Biogeosciences Discuss., 12, C7133–C7138, 2015
www.biogeosciences-discuss.net/12/C7133/2015/

Biogeosciences
Discussions
Open Access

Interactive comment on "Effects of management thinning on CO_2 exchange by a plantation oak woodland in south-eastern England" *by* M. Wilkinson et al.

Anonymous Referee #1

Received and published: 27 October 2015

*** GENERAL COMMENTS

This paper assesses the influence of thinning on the magnitude of CO2 exchanges between a temperate Oak forest and the atmosphere. Using a 9-year time series, the authors aim at evaluating the influence of a thinning event, comparing CO2 flux values before and after the thinning

A strength of the dataset is that the thinning was deliberately operated on a specific wind sector (Eastern part of the tower footprint) while the remaining sector (Western part of the footprint) remained unthinned. This experimental design helps disentangling the influence of meteorological variations from the proper influence of thinning.

However, this design is not able (as the authors acknowledge) to rule out the fact that easterly and westerly air masses are associated with different weather conditions.

Flux data are not really easy to deal with, since they require many corrections, filtering and possibly gap-filling before being used. My main concern with the paper lies there. The authors base a large part of their analysis on the comparison (between sectors) of:

1. monthly or annual flux sums (Fig. 4-5), obtained from gap-filled and partitioned data.

The intention is good, but the authors completely overlook the uncertainty associated to such aggregated data. As appears from Table 2, there are 72% and 65% gaps respectively in the half-hourly data from the West and East sectors. These gaps are filled before calculating time integrals, but the authors say nothing about the uncertainty associated to the gap-filling (GF), the precise method used (simply referring to the website of the online tool they used), nor do they intent to estimate the uncertainty on the time integrals induced by GF.

This point must be addressed since the paper relies on comparisons between fluxes integrals (from the unthinned and thinned sectors) that may in fact not be significantly different.

I perfectly understand that the same method was used to gap-fill the data from both sectors, but this is not an argument to insure that gap-filled fluxes can readily be compared.

I further question the absence of "bias in the data availability (e.g. day or night, or seasonal distribution) that might have affected our gap-filled annual total CO2 flux component estimates" the authors mention on p. 16212, since there is likely a seasonal predominance of W or E winds, associated to different meteorological conditions. A way to rule that out is to make a figure presenting the seasonal windrose / the availability of data from each sector along the season.

I recommend the authors to either (1) stick to the comparison of average, non-filled, fluxes or (2) assess the influence of gap-filling methods on the calculated fluxes (see e.g. Moffat et al., 2007; van Gorsel et al., 2009). If the authors chose option (2), they should further consider the fact that the gap-filling of fluxes is influenced by EC "random errors" (e.g. Ollinger & Richardson, 2005), and run multiple iterations of each GF algorithm to account for such uncertainties.

I would recommend option (1). Do not forget that using gap-filled values translates in using a dataset that contains 65% to 72% of model-derived, uncertain data... Consider further the fact that the statistical model used to GF the data is data-driven...

2. parameters of light-response curves of NEE (Fig. 6)

In that case, the reader wants to see the uncertainties, assessed as the standard error, of the parameters. Please make proper statistical comparisons of the values before jumping to the conclusion that NEE800 from one sector is different from NEE800 from the other.

Other aspects which deserve attention before the paper can be accepted for publication are:

A) The paper is divided in 2 parts: in the first part of results, the authors deal with flux data, while in the second part, they analyze Lidar data, to assess changes in the structure of the stand.

The junction between the Lidar data and flux data is not obvious. Lidar provides assessment of changes in the spatial structuration of the stand, not of changes in the surface properties susceptible to impact CO_2 fluxes. In other words, those Lidar data are interesting to view per se, but bring little to the comprehension of the influence of thinning on CO_2 exchanges of the forest with the atmosphere. A more direct link with fluxes could be through Leaf Area Index (LAI) and biomass data.

B) The authors assessed the influence of thinning on CO_2 fluxes. Why not on H_2O

fluxes?

Last, throughout the text, some assertions or even conclusions of the paper are not really discussed, or stated gratuitously: - the authors refer several time to a caterpillar-caused defoliation, occurring in 2010, which would have impacted more strongly fluxes from the thinned than from the unthinned sector (L20 P 16198, L10 P 16208). Are there data justifying the differential influence of caterpillars among sectors? What may explain that the grazing was more important in the thinned sector?

- How to explain that the largest differences in Reco between sectors were observed 2 years after the thinning (L9 P 16209)? Due to a lagged respiration of debris? I notice from Fig. 5 that Reco is even lower in the E (thinned) sector in 2008, as compared to the W sector.

- Linked to what precedes: how to interpret the higher sensitivity of Reco to Temperature in 2009 in the E sector? It is not just a question of higher availability of substrate to decompose (i.e. woody debris) but of the sensitivity of Reco to T.

*** SPECIFIC COMMENTS

Throughout the text, and on Figures, make clear and univocal distinction between west (=unthinned) and east (=thinned) sectors. Name them W/E or U/T once and keep on with that denomination. Confuse at the moment.

P 16203, L2-6: Has the influence of instrument changes on flux calculation estimated? Measured throuhgout an overlapping interval?

P 16203 L21-23: U* threshold depends on the surface structure. Hence it is not necessarily relevant to use the same threshold for both sectors. Can we see the u*-threshold selection plots (e.g. as suppl. material) ?

P. 16205, Eq. 2: Please revise the definition of NEEmax. In the actual form of the equation, the asymptote of the relationship is not NEEmax but GPPmax (be careful that there is an offset by Rd).

P 16205, Eq. 2: “incident” quantum yield should be replaced by “apparent” quantum yield. Remind that Quantum yield is originally defined at the leaf scale. So at the canopy scale, the relevant expression is “apparent QY”. (Please check, there are other occurrences of “incident QY” in the text).

P 16207, L4-6: this sentence is pleonastic. Rephrase.

P16208, L27: this statement is gratuitous.

P16209, L 4-11: what is Rs? Is it different from Reco? Use consistent denominations for the same flux throughout the manuscript

P16209, L21-22: tricky way to present those results... The sentence “values of NEEmax were generally larger (more negative)” is arithmetically wrong. Why not working with NEP=-NEE? It would simplify much the interpretation, and avoid sign confusion (same remark: rephrase L9 P 16214).

P 16211 L 19: the main result of the paper is not “surprising”, considering what we know from the literature (and the authors remind: Vesala et al., Granier et al.)

P16213 L1: uncertainties of the light-response curve parameters do not appear in the paper (though it is much needed to allow the reader interpret the results).

Table 1: there clearly is a need for numbers before thinning. What about the influence of thinning in terms of removed (1) # of stems (2) biomass? A post-thinning W/E comparison is not enough.

Table 3: Q10: which base temperature?

Fig. 1: date the photograph. I assume this was taken after thinning.

Figs 3-7: blue (east= thinned) / green (west= unthinned). Use a systematic color code and make the legend apparent on each figure.

Fig. 6: indicate confidence intervals of the regressions on the graphs

---

Interactive comment on Biogeosciences Discuss., 12, 16197, 2015.