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Interactive comment on “Internal respiration of Amazon tree stems greatly exceeds external CO₂ efflux” by A. Angert et al.

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General comments

This is a well written paper on a highly relevant topic. It adds to previous evidence on the difference between stem CO₂ efflux and stem respiration. Usually, stem respiration is measured with chambers placed over the stem surface and is then estimated from the CO₂ efflux into the chamber. These measurements will underestimate the stem respiratory rate if some of the CO₂ produced in a given stem section is dissolved in the xylem water and is carried up in the transpiration stream. This study suggests that adding measurements of the O₂ concentration within the stems informs us about the proportion of the respired CO₂ that is transported within the stem rather than emitted. If carbohydrates were the main substrate for respiration, the proportion of CO₂ evolution

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and O₂ consumption, termed apparent respiratory coefficient, will equal 1. Ratios >1 will indicate a change of respiratory substrate, while ratios <1 will indicate that some of the produced CO₂ is removed by the transpiration stream. Support for the feasibility of their method comes from the fact that they performed similar measurements in soil, where they obtained respiratory quotients close to 1. Stem ARQ values were always lower, indicating that some of the respired CO₂ was missing. The authors conclude that this missing CO₂ is most likely carried up in the transpiration stream, released in the canopy, and re-fixed during photosynthesis. They estimate that this source of C can account for up to 10% of the gross tree fixation, which is quite significant.

Specific comments

While I agree with the overall results of this study, I have a few comments:

1. This method assumes that the CO₂ concentration within the stem is affected by xylem flow, but the O₂ concentration is mostly insensitive to transpiration because of the much lower water solubility of O₂ than CO₂. Thus, it is assumed that CO₂ can be removed by the transpiration stream but there is no O₂ supplied or removed by the xylem water. The authors discuss the possibility of O₂ removal by the transpiration stream and conclude that this has an insignificant effect with a maximum of 10% of the O₂ consumed by respiration removed at a height of 1 m. Alternatively, O₂ could be released by the transpiration stream to support respiration in deep areas where there is very little O₂. This possibility is not discussed in the paper, but there is previous research suggesting that both radial diffusion through the phloem and vertical transport within the transpiration stream can supply O₂ to the stems. Support for the supply of stem O₂ by xylem water comes from research that revealed reduced stem O₂ concentrations during drought (Eklund, 2000), at night (Mancuso & Marras, 2003) or when roots were flooded (del Hierro et al., 2002). Additionally there have been reports of correlations between daily patterns in O₂ concentration with transpiration and temperature (del Hierro et al., 2002). How much O₂ can be supplied from the sapwood to the cambial zone/phloem might depend on the structure and diffusive properties of

the wood in different species. I think that the outer stem layers most likely use ambient O₂, however xylem living parenchyma might rely at least to some extent on xylem transported O₂. This could affect the comparison between deeper and outer chamber readings.

2. I know this might be difficult, but it would help if the authors could include some estimate of the potential for removal of CO₂ by sapflow, similar to the estimate included for O₂ (lines 1 to 8, page 11460), in order to assess if it can account for the 35% missing CO₂ reported.

3. ARQ is calculated as $(0/76) \cdot (\Delta\text{CO}_2 / \Delta\text{O}_2)$ under steady state conditions but as $\Delta\text{CO}_2 / \Delta\text{O}_2$ for non steady state. This is based on the equilibration process presented in Figure 2. I see how this is appropriate at both extremes of the curve. However, in the middle (after 2 to 8 hours), which corresponds to the time period when the first set of samples were collected, values for ARQ do not appear to be derived by either $\Delta\text{CO}_2 / \Delta\text{O}_2$ or $0.76\Delta\text{CO}_2 / \Delta\text{O}_2$. The values in this portion of the figure appear to lie somewhere in between. Do the authors have any suggestions as to what kind of error this simplification introduces?

4. Were the ambient [CO₂] and [O₂] values used in the calculations the same for stem and soils? The CO₂ concentration of the air above the soil surface is likely to be higher than ambient values, especially if [CO₂] values for the soil were as large as 2.6%.

5. Table 3 shows some seasonal patterns that are not subsequently discussed. Is this method sensitive enough to reveal seasonal or species differences?

6. In lines 20 to 25 of page 11463, it is argued that that re-fixation is expected to increase the plant resilience to drought, since when stomata close, there is still a supply of CO₂ from stem respiration. However, when stomata close, there will also be less transpiration and thus less CO₂ supplied to photosynthesis from internal re-fixation.

7. The authors conclude that an important photosynthesis flux is mixing from the cur-

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rent carbon balance estimates (page 11465 line 10). I agree that stem respiration fluxes are underestimated. However, I think there is no change in the total C budget; it only changes the partitioning of the C budget.

Technical corrections

- I think Figure 1a would be clearer if the fluxes E_o and E_c would have been drawn between the stem and the ambient instead of between the stem and the chamber.
- Page 11446, lines 25 and 26 and page 11447 line 6: C3 and C4, the 3 and 4 should be subscript.
- Page 11456, line 20, Eq 5: the conductance (g), should be written as "go" to be consistent with the nomenclature previously used.
- Page 11454, line 17: partial pressures should be replaced by molar fraction if the units used are $\mu\text{mol mol}^{-1}$.
- Page 11460, line 1: m mol⁻¹ should read mmol⁻¹.
- Page 11462, line 9: "would creates" should be replaced by "would create".

References

- del Hierro A.M., Kronberger W., Hietz P., Offenthaler I. & Richter H. (2002) A new method to determine the oxygen concentration inside the sapwood of trees. *Journal of Experimental Botany*, 53, 559-563.
- Eklund L. (2000) Internal oxygen levels decrease during the growing season and with increasing stem height. *Trees*, 14, 177-180.
- Mancuso S. & Marras A.M. (2003) Different pathways of the oxygen supply in the sapwood of young *Olea europaea* trees. *Planta*, 216, 1028-1033.

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