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Atmospheric Chemistry and Physics 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.

Anonymous Referee #2

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Review of Eerdekens et al., ACPD 2008

The manuscript represents an overview of the Learjet VOC measurements during the GABRIEL campaign. It contains a lot of important information from an area undersampled by the atmospheric chemistry community, and therefore highly relevant. The text is well written for the most part, but should be proofread for typos and grammar mistakes. The contents covers several important analyses from both atmospheric chemistry and biogeochemistry perspectives.

I have only suggestions for minor corrections, improvements, and clarifications as described below:

1. The Jacob et al, 2005 reference on page 12906, top, should not be used to describe the generally higher growth stage methanol emissions as compared to mature leaves, as these authors neither made the measurements, nor is there evidence as to the universal applicability of the seasonality function they used. I suggest to eliminate that sentence, or be less or more specific.

2. Eliminate the capitalization of the word "no" in the manuscript.

3. The sentence "The sample tubes were fitted into the flowpath approximately 80cm after the pump with ..." is unclear. Was the sample routed through the pump into the cartridges? If so, did the pump have any effects on sample integrity?

4. Figure 1 does not show all 62 points described in the text. Its caption speaks of an "interval comparison", which each such interval seemingly having a number on the graph corresponding to flight number. Is there a compelling reason doing it that way? Is the bias larger or smaller using all points individually? I do not follow the meaning of the error bars on the interval points. It seems that if those intervals reflect periods during a flight, then they should also include a temporal change factor which is possibly larger than the measurement uncertainty. Several numbers appear more than once, other numbers are missing. There are several points along the 1:1 line showing no bias, while others are off that line. What was the rational for the intervals? Were only the values off the 1:1 line corrected? Please provide more information.

5. Rewrite/edit this sentence: "In the SCM, methanol and acetone biogenic exchanges are described using a compensation point approach since bi-directional exchanges (emissions and deposition) have been reported in the field e.g. Kesselmeier 2001 in this approach, the compensation point concentration is defined as the concentration for which the uptake 15 reaction is equal to the simultaneous operating production rate for a net zero flux."

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6. Regarding the statement "The latter are estimated from the solubility and reactivity of methanol and acetone according to the Wesely (1989) approach (see also Ganzeveld et al., 2006)" on page 12916: Note that recent measurements have shown that this approach can strongly underestimate the possible dry deposition velocities for OVOCs. This is also relevant for the model-observation comparison in Figure 8c.

7. Maybe section 5.7 should be moved ahead due to its importance on discussions in earlier sections?

8. I suggest to remove the green bar in Figures 6+7 as it covers some data points and is obviously too high compared to reality.

9. Section 5.6: "Aloft, the diurnal pattern of the [MACR+MVK]/[isoprene] ratio between 09:00-16:00 LT is similar, even though the median and mean ratios are distinctly lower." I think it should say "higher" here, because [isoprene] decreases and [MACR+MVK] increases with elevation ("aloft").

10. I have a problem with this sentence in section 5.7: "In order to estimate the surface flux of isoprene we therefore proceeded as follows. We correct for any chemical loss terms by using the conserved tracer (ISOP+MACR+MVK+others, see Fig. 10a) described previously in Eq. (2)." First, I am unsure to which equation 2 the authors refer to (seemingly not the equation 2 from the previous page). Second, the term [ISOP+MACR+MVK+others] is not conserved unless "others" contains at least the oxidation products of MACR and MVK. As defined by the authors earlier, "others" is supposed to contain only the 1st level oxidation products of isoprene, not the second level. Thus, there will be a slight offset in these results, dependent on [OH], if the authors meant what they wrote. Surely, you can argue that the term is nearly conserved, but realize that the results are then not "upper limits" as stated further down in the text.

11. Page 12938: I do not understand the last part of this sentence: "... but are higher than their average flux of 0.09mg acetone m-2 h-1 for 3 weeks of disjunct eddy covariance measurements in Costa Rica during the dry season, bearing the much lower

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isoprene emission flux from this region in mind."

12. Page 12939: The higher methanol fluxes as compared to the ground-level measurements can be explained either by the different concentration footprints of these measurements and/or by in-situ production of methanol from methylperoxy radical reactions (–> highest contribution after noon time).

13. I think much of the discussion in this last section can be shortened when you realize how uncertain the calculations are, and that the comparisons to previous measurements are rather good, maybe converging upon flux values between 3 and 8 mg isoprene m-2 h-1. Further shortening can involve eliminating the last paragraph, which contributes very little if anything to the main results of the study (also in the conclusions section).

14. There appears to be a general issue with isoprene emission modeling in the SCM: On the one hand, the authors calculated a total rainforest emission equal to what MEGAN calculates for the world (600 Tg isoprene per year), on the other hand, they had to reduce the emission potential by a factor of two to fit those same measurements those total emissions were calculated from. How can this be? If the SCM uses the MEGAN input values (as described by the authors) , it should calculate what MEGAN calculates, but this comparison suggests that the SCM calculates about 4 times as high rainforest emissions compared to MEGAN, assuming that global emissions are twice the rainforest emissions. Even considering very large rainforest emission heterogeneity, this is difficult to reconcile. Please explain.

15. The compensation points for methanol and acetone are mentioned at a few points in the manuscript, but are not listed, neither discussed. The abstract's last sentence suggests that the compensation point approach was tested, but the manuscript does not contain this information. I suggest to either eliminate/shorten parts of the manuscript, or to expand upon this topic by explaining how the measurements and model were used to infer compensation points, what they were, and how they compare

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to previous measurements.

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