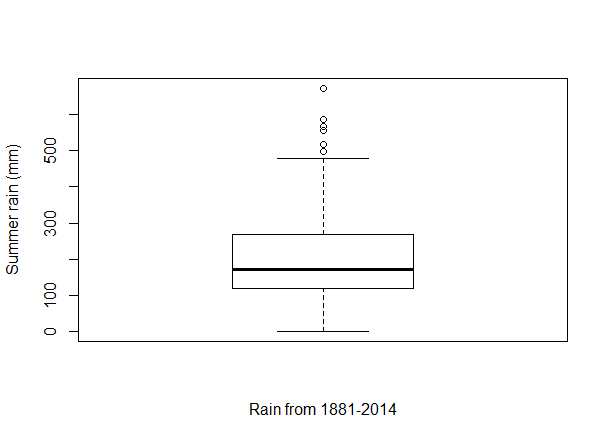
**1. The main point I want to put a little extra emphasis on is that the authors did not test the impacts of extreme drought on tree physiology because they were not successful at droughting the trees as much as they had intended. The authors are fairly up front about this in most locations, but should avoid phrasing with ‘extreme drought’ in it. I am convinced from most of the results that the drought treatment was somewhat successful, but given that predawn water potentials did not become that much more negative relative to the controls, the severity of the drought should be de-emphasized.**

We understand this concern, and we are happy that the reviewer noted our upfront approach to this issue.

Whether the drought was “successful” depends on what metric you focus on. We were quite successful in eliminating precipitation for nearly three months and drying the soil to a very low volumetric water content from the surface down to 1-m-depth (Fig. 4a-c). We have a long-term rain record for this location (1884-2014), and the average rainfall during this period is 208 mm (see plot below). There are only three exceptionally dry years in this record where the total rainfall during the summer months of February-April was near zero (less than 10 mm), which is ~2% of the years on record. Thus, we successfully implemented an extreme drought in terms of surface water addition that occurs in this location on an approximately 1:50 year timescale. We added this content to the methods at ~line 170.

The reviewer is quite correct, however, that the leaf water potentials did not decline to strongly negative values that would be indicative of acute physiological water stress, which we attribute to deep soil water use, particularly from 1-2 meters in depth. We understand the reviewer’s perspective that we did not “successfully” drought the trees, but that is not our interpretation. Our interpretation is that the trees “successfully” avoided acute physiological water stress via a biological drought response that involved (1) reduced growth rates, (2) reduced transpiration rates, and (3) facultative deep soil water usage. We suggest that the lack of strongly negative water potentials is part of an interesting biological response, rather than an unsuccessful treatment.

We have removed the word “extreme”, as suggested, and added further discussion on this point in the discussion (~line 468).



**Other minor comments are listed below:**

**2. L27, write out ‘carbon’ instead of C the first time**

Done.

**3. L31, the authors say later that the trees were perhaps not droughted much due to access to groundwater. This needs to be made clearer throughout. And here in particular the use of ‘extreme drought’ seems to be mis advertising in this context**

We removed “extreme” from this section. We expect that our clearer explanation of the “drought” issue has addressed this issue.

**4. L46, write out ‘carbon’ instead of C the first time**

Done.

**5. L85-87, it would be nice to say what the effect on physiology is**

This was a topic sentence for an introductory paragraph that read: “Temperature is a fundamental aspect of climate that affects many aspects of tree physiology (Way & Oren, 2010; Lu *et al.*, 2013).” We intentionally used broad language here, as the effect of temperature on tree physiology is highly-multifaceted. We added some specific examples to the text, as requested.

**6. L90, ‘Find’ not ‘found’**

Done.

**7. L96-108, Some additional references should be included. See the below list**

**Yang, J., Medlyn, B. E., De Kauwe, M. G. & Duursma, R. A. Applying the concept of ecohydrological equilibrium to predict steady-state leaf area index. J. Adv. Model. Earth Syst. (2018). doi:10.1029/2017MS001169**

**Martinez-Vilalta, J. et al. Hydraulic adjustment of Scots pine across Europe. New Phytol. 184, 353–364 (2009).**

Done (~line 99).

**Trugman, A. T. et al. Tree carbon allocation explains forest drought-kill and recovery patterns. Ecol. Lett. (2018).**

Done (~line 52).

**Detto, M., Wright, S. J., Calderón, O. & Muller-Landau, H. C. Resource acquisition and reproductive strategies of tropical forest in response to the El Niño-Southern Oscillation. Nat. Commun. 9, 1–8 (2018).**

We chose not to incorporate this one. It presents an index that is arguably related to allocation (leaf litter relative to seed litter), mostly in a phenology context. We could not find a way to naturally integrate this into our manuscript.

**Jump, A.S., Ruiz-Benito, P., Greenwood, S., Allen, C.D., Kitzberger, T., Fensham, R., et al. (2017). Structural overshoot of tree growth with climate variability and the global spectrum of drought-induced forest dieback. Glob Chang Biol.**

Done (~line 100).

**8. L158, Do the authors know how deep the roots go? This would be useful information if available**

No, we don’t know the maximum rooting depth. This is quite challenging to measure in practice. We know that when we excavated the soil and roots upon harvest, a few small sinker roots in each chamber did penetrate through the hard layer (i.e., were >1 meter deep). We also documented soil water depletion from 1-2 meters depth (Fig. S1), which is suggestive of roots at those depths. We added some of this information (~line 235).

**9. L163, The authors mention later that the droughted trees may not have actually been fully droughted. It would be good to mention this here**

It would be impractical to describe this issue here in the methods section, as doing so would require describing the leaf water potential results. We think that this is best described in the results and discussion sections. We specifically mention this in the results (~line 341) and the discussion (~line 470).

**10. L185-187, It would be good if the authors gave a brief 1-2 sentence summary on this technique here in their text to orient the reader and then reference the Drake et al study**

This entire paragraph is devoted to describing this technique, and finishes with a reference to the Drake paper. Thus the original manuscript accomplished what the reviewer is requesting, although there was apparently some confusion on this point. We addressed this confusion by removing the first reference to the Drake paper early in the paragraph, making it clearer that all of this text refers to the approach, with the Drake citation at the end.

**11. L193, the Drake study ref has a formatting problem**

This line read: “The underlying flux data and the partitioning approach were published previously (Drake et al., 2016ab).” This citation refers to a 2016 *New Phyt* paper, and an associated dataset published on Figshare. Both are important, hence the “a” and the “b”. The figshare citation is unusual, and we will work on getting this cited correctly during the typesetting process.

**12. L233, if the authors are claiming that climate changes allocation, how can they use allometries based on pretreatment trees to predict branch wood mass. At least justify this or state the implicit assumption in using non-specific allometries (or maybe I am mistaken in which case the authors should clarify this in the text)**

We sampled branches for the branch allometry from each of the experimental trees near the end of the experiment. Thus we did not use pretreatment data. We clarified this in the methods (~line 250).

**13. L255, did any carbon go to reproduction? If not, this is probably reasonable but it should be explicitly stated that no carbon went to reproduction**

Allocation to reproduction was not quantified, as these trees did not produce any reproductive structures. We added this to the methods (~line 243).

**14. Question for the growth measurements section, are the authors using treatment-specific allometry/SLA or is this an average over all trees? I realize that it is asking for a lot work to have treatment-specific values, but if the premise of the paper is that climate impacts allometric traits then this needs to be done or else listed as a caveat**

Allometry was not used to measure stem mass. Branch mass was estimated by a general allometry across all treatments (we tested to treatment effects on this allometry and found none; line 254). SLA values were better than treatment-specific; they were tree specific (line 261).

**15. Fig 1, Fig 2, Fig 4, Fig 5, Fig 6 it would be nice to include a dashed vertical line to indicate when the drought treatment began on each time series panel as well**

We chose not to implement this suggestion. All of these graphs show the drought data explicitly- it is quite straightforward to see when the drought began. Thus we feel that it is better not to add another element of clutter to the graph. We will of course defer to the editor if these additions need to be made.

**16. L311, ‘significantly differ’ instead of ‘differ’**

Done.

**17. Fig 3, what is the dashed black line?**

We expect the reviewer meant “Fig. 4”, as there is no dashed black line in Fig 3. This dashed black line reflects the volumetric water content at which the soil matric potential is -1.5 MPa, which is often defined by agronomists as the soil permanent wilting point. As wilting points depend on the plants as well as the soil, we have avoided using that specific term. We added a sentence to the Fig. 4 legend to describe this.

**18. L331-333 something of this effect should be mentioned in the methods**

This comment refers to neutron probe measurements of soil moisture in deep soil. These measurements were described in the methods of our original submission (line 184).

**Referee #3:**

**1. "The partitioning of gross primary production for Eucalyptus tereticornis trees exposed to experimental warming and drought" study was aiming to study the effect of warming and drought, and their interaction to tree C allocation to above-ground (tree crowns) and below-ground (fine and coarse roots), in E. tereticornis, one the most abundant Eucalypt species, in eastern Australia. The novel aspects of this study are that, the authors studied the effect of global change factors, temperature and drought, on carbon allocation under ecologically realistic field settings. In addition, the CO2 and H2O flux measurements as well as growth measurements have a very high resolution. The study concludes that there was no interaction of warming and drought to C partitioning terms. Warming did increase the C allocation to aboveground biomass and respiration. However, drought treatments did not affect the GPP partitioning. There was an interaction of warming and drought on final root mass ratio.**

**To the best of my knowledge this is the first study that attempts to investigate the combined effect of warming and drought on carbon partitioning in trees under realistic field conditions. In general, the research paper is written in a way that is to understand and follow the logic of the research, except in a few areas of the methodology section that needs more clarifications.**

**In my opinion, this research comes in time and add new knowledge on tree C partitioning. The following minor issues should be reviewed:**

We thank the reviewer for his or her summary of our study. We are happy that this reviewer found our work to be a valuable addition to the field, and we appreciate the suggestions for changes.

**2. line 120, I would suggest to phrase that the study looks at effects of warming, water availability on the tree carbon allocation.**

Done. We also used this suggested language of “water availability” in the title and abstract in place of “drought” in an attempt to address the issue of whether the trees were droughted in this experiment.

**Methodology**

**3. line 201, could you please clarify how many 1-cm-thick cross sections were sampled at each layer? This will provide a clear idea of how many points were used in the regression analyses.**

We sampled a single cross section at each layer for each tree. We addressed this specifically on line 224.

**4. line 204, were the wood and bark densities similar across the 3 sampled crown layers? It is not clear to me, could help if clarified much better.**

As described in this section, bark and wood density declined slightly as stem diameter increased. This means that these densities were higher in the upper regions of the tree crowns relative to the tree bases. We added a sentence to this section to clarify this (line 232).

**5. line 217, it would be good to clarify on how you measured the root density.**

We measured the density of fine roots in the soil (i.e., grams of root per cubic meter of soil). We did not measure the structural density of the roots themselves. We clarified this on line 242. Note that this entire paragraph was devoted to describing how we measured fine root biomass.

**6. line 224, how did you estimated exactly the biomass? It would be better to add details on how you measured it or clarify a bit more.**

This line refers to the topic sentence of this section: “Aboveground biomass was estimated every two weeks for each tree as the sum of leaf, branch, wood, and bark mass…”. The following paragraphs describe the methodologies for each component in detail. We added a sentence here such that subsequent readers don’t have the same confusion (line 254).

**7. line 232, I understand you calculated the stem volume every two weeks during the experiment, and then you also mention that to calculate the wood and bark mass you multiplied the volume by the density, but my question here is, doesn't the density changes during the growth of a tree? It could be better if you could clarify here and if there is any assumption implied regarding changes of wood density over time.**

We could only measure wood and bark density at the end of the experiment, during the final harvest. Our approach assumes that wood and bark density did not vary through time. We added this assumption to line 262.

**8. On the Figure 4 (a-c), I see empty circles for both ambient and warmed treatment from Dec to Feb, but the drought experiment didn't start until mid-Feb. So it is unclear to me why drought treatment is represented during the period it wasn't started yet.**

This figures shows the soil volumetric water content for all four treatment combinations prior to the drought, during the drought, and during the recovery phase. We show these treatments to illustrate that trees assigned to the “drought” treatments had equivalent soil water content values as the “control” trees prior to imposing the drought. We think this will be useful for some readers, so we have left this figure as it is.

**9. In the conclusion I'd prefer to see a conclusion on drought as well, as for now the focus is on warming only.**

Done.

References

**Amthor JS**. **2000**. The McCree–de Wit–Penning de Vries–Thornley Respiration Paradigms: 30 Years Later. *Annals of Botany* **86**: 1–20.

**Lu M, Zhou X, Yang Q, Li H, Luo Y, Fang C, Chen J, Yang X, Li B**. **2013**. Responses of ecosystem carbon cycle to experimental warming: a meta‐analysis. *Ecology* **94**: 726–738.

**Medlyn BE, Kauwe MGD, Zaehle S, Walker AP, Duursma RA, Luus K, Mishurov M, Pak B, Smith B, Wang Y-P, *et al.*** **2016**. Using models to guide field experiments: a priori predictions for the CO2 response of a nutrient- and water-limited native Eucalypt woodland. *Global Change Biology* **22**: 2834–2851.

**Reich PB, Sendall KM, Stefanski A, Wei X, Rich RL, Montgomery RA**. **2016**. Boreal and temperate trees show strong acclimation of respiration to warming. *Nature* **531**: 633–+.

**Way DA, Oren R**. **2010**. Differential responses to changes in growth temperature between trees from different functional groups and biomes: a review and synthesis of data. *Tree Physiology* **30**: 669–688.