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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Isoprene and monoterpene fluxes from Central Amazonian rainforest inferred from tower-based and airborne measurements, and implications on theatmospheric chemistry and the local carbon budget" *by* U. Kuhn et al.

Anonymous Referee #1

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The paper presents isoprene and monoterpene emissions from Amazonian tropical rainforest. The paper is well written and presents important data and analysis on the HC emissions from the Amazon region, which is very important for the atmospheric chemistry of the region and possibly even has global importance through oxidative capacity and removal of long lived greenhouse gases. I enjoyed reading the manuscript and I believe this paper will be much cited in future.

My comments are mostly minor in nature and recommend publication of this paper in the ACP.

Main comments:

On the pages 650-652 the authors describe the surface layer flux measurement techniques used. However, they don't mention the effect of the roughness sub-layer (e.g. Garratt, 1980) on the surface layer gradient measurements. The possible effect of the roughness should be discussed as it may have a major impact on the fluxes measured above such a rough surface as tropical rainforests are. The effect of roughness sub-layer tends towards lower absolute values of trace gas gradients. Therefore, the observed SLG fluxes, which were higher than REA fluxes (page 658), should be even higher.

The discussion on the effect of VOC fluxes to the carbon budget (pages 660-663) is somewhat confusing. The estimation of the fraction of the assimilated carbon lost as VOCs does not seem to make much sense if the estimates of the carbon balance vary between strong sink and a weak source. As the magnitude of the VOC emission compared to the carbon balance is an interesting and important topic this discussion should be somewhat reformulated. As the uncertainty and variability of the CO2 exchange seems to provide most of the variation in the comparison, it might be better to start with trying to get the best estimate of CO2 exchange, with range of variation. Also the non-terpenoid VOCs should be mentioned as they may contribute as much as the terpenoids to the carbon balance.

It is not clear how well the correction for the effect of chemical transformation on the vertical profiles (Page 671, lines 20-22) works for the site with deep and dense canopy. Is the z in the equation (10) height from the ground or from the displacement height? Discussion on how well this correction works would improve the manuscript.

Garratt, J. R., 1980: Surface influence upon vertical profiles in the atmospheric near-surface layer. Q. J. R. Meteorol. Soc., 106, 803-819.

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Minor details:

The title of the paper is quite long. The authors should try to shorten it.

In not sure if the nomenclature of the spatial scales is correct. Does the MLG technique measure emissions in the regional scale (p. 643, lines 8-9) or in landscape scale?

On page 644, lines 21-24 the authors attribute the dominating role of the BVOCs in the lower tropospheric chemistry to their higher reactivity, but do not mention the fact that their emissions are also much higher than thode of AVOCs.

On page 647, lines 20-21, the authors give values for LAI and leaf area density of the forest at Cuieiras site. Are these one sided or two sided?

Page 648, line 18: What exactly were the 2-bed graphitic carbon adsorbents used?

Page 648, lines 22-24: The sentence, “The detection limit of the method was estimated as the greater of the variability in the blank levels (at the 95% confidence level) or a chromatographic peak three times the noise for each compound...”, was not clear to me. What does the variability of the blank level at 95% confidence level mean? Does it mean 1.96 times the standard deviation of the blank values? Also I do not understand what the chromatographic peak three times the noise means. Could the authors elaborate this.

Page 649, lines 15-16: I wouldn’t call the agreement good when quite often one method gives isoprene concentrations that are at least twice that given by the other method and r2 is only 0.62.

Page 649, line 17 and page 652, line 21: Are the figures in right order? To me it seems that now figure 2 preceeds figure 1

Page 649, lines 18-20: The length of the sampling into the cartridges could be mentioned here as the differences in the sampling times are mentioned as a possible source of discrepancy between adsorbent and canister samples.

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Page 649, lines 24-28: A more elegant way for estimation of the sizes of the convective eddies in the boundary layer would be the use of spectral analysis. Was there a fast wind sensor mounted on the aircraft and if so, have the authors tried to conduct spectral analysis on e.g. vertical velocities measured in the mixed boundary layer?

Page 650, lines 1-2: From the sentence “For isoprene, however, they indicated similar trends in vertical profiles, but with a systematic relative underestimation by a factor of 0.55” it is not right away clear what is underestimated, the trend, the absolute concentrations, or both.

Page 650, line 16: Reference Valentini et al. 1997 is missing from the reference list.

Page 650, lines 18: What does the “...equal to zero...” mean. The mean wind surely is never exactly zero. What interval around zero is accepted.

Page 650, lines 20-21: What does the sentence “Ěthe REA theory was followed when thresholds of w = +-0.65 sigma; were used for conditional sampling” mean?

Page 651, lines 5-6: Authors state “...the samplers did not produce any bias within the analytical uncertainty”. Maybe they mean “...the samplers did not produce any bias exceeding the analytical uncertainty”.

Page 652, lines 6-8: Authors state “According to the footprint analysis of Araujo et al. (2002) the measured daytime fluxes at the K34 tower are representative of a 2-3 km2 area around the tower, although a smaller proportion of the fluxes originates from an area as large as 70-80km2”. What percentage of the total flux comes from these distances?

Page 654, line 14: Could the authors specify what exactly they mean by discontinuity and inflection.

Page 655, lines5-15: Are the VOC emissions in the model calculated using Guenther-type parameterizations?

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Page 656, line 20-21: Maybe it should be mentioned that the site can be expected to be representative of undisturbed rainforest during easterly winds.

Page 657, lines 6-7: How low the benzene and toluene mixing ratios were?

659: lines 4-6: “The diel course of mean modelled fluxes of all measurement days shows good agreement with observations, and revealed a range in between those calculated by REA and SLG”. The wording of this sentence seems odd. What did the modeled fluxes reveal?

Page 666, lines 5-9: This section is confusing. Could it be rephrased to make it more understandable.

Page 673, lines 8-10: “Presuming that nonanal is biogenic and can be considered as a conserved scalar within the time scales of CBL convective turnover, its atmospheric depletion is largely driven by turbulent mixing”. I assume that the authors mean that its surface layer depletion is mainly driven by turbulent mixing.

Page 675, line 19: Expression “principal difficulty” sounds strange to me.

Page 676, line 5: Do the authors mean chemistry corrected gradient by “effective gradient”?

References: The references are incomplete. In many cases page numbers or doi is missing, making it hard for the reader to find the cited papers.

Table 1: In the study by Rinne et al. (2002) isoprene flux was measured by eddy covariance (using FIS), and a-pinene by disjunct true eddy accumulation, not disjunct relaxed eddy accumulation.

Figure 6: Would it be possible to show also errorbars for the measured fluxes.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 641, 2007.