Anonymous Referee #1
Received and published: 27 October 2015
The authors would like to thank the referee for their time and valuable comments, which we respond to below. The referee's comments are shown in *italics* whilst our response is in normal blue type. A detailed description of our planned changes to a revised manuscript in response to these comments is given below.

Referee's comment
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.

Author's response
We are pleased to note that the referee acknowledges the strength of our data set and the benefits of the experimental design.

Referee's comment
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.

Author's response
We acknowledge that our experimental design has resulted in large gaps in the half-hourly data and that this is rightly of concern to both referees. We also acknowledge that using the standard MDS method (which we did name) to gap fill from low amounts of original data is likely to result in large uncertainty, which we did not address in the manuscript. Our planned revisions in order to address this main concern are detailed below, following the referee's recommendations.

Referee's comment
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.

Author's response
We acknowledge the referee's concern about, "bias in the data availability……" in a revised manuscript we will provide more information about the data availability, the relative contribution of fluxes during the day / night and during each season by sector as suggested.

Referee's comment

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...

Author's response

We acknowledge the weakness in gap filling this type of data where capture is low. In our revised manuscript we will therefore adopt recommendation (1) and remove all reference to the gap filled data including the integrated annual sums. Furthermore, we will restructure the paper with a greater emphasis of the seasonal average fluxes, including more information about their uncertainty. The restructured manuscript will use these data to examine changes in the processes pre and post thinning.

Referee's comment

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.

Author's response

The lack of presentation of uncertainties and statistical tests was also pointed out by referee #2. Where possible, we will incorporate into a revised manuscript more detail about the statistical analysis and significance tests that have been carried out. Specifically, we will address the issue relating to Fig. 6, highlighted here, by providing more detail of the uncertainties in these parameters and updating the figure accordingly.

Referee's comment

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 CO2 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 CO2 exchanges of the forest with the atmosphere. A more direct link with fluxes could be through Leaf Area Index (LAI) and biomass data.

Author's response

With respect, we strongly disagree with this comment. The Lidar data was important to demonstrate a large increase in the frequency of gaps in the forest canopy gap fraction, an indication of the structural changes that occurred within the forest canopy during the thinning operation. In a revised manuscript we will strengthen the link between canopy structural changes and GPP / LUE differences.

Referee's comment

B) The authors assessed the influence of thinning on CO2 fluxes. Why not on H2O fluxes?

Author's response

The focus of the paper from the title onwards is on C balance of this deciduous forest. Were we to include analysis and discussion of the H2O fluxes, we believe that this would not be beneficial as it would fundamentally change the nature and aims of the paper.

Referee's comment

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?

Author's response
We will clarify the text where appropriate. We have no quantification of the caterpillar defoliation and its variation across the forest. We were only pointing out that a) it is one of the factors contributing to inter-annual variation in C fluxes, and b) when defoliation is high, and LAI low, it may result in increasing the sensitivity of GPP to thinning.

Referee's comments
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.

Author's response
We think that there has been a misunderstanding in the referee's interpretation of this graph. Fig. 5(b) shows 2009 not 2008, secondly Fig. 5(b) shows that Reco was *higher* (not lower) in the E (thinned) sector in 2009, as compared to the W sector, in the discussion we consider the possible reasons for this at length.

Referee's comment
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.

Author's response
There is a slight difficulty in using thinned and unthinned sectors, when referring to data from prior to the thin, but we will revise the manuscript to clarify the terminology.

Referee's comment
P 16203, L2-6: Has the influence of instrument changes on flux calculation estimated? Measured through out an overlapping interval?

Author's response
Logistically it was not possible to run both sets of instrumentation in parallel. However we have analysed the data pre- and post-changes in detail and can find no evidence of instrument induced bias.

Referee's comment
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) ?

Author's response
We will provide these plots as supplementary material in the revised manuscript.

Referee's comments
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).
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

Author's response
We thank the referee for these corrections and suggestions and will revise the manuscript accordingly.

Referee's comment
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.)

Author's response
We will rephrase these sentences accordingly.

Referee's comment
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).

Author's response
As described above we will incorporate uncertainty estimates where appropriate.

Referee's comment
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.

Author's response
In a revised manuscript we will provide more information relating to the stems and biomass removed during the thinning operation.

Referee's comment
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

Author's response
We will incorporate all of these technical corrections in a revised manuscript.