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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chemistry, transport and dry deposition of trace gases in the boundary layer over the tropical Atlantic Ocean and the Guyanas during the GABRIEL field campaign" *by* A. Stickler et al.

Anonymous Referee #2

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Chemistry, transport and dry deposition of trace gases in the boundary layer over the tropical Atlantic Ocean and the Guyanas during the GABRIEL field campaign.

Stickler et al., 2007

This paper examines the photochemistry of the tropical rainforest over Northern South America (2-6N; 60W-50W)using measurements from the GABRIEL campaign interpreted with a photochemical box-model. In particular, the authors use median observations over ocean and land to compute chemical gradients from ocean to rainforest.

Doing this, they find significantly larger CO and lower HCHO in the measurements than can be explained from photochemical theory. They deduce this is likely from entrainment of LRT biomass plumes to the BL. They deduce an entrainment rate of 0.12 h-1. Using this constraint, they calculate new deposition velocities for H2O2, and find organic peroxides are higher than expected identifying some unknown loss pathway. They then examine net ozone production tendencies.

General Comments

I think this paper has significant results that should be published. The authors, however, need to cite the literature more in terms of their results for the reader to understand the contribution to the science as I discuss below. The science is good, I think the paper must be significantly shortened for ease of reading as I mention in my specific comments below. The authors seem to go into heavy detail on aspects not crucial for analysis.

1) The mission description and observations section feel like a mission overview paper in the level of detail. If this is the mission overview paper I would suggest putting flight information into tables etc. If it is not then the level of detail feels overwhelming.

2) Using standard notation would make it easier for the reader to follow. "ppbv and pptv" is standard notation in the atmospheric chemistry modeling community, which the lead author has used in previous papers, I would suggest changing from pmol/mol and nmol/mol to that notation.

Specific Comments

Introduction

It is my feeling that the introduction does not adequately pose the problem and previous literature.

1) As it stands the introduction is predominantly box model and mission description., the later of which I strongly suggest should be its own section (i.e., Introduction,

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GABRIEL Mission, Measurements, Model Description, Ĕ).

2) I think the introduction would benefit including the following background: a. When is the dry season for the region? What is the fire season for the region? How is this location impacted by long range transport? b. I suggest a sentence that says, " In this study we use a box-model constrained with observations to examine Ĕ" something like my overview above. c. What previous missions have taken place in the region and how do you go beyond. You mention LBA-CLAIRE, but not what they learned and what you need to further accomplish here? Perhaps also including other studies in the tropical forest.

Mission Description

3) The discussion of the individual flights and their altitudes is hard to follow and is unnecessary for your analysis. Is there an overview paper you can cite? I suggest saying that the flight tracks were primarily jogs between 300-500 m and 3.5 km. and that's it. Including more information later in the text where it is used.

4) Also include information of time of flights in this section, were they mostly during the day?

5) In figure 1, I suggest expanding the view a little bit more to show where in South America we are looking, including some country borders. (particularly Guiana and Suriname)

Measurement

6) I would suggest focusing your measurements section solely on what is important for your paper. Reference other papers (mission overview?) for peripheral information. The table is great. The level of detail in text is too much.

Box Model

7) You reference Stickler (2006) twice, should there b and (2006a) and (2006b). If

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EGU

possible I would suggest not referencing your thesis unless it is readily available online. If it is include url in reference.

8) I suggest moving the fixed hydrocarbon concentration information to a table with references to where you got them. This really clutters the text.

9) There should be a line saying “we calculate CO, HCHO, and peroxide mixing ratios. The following are fixed by observed concentrations:Ĕ.. Other species are held constant as described in Table xx.” Its in here but took two or three reading for me to figure out.

10) I would suggest just using local time (LT) not second past midnight. I know a paper should be reproducible but that level of detail is too much.

11) The equivilent longitude is difficult to understand. From what I see it is just modifying the start time over the ocean to account for different photochemistry enroute? Please include an opening statement clarifying its purpose at top of page 4793. “We vary initial conditions in order to account forĔfollowing Stickler et al., 2006”

Results and discussion

12) In Figures 2-5. I think it would be helpful to put a line where you are predominantly over ocean versus over land?

13) Similarly, It would be nice to include “longitude” as a second x-axis in figures 6-16, also with a line where ocean/land. I would suggest combining the comparable figures e.g., Fig 8, 9 overlaying them or at least putting them on the same y axis for ease of comparison.

14) Sect 4.1: You compare CO gradients for GABRIEL and LBA-CLAIRE. Are the fire impacts different during these different seasons? Does that explain the gradient difference?

15) Sect 4.1:. For ozone do you get different answers if you only look at afternoon data (i.e., removing nocturnal surface layer loss)?

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16) Sect 4.2.3: You do a lot of work on CO and HCHO to get at this entrainment rate. How uncertain is this rate? What met conditions is this valid for? Please cite others. Do you use this rate along the entire path or would you expect it to be different over the stable ocean MBL, I would think it would be?

17) Im trying to think what else you can get out of this CO gradient. Can you say anything about CO yield from VOCs compared with previous assumptions?

18) Sect 4.3.1: Can you speak to how good this deposition velocity for HCHO is compared with other studies? Can you cite studies suggesting how much heterogeneous removal of HCHO could be responsible?

19) Section 4.5: Similarly, can you talk to how much we would expect the aerosol removal of organic peroxides from previous literature? This for HO22 from lab measurements is smallĚare there estimates for this mechanism?

20) Section 4.6: Is the NPP difference between the two studies due to biomass burning, similar to the CO? It would be interesting if the extra NOx necessary was from entrainment of PAN from aloft.

Conclusions

The conclusions section is v. well done.

21) In the abstract you say 0.12 h-1, but in the conclusion you say 0.15 h-1. Be consistent in what you report/suggest.

Other Editorial details

22) On page 4789, Instead of calling TUV an “external module”, I suggest just saying TUV radiation model.

23) I would suggest walking through manuscript and removing excess wording such as page 4790 “could be calculated”, Ěyou can just say.. “Ě CCF, compensating for cloud an aerosol effects, was then applied.”

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24) Instead of short-dry season say April-May 1998

25) Sometimes you say methylhydroperoxide and sometimes you use CH3OOH, sometimes you say peroxides, sometimes organic peroxidesĚbe consistent for ease of reading.

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