

Response to the Editor

All comments by the Editor and the Reviewers are reported in *Italic* and our responses are in roman.

- 5 *The original two referees have now re-assessed your manuscript, and are mostly satisfied with the changes that you have made. In particular, referee 2 highlights the need of clarifying whether leaf-level responses in transpiration are an explicit function of the assumed changes in LAI. Please address this important aspect, and carefully consider the raised concern about the thermal optimum of photosynthesis being considered constant in the models.*
- 10 We amended the text to address the remaining comments by Referee #2, as detailed below.

Response to Comments by Referee #2

- 15 *Authors have revised the manuscript in response to the review comments from the first round. I thank the authors for the detailed replies. Revisions have been made on the text and this has resulted in a much clearer presentation of the scope of the research questions and motivation of the model experiments. I think the paper is ready to be published after authors address two remaining points and a few minor corrections in the manuscript.*

The comments received by both reviewers were spot on, and indeed helped us improve the manuscript. The remaining issues are addressed as explained below.

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- *Abstract: *Both models predict [...] negligible (PETA) or no (optimization) changes in canopy-level transpiration due to the compensatory effect of increased leaf area.* I am still confused about the extent to which this is subject to the assumed LAI change, in which case this result is not a pure prediction. It would help if it was better clarified whether the leaf-level response in transpiration is an explicit function of LAI and whether the compensatory effect, leading to no or negligible changes in canopy-level transpiration is a result, emerging irrespective of the magnitude of assumed LAI changes. Or does it just happen to just compensate prescribed LAI changes but would not if a different magnitude of LAI change was assumed? I looked at the equations provided in the main text now but this didn't answer my question. Please clarify and make sure the text in the abstract does justice to whether the compensatory effect an actual prediction or whether it just happens to compensate here, but is subject to the assumed LAI change.*
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- In the optimization models OPT2 and OPT3, changes in canopy scale transpiration rate are (analytically) independent of changes in LAI. This can be seen in Eq. (16), where $\frac{\Delta E_{opt}}{E_{opt}}$ is only a function of the change in dry period duration, $\frac{\Delta t_d}{t_d}$. In

contrast, in the PETA model, Eq. (7) shows that $\frac{\Delta E}{E} \sim \left(1 + \frac{\Delta E_L}{E_L}\right) \left(1 + \frac{\Delta L}{L}\right)$, where $\frac{\Delta E_L}{E_L}$ is independent of LAI. Therefore—all else being equal— $\frac{\Delta E}{E}$ depends linearly on changes in LAI. However, the combined increases in atmospheric CO₂ concentration (decreasing $\frac{\Delta E_L}{E_L}$) and LAI result in a compensatory effect and thus minor variations in $\frac{\Delta E}{E}$ with increasing CO₂ concentration. This small sensitivity is shown in Fig. 5B for any choice of the parameter α (actually, the largest variation in E occurs at intermediate α , not when LAI increases the most with atmospheric CO₂; i.e., at $\alpha = 0$). In both models, the lack of (for the optimization models) or small (for the PETA model) sensitivity of E to changes in CO₂ concentration are thus a result of compensatory effects that analytically eliminate LAI from the equation for $\frac{\Delta E_{opt}}{E_{opt}}$ in the optimization model, or numerically cause $\frac{\Delta E}{E}$ to be almost insensitive in the PETA model. Based on this rationale, we would keep the abstract as is, as the compensatory effects are indeed a theoretical result, and are largely independent of the magnitude of LAI change.

Still, we agree with the reviewer that it is worth clarifying these points in the manuscript. After Eq. (7) we now clarify that: “Specifically, E is expected to increase with L if all else is held constant, but the simultaneous changes in c_a (negatively affecting E_L) and L compensate each other, leading to small variations in E .” Moreover, we now explain after Eq. (16) that: “In particular, Eq. (16) shows that changes in canopy transpiration are predicted to be independent of changes in LAI or atmospheric CO₂ concentration, but only depend on changes in dry period duration.”

- Simulated effects of exceeding a thermal optimum of photosynthesis are mentioned prominently in the abstract and in the conclusion. However, the thermal optimum of photosynthesis is considered here as a global constant, while there is strong evidence that the thermal optimum is highly plastic - even within individual plants over the course of a season (Kumarathunge et al., 2019). This is relevant in the following context: The study looks at dry-down time scale responses, explicitly separating respective time scales from (assumed) longer time scales of LAI changes (~seasonal-annual). Following this reasoning, LAI changes are prescribed. Then, interactive effects of simultaneous changes in air temperature and VPD are investigated, motivated by trying to understand climate change effects. However, those would unfold also on a long time scale (decadal), even longer than LAI changes. At these time scales, plants will acclimate, i.e., the temperature optimum of photosynthesis has to be assumed to shift accordingly. In the present study, as I understand, this temperature optimum is assumed to be stationary, and thus representative for fast time scale responses, and not at the climate change-time scale. The discussion of this point on l. 590 should be complemented to clarify this point with respect to the separation of responses on different time scales. I consider the assumed constancy of the thermal optimum a caveat of this study and therefore recommend to avoid strong statements about related effects in the abstract and conclusion. (I should have raised this point already in the first round - please excuse that I'm doing so only now).

We agree that thermal acclimation of photosynthesis was not considered. It is indeed a limitation, which we have now
65 acknowledged in Section 4.2 of the Discussion. Acclimation is expected to shift the thermal optimum of photosynthesis (as a
net result of shifts in a number of photosynthetic parameters) by approximately 0.3 (Vico et al., 2019) to 0.4 (Kumarathunge
et al., 2019) °C/°C change in growth temperature. In addition, species adapted to warmer climates have higher thermal
optima. In our analysis, a fixed thermal optimum was considered, slightly higher than 30 °C, to be compared to baseline
70 growth temperatures of 15, 20 or 30 °C. Therefore, all our results refer to growth temperatures lower than the thermal
optimum, for which a moderate increase in temperature improves photosynthesis. However, warming causes temperature to
increase above the optimum in some of our analyses (Figure 7, bottom panels), leading to growing conditions beyond the
thermal optimum. We emphasized that the combined temperature effects on VPD and on (not acclimated) photosynthesis
decrease the sensitivity of gas exchange to atmospheric CO₂ concentration compared to a case where only VPD is varied at
constant temperature, except when the growth temperature is much lower than the thermal optimum. Assuming that adapted
75 and acclimated thermal optima partly track the growth temperature, in most cases growth temperature would not be much
lower than the thermal optimum, so that the mentioned decrease in sensitivity of gas exchange is expected to be the
dominant effect.

To avoid emphasizing an effect caused by lack of explicit consideration on acclimation in our model, we deleted “except at
80 growth temperatures lower than the photosynthetic thermal optimum” in the Abstract and modified the Conclusions as
follows:

“Drier air is expected to decrease the positive effect of elevated CO₂ concentrations on net CO₂ assimilation and increase the
CO₂ effect on water use efficiency. However, at growth temperatures close to the photosynthetic thermal optimum, the
positive effect of rising CO₂ concentration on net assimilation is reduced because warming might cause a decline in
85 assimilation rates.”

In the Discussion, the relevant point raised by the reviewer on the time scale of acclimation is now mentioned explicitly:

“At time scales beyond week to months, photosynthesis is expected to acclimate to warming, increasing the thermal
optimum, although not as much as temperature itself (Vico et al., 2019; Smith et al., 2020; Kumarathunge et al., 2019).
90 Accounting for thermal acclimation (which we have neglected) could thus partly compensate for the warming-induced
decline in sensitivity of net assimilation to c_a , but warming could also have other consequences that are not considered
here...”

MINOR

95 *l. 39: “... and thus leaf area ...”*: remove ‘thus’. There is no direct, hard-wired link between growth and LAI changes.

Corrected as suggested.

100 *l. 158-159: Still confusing. Are time scales relevant? c_i : c_a assumed to remain constant as c_i varies in response to fast g changes? If so, it would be clearer if formulated e.g. as: “As a result, A_L is a linear function of c_i at short time scales, but, as atmospheric CO_2 varies over long time scales, resulting changes chi lead to a modification of its slope.” (If my understanding is correct.)*

We propose a slight variation of the suggested re-phrasing: “As a result, A_L is a linear function of c_i , but as atmospheric CO_2 concentration varies over long time scales, resulting changes in k lead to a flattening of the A - c_i slope.”

105 *Table 2: Is it correct to factor porosity into root zone water storage capacity parameter w_0 ?*

Water storage capacity, expressed a depth of water in the rooting zone, is given by the depth of the rooting zone, times the porosity (providing the depth of water that can be stored irrespective of how much plants can actually use), times the difference in saturation (fraction of pore space filled with water) between field capacity and wilting point (accounting for the fact that plants cannot use water that percolates rapidly beyond roots’ reach, and water tightly bound to minerals). Because
110 the soil moisture variable we use is rescaled between field capacity and wilting point, accounting for porosity is necessary. We thus think our definition is correct.

Fig. 6, bottom row; Fig. 7: I don’t understand the y-axis label. Is this a product of relative changes in D and T ?

The label indicates that relative changes in VPD are a function of relative changes in temperature. To clarify, we modified
115 the captions of both Figure 6 and 7: “Changes in D are expressed as a function of changes in temperature T_a at constant relative humidity...”

l. 539-540: Is this referring to a possible effect in real ecosystems investigated by Lu et al. and Fay et al, cited just before? Or is this a general statement?

120 We were referring to the cited studies, as we now clarify: “Water availability increases as observed in these studies might occur only in the short-term because CO_2 enrichment had not been running long enough for plants and communities to fully acclimate.”

Titles of sections 4.2 and 4.3 are identical.

125 These titles are slightly different and we would prefer to keep them as are now:

“4.2 Atmospheric CO_2 and vapor pressure deficit interact...”

“4.3 Atmospheric CO_2 and dry-down duration interact...”

l. 693: “least” instead of “lease”?

130 Corrected.

References

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