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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurements of reactive chlorocarbons over the Surinam tropical rain forest: indications for strong biogenic emissions" *by* H. A. Scheeren et al.

Anonymous Referee #2

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The manuscript describes what appears to be a well designed study and reports some interesting results concerning fluxes of halocarbons from the Amazonian rainforest. Unfortunately the work is flawed by superficial and uncritical approach to the literature, a rather naive and simplistic extrapolation of the data to estimate global fluxes from tropical forests, and some gratuitous and highly premature speculation concerning reasons for possible trends in atmospheric CH3Cl concentration.

DETAILED COMMENTS

Pg 2 and 3. The second and third paragraphs of the introduction are poorly structured and rather muddled. The 3rd paragraph appears to move on to discuss reactive halocarbons other than CH3Cl but then reports CH3Cl emission fluxes from peatlands,

wetlands, salt marshes and coastal waters. Moreover the paper by Watling and Harper (1998) mentioned deals with fungal fluxes of CH3Cl from forests not CHCl3. I am surprised that there is no reference in the introduction to Montzka et al. Scientific Assessment of Ozone 2002 (2003) which provides a useful summary of current thinking in this area.

Pg 7 para 2 line 4. should UT be MT?

Pg 10 para 4 line 2-4. On what basis is the outlier excluded? Where are the error bars on the outlier? Would there be a significant increase in C2Cl4 if the outlier was included in the data. If not, I do not think that it is acceptable to assume an increase in C2Cl4.

Pg 11 para line l3-6. Emission of CH3Cl may also occur from rotting wood whether it be fallen timber, standing dead trees, or wood litter lying on the forest floor (Watling and Harper, 1998). Moreover, the authors do not appear to be aware of the work of Hamilton et al. Science 301, 206 (2003) concerning the production of CH3Cl by senescent and dead leaves.

Pg 13 para 2 line 2. The data in Yokouchi et al. (2002) is simply not detailed or comprehensive enough to draw any conclusions about the diurnal variability or seasonal variability of emissions. The work on diurnal variability was based on two plants in a glasshouse over several days. Indeed other work on plant sources e.g. Rhew et al. (2000) and Dimmer et al. (2001) suggest strong diurnal variability in CH3Cl emissions. The work by Hamilton et al. (2003) on abiotic formation of CH3Cl from plant pectin also implies diurnal variations in emissions.

Pg 13 para 2 and pg 14 para 1. I simply do not find it credible that, as a result of transport of sea salt aerosols embedded in dust plumes, Cl- concentrations in soil in areas of tropical rainforest far from the sea are as high as those in maritime areas. This is certainly not the case on other continents! I therefore cannot accept that soil salinity is not a limiting factor in CH3Cl production in highly leached environments such

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as tropical rainforests. Adjustment of fluxes for this factor must be attempted but is likely to prove difficult.

Pg 13-15 section 5.3, Table 4. I am far from happy at applying estimates for CH3Cl emissions from Amazonian rainforest to such forests in Africa or Asia. It appears from Yokouchi et al. (2002) that emissions in lowland E Asian rainforest are confined to a few genera of ferns and dipterocarp trees and hence are not a universal characteristic of tropical plants and trees. Any estimates calculated in this paper on fluxes based on the Amazonian data collected are therefore, at best applicable to the Amazon basin, and, in the worst case scenario, only to the area in which the study was conducted.

The authors also makes the entirely unjustified assumption that all tropical forests are of the rainforest type. Around 30% of tropical/subtropical forests are seasonally dry and represent a completely different ecosystem from rainforest.

Pg 15 para 3 line 9-12. Lee-Taylor et al adopt a figure of 128 Gg yr-1 for the global fungal CH3Cl flux. This estimate appears to be derived entirely from Watling and Harper (1998) and Khalil et al. (1999) except that a different modelled assessment of woody decomposition is employed for which no good justification is provided. I would suggest that the Watling and Harper (1998) and Khalil et al. (1999) estimates of the fungal flux are used.

The annual global emission flux of CH3Cl from wood-rotting fungi is calculated by the latter authors at 163 Gg yr-1 of which 115 Gg yr-1 is from tropical forests excluding Australia. The total Australian flux is estimated at 9 Gg yr-1 of which approximately 60% is tropical/subtropical. Hence the total global flux of CH3Cl from tropical/subtropical is 120 Gg yr-1 and this is the estimate that should be used as the fungal flux from tropical rainforest in the paper.

Moreover, the authors have totally ignored in Tables 3, 4 and Fig 7 the fungal emission flux of CH3Cl from temperate forests calculated in Watling and Harper (1998) and Khalil et al. (1999) at 43 Gg yr-1 (after making the appropriate correction for the Australian

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flux). This should be included in the Tables and Figure.

Pg 18 para 4 and Fig 8. There is a discrepancy between the arguments on page 18 and Fig 8. I can only assume that the Montzka et al. (2003) correction has not been applied to Khalil and Rasmussen (1998) data in Fig 8. Proper referencing of all the data in this figure is necessary. I really do not believe that the data presented is adequate to attribute any decline in atmospheric CH3Cl concentrations to tropical deforestation especially in view of the work of Butler et al. (1999) which has suggested an increase over the last century.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5469, 2003.