Saturating response of photosynthesis to increasing leaf area index allows selective harvest of trees without affecting forest productivity

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Authors' response to

RC1: 'Comment on egusphere-2024-3092', Anonymous Referee #1
and
RC2: 'Comment on egusphere-2024-3092', Anonymous Referee #1

Again we are very thankful for the reviewer's suggestions and for the editorial work.
The major changes underwent were:
- shortening the introduction according to RC2's suggestions and critics, which is now more focused;
- made the necessary complements to the methods regarding the LAI estimation, to the fluxes and the model;
- redrew the figures 1, 3 and 4;
- made several changes to the discussion in order to incorporate the elements suggested, such as the uncertainty in LAI and fluxes, uncertainty in the location of the LAI threshold and its variations among plant functional types (or other factors);
- discussed the limitations of the model;

Modifications made in response to the Major comments by RC1:

With regard to LAI values, it is difficult to understand whether the threshold value indicated by the authors is relevant whatever the PFT. In fact, the definition of LAI varies between deciduous and coniferous stands, due in particular to a difference in clumping index. As a result, its impact on carbon fluxes can also be expected to be different. This point deserves to be discussed. In addition, the results based on the analysis of carbon fluxes measured by eddy covariance technique should be further discussed in the light of the 'known' uncertainties concerning the estimation of GPP and Reco during the day.

> We have highlighted the existence of variations among PFTs in the threshold in the results section. We also highlighted the uncertainties in both the LAI and the fluxes estimates, present them in the Figure 1, in the text and discussed these uncertainties in the discussion section.

In short, we suggest that the location of the threshold is not precisely determined by our data, and thus present a range rather than a single value, and mention the possibility that this threshold varied among PFTs as suggested by the data and the model.

In general, it is difficult to assess the contribution of using the LPJ-GUESS model. This tool was mainly used to confirm the non-linear relationship between GPP and LAI and to confirm the LAI threshold value, but it could have been used to go further in analysing the weak impact of forest management (competition for light, for example).

The model was used in order to verify that it could represent a saturation in the responses of GPP and NEP to increasing LAI. It confirmed this and also highlighted the variation among sites in the location of the threshold value. We inserted some lines to better explain the expected outcome of the current modelling work, and, in accordance to your very pertinent suggestion, added the total LAI and total GPP numbers to the plot (with their uncertainties).

We added a statement regarding next steps to the discussion, as well as further explanation on the choice of the modeling (L507-515).

We lately found a satisfying way to model management, but could not incorporate it in this revision because we lacked the time to do so. It allows to compare managed/unmanaged time series of LAI, GPP and NEP, and could be added to the manuscript at a later stage.

Specific comments:

151-153: For the sites studied, are the age and forest management of the plot described, and how have these characteristics been taken into account in the analysis?

We made no particular changes.

162-166: For estimating LAI based on remote sensing, is the spatial resolution of MODIS images sufficient, particularly in relation to the size of the plots (consistent with the comparison with carbon flux measurements), to detect differences in LAI between managed and unmanaged sites?

We tried to make it clearer in the text, that the spatial scale of the study is that of stand, the typical footprint area of eddy covariance being in order of 100 ha (L272-274).

We highlighted the uncertainties concerning the estimation of LAI both in the text and the figures.

223-227: In general, it is difficult to assess the contribution of using the LPJ-GUESS model in this study because the description is not very detailed: how is competition for light taken into

account, in particular as a function of tree density, the age of the tree stand, etc.? how do photosynthesis parameters vary as a function of age, as a function of PFT? how does a reduction in soil water impact photosynthesis and/or production?

We provided more details to explain the LPJ-GUESS processes, for instance concerning its representation of the canopy, its internal photosynthesis model (L272-274, 282-286).

228-230: Does this mean that carbon allocation is only calculated on an annual time step in the model? There are seasonal dynamics that affect the respiration rate associated with organ growth and therefore the NEE. This point needs to be clarified in relation to the conclusions of this study.

We introduced new sentences to disclose the functioning of the model (L282-288) and as suggested as discuss these limitations in the discussion (L510-515).

230-232: Does the SLA vary with position in the canopy (profile of SLA?) relative to leaf exposure to incoming radiation? This is an important point to take into account when considering light competition and its impact on NEP in relation to tree density.

Variations in SLW could not be included in our current study. We explicitly mention this limitation L282-283.

237-238: How does clumping index vary between PFTs, stand age and tree density? Is this variation taken into account when analysing the results?

We introduced several sentences to remind this problem of clumping. We present it in the Material and Methods section along with the reference by Gielen et al. (2018) where the estimation of the clumping index is described (L197-198). We discuss these problems further in the discussion, around the uncertainties of estimating LAI.

Figures 1 and 4 now present the uncertainties in LAI and fluxes.

267-268: Is the threshold of 4.5 m^2/m^2 the same regardless of the clumping effect? Is this value the same for coniferous stands? Generally speaking, there is no discussion of the definition of LAI for a deciduous stand and that for a stand of conifers (see lines 285 & 305-307).

We modified Fig. 1. in order to highlight the differences between conifers and broadleaved. We also discuss the matter, for instance L 431-434.

273-275 & 394-396: This result is relatively expected because if the LAI value increases, we expect an increase in biomass (linked to an increase in canopy photosynthesis) which leads to an increase in growth respiration, one of the two components of autotrophic respiration. Why

not use the model to deeply analyse the differences in partitioning of the two components of autotrophic respiration (respiration due to the energy cost of tissue maintenance and respiration due to the cost of tissue construction during the growth phase) between sites and forest management to confirm the hypotheses proposed by the authors? Can the model support the hypotheses mentioned, particularly with regard to the non-linear relationships found with GPP, the distribution of NEE between GPP and Reco, and even the distribution of Reco between growth respiration and maintenance respiration?

> The photosynthesis model is presented in the text L287-288. Further, we introduced new sentences to discuss the limitations inherent to the model (it being a big-leaf model, with no SLA variations throughout the canopy, and the lack of daily allocations).

417-418: Yes, a discussion on the uncertainty of the GPP estimate could be added, as well as for Reco values during the day (see also lines 304-305). The impact of the age of the stands selected for this study on the growth respiration rate in terms of the amount of living tissue (not total above-ground biomass) should be discussed. An increase in growth respiration could also be expected if there is a stand management practice such as pruning.

> We present uncertainties in GPP estimates and show them in the Figures 1 and 4. More on the effects of management could be done in the future, by comparing the simulated fluxes under management/no management.

420-421: Why didn't the authors try to validate the model's predictions of NEE, GPP and Reco on these two sites? Once this had been done, the model could have been used to validate the hypothesis of an equilibrium LAI and to confirm the threshold value of 4.5 m^2/m^2, and to test the impact of a change in the clumping index due to forest management.

> This is a reasonable point. However, the problem is that LPJ-GUESS is not designed to perfectly capture highly site-specific properties. We aligned the simulations in this study with the observations to get model results also for the various climates, soils, and species types. Capturing the exact details of a site, including exact age distributions, and management impacts, would require detailed data for the sites that are not available and even then probably not capture the exact properties of the sites. We used the model here to show the non-linear response of GPP to LAI.
>
> In fact, the default management scheme in LPJ-GUESS is based on executing thinning when LAI gets above a threshold, therefore this cannot be used for this experiment.
>
> Nevertheless, we will add further validation of the model (see also our answer below) and try again whether we can include more model results to back the claim for management as well.

Fig 1: The GPP/LAI relationship is difficult to interpret due to the high variability of GPP values (e.g. for managed conifer/mixed). No point corresponds to the case of managed broadleaves (mentioned in the legend). For the Reco/LAI relationship, it would be interesting to indicate the uncertainties on the graph in the same way as for the GPP/LAI relationship.

We have redrawn the figure.

Fig 3: as for figure 1, it would be interesting to identify coniferous sites from broadleaves sites.

The figure 3 was redrawn and now shows the conifer/broadleaves sites.

Fig 4: Why not show the measured NEE in addition to the simulated NEE?

We added measured and modelled NEP in the figure 4 in accordance to this suggestion.

Modifications made in response to the Major comments by RC2:

54: value should have associated uncertainty for management guidance. Is 4 m2/m2 a minimum?

We made several changes that go in that direction, by indicating a likely range of values for the threshold, which could vary according to functional plant types and other factors. We also generally present in more details the uncertainties in the LAI and fluxes values. We have redrawn Fig 1 and 4 in this sense too.

57: note that this applies to temperate forests

We changed the text accordingly (L60).

67: 'counteracts climate change mitigation' sounds like a bit of a double negative and was confusing to read to start of the manuscript. I get it, but had to pause. The next sentence discusses mitigation rather than counteracting mitigation so one's mind is pulled in two directions.

Changed as planned.

71: who assumes this? I wasn't aware that this was a common perception amongst scientists, at least forest scientists.

These parts were removed.

77: this isn't always the case e.g. https://www.nature.com/articles/nature12914 and 'very low' is at a minimum qualitative. There's a huge literature on this topic (https://www.nature.com/articles/nature07276) with lots of controversy (https://www.nature.com/articles/s41586-021-03266-z) as the authors are well aware and the statement as written discounts this rick literature. I've come to the opinion that people with forest management training think that old stands stop growing because monoculture forests basically do, but natural forests can keep taking up carbon even if at a slightly slower rate. Having been in temperate forests where the mid story is comprised of trees that we would think of as fully grown mature adults with overstory trees proper old growth giants, I've always been mystified at the idea that old growth forests don't still take up carbon. Obviously there is some physical limit. I don't disagree that older forests might take up less carbon, rather the assumption that they always do; for example take a look at the data points in Fig. 1 here instead of the curves that were hacked through them: https://assets-eu.researchsquare.com/files/rs-5183310/v1_covered_49c18487-5655-429a-a89d-b2e4d64fa22e.pdf?c=1735007962

These parts were removed.

88: I somewhat that wood provision is considered a disturbance if sustainably harvested to simulate natural forest processes. The passage could easily be re-written to emphasize what the paper is actually about: that harvesting can occur with minimal disruption to carbon sink strength.

We shortened and tried to focus as suggested.

95: disagree that selective harvesting is a disturbance or at a minimum that a disturbance is a bad thing; forest harvesting can simulate 'natural' forest processes as noted above.

Revised accordingly (L116).

As a whole, the Introduction makes a number of valid points, but is weakened by assumptions and poorly-cited statements. It should be re-written to focus more strongly on the matter at hand, and can be guided around the LAI of 3.5 m2/m2 found by Schultze to expand this argument to carbon gain in addition to conductance.

The introduction was shortened and is hopefully better now.

151: couldn't most of the Canadian sites from BOREAS be considered unmanaged conifers?

As mentioned we focused exclusively on temperate sites.

153: how was LAI estimated? I see some text on line 162 but this could be written in a much more systematic way for a methods section. Remote sensing and ground-based estimates should be compared to understand their differences (and both have substantial uncertainty that should be estimated if possible).

We introduced several sentences to remind this problem of clumping. We present it in the Material and Methods section along with the reference by Gielen et al. (2018) where the estimation of the clumping index is described (L197-198). We discuss these problems further in the discussion, around the uncertainties of estimating LAI.

168: the flux is transport across an area, so yes by measuring transport across the sonic anemometer and gas analyzer the eddy covariance system is physically measuring a flux. Calculating a surface-atmosphere flux does require some assumptions, I agree. Most instruments really just measure voltage differences so one could also argue that nothing measures a flux.

These sentences we rephrased (L203-205).

172: low ustar needn't represent an error in measurement, it just seeks to represent a case of insufficient turbulence where the assumptions that underlie the eddy covariance technique are not good assumptions. For all we know the sensor measurements themselves can be of the highest quality.

These sentences we rephrased (L204-208).

Figure 1: surprised that red and green are being used at the same time. Please use different choices for our colorblind colleagues.

The figures were redrawn correspondingly.

194: please cite the R package

Done, L242.

210: this can vary widely based on tower height and environmental factors; Chu et al. (2019) have the most systematic study of footprints across multiple sites and citing this study here can help clarify quantitative aspects of flux footprint dimensions at the network scale.

We do not present specific estimations of the footprint area, but mention its order of magnitude L275-277. It is unclear if each site has the same methodology to estimate the footprint, and if the area of the footprint could be a covariate in this study.

221: 'demonstrated' instead of 'proven' is probably a better verb here.

Done, thank you.

263: in the results section define what is meant by 'near'. The manuscript should be strengthened by including uncertainty estimates in multiple locations including here (also line 266, etc.).

Done and changes made to Fig.1.

267: yes, because it's not a location, it's a mean value with uncertainty. That could and should be quantified, either as a single value or a threshold that varies as a function of climate or forest type.

We revised Fig. 1 according to this suggestion, and show the threshold with its uncertainty. We also introduced new sentences to discuss the

279: if it's not significant, it didn't tend to be higher. But it could be higher in future studies with more statistical power perhaps.

Indeed, if it had been significant, it would have been significantly higher. Since it's not, only a tendency can be evoked. The discussion will highlight the fact that significance is not reached and we will call for more experimental data to increase the statistical power.

292: there are more significant digits reported than warranted for a study of dry matter at a plot scale.

The mistake was to use a dot instead of a space or a coma: the values are very high and not presented with decimals, expressed directly in g.

321: this is a great rule of thumb but adding an improved statistical analysis could further improve it, or note that the analysis points toward a rule of thumb that could be valuable guidance for foresters with additional research.

We introduced new sentences in the discussion in this sense L401-404 and 438-440.

331-332: add scientific names. Note also that where the authors are writing from there is a single type of Fagus, but another in the eastern Mediterranean (although the eastern European

one is now recognized as a subspecies), and quite a few species in Asia such that simply stating 'Fagus' might cause unnecessary confusion for an international study. Note also that Fagus is italicized on line 420 but not in other places.

We revised the text accordingly.

Fig. 1: are these data points from the eddy covariance data or the model? If the former, what partitioning method was used to infer GPP and Reco?

We made complements to the presentation of the flux methods (L).

Figure 3: is <10 etc. the age of the forest since last stand replacement or the time since last management prescription?

The figure was changed to present distinctly the conifers and broadleaved. This new version is also less confusing than the duration presented in the earlier version of the figure.

Fig. 4: slightly larger font sizes would make this easier to read.

Changed accordingly.

735: species name

Changed accordingly.