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

This contribution is an important one, in that it provides a functional insight into a long-attested empirical demographic relationship relating plant/tree population density and yield. In spite of its robustness, this fundament of yield science and silviculture (being able to extract trees from a forest without sacrificing total yield, and increase growth of remaining trees in a process that fastens the acquisition of size, thus preserving its long-term sustainability) remains poorly perceived by ecologists and the general public, causing ill-posed debates in forest sustainability. For this reason, being able to support it by functional process-based grounding is major. This is an originality of the contribution.

That said, we also wish to share some matter of concern regarding specific aspects of the contribution, in order to give it the broadened perspective it deserves.

1. Constant final yield

One first aspect is the apparent disconnection there is, between this "modern" approach relating C fluxes and NEP from the EC methodology, and the bunch of historical work performed in forestry science to relate yield (ANPP) to stand density, also in agronomy science. The same saturation has been described as "law of final constant yield", "Langsaeter's plateau", the "thinning response hypothesis". Major texts include Yoda et al 1963, Assmann 1970, Pretzsch 2009, but also and more recently the nice synthesis by Weiner and Freckleton 2010 (named "constant final yield").

➔ Reference to these texts and concepts would allow better connect these findings to a long-established reality.

This also raises an issue. The following conclusive statement: *This threshold can be used to define sustainable metrics for sustainable harvesting, as those that do not impact the carbon sink strength of the forest stand* questions how it can be achieved. When this plateau was established in terms of ANPP = f (stand density), at least direct targets could be formulated for the prescription of silvicultural regimes. Yet, how may a forest manager easily pilot a LAI target?

➔ Here, I wonder whether plot density data are available, and to what extent the correlation between LAI and stand density (say BA, or N) would allow capture/confirm the alleged LAI threshold invariant identified, and may pave the way toward these metrics. In would rather incline toward acknowledging the merit of this transposition of initial stand density / yield relationship to LAI / photosynthetic C fixation to ground the previous relationships, and deliver the message that forestry science is justified to do so. Perhaps the aspect may be expanded both in the introduction and discussion.

2. Tree species diversity and species traits

As written, the paper gives an impression that finding the threshold is so motivating that the issue of variability is a bit discarded. At L277-279, it is even suggested that cross-species variation may be minor, but no reference is suggested for this aspect. By contrast, I would be of the primary opinion that the shade-tolerance of tree species may influence that potential threshold, light-demanding species meeting an earlier LAI threshold than others. This would be consistent with the notion of clear-forest silviculture as envisioned in the 19th century for the management of light-demanding species (e. g. pines, oaks).

➔ May it be useful to discuss the invariance of this LAI threshold in view of species traits? How far can we go? What may be the implications in terms of experimental designs based on current tower flux facilities? Is it an issue for a near future, or currently out of reach? This discussion would be welcome in the discussion.

Further, may the design structure of EC sites allow to explore, at least a bit, the issue, e. g. by ranging sites according to some community weighted mean of the shade-tolerance trait (TRY) and see whether it correlates to some parameter related to the fit of the non-linear responses to LAI? I have this impression that a prescriptive unique threshold value should be avoided.

➔ Is it possible, or prohibitive, to explore a quick statistical relationship between the position of the threshold and a community weighted-mean of shade tolerance?

3. The role of excess LAI

In the discussion, the adaptive significance of "excess LAI" is discussed, which suggests that excess LAI may serve the purpose of better tree resilience to abiotic or biotic disturbances. If so, while saturation is good news for the C storage, it is not necessarily the same for other functions related to resilience, in the present environment. Since it is indicated that some monitoring plots experience heat/drought or other disturbances over the period, may this be tested, or is it out of reach?

➔ I am uncertain about whether this test may be easy. At least, a better balance of the corresponding paragraph, and the trade-off it may induce in a silvicultural approach, is welcome.

4. Methodological aspects

In general, it is quite hard to grasp the monitoring period under study, precisely. And also to compare it to management events, in order to perhaps question the ranking of managed/unmanaged plots. It may seem worrisome that only 11/19 plots classified as "managed" did not show any management event over the monitoring period in view of the rapidity of LAI reconstitution in general.

➔ May this have contributed to dampen the difference between managed / non-managed forests? Shouldn't this be discussed a bit (and if possible, tested? And if untested, what would be the perspectives ?)

It was also very difficult to me to understand whether GPP/NEP data come from field plot measurement, or from LPJ-guess, and also what specific role does the model indeed play.

➔ I have this impression that the modelling objective may be specified more clearly in the dedicated section, and early in the introduction. E. g., is it because it is feared that the integration scale difference between plot and EC towers may have a role?

5. Local detailed comments

Line 78-79: Bontemps 2021 (Plos) does not suggest any saturation of the carbon stock in European forests

Line 95-98: the saturation of forest productivity with stand density has been a cornerstone of silviculture since the elaboration of the scientific principles of forest management. It is a bit frustrating it is not mentioned a bit more in-depth, a law known as the thinning response hypothesis, Langsaetter's law, and more generally in agricultural yield science, as the law of final constant yield.

In particular, *Saturating response of photosynthesis to increasing leaf area index allows selective harvest of trees without affecting forest productivity* may be rephrased as *Saturating response of forest ANPP to increasing stand density allows selective harvest of trees without affecting forest productivity.*

Line 122: why it would have impact on CO2 assimilation is less clear.

Line 128: and certainly also, across tree species, given that their sensibility to light (shade-tolerance) may have a contrasted effect on a unique LAI threshold.

Line 133-135: *In particular, the interactions between management and LAI, and their consequences for the carbon sink strength need to be determined in order to examine the consequences of wood harvesting on forests carbon sink strength.*

+ line 138-140: *explore the possibilities of defining levels of sustainable partial cuttings from the perspective of carbon 140 fluxes, key to designing forest managements strategies*

But how in practice, can we transfer a target N trees / ha under management into a LAI reduction / ha, especially since selective thinning can be deployed for different strategies (thinning from above / below)?

Line 154-155: what is the monitoring period exactly? Is it 2000-2020 as suggested by LAI measurements? If so, one may wonder about the significance of being classified as managed forests for those 8 study sites out of 19 that were matter of no harvesting over the period.

Line 164-166: were these MODIS-based estimates compared against measurements for those sites where site measurements for LAI do exist?

Line 168-173: in addition, may it be possible to specify a bit across which spatial range do the flux measurements integrate and to what extent there are representative of fluxes within the perimeter of the study sites?

Line 183-185: then if GPP and Reco are gaussian, why the difference = NEE would not be so? Are their discrepancies to be expected in this budget?

L 203: but also suppressed trees, with varying LAI

Line 209-210: then how can it be that measured fluxes are representative of forest plot dynamic?

Line 218: on monitoring plots, wasn't it possible to more directly measure tree-driven GPP, or at least compare?

Line 145-246: at this point, the issue of variability across tree species turns an issue. What are the species covered in the 30 monitoring plots? How do they match the PFT of LPJ-guess? To what extent there is a risk to remove this variability?

L253-254: can it be specified whether the management operations described on the monitoring plots were introduced into LPJ-guess? Or else?

L262-263: I would say this (should) has(ve) been the main concern of silviculture since its foundation!

L299: finally, what is the monitoring period for the study? 10 years? 20 years? Else?

Fig1a: full red squares not specified. Further, only clear footprint of a saturation given by open red squares.

Fig4: only the first column is informative of a threshold. Perhaps needed to re-inform about the lack a sufficient LAI gradient on the right (but may LPJ-guess be pushed toward extrapolation anyway?)

L324: A bit puzzling that the 1st paragraph seeks to establish this result and ground it in the recent literature, with no reference at all to the classical forestry literature. Could this be a little bit better balanced, for the sake of enlarging the audience?

L335-339: how may this be evaluated properly? Are there any data possibilities available to support the issue?

L377-379: yet what allows to justify this statement? It may be conversely expected that the saturation point is reached at lower LAI for light-demanding tree species, in line with the clear forest management strategy. Not?

L399-401: and also, this has been observed in empirical studies of forest yield, whereby an optimum, instead of a plateau, has also been detected.

I am a bit puzzled with two conclusions:

Above its saturation value of ~ 4 m2 m-2, additional increases in LAI are not linked to increased productivity, but may contribute to other functions selected in evolution, such as competition with adjacent trees, resource storage and buffering against herbivory

Then, should this additional LAI have adaptive functions, some of these being adaptations to biotic or abiotic pressure of the environment, should we understand that – while decreasing LAI may be of minor impact on the strength of the C sink – it may impact stand/tree resilience to abiotic/biotic disturbances. Isn't it important as well? Shouldn't it be discussed?

This threshold can be used to define sustainable metrics for sustainable harvesting, as 448 those that do not impact the carbon sink strength of the forest stand.

And how? When this plateau was established in terms of ANPP = f (stand density), at least direct targets could be formulated for the prescription of silvicultural regimes. Yet, how may a forest manager easily pilot a LAI target?

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Statement of conflicts of interest

The author declares to maintain a recurrent professional relationship with the first author of this paper. Due to its interest in forest dynamics (demographic, not functional), he first volunteered a few remarks about the paper, which the first author considered worth of interest to be posted publicly in the discussion of this paper.