Biogeosciences Discuss., 7, C1859–C1867, 2010
www.biogeosciences-discuss.net/7/C1859/2010/

Biogeosciences Discussions

Interactive comment on "Carbon dioxide fluxes over an ancient broadleaved deciduous woodland in southern England" *by* M. V. Thomas et al.

Anonymous Referee #2

Received and published: 16 July 2010

/// GENERAL COMMENTS ///

With this contribution, Thomas and co-workers provide estimates of the carbon balance of an European temperate mature woodland, completed by classical statistical modelling of flux time series, with the aim to interpret the observed variability in high frequency (hourly to daily) data. This study would purposely complete the set of data already published in syntheses papers documenting the role of mature forests in the global C balance (e.g. Luyssaert et al., 2008, Nature) if based on longer time series, which would help assess the dependence of interannual variations of C exchanges (balance and elementary fluxes) on meteorological and biological (e.g. biomasses, canopy properties) factors.

In the present state I would recommend this paper not to be published.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). Yet 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. 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.

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). I personally conducted such corrections for an analogous EC site (also temperate DBF) and observed the transition from net sink to net source for most of the wintertime originally-measured-as-net-sink periods.

/// SPECIFIC COMMENTS ///

Introduction

L. 15-end p. 3767: useless to remind the basic principles of EC in the introduction. This technique is extensively documented in dozens of dedicated papers.

L.9 p. 3768: add ref to Luyssaert et al., 2008 Nature

L. 11-14 p. 3768: The reference to the use of EC data for modelling is vague. EC data can be used to parameterise and / or validate ecosystem models.

Methodology

L. 6 p. 3769: Please precise the altitudinal amplitude at the study site (e.g. over the main footprint). This may help evaluating qualitatively the likeliness oh horizontal advection to occur.

L. 21- end of paragraph p. 3769: unnecessarily long. Write more concisely.

L. 22 p. 3770: Merge figs 1 and B1 (first graph of the latter, other 3 subgraphs are useless)

L.1 p. 3771: All data acquired from the LI-7500 should be processed considering the additional correction term proposed by Burba et al. (2008). Subsequent lines regarding the IRGA device are useless.

L. 14 p. 3771: Remove fig. 2

L.3 p. 3772: replace by "the mean product of mean-centred time series of vertical wind speed and mixing ratio of CO2", which is the definition of the covariance

L. 7 p.3772: Replace "Webb-Pearson-Leuning" by "Webb-Pearman-Leuning"

The authors should summarise all section 2.3.2 in a single table. Would shorten sub-

BGD

7, C1859–C1867, 2010

Interactive Comment

stantially the paper, while allowing conserving some details of the data treatments.

L.6 p. 3774: SEB is not, following to eq. 2, a budget. It's a ratio. I think mention to energy balance closure is not necessary in the paper, but was it to be conserved please provide a meaningful definition.

L. 23 p. 3774: Advection problems are not restricted to open-path IRGAs. Though i have no doubt the authors know this well, the sentence formulation is ambiguous. Refer here to papers having tried to document advection magnitude (e.g. papers from the ADVEX experiments lead by Aubinet and Feigenwinter).

Section 2.3.3: Shorten to a mention to the percentage of missing data + mention to the used gap-filling technique in one line (the techniques are already published, no need to document them in details again).

Section 2.3.4: No need to detail the calculation of u* thresholds. Refer to the corresponding Papale et al. (2006) and Reichstein et al. (2005) papers.

Section 2.3.5: Partitioning of net fluxes to elementary components (in first instance calculation of ecosystem respiration) was done through using a relationship between soil temperature and night-time fluxes. Soil Temp was measured in a grassland close to the woods of main contribution to EC measurements. The thermal amplitude in a grassland is higher than in a forest, as noted by the authors subsequently. As such, the use of a convex (exponential) relationship between night-time fluxes and (grassland) soil T leads to an overestimation of the ecosystem respiration (as a direct consequence of Jensen's inequality for convex functions) and could participate to the high Reco (and hence GPP) flux values reported for this site. I would encourage the authors to evaluate the sensitivity of the inferred elementary fluxes to different temperature time series (e.g. by artificially modifying the daily amplitude of T variation though conserving the same daily mean).

L. 1-5 p. 3778: why confining the statistical analysis to the use of polynomials? Is it a question of parsimony in the number of parameters with respect to other, more classical forms (saturating light-response curve, Q10 equation for respiration fluxes)? In any case the use of such forms would eventually help comparison with other studies.

Results

Section 3.1: a detailed description of meteo data is definitely not needed in the paper. Mention the principal meteo characteristics in site description is sufficient. Hence remove this section.

Section 3.2: I'm not particularly at ease with the idea of simply removing the negative night-time data in EC time series. The authors advocate that these non-realistic fluxes are likely to be marked by advection. I agree with this argument. However the use of such a threshold may not completely remove the bias. I encourage the authors to apply an algorithm recently developed by van Gorsel et al. (2009 AFM) based on the selection of dusk data less likely hindered by advection.

Section 3.3.1: This section contains a very classical description of flux fingerprint for a temperate deciduous forest. This does not increase our understanding of ecosystem functioning. As a more general remark, it is useless to give every graph details in the text section (e.g. "blue colours refer to highest C sink force"). Figure captions are most of the time self-sufficient.

L. 23 p. 3780: Can you precise what a "very small proportion" is in this case. This could help us interpreting the subsistence of significant net uptake in wintertime (if not due principally to the non-application of Burba correction).

L. 18 p. 3781: refer to fig caption

L. 7-8 p. 3782: i see no evidence for such "periods of increased temperatures" in the appropriate figure (Fig 3). The authors should further explain the occurrences of low summer Reco values (Aug-Sep) in 2007. Due to superficial drought? Measurement bias (e.g. large gap filled with low confidence)?

BGD

7, C1859–C1867, 2010

Interactive Comment

L15-end p. 3782: these are textbook explanation for the seasonal variations of GPP

L 3-11 p. 3783: remove this paragraph and figure B3.

Section 3.3.3: Reported values for Reco and GPP are the highest reported to date for a temperate deciduous forest (see e.g. table 3). The GPP fluxes are particularly high and comparable to those of temperate coniferous forests (see e.g. Grunwald & Bernhofer 2007 Tellus for a temperate spruce forest in Eastern Germany). I would recommend a careful examination of the plausible technical causes for such fluxes (analysis similar to results presented in Fig 4 but for annual integrals) + recalculation of all fluxes including the Burba correction as mentioned above.

L. 26 p.3783: reference to Fenn et al. 2010: which of both papers? Distinguish using letters a/b.

L1 p3784: It is not possible to explain interannual variance with only 2 years of measurement (i.e. 2 data points). The authors refer here to variance at higher frequency (hourly or daily, impossible to infer from text), possibly measured for 2 years, but certainly not representing interannual variations in flux time series. Please rephrase. L 10-18 p 3784: Valentini et al. 2000 (Nature) suggested based on limited evidence (low number of site-years) that intersite variations in NEP were mostly driven by variations in Reco. These results were questioned in more recent work, based on more extensive datasets (van Dijk et al. 2005 GBC, Reichstein et al. 2007 JGR). Further, the limited extension of the time series does not allow to infer firm conclusions with respect to the relative influences of GPP or Reco on interannual variations of NEP. Please reword this paragraph and temper your conclusions accordingly.

L 19-end of section 3.3.3 p3784: This paragraph is particularly confusing with respect to numbers. Obviously a mean annual flux value of 0.53 umol/m2/s does not correspond to 265 gC/m2/y (merely to 0.52*12*1800*1e-6*17520 = 200 gC/m2/y). This confusion stems i guess from the respective definitions of NEP and NEE used by the authors, which are not equivalent to NEP=-NEE as classically defined. If the authors are to use other definitions, these should be clearly stated in the methodology. Yet i see no clear paragraph on this subject (2 equation lines would suffice) in the present version of the paper.

L4 p3785: I have doubt with respect to the reported annual integral including NEEnight<0. Such a value of 8.2 tC/ha/y is too different from the 2.7-3.6 tC/ha/y reported whence the NEEnight <0 have been excluded to be realistic. Is such a difference caused by a few peaks in the night-time data (e.g. values around -100 umol/m2/s)?

Section 3.3.4: Most of this section is not new and bring no additional info to our current knowledge of ecosystem response to meteorological variables in temperate deciduous species (e.g. light saturation of NEE, compensation point etc.).

L.2 p.3786: the assertion that 90% of the PAR is absorbed by a canopy is a gross simplification... this may be the case for a close canopy. But it primarily depends on LAI, which is not documented for this forest. I would like to see such a LAI value in the paper in order to be able to evaluate the likeliness of this absorbance ratio.

L 13 p3786: I doubt that the magnitude of secondary leafing is sufficient to explain the increase in GPPmax observed by the authors. Would be good to provide estimates of this value, which I expect to be very low. The question of late occurrence of GPPmax may more likely be related to questions of leaf maturation (see papers from Morecroft et al. on these aspects) and/or modifications of ambient conditions which would favour higher values of photosynthesis to be achieved (e.g. higher, milder temperatures). The authors should discuss these points.

L7-end of section p3787: this paragraph referring to residual analysis is much confusing. If i understand well, the authors mix up residual analysis between LRC residuals (dependent variable) and meteo or respiration data as independent variables. I see no interpretation of the results in the following and the inferred relationships are pretty difficult to understand. Please clarify your points. If you are just to report R^2 of residual analysis and not interpret the results, just do not mention them in the paper.

BGD

7, C1859–C1867, 2010

L11 p3787: I do not understand the meaning of “respiration at 300 mm”... please clarify

L20 p3789: remind that Kutsch et al. (2008 Ecol. Appl.) recently questioned the early NEP numbers published by Knohl et al. (2003 AFM) after quantifying advection fluxes at their site.

L6 p3791: remove “despite”

L10-12 p3791: “the degree to which temperate deciduous forests can ameliorate future emissions”. This sentence is non-sense. Forests can do nothing about CO2 emissions in themselves, but reducing the amount of CO2 located in the atmospheric reservoir.

Tables and Figures

Table 1: i’m not sure this table is useful. Infos contained should be condensed and included in the caption of figure 1 describing the site.

Table 3: compile further estimates of actual evapotranspiration (as measured by EC latent heat fluxes) where available.

Figure 2 is useless.

Figure 4: Why is gap-filling presented in the second stage of flux correction here? Should appear after spikes, negative nights, u* correction and LE+H thresholds have been applied (all these latter stages in the data post-processing generate gaps in the time series which are subsequently filled by the gap-filling procedure). In the caption of the figure make clearly apparent the words “negative nights removed” and not “-ve nights removed”.

Figure 5: This is not a “pulse” figure. We classically refer such graphs as “fingerprints”. Please make reference to online GF tool apparent.

Figure 6: Respiration time series: I see no explanation in the text with respect to the sudden reduction of Reco observed by August 2007 and to a lesser extent Aug-Sep 2008. Might these reductions be caused by soil dryness? If measurements of soil

BGD

7, C1859–C1867, 2010

water are lacking, an index of moisture may be grossly derived as P-AET or P-PET (AET= actual evapotp, PET= potential evapotp).

Figure B1: I see no use in conserving subgraphs b-d. Subgraph a should be merged with fig 1.

Interactive comment on Biogeosciences Discuss., 7, 3765, 2010.