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Atmospheric
Chemistry
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Discussions

Interactive comment on “Flux estimates of isoprene, methanol and acetone from airborne PTR-MS measurements over the tropical rainforest during the GABRIEL 2005 campaign” *by* G. Eerdekens et al.

G. Eerdekens et al.

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We are very grateful for the interesting comments of the reviewer. We believe we have addressed them all in the revised version.

Minor Comments

1: We refer now to the most recent work available by Millet et al., (2008) and references therein. The determination of the methanol emission dependence on leaf age was done by e.g. Macdonald and Fall, (1993) and Nemecek-Marshall et al., (1995) and these references are cited in the above work.

2: This typo, i.e. capitalized 'NO' has been changed to 'no' in all four sentences.

3: We believe that the sentence highlighted is clear as we state the cartridge was placed after the pump. However, for greater clarity and to address the reviewer's question we now add the following text "The high pumping rate through the system prevented the testing of sample integrity with a gas standard. If a reactive compound such as isoprene had been destroyed in the pump then isoprene would have been underestimated by the TD-GCMS analysis against PTR-MS and this was not observed."

4: Reviewer 2 has found Figure 1 difficult to interpret and we have now simplified it accordingly. We have now included all GC-MS data points (open markers) including those over the ocean. We still exclude data for which less than 5 out of a possible 10 PTRMS data points were averaged (solid markers) in the regression analysis. We have removed the flight number as there was no significant difference between each of the individual flights. The error bars on the x-axis represent the total error on the GC-MS measurements. The error bars on the y-axis are calculated by the root mean square of the total measurement errors of all PTRMS data points within each interval, which was the time over which the GC-MS sampled. We have treated all data in the same way. The PTRMS data has now been corrected for the humidity dependence of the calibration factor as noted in the replies to reviewer 1.

5: The sentence quoted by the reviewer was missing a full stop which is now included, as follows: "... Kesselmeier (2001). In this approach, ..." .

6: We are aware of the fact that the commonly applied Wesely approach needs to be revised based recent observations suggesting substantially larger removal rates of a number of reactive species not only for OVOC's (Karl et al., 2004) but also for peroxides (Ganzeveld et al., 2006). In an accompanying paper by Ganzeveld et al., (2008), this is discussed in more detail and additional analysis with the SCM have been conducted using a small surface uptake resistance for methanol an acetone (cuticular uptake resistance of 100 s m-1) to indicate how much a difference this enhanced uptake would make compared to the simulations with the default Wesely approach estimates of uptake efficiency. This analysis shows indeed a substantial decrease in concentrations but limited to the canopy and surface layers because of the limited nocturnal turbulent transport between the surface and the atmosphere aloft. In order to restrict the length of this paper we refer the reader to the Ganzeveld et al., 2008 paper for more details on this point.

7: The paper is organised such that first we discuss the gas phase measurements and then afterwards use them to calculate the flux. It therefore seems more logical to us to retain the order. This is now explained at end of the introduction thus: "The paper is structured as follows, first we examine the boundary layer dynamics and meteorology for measurements and modelling. We then examine vertical and diurnal profiles of mixing ratios and relate them to chemical and mixing timescales. Finally we use the measurements to calculate a flux from the rainforest using several approaches, the results of which are compared and discussed."

8: The green bar has been removed.

9: The reviewer is right and we have rectified the mistake.

10: The reviewer is right about the mistake in the cross-reference. "in Eq. (2)" has now been replaced by "in section 5.3". In answering minor comment 13 from reviewer 1, we have compared ISOP+(MACR+MVK)+ ((MACR+MVK)*61/39) to isoprene back-calculated to the source level by including the secondary oxidation processes. We conclude from this that ISOP+(MACR+MVK)+ ((MACR+MVK)*61/39) is conserved and perhaps overestimates isoprene compared to the back-calculated isoprene. However, this is a purely chemical calculation and does not compensate for the loss of isoprene and oxidation products due to mixing. Therefore, we believe that ISOP+(MACR+MVK)+ ((MACR+MVK)*61/39) is a reasonable approximation in case OH has not been measured and returns an "upper" estimate of the flux keeping the dependence of the estimate on the assumed boundary layer height in mind.

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11: The sentence referred to by reviewer 2 has been revised for clarity to "These fluxes are comparable to the maximum surface layer fluxes of 0.36 mg acetone m-2h-1 and 0.5 mg methanol m-2h-1 reported by Karl et al., (2004) and higher than the average flux of 0.09 mg acetone m-2h-1 and 0.13 mg methanol m-2h-1 observed during these 3 weeks of disjunct eddy covariance measurements in Costa Rica. "

12: We thank the reviewer for the suggestion and agree that several processes (e.g. direct emissions and in-situ secondary production) may be involved in methanol production. This point has been included in the text.

13: We have shortened this section as suggested.

14: This issue has been addressed in point 30 to reviewer 1.

15: The compensation point approach is discussed in detail by Ganzeveld et al., (2008). We refer the reader to this work rather than further lengthening this manuscript.

References

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