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Interactive comment on "CO_2 uptake of a mature *Acacia mangium* plantation estimated from sap flow measurements and stable carbon isotope discrimination" *by* H. Wang et al.

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Dear Dr. López:

Thank you for your kind attention and comments.

The title of the revised manuscript we submit to you is "CO2 uptake of a mature Acacia mangium plantation estimated from sap flow measurements and stable carbon isotope discrimination". The Paper was prepared by Hua Wang from Beijing Academy of Agriculture and Forestry Sciences, Ping Zhao, Lvliu Zou, Xiaoping Zeng, Guangyan

Ni, Xingquan Rao from South China Botanical Garden, Chinese Academy of Sciences, and Heather R. McCarthy from University of Oklahoma. Our manuscript was numbered as Manuscript # bgd-10-11583-2013.

We revised the above referenced manuscript according to comments provided by your instructions. The contents of manuscript have been significantly improved. It is submitted herewith for consideration by you, and by Biogeosciences.

Interactive comment on “CO2 uptake of a mature Acacia mangium plantation estimated from sap flow measurements and stable carbon isotope discrimination” by H. Wang et al. R. Poyatos López (Referee) r.poyatos@creaf.uab.es Received and published: 26 August 2013

General comments

This manuscript presents the results of a 4-year study of continuous sap flow, seasonal 13C discrimination in leaf sap and leaf-level gas exchange in Acacia mangium. Sap flow and leaf sap 13C values, together with air CO2 and 13C, and other biometric measurements, were used to estimate canopy-scale gross photosynthesis (FCO2). Instantaneous FCO2 was compared with leaf-level measurements and time-integrated (seasonal, annual) FCO2 estimated were compared with literature values. The dataset is indeed very complete, spanning a 4-year period, and the measurement techniques employed were, in general, adequate. (Q1) However, some aspects of sap flow measurements could be improved, such as the estimation of dtmax under zero flow. (Q2) Other aspects, such as the influence of natural stem temperature gradients were not even mentioned. (Q3) Clearly, the study would have benefited from having an eddy covariance tower simultaneously measuring net ecosystem CO2 exchange, and from which GPP could have been estimated for comparison with the SF/SI approach. Although I am aware that it is not easy to maintain this infrastructure for a 4-year period without specific support, the authors could have alternatively used a model to provide an independent validation for their estimates of FCO2 (Hu et al., 2010; Rascher et al.

2010). (Q4) The presentation of the results is good, although some of the data could have been better displayed in tables rather than in the main text. (Q5) The statistical treatment of the data could have been more robust by using linear mixed models for some of the analyses. The application of a sensitivity analysis is appreciated, although the results are very similar to those by Hu et al. (2010), and therefore lack novelty. (Q6) The boundaryline analysis of Gs seems unconnected to the rest of the study. Is it used to gap-fill the Gs time-series? (Q7) The authors should focus the study better in the introduction and improve the discussion (see specific comments). (Q8) This includes the choice of appropriate references. In its current state, the manuscript appears just as the application of an already developed methodology, albeit in a different species and using a relatively long and complete dataset. (Q9) Perhaps the authors could make more emphasis to discuss the interesting seasonal interannual variability in FCO2, and test whether one or various processbased models are capable of reproducing it (in line with the validation issue mentioned above). (Q10) Overall, the authors should carefully address all the issues suggested here (and those mentioned by T. Klein in his review) to produce a manuscript that is acceptable for publication in Biogesciences.

Response:

(R1) In field experiments by sap flow measurements, estimation of dtmax under zero flow are needed to be carefully and correctly considered. We did not do a good job in writing the original manuscript and was not able to enunciate our estimation clearly. In the revision, we added some explanations to make our estimation more persuasive (Specific comments R9). In our study, dtmax was estimated by using the maximum temperature difference for each night (using Baseliner program developed by Dr. Yavok from Duke Uni.). On the one hand, the characteristic of nocturnal water use of Acacia mangium can't lead to underestimating sap flow (Wang et al., 2012). On the other hand, when analyzing canopy stomatal conductance (Gs), data were selected to keep errors small (Ewers and Oren, 2000; Granier et al., 2000).

(R2) We considered the question you raised before initiating the experiment. In the

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revision, we added these detailed contents describing our way of avoiding the influence of natural stem temperature gradients. 'The sensors and adjacent portions of stem were wrapped with plastic insulation to protect the probes from mechanical damage, and the entire assembly was enclosed in an outer layer of aluminum film in order to minimize spurious temperature gradients caused by radiant heating of the stem, as well as to protect against water running down the trunk (Zhao et al. 2005b).' was added in the Method section 2.3 (P11589, L15).

Besides, sap flow measurements were also improved by reinstalling sensors at proper time. 'To ensure good measurements, new sets of sensors were reinstalled in positions 2–5 cm from the original holes at both the beginning and end of the dry season (Ma et al. 2008a).' was added in the Method section 2.3 (P11589, L15).

(R3) We think it is difficult for comparison of eddy correlation (EC) observation system with the SF/SI approach. Just as you said, it is not easy to maintain such an infrastructure for a 4-year period without specific support. We didn't have such a condition at that time.

We pondered on using an appropriate method. We think the SF/SI approach is proper than EC for our study, since it is suitable for upper-canopy species, and especially for single-species plantations, such as the Acacia mangium plantation in our study. EC application in measuring C uptake of forest faces some restrictions. Of them, the requirement on a flat landform restrains its application in mountain areas. Moreover, owing to high expense and low mobility, EC is at this moment not an appropriate observing system for our study. Additionally, this method is helpless in studying the contribution of different species to and the influence of population renewal on C balance within a mixed forest, since it is incapable of separating C fixation contribution by canopy from that by understorey and by soil, and unable to determine how these processes are affected by environmental factors as well. Herein, SF/SI is not comparable with eddy correlation (EC) observation system to some degree. CO2 flux estimated from EC represents ecosystem-scale flux, while CO2 flux from SF/SI represents canopy-scale flux.

It will be better to add this information in the Introduction section ‘This SF/SI can cover the shortage of eddy correlation (EC) observation system. It could avoid low mobility of EC, and suitable for upper-canopy species, especially for single-species plantations, such as the Acacia mangium plantation in our study (Zhao et al., 2005a).’(P11585, L29).

Notably, our approach is typical of experimental ecology research methods, which avoids over-reliance on model. However, there is greater uncertainty in the model approach due to estimations of a lot of parameters.

(R4) Changes have been made as suggested. Some of the data were displayed in tables and the main text was revised accordingly. Please see the specific changes in (Specific comments R12).

(R5) A great deal of data was accumulated during our study. Handling, organization, and analyses of the data set were a major task and were imperative for our study. The data were meaningful only after they were “correctly” analyzed. In this regard, the independent t-tests, partial correlation analysis, linear regression analyses, and non-linear regressions were included in our data handling. In the meantime, we applied a Monte Carlo method to quantify error propagation when modeling FCO2 based on measurements of Gs, Ca and Δ. Through these processes, the massive data sets were reduced to interpretable results leading to the final conclusions.

The linear mixed models are widely used in data analysis. Krauss et al. (2007) used a three-way, fixed effects, repeated measures analysis of ANOVA to reveal the effects of flooding on water transport in mangroves. Moore et al. (2010) applied a random-slope, linear mixed-effect model to quantify potential temporal sampling errors in Douglas-fir and red alder. Faubert et al. (2010) used linear mixed models to indicate the effect of the warming and litter addition treatments on the quantified BVOC emissions in repeated measurements. The linear mixed models are not suitable in our study, because in our opinion, they are more concise and succinct description of the data sets included response and variables, such as the effect of sapwood depth, flooding et al. on sap flux. In our study, we applied a simple, nondestructive method for the estimation of canopy CO2 uptake. This estimation was based on canopy averaged conductance and Ci/Ca. No treatments such as drought, warming were included in our study. In this regard, error propagation when modeling FCO2 based on measurements of Gs, Ca and Δ was in essence steps in data handling. For this reason, we chose to present our data with these methods.

Krauss, K. W., Young, P. J., Chambers, J. L., Doyle, T. W., and Twilley, R. R.: Sap flow characteristics of neotropical mangroves in flooded and drained soils, Tree Physiology, 27, 775-783, 10.1093/treephys/27.5.775, 2007.

Moore, G., Bond, B., Jones, J., and Meinzer, F.: Thermal-dissipation sap flow sensors may not yield consistent sap-flux estimates over multiple years, Trees, 24, 165-174, 10.1007/s00468-009-0390-4, 2010.

Faubert, P., Tiiva, P., Rinnan, Å., Michelsen, A., Holopainen, J. K., and Rinnan, R.: Doubled volatile organic compound emissions from subarctic tundra under simulated climate warming, New Phytologist, 187, 199-208, 10.1111/j.1469-8137.2010.03270.x, 2010.

(R6) When we discussed our results, we found it is necessary to reveal the stomatal control on canopy CO2 uptake, adding that the data was large, hence we chose the boundary line analysis of Gs. Besides, following the sensitivity analysis using a Monte Carlo approach for canopy CO2 uptake (FCO2), we found that the dynamics in the estimates of FCO2 were mainly driven by dynamics in Gs, compared with Ca and Δ. However, we have revised our discussion. Please see the specific changes in Specific comments R18, R19, R20.

(R7) A detailed response to specific comments The responses to all the points raised by the reviewers were showed as follows.

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(R8) The choice of appropriate references We have included appropriate references in the revised manuscript according to your suggestions. Also, we added related references on the basis of the revised content. Please see the specific changes in the response to the specific comments.

(R9) The interesting seasonal interannual variability in FCO2 Changes have been made as suggested. More explanations were added in the discussion section 4.2. 'Further, lower annual FCO2 and maximum monthly FCO2 in May in 2005 may be due to the frequent rainfall occurred in June and July' was revised as 'Further, lower annual FCO2 and maximum monthly FCO2 in May in 2005 may be due to the frequent rainfall occurred in June and July, or to both the SWC and the stand transpiration that are affected by some carryover effect' (P11602 L 14-16).

(R10) We have carefully revised manuscript raised by the reviewers in detail.

Specific comments

(Q1) P. 11584, L. 5. The abstract lacks one or two opening lines on the general motivation of the study and the presentation of the main research question(s) addressed.

(R1) The opening line on the aim of the study and the presentation of the main researched questions 'A simple, nondestructive method for the estimation of canopy CO2 uptake is important for understanding the CO2 exchange between forest and atmosphere.' was added as suggested (P11584, L2).

(Q2) P. 11584, L. 25. You could cite some more recent and relevant references such as IPCC's 4th assessment report.

(R2) 'IPCC's 4th assessment report' was cited as suggested. The specific changes were listed as follows. 'Schneider, 1989' was revised as 'Schneider, 1989; Pachauri and Reisinger, 2007' (P11584, L25). 'Pachauri, R., and Reisinger, A.: IPCC fourth assessment report, IPCC, Geneva, 2007.' was added in the reference section (P11610, L30).

(Q3) P. 11585, L. 9. The reference by Oren et al (2001) does not deal with the balance between C uptake and release in mature forests. Besides, a more recent synthesis suggests that mature forests are not carbon neutral (Luyssaert et al 2008).

(R3) The reference by Oren et al (2001), reference by Luyssaert et al (2008), and reference by Fang et al (2001) were carefully read. Changes have been made as suggested. The specific changes were listed as follows.

‘Additionally, studies have frequently shown that primary or mature forests reach a balance between C uptake and release, after which their further contribution to carbon fixation are no longer significant (Fang and Chen, 2001; Oren et al., 2001).’ was revised as ‘Additionally, more recent studies have frequently shown that planted forests (afforestation and reforestation) sequester carbon, old-growth forests continue to accumulate carbon, while natural forests loss carbon (Fang and Chen, 2001; Luyssaert et al., 2008).’ (P11585, L6-9).

‘Luyssaert, S., Schulze, E. D., Borner, A., Knohl, A., Hessenmoller, D., Law, B. E., Ciais, P., and Grace, J.: Old-growth forests as global carbon sinks, Nature, 455, 213-215, doi:10.1038, 2008.’ was added in the reference section (P11609, L23). ‘Fang, J. Y. and Chen, A. P.: Dynamic forest biomass carbon pools in China and their significance, Acta Bot. Sin., 43, 967–973, 2001 (in Chinese with English abstract).’ was replaced by ‘Fang, J. Y., Chen, A. P., Peng, C. H., Zhao, S. Q., and Ci, L. J.: Changes in Forest Biomass Carbon Storage in China Between 1949 and 1998, Science, 292, 2320-2322, 10.1126/science.1058629, 2001.’ (P11607, L21-22).

(Q4) P. 11585, L. 15-16. The following statement is not strictly true: ‘there is currently no commonly accepted estimation method for photosynthesis at the canopy scale’. Whole-tree chambers for CO2 exchange measurement do exist, although their use is limited to specific experiments (i.e. they are difficult to install in tall trees).

(R4) ‘There is currently no commonly accepted estimation method for photosynthesis at the canopy scale.’ was revised more precisely in statements as ‘there is currently rarely accepted estimation method for photosynthesis at the canopy scale.'(P11585, L15-16).

(Q5) P. 11585, L. 19-20. You forget to mention direct CO2 exchange in branch bags (e.g. Rayment and Jarvis 1999).

(R5) It was added as suggested. The specific changes were listed as follows.

'(1) combining water use efficiency obtained with branch bag techniques and whole-tree transpiration (Morén et al., 2001),' was revised as '(1) directly adopting branch bag techniques, or combining water use efficiency obtained with branch bag techniques and whole-tree transpiration (Rayment and Jarvis, 1999; Morén et al., 2001),' (P11585, L19-20).

'Rayment, M., and Jarvis, P.: Seasonal gas exchange of black spruce using an automatic branch bag system, Canadian Journal of Forest Research, 29, 1528-1538, doi:10.1139/x99-130, 1999.' was added in the reference section (P11611, L10).

(Q6) P. 11586, L. 3. Maybe it would be informative to be more precise as to where 13C discrimination is measured: bulk leaf organic matter, stem or leaf phloem sap, treerings?

(R6) '13C discrimination' was revised as '13C discrimination in leaf sap' as suggested (P11586, L3).

(Q7) P. 11586, L. 28 P. 11587, L. 1-2. These two last statements seem to appear out of nowhere; why do the authors have such expectations? This should be stated somewhere in the introduction.

(R7) The last two statements 'We also expected that FCO2 would be more sensitive to changes in Gs than in Ca and Δ. Finally, we expected that the FCO2 would be the highest in the mid-morning during summer months.' was deleted to make the whole paragraph more consistent and concise (P11586, L28; P11587, L1-2).

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(Q8) P. 11587, L. 22-23. This range in tree height (2 to 23 m) comprises trees originally planted in 1984 and naturally regenerated trees?

(R8) Yes. This information was added as ‘These trees comprise both trees originally planted in 1984 and naturally regenerated trees.’ as suggested (P11587, L23).

(Q9) P. 11589, L. 20-23. Using the maximum temperature difference for each night can lead to serious errors in the estimation of sap flow if conditions for night-time transpiration occur (relatively high VPD’s). Moreover, using this approach you could be underestimating sap flow, and hence Gs during the day. Have you tried alternative methods of dtmax estimation that account for this? (e.g. Oishi et al. 2009).

(R9) The nocturnal water use of the same Acacia mangium plantation was studied by combining sap flow measurements and gas exchange method (Wang et al., 2012). Granier’s thermal dissipation method was used to determine the nighttime sap flux of A. mangium. Gas exchange system was used to estimate nighttime leaf transpiration and stomatal conductance of studied trees. It was found that the nighttime sap flow of the same Acacia mangium plantation was mainly used for refilling water in the trunk, rather than nighttime transpiration. Hereby, using the maximum temperature difference for each night can’t lead underestimating sap flow. Besides, when analyzing Gs, we used all data except for those that were excluded based on the following criteria: (1) data collected during and 2 h after rainfall were excluded to avoid the discrepancy between evaporation and tree transpiration (Granier et al., 2000); (2) data were excluded when global radiation, or vapour pressure deficit, or stand transpiration were too low (< 5% of the maximum value), because of the large relative uncertainties in computing Gs under these conditions (Granier et al., 2000); (3) data were excluded when D < 0.6kPa for Gs estimation, to keep errors in Gs estimates to less than 10% (Ewers and Oren, 2000) (P11591, L9-16). Therefore, we selected the highest temperature difference between heated and unheated probes during times of zero flux as the baseline for each day (Granier, 1987; Phillips et al., 1997).

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To make this method of dtmax estimation more persuasive, some explanations were added. The specific changes were listed as follows.

‘We selected the highest temperature difference between heated and unheated probes during times of zero flux (ΔTm) as the baseline for each day using Baseliner program developed by Dr. Yavok from Duke Uni. (Granier, 1987; Phillips et al., 1997).’ was revised as ‘We selected the highest temperature difference between heated and unheated probes during times of zero flux (ΔTm) as the baseline for each day (Granier, 1987; Phillips et al., 1997).’ (P11589, L20-23).

‘It was found that the nighttime sap flow of the same Acacia mangium plantation was mainly used for refilling water in the trunk, rather than nighttime transpiration (Wang et al., 2012). Hereby, using the maximum temperature difference for each night can’t lead underestimating sap flow. Besides, when analyzing canopy stomatal conductance (Gs), data were selected to keep errors small (Ewers and Oren, 2000; Granier et al., 2000) was added at the end of the paragraph (P11589, L23).

Wang, H., Zhao, P., Hölscher, D., Wang, Q., Lu, P., Cai, X. A., and Zeng, X. P.: Nighttime sap flow of Acacia mangium and its implications for nighttime transpiration and stem water storage, Journal of Plant Ecology, 5, 294-304, 10.1093/jpe/rtr025, 2012.

(Q10) P. 11590, L. 22-24. Could you confirm the strong coupling by checking whether there is an effect of wind speed on Gs?

(R10) The effect of wind speed was carefully analyzed using curve estimations as suggested. In addition, to support the strong coupling, both the daily average wind speed and decoupling coefficient were calculated. The specific changes were listed as follows.

‘The A. mangium plantation has a low LAI, and thus an open, well-ventilated, aerodynamically rough canopy. Therefore, a strong coupling of canopy surface to the atmosphere can be assumed (Schäfer et al., 2000).’ was revised as ‘For our stand, a

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strong coupling of canopy surface to the atmosphere can be assumed. There may be three attributes. Firstly, the A. mangium plantation has a low LAI, and thus an open, well-ventilated, aerodynamically rough canopy. Secondly, the wind speed (w) has an effect on canopy stomatal conductance (Gs) of the A. mangium plantation. On the one hand, Gs significantly decreased with w in July and December from 2005 to 2007 ($p<0.05$; R2=0.106; Gs = -7.3437w + 62.548). On the other hand, most of the time, the daily average w above the plantation was greater or close to 2 m s-1 from 2005 to 2007. In this case, the boundary layer conductance was much higher than canopy stomatal conductance (Larcher, 1983). Finally, decoupling coefficient for our stand was less than 0.2 in 2005 (Ma, 2008).' (P11590, L22-24).

'Larcher, W.: Physiological Plant Ecology, Springer-Verlag, Berlin, 1983.' was added in the reference section (P11609, L17).

(Q11) P. 11596, L. 3-6. Could the authors use a more statistically robust method to deal with repeated measures and to include tree identity as a random factor? (linear mixed models?).

(R11) When we tested whether there were statistically significant differences in Ca, Ci, Ci/Ca, ïĄď13Cp, ïĄď13Ca, and Δ between the wet and dry seasons, in Ci/Ca between the sunlit leaves and shaded leaves, we chose the independent t-tests. Through this process, the data sets were led to the final conclusions. The linear mixed models are widely used in data analysis (Krauss et al., 2007; Moore et al., 2010; Faubert et al., 2010). However, in our opinion, they are more concise and succinct description of the data sets included response and variables, such as the effect of sapwood depth, flooding et al. on sap flux. For this reason, we chose the independent t-tests.

(Q12) P. 11598, L. 3-18. Maybe the data presented here is clearer in a table.

(R12) The content (P11598, L3-18) was presented in Table 2 and the text was changed accordingly. The specific changes were listed as follows.

The content (P11598, L3-18) was replaced by ‘The diurnal Ca, Ci, Ci/Ca, ïĄď13Cp, ïĄď13Ca, and ∆ fluctuations were observed (Table 2). CVs of diurnal Ca, Ci, Ci/Ca, ïĄď13Cp, ïĄď13Ca, and ∆ were below 5.5%, 11.6%, 10.1%, 6.4%, 12.7%, and 11.2%, respectively. Because of the lack of strong variability in diurnalïĂă∆ values, the average ∆ value was used in subsequent calculations of FCO2 for the whole season. Further, values of diurnal Ci/Ca for sunlit leaves ranged between 0.55 and 0.94, while those of shaded leaves ranged between 0.68 and 0.95. Mean daily Ci/Ca value of 0.76 for sunlit leaves was significantly lower than that of 0.83 for shaded leaves (p = 0.001). At the seasonal scale, CVs of seasonal Ca, Ci, Ci/Ca, ïĄď13Cp, ïĄď13Ca, and ∆ were below 5.1%, 5.0%, 3.3%, 5.1%, 14.4%, and 9.8% (Table 2). Furthermore, there were no significant differences in Ca, Ci, Ci/Ca, ïĄď13Cp, ïĄď13Ca, and ∆ between the wet season and dry season (p = 0.212, p = 0.781, p = 0.238, p = 0.420, p = 0.118, p = 0.988).’(P11598, L3-18).

‘Table 2’ was added (P11614). Table 2. The diurnal ranges, seasonal means, and coefficients of variation (CV) in ambient CO2 concentration (Ca), intercellular CO2 concentration (Ci), the ratio of the intercellular and ambient CO2 concentrations (Ci/Ca), and carbon isotope compositions in leaf sap (ïĄď13Cp), isotope compositions in the canopy air (ïĄď13Ca) and the photosynthetic 13C discrimination (∆). Diurnal scale Seasonal scale Range CV Mean wet season vs. dry season Maximum CV Ca (ïĄ■mol mol-1) 381.51 – 416.58 0.02 – 0.05 399.06 vs. 386.85 0.05 Ci (ïĄ■mol mol-1) 279.35 – 356.30 0.02 – 0.12 308.30 vs. 311.74 0.05 Ci/Ca 0.72 – 0.90 0.04 – 0.10 0.81 vs. 0.78 0.03 ïĄď13Cp(‰ -31.96 – -29.91 0.03 – 0.06 -31.42 vs. -30.75 0.05 ïĄď13Ca(‰ -10.59 – -8.82 0.04 – 0.13 -9.62 vs. -8.92 0.14 ∆(‰ 20.47 – 23.89 0.05 – 0.11 22.51 vs. 22.52 0.10

The former ‘Table 2’ was changed into ‘Table 3’ (P11598, L22). The former ‘Table 2’ was changed into ‘Table 3’ (P11614, L1). The former ‘Table 3’ was changed into ‘Table 4’ (P11599, L3). The former ‘Table 3’ was changed into ‘Table 4’ (P11602, L26). The former ‘Table 3’ was changed into ‘Table 4’ (P11615, L1). The former ‘Table 4’ was

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changed into ‘Table 5’ (P11599, L24). The former ‘Table 4’ was changed into ‘Table 5’ (P11603, L25). The former ‘Table 4’ was changed into ‘Table 5’ (P11604, L23-24). The former ‘Table 4’ was changed into ‘Table 5’ (P11616, L1). The former ‘Table 5’ was changed into ‘Table 6’ (P11600, L17). The former ‘Table 5’ was changed into ‘Table 6’ (P11617, L1).

(Q13) P. 11599, L. 23-27. This comparison with published values may be better placed in the discussion section.

(R13) The published values for comparison ‘For comparison, the gross primary production (GPP) of forests in different climate zones, using several different methods, was summarized in Table 5. GPP in tropical forests (8.35 ± 0.23 gC m−2 day-1) was significantly higher than that in sub-tropical forests (5.38 ± 0.46 gC m−2 day-1, $p<0.001$), and temperate forests (2.85 ± 0.45 gC m−2 day-1, $p<0.001$).’ was moved to the discussion section 4.4 as the first sentence (P11603, L24).

(Q14) P. 11600, L. 1-13. I would combine the leaf-level data with the instantaneous FCO2 (i.e. the beginning of section 3.3) and then move to seasonal and annual values. The way it is written now does not seem very logical to me: FCO2, time-integrated values, literature comparisons and leaf-level values.

(R14) The leaf-level data was combined with the instantaneous FCO2 to present the reader with an organized, logical result as suggested (section 3.3). The specific changes were listed as follows. The two paragraphs in the section 3.3 were rewritten as follows.

Paragraph 1 ‘At the leaf level scale, diurnal leaf stomatal conductance (gs), net photosynthesis (Pn) of A. mangium obtained through gas exchange measurements reached its peak around 9:00 in the wet season, and around 11:00 in the dry season (Fig. 5). The diurnal means of gs in all leaves, sunlit leaves, and shaded leaves were 136.40, 129.42, and 144.34 mmol m-2 s-1 in the wet season, and 58.65, 56.74, and 60.42 mmol m-2 s-1 in the dry season, respectively. The diurnal means of Pn in all leaves, sunlit leaves, and shaded leaves were 3.42, 3.93, and 2.84 μmol m-2 s-1 in the wet season, and 1.85, 2.17, and 1.34μmol m-2 s-1 in the dry season, respectively. Notably, CVs were high (ranging mostly from 40 % to 60 %) for gs, and Pn. At the canopy level scale, diurnal canopy CO2 uptake rate (FCO2) peaked around 9:00, with a maximum of 7.3 μmol m-2 s-1 in the wet season (Fig. 6a). Diurnal FCO2 was more symmetrical and smoother, reaching a maximum of 3.9 μmol m-2 s-1 around 11:00 (Fig. 6a). The diurnal means of Gs and FCO2 in all leaves were 56.70 mmol m-2 s-1 and 2.78μmol m-2 s-1 in the wet season, 28.81 mmol m-2 s-1 and 1.32μmol m-2 s-1 in the dry season. Further, as shown in Fig. 7, values of both Gs and FCO2 were lower than those of gs, and of Pn, with regression coefficients of determination (R2) of 0.58 and 0.19 in the wet season, 0.70 and 0.65 in the dry season, and 0.44 and 0.35 in all seasons, respectively .' (11599, L9-27).

Paragraph 2 'Based on daily FCO2, monthly, seasonal, and inter-annual FCO2 were calculated. Maximum monthly FCO2 occurred in July (155 gC m-2), May (98 gC m-2), July (129 gC m-2), and August (117 gC m-2) in 2004, 2005, 2006 and 2007, respectively (Fig. 6b). FCO2 of the A. mangium stand in the wet season (522 gC m-2) was considerably higher than that in the dry season (255 gC m-2) (Fig. 6c). Annual FCO2 was 820, 564, 895 and 828 gC m-2 in 2004, 2005, 2006 and 2007, respectively. FCO2 in the wet season contributed 72, 71, 66, and 61% of the total annual CO2 uptake (Fig. 6d). Across years 2004, 2005, 2006 and 2007, the daily and annual means of canopy CO2 uptake for mature A. mangium plantation was 2.13 ± 0.40 gC m-2 day-1 and 776.89 ± 145.76 gC m-2, respectively.' (P11600, L1-13).

The former 'Fig. 6' was changed into 'Fig. 5' (P11600, L3). The former 'Fig. 6' (P 11623, L1) was changed into 'Fig. 5' (P11622, L1). The former 'Fig. 5a' was changed into 'Fig. 6a' (P11599, L10). The former 'Fig. 5' (P 11622, L1) was changed into 'Fig. 6' (P11623, L1). The former 'Fig. 5b' was changed into 'Fig. 6b' (P11599, L16). The former 'Fig. 5c' was changed into 'Fig. 6c' (P11599, L18). The former 'Fig. 5d' was changed into 'Fig. 6d' (P11599, L20).

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(Q15) P. 11601, L. 3-13. What is the purpose of this paragraph? You use D to estimate Gs while Hu et al (2010) also use D to calculate WUE. Therefore, an estimate of D is involved in both approaches.

(R15) This paragraph aims to indicate the development and the theory basis of this method. As you suggested, as D is the driving force for sap flow, changes in this parameter directly affect conductance estimates and, thus, canopy photosynthesis. Herein, we rewrote this paragraph as follows.

The content in P11601, L3-13 was written as ‘To our knowledge, all methods for direct measurement of canopy CO2 uptake rely on scaling. Morén et al. (2001) combined the water use efficiency (WUE) on the basis of branch gas-exchange measurements and canopy transpiration (E) as conversion scalar to get canopy carbon assimilation of a boreal forest, and also found canopy WUE showed a strong dependency on vapour pressure deficit (D). Catovsky et al. (2002) combined whole-tree sap flow measurements with micrometeorological monitoring and leaf-level gas exchange to determine whole-tree carbon gain, and showed that estimates of canopy photosynthesis were most sensitive to measurements of D and the relationship between photosynthesis and conductance. However, Catovsky’s and Morén’s methods require a lot of measurements of gas exchange on leaves or branches. More recently, to solve the difficulties mentioned above, Zhao et al. (2005a) made use of sap flow technique for scaling and isotope technique for plant water use, and then proposed a new approach combining the sap flow measurement and the stable 13C techniques to estimate forest C assimilation. Herein, the relation between Δ and Ci/Ca raised by Farquhar et al. (1982) is integrated with that between FCO2 and stomatal conductance for CO2, so that a new relationship linking Δ, FCO2 and Gs was obtained. Consistent with us, Hu et al. (2010) used observed transpiration rates and needle sugar carbon isotope ratios to estimate whole-tree carbon assimilation rate, and then combined with species distribution and tree size to estimate gross primary productivity (GPP) using an ecosystem process model.’ (P11601, L3-13).

(Q16) P. 11601, L. 26 - P. 11602, L. 1-6. See also my previous comment on the estimation of dtmax. Check whether an alternative calculation of dtmax and sap flow improves the relationships between leaf level gs and Gs.

(R16) We responded to (Q9) in detail to make this method of dtmax estimation more persuasive. Aslo, some related explanations were also added. Please see our responses and the changes in (R9).

(Q17) P. 11602, L. 11. What do you mean by ‘heat load’? Is it ‘evaporative demand’?

(R17) ‘heat load’ was revised as ‘evaporative demand’ as suggested (P11602, L11).

(Q18) P. 11602, L. 17-21. Is this relevant? It should be fine mentioning that the equation is a simplification of the PM equation in the methods section.

(R18) It was revised as suggested. The specific changes were listed as follows.

‘Estimates of canopy conductance from the Penman-Monteith formula were very close to those obtained with the simplified equation by Köstner et al. (1992) (Ma, 2008), as would be expected given that the Köstner et al. (1992) equation is based on Penman-Monteith.’ were deleted (P11602, L17-21).

‘Therefore, the mean canopy stomatal conductance (Gs, mmol H2O m-2 s-1) of the study stand can be calculated based on a simplified equation (Köstner et al., 1992):’ in the methods section 2.4 was revised as ‘Therefore, the mean canopy stomatal conductance (Gs, mmol H2O m-2 s-1) of the study stand can be calculated based on a simplification of the Penman-Monteith equation (Köstner et al., 1992):’(P11591, L3-5).

(Q19) P. 11602, L. 24-25. This sentence is not very accurate; the hydraulic system needs to be protected from what?

(R19) It was revised as ‘The increase of D led to a decrease in Gs despite the increase in PAR, because the hydraulic system needed to be protected from cavitation.’ as suggested (P11602, L24-25).

(Q20) P. 11602, L. 29 - P. 11603, L. 1-2. Why do you refer to the Fernandez et al. 2009 study here? Moreover, high Gs at low D is what you also find? In what sense is this inconsistent with your results?

(R20)'Gs stayed high at low D' was revised as 'Gs stayed high at high D' as suggested (P11603 L1).

(Q21) P. 11603, L. 5. Maybe the definition of the abbreviation SF/SI should have appeared earlier in the text.

(R21) The definition of the abbreviation SF/SI has been revised to appear in the beginning of the Introduction section. The specific changes were listed as follows.

'a novel approach combining sap flow measurement and stable carbon isotope techniques' was revised as 'a novel approach combining sap flow measurement and stable carbon isotope techniques (SF/SI)'(P11585 L28-29). 'new approach' was revised as 'SF/SI'(P11586 L3-4). 'this sap flux/stable isotope combination' was replaced as 'SF/SI'(P11586 L10).

(Q22) P. 11603, L. 7-9. You should be consistent with what you stated in P. 11597, L. 6-7 about the seasonal variation in LAI.

(R22) The independent samples t-test was performed to analyze whether there's significant difference between LAI in the dry season and that in the wet season. LAI in the dry season (1.91 ± 0.38) was lower than that in the wet season (2.04 ± 0.22), but no significant difference was found ($p=0.151>0.05$). Based on these above analysis, we revised the text to make them to be consistent as follows.

'in contrast with the relatively small seasonal variation in LAI' was revised as 'in contrast with the relatively small seasonal variation in LAI ($p=0.151>0.05$)'(P11597 L6-7).

'Better agreement between FCO2 and Pn in the dry season was likely due to the fact that the lower leaf area index in the dry season would tend to minimize the effects of leaf shading, which can diminish conductance in lower-canopy leaves.' was revised as

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‘Better agreement between FCO2 and Pn in the dry season was likely due to the fact that the lower leaf area index in the dry season (1.91 ± 0.38 in the dry season vs. 2.04 ± 0.22 in the wet season) would tend to minimize the effects of leaf shading, which can diminish conductance in lower-canopy leaves.’(P11603 L7-9).

(Q23) P. 11604, L. 6-10. It is not clear at all what leads to a decline in photosynthetic capacity of A.mangium.

(R23) The reasons for the inference that high Ci/Ca values leads to a decline in photosynthetic capacity of A.mangium are given in detail. The specific changes were listed as follows.

The sentence ‘Our Ci/Ca ratio varied from 0.76 to 0.84, which was higher than the ranges found in cottonwoods in a riparian woodland (0.75–0.78) (Letts et al., 2008), in 13-yr-old loblolly pine (Pinus taeda) trees (0.45–0.80) (Maier et al., 2002), and in nine well-watered conifer species (0.57–0.68) (Brodribb, 1996), leading to a decline in photosynthetic capacity in A. mangium. Relatively high Ci/Ca ratios have also been found in a Canarian laurel forest tree species (Laurus azorica) (0.67–0.95) (González-Rodríguez et al., 2001). (P11604 L6-12)’ was deleted. The related content was rewritten and moved to the end of the paragraph (P11604 L15).

The reasons for the inference that high Ci/Ca values leads to a decline in photosynthetic capacity of A.mangium are given in detail as ‘The Ci/Ca ratio, which is maintained at a constant or near-constant value in many plant species, represents a balance between the rates of inward CO2 diffusion (controlled by stomatal conductance) and CO2 assimilation (controlled by photosynthetic light/dark reactions) (Ehleringer and Cerling, 1995). In our study, the Ci/Ca ratio varied from 0.76 to 0.84, which was higher than the ranges found in cottonwoods in a riparian woodland (0.75–0.78) (Letts et al., 2008), in 13-yr-old loblolly pine (Pinus taeda) trees (0.45–0.80) (Maier et al., 2002), and in nine well-watered conifer species (0.57–0.68) (Brodribb, 1996). Relatively high Ci/Ca ratios have also been found in tropical rain forest species (Lloyd and Farqhar, 1994; Ishida et al., 1996), a Canarian laurel forest tree species (Laurus azorica) (González-Rodríguez et al., 2001). There are three possible reasons for the inference that the Ci/Ca ratio may result in a decrease in photosynthetic rate of our study species. Firstly, high Ci/Ca values may suggest a relatively small stomatal limitation to net photosynthetic rate and non-conservative water use (González-Rodríguez et al., 2001; Ishida et al., 1996). Consistent with this inference, Cienciala et al. (2000) also observed no apparent limitation to water flux in A. mangium. It is likely that high transpiration rates may cause a localized leaf water-deficit, which depletes photosynthetic capacity (Sharkey, 1984). Secondly, non-stomatal limitations were may also be responsible for the decline in assimilation rates (Lauer and Boyer, 1992). Thirdly, the reduction in photosynthesis associated with leaf senescence should have high Ci/Ca values (Ponton et al., 2006).' (P11604 L15).

The reference 'Cienciala, E., Kučera, J., and Malmer, A.: Tree sap flow and stand transpiration of two Acacia mangium plantations in Sabah, Borneo, Journal of Hydrology, 236, 109–120, doi:10.1016/S0022-1694(00)00291-2, 2000.' was added in the reference list (P11607 L12).

The reference 'Ehleringer, J. R., and Cerling, T. E.: Atmospheric CO2 and the ratio of intercellular to ambient CO2 concentrations in plants, Tree Physiology, 15, 105–111, doi:10.1093/treephys/15.2.105, 1995.' was added in the reference list (P11607 L18).

The reference 'Ishida, A., Toma, T., Matsumoto, Y., Yap, S. K., and Maruyama, Y.: Diurnal changes in leaf gas exchange characteristics in the uppermost canopy of a rain forest tree, Dryobalanops aromatica Gaertn. F., Tree Physiology, 16, 779–785, doi:10.1093/treephys/16.9.779, 1996.' was added in the reference list (P11608 L26).

The reference 'Lloyd, J., and Farquhar, G.: 13C discrimination during CO2 assimilation by the terrestrial biosphere, Oecologia, 99, 201–215, doi:10.1007/bf00627732, 1994.' was added in the reference list (P11609 L20).

The reference 'Ponton, S., Flanagan, L. B., Alstad, K. P., Johnson, B. G., Morgenstern, K. A. I., Kljun, N., Black, T. A., and Barr, A. G.: Comparison of ecosystem water-use efficiency among Douglas-fir forest, aspen forest and grassland using eddy covariance and carbon isotope techniques, Global Change Biology, 12, 294–310, doi:10.1111/j.1365-2486.2005.01103.x, 2006.' was added in the reference list (P11611 L7).

The reference 'Sharkey, T.: Transpiration-induced changes in the photosynthetic capacity of leaves, Planta, 160, 143–150, doi:10.1007/bf00392862, 1984.' was added in the reference list (P11611 L21).

(Q24) P. 11604, L. 18-19. Could you explain in more detail why you attribute the low GPP to age-related decline in photosynthesis in 20-year-old trees? If you refer to the lowest C uptake values compared to those in the Brazilian study, the main reason would be their juvenile, faster growing stage vs more mature stage of the trees in the present study. I would only use the terminology 'age-related decline' for older trees, past their maturity.

(R24) The difference between our study and the Brazilian study was explored in more detail as suggested. The growth stage was stated as an important reason for this difference as suggested. Hereby, it was revised as 'Our lower estimates of GPP may be partly explained by the slower growth rates of maturity stage of trees in our study than juvenile stage of trees in the Brazilian study (20 yr-old vs. 4–6 yr-old). It was also probably the result of lower photosynthetically active radiation absorbed by the canopy of trees in our study, due to lower stand density (734 trees ha-1 vs 1111 trees ha-1), and lower LAI (1.95 m2m-2 vs. 3.479 m2m-2) (Ma, 2008a; Nouvellon et al., 2012)' (P11604 L18-19).

(Q25) P. 11605, L. 8-10. I don't understand this sentence; please clarify.

(R25) This sentence was revised as 'As shown in Eq. (15), Δ is the proportioning coefficient between Gs and FCO2.' (P11605 L8-10) to be clear as suggested and combined with the following content (P11605 L12-14). (15)

(Q26) P. 11605, L. 12-14. This sentence is suspiciously similar to what Hu et al (2010) wrote in their discussion.

(R26) It was rewritten to make it clear and fluent as suggested. The specific changes were listed as follows.

'In our model shown in Eq. (15), the proportioning coefficient between Gs and FCO2 is Δ, which is ultimately dependent on the scaling between Gs and FCO2. Therefore, we get a mechanistic relationship linking Δ, Gs and FCO2. Variation in Δ between seasons is insignificant (p = 0.988), compared with variation in Gs, since Δ is mathematically constrained as the ratio of two co-varying fluxes, and partly because stomata function in a way that optimizes Gs and FCO2 toward conserved values in response to environmental changes (Wong et al., 1979). Further, Ci/Ca is a very useful parameter to model FCO2.' was revised as 'Variation in Δ between seasons is non-significant (p = 0.988), compared with variation in Gs. This may be due to the fact that Δ, the proportioning coefficient between Gs and FCO2 as shown in Eq. (15), is constrained as the ratio of two co-varying variables. It may also be due to the evidence that stomata functions in a way to optimize Gs and FCO2 in relation to environmental changes (Wong et al., 1979).' (P11605 L11-15).

'In our model shown in Eq. (15), the proportioning coefficient between Gs and FCO2 is Δ, which is ultimately dependent on the scaling between Gs and FCO2. Therefore, we get a mechanistic relationship linking Δ, Gs and FCO2.' was deleted and combined with the following content (P11605 L11-15).

Other comments

(Q1) P. 11598, L. 27. I think 'insignificant' should be replaced by, 'non-significant'.

(R1) 'insignificant' was replaced by 'non-significant' as suggested (P11598 L27). Similarly, 'insignificant' was also replaced by 'non-significant' as suggested in P11605 L11.

(Q2) P. 11601, L. 24. There is a comma in the end, followed by a new sentence starting with capital letters.

(R2) The comma ‘,’ in the end was replaced by the period ‘.’ (P11601 L24).

(Q3) P. 11605, L. 15-16. Have been reported.

(R3) ‘Seasonally constant Ci/Ca values have reported from a wide range of field experiments’ was revised as ‘Seasonally constant Ci/Ca values have been reported from a wide range of field experiments’ as suggested (P11605 L15-16).

(Q4) References Hu J, Moore DJP, Riveros-Iregui DA, Burns SP, Monson RK. 2010. Modeling whole-tree carbon assimilation rate using observed transpiration rates and needle sugar carbon isotope ratios. New Phytologist 185:1000–15. Luyssaert S, Schulze E-D, Börner A, Knohl A, Hessenmöller D, Law BE, Ciais P, Grace J. 2008. Old-growth forests as global carbon sinks. Nature 455:213–5. Oishi AC, Oren R, Stoy PC. 2008. Estimating components of forest evapotranspiration: A footprint approach for scaling sap flux measurements. Agricultural and Forest Meteorology 148:1719–32. Rascher KG, Máguas C, Werner C. 2010. On the use of phloem sap _13C as an indicator of canopy carbon discrimination. Tree Physiol 30:1499–514. Rayment MB, Jarvis PG. 1999. Seasonal gas exchange of black spruce using an automatic branch bag system. Canadian Journal of Forest Research 29:1528–38.

(R4) Thank you for reminding us of these related references. The specific changes were listed as follows.

The references by Hu et al. (2010), by Rascher et al. (2010) have already been included in the reference list. ‘Luyssaert, S., Schulze, E. D., Borner, A., Knohl, A., Hessenmoller, D., Law, B. E., Ciais, P., and Grace, J.: Old-growth forests as global carbon sinks, Nature, 455, 213-215, doi:10.1038, 2008.’ was added in the reference section (P11609, L23). ‘Rayment, M., and Jarvis, P.: Seasonal gas exchange of black spruce using an automatic branch bag system, Canadian Journal of Forest Research, 29, 1528-1538, doi:10.1139/x99-130, 1999.’ was added in the reference section (P11611, L10).

We appreciate the opportunity of submitting our revised manuscript to Biogeosciences for consideration.

Sincerely,

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