

Dear Editor,

We thank the referees for their time and comments.

We would like to make a strong argument that this paper is an important contribution to the literature because it analyses new eddy covariance data at a temperate deciduous woodland within the unique maritime climate of the UK. Furthermore, we emphasise that our field site (ancient woodland) represents an understudied forest type that is distinct in characteristics from either old-growth or secondary forest. All these points make this paper a significant contribution to the literature that warrants publication in a high quality journal such as *Biogeosciences*.

Our responses to the reviewers' comments on our discussions manuscript are detailed here. Each reviewer's comments are addressed in turn, in the order in which they were received. Reviewers' comments are in *italics* our response is in normal type.

### **Anonymous Referee #1 dated 22/06/2010**

#### Referee comment

*This paper makes a capable contribution to the large literature on CO2 fluxes over forested landscapes. It could be a noteworthy contribution in that fluxes were measured over a 'disturbed ancient woodland', a vegetation type possibly poorly represented in the literature. However, we are given no information on important stand ecological characteristics such as tree age and size distributions, canopy LAI or N content, soil fertility, and so on. Hence, it is difficult to make meaningful comparisons with other datasets apart from coarse temperature/precipitation relationships (e.g., fig 9).*

#### Author response

We are pleased that the reviewer feels that this paper makes a capable contribution to the literature. We are happy to add information on total basal area, maximum LAI, and limited data on soil fertility to the Methods section of the paper (Table 1). Tree size distributions, as a basic proxy for tree age distributions, of the three main species will be provided in the companion paper by Fenn et al and can also be included in the Appendix for this study.

Table 1. Stand ecological characteristics (based upon results from Butt et al., 2009).

<b>Characteristic</b>	<b>Value</b>
Stem density	1128.2 ha <sup>-1</sup>
Basal area	33.3 m <sup>2</sup> ha <sup>-1</sup>
Average tree height	15-20m
Mean soil carbon content	5.32% (S.E. 0.29)
Mean soil nitrogen content <sup>1</sup>	0.395% (S.E. 0.02)
Maximum leaf area index	Acer ~5 Fraxinus ~1 Quercus ~0.5

<sup>1</sup> Please note that additional findings regarding soil nutrient content and nutrient ratios by soil type and depth can be found in Butt et al. (2009).

Referee comment

*The relatively good fit between meteorological and biometric estimates could be coincidental since the 1 ha biometric plot, in an undisturbed ancient semi-natural woodland, would appear to lie outside the footprint of the flux tower.*

Author response

It is estimated, using analysis of wind direction data as described in the paper, that the 1 ha plot lies within the footprint of the flux tower. However, we do stipulate in the paper that fluxes from other parts of the forest will also be represented in the data recorded at the flux tower. We have recently completed sampling of an 18 ha plot stretching south from the tower (Butt et al., 2009). Much of the forest to the south and west (the predominant wind directions) is similar oak-sycamore-ash dominated woodland as that found in the 1 ha plot around the tower.

Referee comment

*As it stands, broader analyses and insights are not possible with such a short term record and limited ecological data*

Author response

We agree that it is difficult to make insights regarding interannual variations with only 2 years of data. We are currently investigating the opportunity to include an additional year of flux data analysis. The new flux equipment installed in April 2009 requires alterations to the EdiRe processing file to allow analysis of the new data files. If we are confident that the new data can be processed using the same steps as the 2007-09 dataset then we will gladly include this in the revised submission. However, while strengthening our analysis of interannual variability, we concede that a three year time series is still of medium length and can downplay somewhat the inference of controls on interannual variability.

**Anonymous Referee #2 dated 16/07/2010**

Referee comment

*In the present state I would recommend this paper not to be published. The analyses are sound and straight, and the paper is overall well written (though wordy and providing a useless amount of details in many instances).*

Author response

We feel that the referee's recommendation to not publish the paper in its current state is a little harsh, especially bearing in mind the comments of the other referees. We are pleased that the referee feels that the analyses are sound and the report well written. As indicated by other referee comments it is difficult to satisfy all with regards to the level of detail provided in the paper. The authors wanted to retain some of the basic processing details, which although not vital we feel provide clear and understandable descriptions of processes that are commonly glossed over in eddy covariance papers. We feel that providing such detail in a well written and clear format will benefit newcomers to eddy covariance studies. However, we are happy to take the Editor's advice on appropriate shortening of the manuscript.

Referee comment

*The most important results (related to the magnitude of C sink for such an ecosystem and assessment of correspondence to independent measurements) were already published and discussed in a companion paper (Fenn et al., in review for BGD) and*

*the statistical flux analyses provide no real improvement in our current knowledge of flux dependence to meteorological and biological drivers.*

#### Author response

The Wytham Woods field site is one of the few temperate woodland sites in the world where flux tower analysis is combined with detailed assessment of scaling up of NPP and autotrophic respiration components, and to our knowledge the only such site in ancient woodland. The power of our analysis comes from the complementary nature of the two papers: this paper shows how seasonal variation in GPP and respiration (not just carbon sink) is related to climatic factors and inbuilt phenological factors, the Fenn et al paper then describes how this seasonal variation and annual budget can be allocated between leaves, stems and roots. The fact that the two approaches match at seasonal resolution is pleasing and rarely reported in the literature. The two papers complement each other; the Fenn et al paper on its own would only tell half the story without the detailed exploration of the flux data that this paper provides.

Furthermore, our findings do add to the current knowledge pool of the flux community (see previous comments).

#### Referee comment

*I encourage the authors to re-orient the objectives of the paper. Since meteorological dependences of C exchanges at high frequencies (timescales preceding the annual cycle) are well known from years now, we could think of taking advantage (1) of a longer EC dataset which would help addressing the question of interannual variability of C exchanges and / or (2) relating the observed fluxes to other data, referring to functional properties of the forest (physiological data as published in paper from Morecroft et al., growth data etc.), which would advantageously complement the work of Fenn et al. on the correspondence between respiration estimates and help us estimating the confidence we have in closing the C budget.*

#### Author response

We agree that it is difficult to make insights regarding interannual variations with only 2 years of data. We are currently investigating the opportunity to include an additional year of flux data analysis, which may help address concern (1). We can also refer more to the physiological and biometric data, while maintaining a distinction from the Fenn et al paper.

#### Referee comment

*Important additional remark: data reported were acquired with an EC system including a LI-7500 IRGA. Papers are appearing (notably one written by Licor engineers: Burba et al., 2008, GCB) explaining the importance of adding a supplementary correction term to the classical WPL correction. This term is not included by default in data processing softwares, and i guess not in EdiRe (used in this paper). The correction may be critically substantial with respect to the magnitude of measured fluxes. I therefore encourage the authors to correct their dataset with the appropriate relationships, as provided in Burba et al. (2008), and consequently re-evaluate the magnitudes of net and elementary fluxes. These corrections are mostly of importance in wintertime (when LI7500 warming and subsequent temperature differential with ambient air is highest) which is characterised in the presented time series by frequent periods of net uptake which are not expected for such a deciduous woodland (even with sparse evergreen vegetation).*

### Author response

We thank the referee for highlighting the potential importance of the 'Burba correction' in our processing of the flux data. Unfortunately it is not possible to undertake the Burba correction for the 2007-2009 flux dataset. This is because neither  $R_g$  nor  $R_n$  was recorded by the flux equipment. Detailed radiation measurements have, however, been recorded by the new flux equipment installed in April 2009. We have applied the Burba correction to basic flux calculations from this equipment. The results show that the maximum correction is in the order of 5% - only for certain times of the year.

Studies by the Centre for Ecology and Hydrology for flux data measured in April/May 2010 shows the most significant correction where cool temperatures (for May) and high solar radiation coincide: these combine to produce instrument heating leading to a false uptake of  $\text{CO}_2$  of the order of 5%. The purple line on Figure 1 shows the difference between the uncorrected and Burba corrected  $\text{CO}_2$  flux (magnified by 100 to show the detail). The thicker dark blue line on the same plot shows the difference on the same scale as the fluxes themselves (shown above in green).

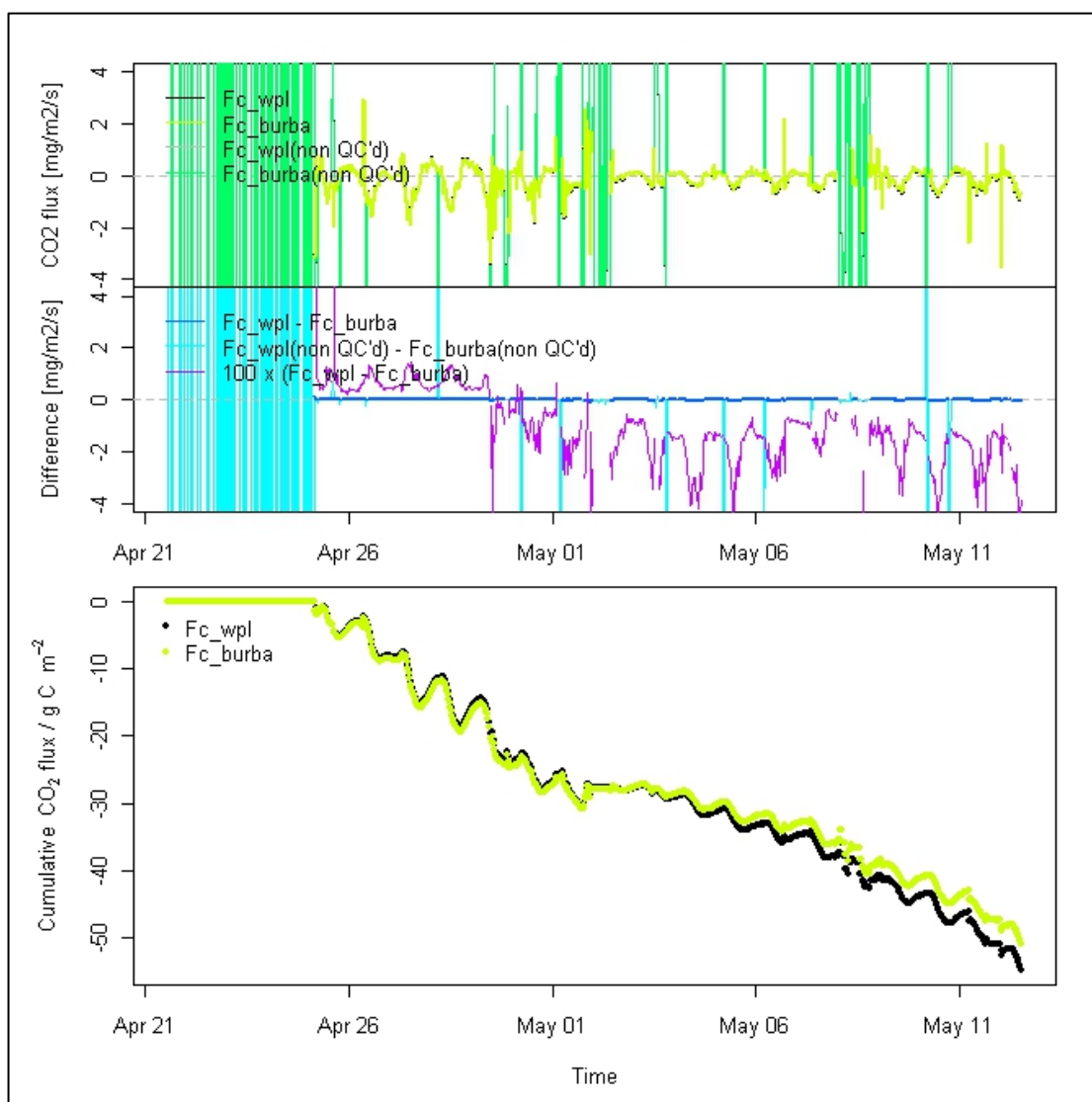


Figure 1. Timeseries of  $\text{CO}_2$  fluxes in April/May 2010 with ( $F_{c\_burba}$ ) and without ( $F_{c\_wpl}$ ) the Burba correction. Source: Centre for Ecology and Hydrology.

During warmer weather the correction is negligible (~0.5%) and the errors in the correction become as significant as the correction itself (Figure 2).

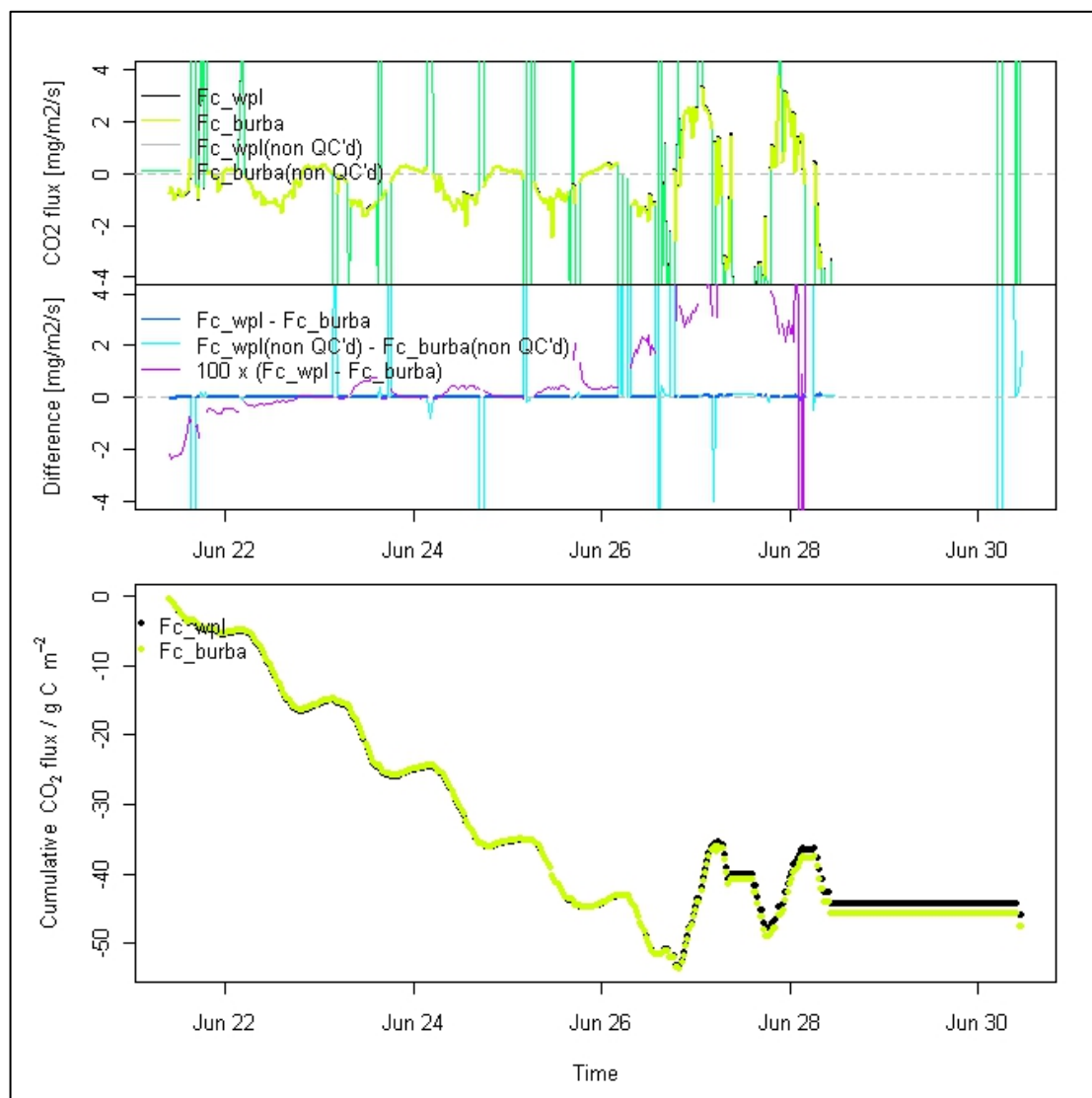


Figure 2. Timeseries of CO<sub>2</sub> fluxes in June 2010 with (Fc\_burba) and without (Fc\_wpl) the Burba correction. Source: Centre for Ecology and Hydrology.

We feel that it is justifiable to assume that the Burba correction would have similar (negligible) impacts on the 2007-2009 flux data. This is not entirely surprising since the Burba correction is of most importance in cold climates, especially where total fluxes are small and thus errors under certain meteorological conditions can have a relatively large impact upon net fluxes. Wytham's climate is mild and so we would not expect the Burba correction to have a great effect. Indeed, the most likely impact would be the eradication of some of the negative night time values estimated – something which we have already attempted to account for.

Referee comment

**The referee continues to make numerous suggestions to undertake small modifications in the text.**

Author response

We are happy to take these modifications on board and update the text accordingly.

**Anonymous Referee #3 dated 15/08/2010**

Referee comment

*In general, I find that the study gives valuable information and a substantial amount of analysis has been applied to the data. The presentation can be improved. Especially, I would like to see a more detailed description of the site and set-up than given here.*

Author response

The level of site detail provided is comparable with other flux papers, but we would happily include additional data on site and forest properties in the revised manuscript.

Referee comment

*The meteorological measurements have been made a substantial distance away from the flux mast and in a different ecosystem. This calls for some careful consideration when interpreting the fluxes in relation to meteorology.*

Author response

Agreed - this is a point that we make in the paper and we are quite fortunate to have an AWS that is so close to the study area. The difference between the two sites is most likely to be manifest in temperature and relative humidity, and in net radiation – the air above the grass site may be warmer (and lower RH) in the day and cooler (higher RH) above the woodland, but the timing of hot or cool events will be the same at both sites. We will emphasise this caveat more strongly in our revised manuscript. There are unlikely to be substantial differences in solar radiation or precipitation over the short distances described.

New flux equipment, installed in April 2009, measures some meteorological data from the top of the flux tower and we can inspect this to estimate what the biases in temperature and humidity may be.

Referee comment

*I have some reservations regarding the methodology as detailed below and also find that it is a bit brave to talk about interannual variability based on only two years of data. Other studies with more years have concluded that it is very difficult to derive simple meteorological explanations of the net carbon uptake in a single year.*

Author response

Please see previous responses to similar comments from earlier reviewers.

Referee comment

*p. 3770, l.20: The flux measurements were made at a height of 25m. No information is given on the height of the trees, but I suspect that the trees of such an old forest can easily be very close to this in height. Please add more detailed information on this and possibly also about the variation in tree height and topography.*

Author response

We are happy to add information on total basal area, maximum LAI, and soil properties to the Methods section of the paper. Tree height was about 15 to 20 m. Tree size distributions, as a basic proxy for tree age distributions, of the three main

species will be provided in the companion paper by Fenn et al and can also be included in the Appendix for this study.

Referee comment

*p. 3771, l. 14: It seems from the text that the only instrumentation on the mast is a sonic and open path LiCor. All other meteorological measurements were measured 840m away and no details are given. How and where e.g. was net radiation measured?*

Author response

Net radiation was measured at the AWS. We will provide details in the Methods section of the paper. See response to referee 2 above.

Referee comment

*p. 3771, l. 25: "standard corrections for open path sensors": Please specify what these are.*

Author response

We can include succinct details of the standard corrections in the revision, while bearing in mind reviewer 1's complaint of too much methodological detail.

Referee comment

*p.3772: Secondary data processing: The description of the procedures to clean up the data needs clarification. It is well known, that open path sensors perform poorly in moist conditions and that a special correction is needed ("Burba correction") to avoid false fluxes under certain meteorological conditions. The procedures laid out here seem to be somewhat subjective. A small table giving each criteria and the percentage of data removed would be helpful. The removal of negative night time fluxes seems a little bit risky to me and calls for a more serious analysis. Ideally there should be no photosynthesis in the dark and thus no CO<sub>2</sub> uptake. However, if the negative values are just results of noise in the measurement system, similarly small positive fluxes should be removed. Otherwise the results will be biased.*

Author response

The referee makes a valid point regarding the removal of negative night time fluxes. Although not ideal, we feel that the justification for undertaking this step is strong:

1. Retaining the negative night time values caused the partitioning process to fail.
2. The removed negative night time fluxes then underwent the gap filling process anyway.
3. It is highly probable that the negative night time values are caused by night-time advection processes from this hill site. As we did not have sufficient data to correct for advection effects, it can be argued that removing negative night time values effectively compensated for this. It could be argued that hill sites should be avoided for flux studies. However, we believe it is important to develop analysis approaches for hill sites, and that our paper demonstrates that the resulting flux estimates are credible when compared with independent data.
4. Retaining negative night time values led to highly improbable estimates of annual NEP, as described in the paper. Removing these and an equivalent NEE of positive data would lead to the same net value of NEP.

We did try numerous other methods in order to ensure that there was no positive bias to the data, as the referee suggests, including symmetrical exclusion of large positive fluxes. However, determining where to make the reductions in positive values was

extremely subjective. The implementation of monthly average reductions to hourly values left many positive values as negative – the very thing we were trying to eradicate in the first place! Implementing a threshold value for positive fluxes leaves too few fluxes and thus the reduction value creeps up to being too high.

With regards to the 'Burba correction', our initial analysis suggests that it makes little substantial difference (see response to Reviewer 2 above).

Referee comment

*p.3775, l.11: A very high percentage of the data has to be gap-filled. Here we are told that 32.5% of the data has to be filled, but after  $u^*$  correction this increases to 43% (p.3780, l. 3). This is a very high percentage of gap-filling and I miss some discussion on the impact of this on the reliability of the annual sums of NEE etc.*

Author response

As shown by numerous papers, gap-filling of around 40% is quite common (such as Moffat et al., 2007; Baldocchi, 2008). We will include some further discussion of whether this introduces any bias (we do not believe it does).

Referee comment

*p. 3778, l.9: As noted above the meteorological data are not measured at the site of the flux measurements. Here we are informed that it is not even in the forest but in open grassland. What does this mean to the calculation of ecosystem respiration (based on soil temperature) and to the derived conclusions regarding meteorological influence on the fluxes.*

Author response

See response to Reviewer 2 above.

Referee comment

*p. 3779, l. 24: The diurnal variation in respiration can be substantial due to temperature differences. Here this variation is assumed to be "small". How small?*

Author response

The challenge here is that although stem and soil respiration may be higher in the day because of higher temperatures, leaf respiration may be lower because of photoinhibition of leaf respiration. It remains an open research area how total ecosystem respiration varies between day and night; the assumption of no difference made here (and in many other studies) is probably as valid as the assumption of a pure temperature sensitivity made in some other flux studies.

Referee comment

*p. 3781, l. 7: Here we are told that the flux tower is on a hill. This is important information that should have been given in the Methodology section.*

Author response

We are happy to add this information to the Methods.

Referee comment

*p.3784, l. 19: I suppose that the study actually gave two complete years of measurements. Whether they are calendar years does not really matter. It is true that many by convention report fluxes for calendar years, but really not necessary.*

Author response



We will add the word 'calendar' to the sentence in question in order to clarify that we are referring to the 'only complete calendar year'.

#### Referee comment

*Figure 9: Since there are no clear relationships between the cumulated ecosystem fluxes and the average precipitation and temperature this figure does not add information over that given in Table 3 and could be left out.*

#### Author response

We feel that Figure 9 provides an important visual representation of the results of fluxes from studies around the world, which has not been attempted by other papers, and hence the figure plays an important context setting role.

#### References

Baldocchi, D.: TURNER REVIEW No. 15, "Breathing" of the terrestrial biosphere: lessons learned from a global network of carbon dioxide flux measurement systems, *Aust. J. Bot.*, 56, 1–26, 2008.

Butt, N., Campbell, G., Malhi, Y., Morecroft, M., Fenn, K. and Thomas, M.V. Initial Results from Establishment of a Long-term Broadleaf Monitoring Plot at Wytham Woods, Oxford, UK. [Online publication, 2009.](#)

Moffat, A. M., Papale, D., Reichstein, M., Hollinger, D. Y., Richardson, A. D., Barr, A. G., Beckstein, C., Braswell, B. H., Churkina, S. G., Desai, A. R., Falge, E., Gove, J. H., Heimann, M., Hui, D., Jarvis, A. J., Kattge, J., Noormets, A., and Stauch, V. J.: Comprehensive comparison of gap-filling techniques for eddy covariance net carbon fluxes, *Agr. Forest Meteorol.*, 147, 209–232, 2007.