Biogeosciences Discuss., 5, S470–S475, 2008
www.biogeosciences-discuss.net/5/S470/2008/

Biogeosciences Discussions

Interactive comment on "The effect of flooding on the exchange of the volatile C_2-compounds ethanol, acetaldehyde and acetic acid between leaves of Amazonian floodplain tree species and the atmosphere" *by* S. Rottenberger et al.

S. Rottenberger et al.

Received and published: 5 May 2008

Comments to Referee Peter Harley

We thank Peter Harley for his substantial work with the review. We think we can sufficiently address all his comments. If not, we will be happy to continue our discussions on the next meetings.

RATES OF PHOTOSYNTHESIS UNDER FLOODING CONDITIONS

The referee was "struck however by the ability of these leaves to maintain rates of photosynthesis and transpiration over 50% of their pre-flooding levels". We also were really surprised about the adaptation potential of tree species to flooding conditions in the Amazonian floodplain areas. There is more excellent information about adaptation mechanism found in the literature. For review see Parolin et al. (2004). The adaptation of plants to such an environment is even more impressive if one has the chance to visit the Varzea and Igapo by boat during the peak of the flooding season when plants get trapped in the flood reaching the upper parts of the stem even of large trees. Some plants have thrown down their leaves to survive. Others are totally flooded for months and survive. In many cases however, morphological and physiological adaptations enable plants not only to survive these long term flood pulses but to maintain a substantial physiological activity. Recent advances in the understanding of different adaptation strategies are in press (Haase and Rätsch, 2008). The consequences of this sort of stress on reactive trace gas exchange between the tree's crown and the atmosphere is unexplored.

LEAF ENZYME ACTIVITIES

We agree with the referee that all discussions about the role of leaf enzyme activities are speculative. We wanted to indicate potential differences in leaf metabolism to understand the differences in emission quality. We will underline the prematurity of these discussions. There is more information needed to link enzymatic activities with emission rates, and our data provide good arguments to invest time into such kind of research.

SUBSTANTIAL DEPOSITION TO THE LEAF CUTICLE

We found a discrepancy between measured deposition velocity and stomatal conductance to acetaldehyde and discussed a significant non-stomatal deposition to explain this observation. Our discussions are based on the earlier publications as cited by the referee. In the meantime we performed further experiments (with other plants) inducing stomatal closure by incubation with abscisic acid, which support the view of a potential cuticular deposition. However, we can not differentiate between pure physicochemical

BGD

5, S470–S475, 2008

deposition on humid surfaces and/or active uptake by biological consumers.

IMPACT AT THE REGIONAL SCALE

We fully agree that this is only a first step towards characterizing the impact of such emissions from seasonally flooded tropical forests at the ecosystem or regional scales. We fully agree with the referee to consider that all such experiments in the past were performed on potted trees under artificial conditions in the laboratory/greenhouse. Before we transfer conclusions to natural ecosystems we should go out into the floodplains to measure mature trees under natural conditions. We can however confirm that well flooded pots are not far away form the perfect and long lasting flooding of the floodplain areas. The rising water table covers the lower parts of the trees completely. The plants react by developing adventitious roots, aerenchyma and fermentation. These phenomenona are found in the field as well as in potted plants. At the moment we can not answer the question which strategy is more successful, avoidance of roots hypoxia or fermentation? All we can say is that plants such as S. martiana are growing obviously at places where others fail to find their niche. We need long term field experiments including primary emission measurements as well as flux studies. We are currently discussing such studies and would be glad to get support from other readers of the paper or the discussion contributions.

MORE OR LESS SIGNIFICANT COMMENTS (Phrasing of the referee!)

We do not comment on all remarks here but we will carefully work on them to improve the paper. Here are some answers to the more prominent questions:

p. 464, l. 8 no significant emissions? What is significant? AUTHORS: Emissions were not detected within the uncertainty of the exchange measurements. We will skip the word significant and rewrite: none of the species exhibited a measurable emission.

p. 464, l. 21 sources of although C-2 emissions not necessarily from ethanol: AUTHORS: Accepted, discussion will be included.

p. 465, l. 6 toxicity of acetaldehyde and ethanol: AUTHORS: The toxicity of ethanol is not finally accepted. There are reports in the literature that the oxidation product acetaldehyde is highly toxic, however. Preventing the oxidation step no toxicity of ethanol was found (Perata and Alpi, 1991).

p. 469, l. 12 Although I'm no expert in PTR-MS techniques, it is not clear to me why a signal at m/z 29 is "obviously not detectable". Are there any known interferences with the measurement of acetic acid at m/z 61? Please make clear whether the concentrations/ emissions of ethanol are based solely on m/z 47 or whether any correction was applied. AUTHORS: m/z 29 was obviously not detectable. In the present case, where emissions of ethanol were investigated, dissociation occurred during the proton transfer. Under standard conditions, only 10-20% of ethanol is expected not to fragment upon protonation, and is detected at mass 47 (R. Holzinger, personal communication). The remaining fraction of protonated ethanol molecules lose a H2O fragment, yielding C2H5+, which should be detected at mass 29. Since C2H5+ has a lower proton affinity than water, however, the fragments cannot be detected (collision of H2O and C2H5+ leads to reformation of H3O+), and ethanol concentrations are highly underestimated.

p. 473, l. 13 There seems to be some discrepancy between the PTR-MS data (Fig. 2) and the HPLC data (Fig. 3). AUTHORS: We can not explain this difference but trust the HPLC data. The ambient acetaldehyde concentrations varied between 0 and 3 ppb. Drivers for these fluctuations may be both, direct and indirect emissions.

p. 478, l. 24 I don't understand how the increased solubility of acetic acid might explain the different deposition behavior of acetic acid and acetaldehyde. Higher solubility might decrease the mesophyll resistance term, but since acetaldehyde deposition already greatly exceeds stomatal conductance, Rm appears negligible to begin with. Why might acetaldehyde be deposited to the cuticle, as suggested by the authors, but not acetic acid? AUTHORS: Our calculation is based on the assumption that the uptake of water-soluble gases is primarily by way of their solution into the aqueous phase being committed within the stomatal cavities of the leaves (see e.g. Niinemets and

Reichstein 2003, doi:10.1029/2002JD002620), up to the extend the physical transfer of stomatal conductance / cell wall resistance allows. The latter was assigned from the measured exchange of water vapor and stomatal conductance to H2O, respectively; and assuming preferential partitioning of these gases to the aqueous phase. Deposition rates exceeding this upper bound estimate for the potential uptake of water-soluble gases were attributed to be non-stomatal, hence (quasi by default) to surface deposition. As acetic acid is significantly better soluble in water than acetaldehyde, the stomatal conductance should be better for acetic acid than for acetaldehyde. Furthermore, owing to the more hydrophobic nature of the cuticle more acetaldehyde might get stuck in the cuticular membrane. Based on the reported experiments we can not add more to this discussion than already published in Rottenberger et al. (2004).

p. 485, l. 1 If the data are available, I'd be curious to know how isoprene emissions in L. corymbulosa responded to the flooding conditions imposed in this study. AUTHORS: We have data of isoprene emissions. They are under evaluation now. We are also curious to see, but this will take some more time. We will keep the editor informed.

References

Haase, K. and Rätsch, G. (2008; in press) The morphology and anatomy of tree roots and their aeration strategies. In: Central Amazonian Floodplain Forests: Ecophysiology, Biodiversity and Sustainable Management (Wolfgang J. Junk, Maria T. F. Piedade, Pia Parolin, Florian Wittmann and Jochen Schöngart (eds.), Springer-Verlag, Berlin, Heidelberg, New York

Parolin P., De Simone O., Haase K., Waldhoff D., Rottenberger S., Kuhn U., Kesselmeier J., Kleiss B., Schmidt W., Piedade M.T.F. and Junk W.J. (2004) Central Amazon floodplain forests: tree adaptation in a pulsing system. The Botanical Review 70(3), 357-380.

Perata P. and Alpi, A. (1991) Ethanol-Induced Injuries to Carrot Cells. Plant Physiology 95:748-752 (1991).

Rottenberger, S., Kuhn, U., Wolf, A., Schebeske, G., Oliva, S.T.,Tavares, T.M., and Kesselmeier, J. (2004) Exchange of short-chain aldehydes between Amazonian vegetation and the atmosphere at a remote forest site in Brazil. Ecological Applications 14(4) S, 247-262.

Interactive comment on Biogeosciences Discuss., 5, 463, 2008.