

Interactive comment on “Airborne measurements of trace gas and aerosol particle emissions from biomass burning in Amazonia” by P. Guyon et al.

Anonymous Referee #2

Received and published: 13 June 2005

Review of manuscript 1680-7375/acpd/2005-5-2791

“Airborne measurements of trace gas and aerosol particle emissions from biomass burning in Amazonia”

This work was performed as part of the LBA-SMOCC program and reports CO, CO₂ and CN (particles) measurements in fire plumes over Amazonia. Since the nineties, biomass burning has been extensively investigated for emissions and atmospheric influence at regional and global scales. Basic informations are given and reviews now available (eg: Andreae and Merlet, 2001). In this context, the manuscript appears much too LONG and talkative. Inflation is almost everywhere: number of authors, ab-

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

stract length, introduction length, manuscript length, reference list. I suggest too to introduce the case studies (3.4) in the body of the discussion this would avoid repetitions in the text. 1) The first thing authors should do is to answer the following question: what is new in our work? The answer is obvious: particle EF and particle behaviour when detrained in the CDL and FT. So it is most desirable that the manuscript is recentered on that topic. The first part (part 3-1 and associated figures which represents 3 pages!!) could almost be deleted and appear as an introduction to explain that CO/CO₂ ratio values are biased (especially in the morning) by respiration of the vegetation. 2) Second: avoid to present well known assessments. It is well know that fires are heterogenous (forest fires but also savanna fires). With aircraft measurements there is a chance to sample either individual plumes at a given phase of the burn, and combustion layers which may be more integrated airparcels. Instead of claiming “we need more measurements to capture fire heterogeneity”, I would have prefer to see a work on tracing the origin of pollution layers (with backtrajectories). Assessment that BL is more influenced by smoldering than CDL or FT so that airborne measurement are biased is obvious and don't need several sentences over the manuscript. 3) Check assessment consistency Although situated in the paragraph suggested to be deleted, I don't understand how “figure 4 presents a frequency plot of ER CO/CO₂ with very good correlation between CO and CO₂ (line 3 p 2800) and line 17: ER CO/CO₂ showed a large variability. WHY do you find smoldering material above the BL (figure 4)? Explanation is too “light”. 3) Insist on new findings. Make hypotheses I could retain some interesting findings which need further explanations: The remarkable narrow range of ER CN/CO Authors are always referring to savanna fire data with often similar values for EF than deforestation Why? Are there any differences between prescribed fires and deforestation fires? Why pyrocloud detrained smoke particles remain unchanged (same angstrom coefficient) whereas they grow in size and scatter more (p2810, line 12?). 4) finally, I would have appreciated more comprehensive assessments on consequences of the presence of the numerous pyrolytic aerosols in the FT . This is a strong and new finding of this work.

In conclusion the manuscript relies on a great amount of work (and certainly of money!). It deserves further work to be tightened and presented with new and strong interesting new findings. I recommend major changes to reach a good presentation of the new interesting findings .

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 2791, 2005.

ACPD

5, S1152–S1154, 2005

Interactive
Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

S1154

EGU