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Interactive comment on “Halogenated organic species over the tropical rainforest” by S. Gebhardt et al.

Anonymous Referee #2

Received and published: 11 March 2008

MS-NR: acpd-2007-0525 Version: 1 Received: 13 November 2007, 14:46 CET Title: Halogenated organic species over the tropical rainforest Author(s): S. Gebhardt, A. Colomb, R. Hofmann, J. Williams, and J. Lelieveld

General Comments

This is an interesting and reasonably well-written paper suitable for publication in ACP with a few minor changes. The authors present substantive original results that help to constrain the net fluxes of the halocarbons methyl chloride, methyl bromide and chloroform over the tropical South American rainforest.

The design of the experiment is really quite bold – in that so many flight hours are dedicated to it and I commend the researchers for pursuing it. I am curious about

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what other trace gases were measured at the same time. Further reference to an overview of the GABRIEL project (and/or website) would be appropriate; perhaps on p1162, lines 15-20?

Specific Comments The previous two referees have given excellent detailed specific comments on this paper, so I will restrict mine to reinforcing what I feel are the most important points to have been made, and adding my suggestions for improvement that haven't already been covered.

As pointed out by other reviewers, the authors need to be more careful to be consistent in their use of the term "net flux"; rather than "emission fluxes" (which pre-supposes that the forest will be a net source; when plants and/or soil have sometimes shown to be sinks of such molecules). Similarly, their use of the term "missing source" is not always the best choice of term. It also pre-supposes that the experiment will find net emission. In addition the term "long dry season" is not always appropriately employed (as explained by reviewer 1).

Like Reviewer 1, I would be very interested to see the results from the "up-wind" or T<0 samples added to Figure 5.

P 1164, line 11. Please forgive me if I am wrong, but I was not aware that NCAR was in possession of a NIST standard for halocarbons. Are the authors perhaps referring to the NIST propane standard that is employed as a reference by many analytical groups? Please clarify.

P 1171 lines 21-27. This discussion of the short-comings of GC-ECD and the fact that the samples were stored before analysis feels rather awkward to me and is probably irrelevant, given that the authors go on to state that there is good agreement with the current work and that the canisters have proven to be stable containers for the molecules in question.

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