Atmos. Chem. Phys. Discuss., 5, S3367–S3371, 2005
www.atmos-chem-phys.org/acpd/5/S3367/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

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

P. Guyon et al.

Received and published: 20 October 2005

1) 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).

Reply: We don't agree with the referee's inference that biomass burning has been exhaustively investigated. Given that the senior author is the author of the review mentioned as well as the organizer of many investigations on biomass burning, the current study was designed precisely to explore those issues that had not been addressed adequately before: emissions from deforestation fires (which have been surprisingly little explored), and number emission fluxes of particles. The present study is therefore, to our opinion, most valuable in closing crucial gaps in our knowledge. Some of our

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

EGU

results are in good agreement with previous studies, and we have added considerable new information that was previously not known.

2) Inflation is almost everywhere: Number of authors:

Reply: Max Planck and LBA have strict guidelines on selection of authors, which have been followed. I am surprised that the reviewer feels he has enough insight into the role of the participants of this complex study to question the PI's judgment here.

Abstract length, introduction length, manuscript length, reference listĚ:

Reply: We are not sure what criteria the reviewer applies here. The abstract has 336 words, and the introduction is 2 double-spaced pages long and includes very interesting original (unpublished) data on deforestation rates and burning in the Brazilian Amazon, pointing at the fact that despite increasing awareness on the situation in the Brazilian Amazon, deforestation keeps on increasing.

3) Ě introduce the case studies (3.4) in the body of the discussion this would avoid repetitions in the text.

Reply: We believe this would probably add confusion. The case studies are there to highlight specific issues and to demonstrate that our observations are not only an arrangement of statistics but can also be seen on single case basis, which emphasizes the quality of our data. This purpose is best served at the end after the statistical discussion.

4) The first part could almost be deleted and appear as an introduction to explain that CO/CO2 ratio values are biaised (especially in the morning) by respiration of the vegetation.

Reply: The respiration bias of emission ratio measurements relative to CO2 is a serious problem in the determination of pyrogenic emissions. To date, this bias in CO/CO2 ratio due to the respiration of the vegetation (particularly noticeable in the morning) has never been documented other than in the present paper (in fact we have never

ACPD

5, S3367–S3371, 2005

even heard anyone mention it), so that researchers keep on publishing biased data without knowing it (some examples are available in the text). We feel that this is an important, albeit technical, part of the paper.

5) avoid to present well known assessments. It is well know that fires are heterogenous (forest fires but also savanna fires).

Reply: The point here is that we are not just making this statement in the usual form of a platitude, but that we also put a considerable amount of quantitative statistics behind it. We believe it is a good thing to remind people how diverse fires really are, and that consistent results only emerge after suitable aggregation from a large enough data set.

6) I would have prefer to see a work on tracing the origin of pollution layers (with backtrajectories).

Reply: As written in the manuscript, within the BL, there was the same heterogeneity in fresh plumes (whose origin was a few 100 meter below the aircraft) and in older pollution layers. Demonstrating that was the exact purpose of the case studies at the end of the manuscript (showing that a single fire could be as heterogeneous as fires may differ from each other in this region). Therefore back-trajectories wouldn't have brought any additional valuable information. (Anyway, does the reviewer really believe back-trajectories would have been able to tell us from which part and time (i.e., smoldering vs. flaming) of a distant fire we were measuring?)

7) 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.

Reply: We think the reviewer misunderstands the text here; the "good correlation" refers to simultaneous CO and CO2 measurements taken in some individual 163 plumes (correlation within plumes). For each one of these plumes the emissions of CO and CO2 correlated well (as indicated in the text, 75% of the r2 of the linear fits

ACPD

5, S3367–S3371, 2005

were $>$ 0.7). The large variability refers to the fact that the regression slopes vary between plumes. We have added a few words to the text to make this clearer to readers.

8) WHY do you find smoldering material above the BL (figure 4)?

Reply: There is no reason why smoldering may not be entrained with the hot convection. In fact, anything we measured must obviously have been lofted at least to aircraft height. Once there, it can be readily lofted further and detrained by normal convective activity.

9) Authors are always referring to savanna fire data with often similar values for EF than deforestation. Why?

Reply: In the introduction, we wrote: Andreae and Merlet (2001) Ě point at the lack of reliable data on the number of particles emitted per amount of biomass burned. Recent studies have filled this gap for savanna fires, however, for most other types of combustion, and, surprisingly, especially deforestation fires, available data remain based on estimates from laboratory studies, or are just guessed. This study presents the first quantitative measurements of the emission rates of aerosol particle numbers from pasture and deforestation fires over the Brazilian Amazon Basin. There is no deforestation data to compare with, and we have tried to highlight this a bit more in the manuscript. It is normal scientific practice to compare ones results to previous studies from similar and contrasting environments.

10) Are there any differences between prescribed fires and deforestation fires?

Reply: No, except that we knew about it before it was lit, and were able to study to some extent the type of fuel, size forest to be burned Ě In fact, our prescribed fire was a deforestation fire. We have clarified the text in the manuscript.

11) Why pyrocloud detrained smoke particles remain unchanged (same angstrom coefficient) whereas they grow in size and scatter more (p2810, line 12?).

Reply: p2810, line 12, we wrote: “Compared to the fresh smoke, ERCO/CO2 of the

ACPD

5, S3367–S3371, 2005

pyrocloud-detrained smoke remained unchanged, while the aerosols grew in size (Fig. 8) and scattered more. " Then we added: "In the present case, no difference was found in the backscatter ratios and Angstrom coefficients." Backscatter ratio and Angstrom coefficients depend in complex ways on the shape of the size distribution. Obviously, the particles did not remain unchanged, as inferred by reviewer - the text and Table 4 indicate that size and scattering coefficient increased significantly during cloud processing. On the other hand, the changes in å and b expected from Mie theory are too small to be detected above the instrumental error. A sentence has been added at the end of section 3.4.1.

12) 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.

Reply: Thanks. However, this does not belong to the scope of this paper, as the focus here is on emissions, not radiative or chemical effects. Other aspects of the SMOCC data set have been and will be explored in other papers.

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