

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
C4550

techniques employed were, in general, adequate. However, some aspects of sap flow measurements could be improved, such as the estimation of dt_{max} under zero flow. Other aspects, such as the influence of natural stem temperature gradients were not even mentioned. 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)

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. 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. The boundary-line analysis of G_s seems unconnected to the rest of the study. Is it used to gap-fill the G_s time-series?

The authors should focus the study better in the introduction and improve the discussion (see specific comments). 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. Perhaps the authors could make more emphasis to discuss the interesting seasonal interannual variability in FCO₂, and test whether one or various process-based models are capable of reproducing it (in line with the validation issue mentioned above). 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 Biogeosciences.

Specific comments

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.

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

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).

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).

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

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, tree-rings?

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.

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

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 G_s during the day. Have you tried alternative methods of dt_{max} estimation that account for this? (e.g. Oishi et al. 2009).

C4552

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

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?).

P. 11598, L. 3-18. Maybe the data presented here is more clear in a table.

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

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.

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

P. 11601, L. 26 - P. 11602, L. 1-6. See also my previous comment on the estimation of dt_{max}. Check whether an alternative calculation of dt_{max} and sap flow improves the relationships between leaf level g_s and G_s.

P. 11602, L. 11. What do you mean by 'heat load'? Is it 'evaporative demand'?

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.

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

P. 11602, L. 29 - P. 11603, L. 1-2. Why do you refer to the Fernandez et al. 2009 study here? Moreover, high G_s at low D is what you also find? In what sense is this

C4553

inconsistent with your results?

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

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.

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

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.

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

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

Other comments

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

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

P. 11605, L. 15-16. Have been reported.

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.

C4554

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 $\delta^{13}\text{C}$ 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.

Interactive comment on Biogeosciences Discuss., 10, 11583, 2013.

C4555