Biogeosciences Discuss., 5, S215–S217, 2008
www.biogeosciences-discuss.net/5/S215/2008/

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.

Anonymous Referee #2

Received and published: 25 March 2008

The manuscript submitted by Rottenberger et al. deals with the emission of ethanol, acetic acid and acetaldehyde by the leaves of flooded Amazonian trees. To my knowledge, for the first time four species representative for the Amazonian floodplain were included in this laboratory study. The authors observed different species specific emission rates as well as characteristic daily patterns of VOC emission which were very similar for the different species. These daily patterns showed low to absent emission during night, high emission rates in the morning and decreasing rates in the afternoon. The species specific differences in emission rates were explained by different morpho-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

logical structures which have been published earlier (De Simone et al. 2002a, b).

Unfortunately, the paper is mostly of confirmatory character. Increased acetaldehyde and ethanol emissions following root hypoxia has been studied and published previously also by the same working group (Holzinger et al. 2000). The only new aspects are the observation of acetic acid emission by flooded trees and the decreasing acetaldehyde and ethanol emissions during extended periods of flooding treatment.

The manuscript is nicely written and well structured, and the methods applied seem to be suitable. The paper is within the scope of BG. Nevertheless, I have some serious concerns regarding the performance of the experiments described. My main criticism is the extremely low number of replica which is n=1 for three species and n=2 for the fourth species (L. corymbulosa). The whole discussion is based on this very poor data base and to my opinion it is not possible to draw any reliable conclusions.

With n=1 species specific variation of course cannot be estimated. The possibly high variations are indicated if the results of L. corymbulosa (n=2) are considered. Acetaldehyde and ethanol emission rates varied by a factor of 2 between both individuals (fig. 2), underlining the enormous variation that can occur. To indicate this weakness, the number of trees used should at least be stated in the Materials and methods section.

Another serious problem is the determination of ethanol by PTR-MS. As the authors state, only estimated 10-20% of the ethanol present in air can be quantified by the measuring unit. This underlines that the emission rates presented in the figures are more or less useless if not corrected for this underestimation. To determine correction factors, a cuvette could have been fumigated with known concentrations of ethanol at different relative humidities.

An environmental factor determining alcoholic fermentation and subsequently VOC emission during flooding is the oxygen concentration in the soil. Unfortunately, data are not presented (although determined). The species specific differences observed could be caused by differences in oxygen consumption during the flooding period and

BGD

5, S215–S217, 2008

not only by different root structures as discussed. Therefore, oxygen concentrations should be shown for each experiment. Besides a short description on how oxygen was determined should be included.

As VOC emission depends on alcoholic fermentation in the roots, substrate availability for fermentation (soluble carbohydrates) might be responsible for the observed differences. If there is a lack of sugars in hypoxia treated roots, fermentation cannot run at a high level and as a consequence, C2-VOC emission drops (independent on oxygen availability in roots and on root structures). This aspect should be discussed.

The authors suggest principle differences between species in leaf ethanol metabolism because the ratios of acetaldehyde/ethanol emission rates are different between species. This is an interesting aspect (which could have been further investigated by determining the activities of the responsible enzymes). Moreover, from fig. 4 it is evident that this ratio also changes during the course of an experiment in individual plants. The reason might be changes in enzyme activities. This should also be discussed.

The acetaldehyde compensation points of two species are shown. What about the other species? Were their compensation points in the same order of magnitude?

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