Dear Editor:

We are pleased to submit a revised version of our manuscript, “Thermal sensitivity across forest vertical profiles: patterns, mechanisms, and ecological implications” (NPH-TR-2021-38020), for consideration for publication in *New Phytologist*. We have addressed all points raised by the reviewers, as detailed below. The most significant changes include:

…

Regarding production of the figures, below are some specific criteria:

* It will generally be fine to adjust colors, layout, and spacing on the figures.
* We really like Figure 1 and would like to retain the background painting (by the lead author)

Thank you for considering this revised version, and we look forward to your response.

Sincerely,

Nidhi Vinod & Kristina Anderson-Teixeira (on behalf of all coauthors)

# Response to Reviewer(s)’ Comments to Author:

## Referee: 1

Comments to the Author I am satisfied with the corrections and the responses to the comments. I like the new structure, the writting is very precise while covering broad areas and the paper very interesting.

**Thank you.**

Some minor points :

-PAR units in Fig 2d

-Fig 3 caption: TLeaf and some other discreptances with rest of the paper abreviations or format (tair, gs)

-l304 Not sure about the structure of the sentence and causality. Gravity and hydraulic pathlength (due to height) could mitigate the effect of light alone ?

-Table 2, gb unit, mmol m-2 s-1 ?

-I found the x axes labels in Fig 4.a difficult to understand, there is maybe a more intuitive way to write them. Note that PET is not defined. °C for the units.

## Referee: 2

Comments to the Author The authors seek to tackle a fairly complex topic in this review article. Whilst the topic of vertical variation in forest canopies is fairly specialised, they have pulled together quite a large body of literature and the review cuts across the fields of micrometeorology, physiology and ecology for forests, and many will want to read and perhaps cite the review.

**Thank you.**

The authors have done a good job of addressing the reviewers’ comments from my inspection. This reviewer apologises for needing to raise additional points that were not raised in the previous reviews of the article, but some conceptual aspects and some of the writing are perhaps flawed or require clarification. There are some additions done in the review process that have confused things, or a few aspects that were not picked up in the initial review. The authors and readers both benefit from a clearer picture emerging in this review. There are still a number of places where the authors could improve and one cannot resist letting these past at this point.

The words ‘disproportionate’ or ‘disproportionately’ are used l. 33, 35, 504, 648, 814, 838. For instance, l. 35 ‘disproportionately impacted under hot, humid conditions’: disproportionate to what? The implication is disproportionate to canopy trees, but there is almost no evidence of this. The authors need to be clear what the comparative is in each case that a form of ‘disproportionate’ is used, especially in their abstract.

In fact, the abstract reads like the authors cannot decide whether tall trees ‘… are disproportionately vulnerable to drought and damaging 𝑇𝑙𝑒𝑎𝑓’ (l. 33) or understory trees (l. 35). It cannot be both if one is comparing them. In this aspect the abstract comes off indecisive or unclear. This is an element in the review as well (see below).

I have issue with the ecosystem level being mentioned in the Abstract. There was little true ‘ecosystem level’ assessment done in the manuscript, and most of the ‘ecosystem level’ inferences are based on these statements which are indirect:

l.744 ‘Because canopy trees dominate these fluxes (Fig. 5), their responses will strongly influence the whole-ecosystem response’

l.898 ‘disproportionate role of large trees in ecosystem dynamics’

So perhaps the authors can greatly soften their ecosystem level assessment. To their credit, and what truly should be emphasised, is the further research needed on ecosystem function (l. 757).

l.32 tall trees have higher absolute metabolic capacity and growth at both individual *and ecosystem levels* (please remove the text between the asterices).

I make a major point of revisions around this idea (l. 35-36) that ‘understory trees may be disproportionately impacted under hot, humid conditions’. What evidence is there? There is speculation about what might happen in sunflecks (l. 800-801) but little direct evidence, and there is Fig. 4b which is one study with smoothed results that are not convincing (see above). Yet this statement is contrary to statements made in the actual review, like l. 200-201 to the effect that upper canopy is warming with higher VPD than understory, l. 232 that there are higher leaf-air temperature difference in the upper canopy than understory, l. 275-276 that leaves in the upper canopy have higher leaf-air temperature difference and maximum temperatures than understory, also l. 775-776. If so many arguments have been made for a harsher environment and higher leaf temperatures in the upper canopy than understory, it comes off as illogical to reverse and conclude on the basis of Fig. 4b which is flimsy evidence that, as stated in the abstract l. 35-36: ‘understory trees may be disproportionately impacted under hot, humid conditions’. I would suggest that the authors clean up their contentions and not come off so contrary to their own evidence and logic. Certainly this titillating idea of understory tree damage is not to be highlighted in the abstract if so much evidence to the contrary is presented in the review. I would like to see direct observations of the impacts that they suggest for the understory, but in the absence of strong arguments the balance of the review would support upper canopy vulnerability rather than the understory. This is a major aspect for the authors to clarify.

These are wording issues where the writing can improve:

l.187: ‘but the elevated CO2 quickly dissipates during the day such that differences are negligible for most of the day when photosynthesis is occurring’. Please specify ‘CO2 concentration differences’ where it says differences, to increase clarity.

l.200 ‘upper canopy leaves have higher 𝑔𝑏 and associated potential for water and heat loss’ compared to what??? Please clarify.

l.256: The statement “During drought (hot and dry, with higher-than-average solar radiation) …” is worded in a confusing manner. Meteorological ‘Drought’ is conventionally defined as a prolonged shortage in the supply of water to land or plants, but with no link made to temperature or cloudiness. It is fine if the authors associate drought with high temperatures and high radiation, but the parenthetical should emphasise this association, whereas it could be read to imply a one-for-one relationship which is incorrect.

I suggest revising as ‘associated with conditions that are hot …’.

l.264-265 ‘their environment is less conducive to dissipating excess heat compared to upper-canopy leaves.’ What evidence? This is a logical argument and should either be backed with references or labelled as a logical outcome of the conditions they have stated.

l.269 How does lower VPD in the understory inhibit evaporative cooling, if VPD is the major driver for evaporation? I think I understand how this can be counter-intuitive, but the reasoning isn’t explained so it all becomes opaque to the reader. Please explain very briefly.

l.313 ‘information from adjacent mature leaves or apical meristems, including a “memory” of previous conditions in that location, …’. This was not in the previous version that I can see, and I don’t see the basis for inclusion of this statement. As well, use of the term ‘memory’ seems awkward if not entirely wrong (even reading between quotes). Please revise.

l.579 ‘implies that high 𝑇𝑎𝑖𝑟 should decrease 𝑔𝑠 of upper canopy leaves more than understory leaves’. The authors here are ignoring what they’ve said previously, that high Tair is associated with high VPD (l. 199-200), and that VPD is conclusively a much stronger driver of gs than Tleaf (Leuning 1995, Grossiord). I think the sentence should be corrected and include VPD. I’m not yet aware of a good study that has decoupled VPD and Tleaf to prove that Tleaf intrinsically causes reduction in gs. So the authors cannot conclude this.

l.629 ‘we currently lack mechanistic understanding of how and why the temperature sensitivity of respiration varies across the forest vertical profile’. Other than that it varies according to the prevailing temperature difference? I think the temperature difference between upper and lower canopy positions, the theme of this review, should be mentioned in regard to this.

l.806-807 ‘trees in the understory might be more negatively affected by chronic stress from warming 𝑇𝑎𝑖𝑟’. This is based on such very thin evidence, almost no evidence. Nor do I think that the authors are appropriately highlighting a lack of knowledge very well by their speculation (which is not based on data). They are inferring what would happen with climate warming on the basis of historical evidence, highly indirect in Fig. 4B which only shows modelled results, and when there are highly overlapping CI’s. I hardly find it a basis for a strong statement landing in the abstract, especially when it is contradicted by other statements throughout the review.

## References