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Biogeosciences Discussions

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

Anonymous Referee #2

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This paper investigates tree stem respiration using the ratio of O2 influx and CO2 efflux (here defined as the apparent respiratory quotient, ARQ). This method is more technically demanding than measuring CO2 efflux directly in the field, but CO2 based estimates of tree stem respiration have been questioned because of CO2 concentrations and movement inside of tree stems. Based on the ARQ values, the paper argues that stem respiration is much larger than what is indicated by stem CO2 effux.

This is an interesting area of research because of implications for our understanding of carbon cycling which is of interest to readers of this journal. I am supportive of this research and agree with the paper in that there is a need for refined estimates of woody tissue respiration that move beyond simple gas exchange.

The paper uses concepts, ideas, and tools that were described in a 2011 paper by

the author. Though the original paper was more of a methods/concept paper, the current paper attempts to apply the technique to tropical forest trees. The application to tropical forest trees is important given the importance of these forest systems in the global carbon cycle. However, the actual data contained within this paper represent only a portion of what the experiment actually required and, as a result, only qualitative comparisons can be made. There are no clear hypotheses provided, but it seems like the design described in the methods intended for comparisons of different tree species during wet and dry portions of the growing season. Only qualitative comparisons of results were made because so few data were available that statistical tests could not be performed for comparisons. Are these substantial conclusions? I tend to think not. The data from stem chambers during the wet season seem to be mostly there and relatively consistent. One drawback of presenting only individual tree data is that the reader is left to calculate measures of variation. The data from the in-stem sampling seems noisy when replicated data are provided which makes me wonder how representative these measurements are. See the difference in O2 for Marupa in Period A: 3.66-18.06; or Tangarana in Period B:0.01-10.33.

The title captures the message of part of the paper (Table 5), but I do not think that the data lend great support to the claim. The comparison only seems to be made for 5 individual trees at a single point. This seems rather preliminary to me and the efflux comparison seemed to be the most novel aspect of this work compared to the methods/concept paper.

I have a few concerns about the interpretation of ARQ and how valuable it might be in providing information on the sources or fates of CO2 within tree stems. The interpretation of ARQ<1 indicating aqueous transport of CO2 away from the point of respiration may be valid; however, these measurements do not provide insight into the source of the CO2 that gases out of the stem. The degassing of CO2 from the stem could result from CO2 that was dissolved and transported through the transpiration stream. ARQ>1 would indicate CO2 coming from below the measurement location on the stem or even

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from the root system belowground, but ARQ still could not tell us precisely where. As well, there could be a large amount of CO2 coming from below the measurement point, but if it is dissolved, it would not influence the measured ARQ. I believe this approach is a step in the right direction, but I am not convinced that this approach will tell us the entire story of respiration in tree stems. I believe it adds beyond the typical efflux measurements, but my sense is that we need to measure changes of CO2 concentrations inside the stem, both above and below the measurement location, along with sap flow in addition to measuring the ARQ.

The abstract provides a concise and complete summary; however, I think the statement of novelty regarding "the first time" data like these are presented is perhaps misleading given that similar data have been published in the earlier method/concept paper and the data presented here are somewhat incomplete. This same sentence is also somewhat misleading in that it mentions a considerable internal flux. I do not see that the experiment has measured the internal flux of CO2. The paper uses a technique to determine that some of the CO2 from stem respiration does not flux out. The paper acknowledges that sap flow rates were not measured and that overall flux could not be determined.

The overall presentation of data is well structured and clear. I would prefer to see figures representing mean values in place of the individual tree data, but I understand that this is done because some means would have n = 1 and no measure of variation. The overall presentation is clear, and the paper does not try to hide that so many data are missing.

The language is fluent and, for the most part, precise.

Mathematical formulae, symbols, abbreviations, and units appear correctly defined and used.

The number and quality of references appears appropriate. The key literature appears to be referenced.

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There is no supplementary material.

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