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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Role of convective transport on tropospheric ozone chemistry revealed by aircraft observations during the wet season of the AMMA campaign" *by* G. Ancellet et al.

G. Ancellet et al.

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Answer to the major remarks of reviewer 1

1) The Introduction and background for the paper are inadequate. The innovative nature of the AMMA flights will be better highlighted if the past work is acknowledged. Then the authors can stress what is new in the present study. Likewise, the Conclusions can summarize more explicitly the unique AMMA accomplishments.

The introduction has been rewritten according to the two reviewers recommendations. In particular, past studies have been better acknowledged in the present version. De-

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scription of the campaign strategy has been improved in section 2.1 where one table has been added to summarize the F-F20 flights objectives and positions.

The conclusions was rewritten focusing on the main results of the

paper, i.e. (i) the link established between the variability of the observed chemical composition of the mid- and upper troposphere during the WAM and a large variety of transport pathways (ii) discussion of the conditions where ozone production was observed (iii) the usefulness of complementary airborne measurements combined with trajectory analysis.

2) Peroxide measurements as indicators of convection can be imprecise, largely due to uncertainties in the measurement method. Have these been taken into account? In referring to Snow et al., please discuss.

The technique itself is well-established and has been widely used for the airborne and ground H2O2 measurements. The AEROLASER instrument used in this work has been widely used by other groups. Although some artefacts and interferences were observed, the technique is considered as sufficiently accurate and sensitive for the H2O2 measurements. The discussion of the different aspects related to this method can be found in the reviews of Reeves 2003 and Lee 2000. References to these works were added in the final version. It is also important to stress that the role of convection is diagnosed not only with peroxide measurements but with the full set of data (CO, O3, H2O, NOx) reducing the dependency of our conclusions with possible inaccuracies in the peroxide data.

3) The overall results and their significance are not summarized effectively. The paper reads as an excellent report but it would be more remarkable for readers and the field of tropical chemistry if you made stronger conclusions. One way to do this might be to make a map or diagram in the Conclusions.

The major improvement was to change the conclusion in order to describe more ex-

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plicitly the outcomes of our work. First, the link between the spatiotemporal variability of the chemical tracer measurements and the role of convection during the WAM is discussed. A diagram has been added as suggested by the reviewer. The conditions for significant ozone production is the second outcome and are discussed in a specific paragraph. Finally the usefulness of an approach combining complementary airborne chemical measurements and trajectory modeling is demonstrated even for a very complicated dynamical system such as the WAM.

4) After the Introduction and Conclusion is revised, a more effective Abstract needs to be written that includes context provided by prior work.

This has been done based on a new introduction, conclusion and section 2.1.

Answers to the specific remarks

The authors wish to thank very much the reviewer 1 for his thorough reading of our manuscript and the suggested improvements of our poor English writing. A careful reading of the English writing was done and we have tried to improve this.

Hydroperoxide is somewhat imprecise. Do you mean hydroperoxy radical, H2O2 [hydrogen peroxide], ROOH and H2O2 generically?

Throughout this paper the term "hydroperoxide" includes H2O2 and ROOH generically, it has been defined in the introduction and it was checked that the meaning is really the same everywhere in the paper.

Page 15944: Section 2.1 very confusing.

This section has been rewritten to emphasize the objectives of the flights, namely the MCS outflow studies and the analysis of the meridional cross-sections of the chemical composition during the dry spells. A table is provided to list the flights and their objectives.

Page 15946 - 47. What is the accuracy of the chemiluminescent-based method? The

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calibrations described here are acceptable to prove consistency of the data but the reliability of the basic method needs support.

The accuracy of the presented measurements has been estimated on the basis of the preliminary tests and calibration measurements taking into account well known potential problems, e.g. losses in the sampling lines, uncertainty of the H2O2 concentrations in the calibration solutions, etc.. A short description of the measurement procedure is given in the text, while the discussion of all the details related to the H2O2 measurements is not appropriate in this paper and can be found in the cited review papers (Reeves et al. 2003, Lee et al. 2000) (see l. 154-159)

Page 15949 Line 11. Calibration coefficients have been corrected. HOW? Or add reference

Since the chemiluminescent analyzer have shown a temperature dependence sensitivity, the calibration coefficients, which were determined at a given temperature, have been corrected at each sampling time by linear interpolation. This has been explained now in the text (L. 226-228)

Page 15950 Lines 18-19. Confusing sentence on CO difference method. Do you mean the delta is referenced to 100 ppbv?

Yes the CO excess is the difference between the measured CO and a

threshold value of 100 ppb which corresponds to non polluted air masses according to Sauvage et al. 2007. It is better explained in the text at l. 256-258

Page 15951 Line 15. What is the logic of the RO2 to ozone connection? Please clarify or provide a reference.

We meant here that when one includes the role of RO2 in the NOx partitioning, the steady state expression of the NO2/NO ratio implies the well known shifting towards larger NO2 concentration especially when the NO2 production from the reaction of O3 with NO is lower. This is explained at l. 278-280

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Page 15953 Line 10-17. This is confusing. In discussing the possibility for transport from the southern hemisphere (SH) to Cotonou, no context is provided. Why might it occur? What are the pathways of transport? Are there prior observations for this? References? If it seems important to mention, why not add a diagram that shows possible flow.

References to Mari,2008 and Sauvage,2007 papers have been added to discuss the transport pathway connecting the SH to the Gulf of Guinea. This transport pathway is related to a baroclinic low-level circulations in a 2-km layer at 5°S which can uplift ozone precursors from the Southern Hemisphere (SH). The fire products are then transported from the SH to the NH by the south-easterly trade wind above the monsoon layer. In this paragraph, our point is to show that the sharp gradient in the ozone precursor and ozone concentrations between the air mass upwind and downwind of Cotonou is not related to this transport pathway but can be explained by the low level advection of the Cotonou plume above the continent. The paragraph has been rewritten to clarify this point (l. 323-335). It justifies that an air mass corresponding to the city emissions is included in table 2.

Page 15960 line 22 .Is there any conclusion about Type III or does it not occur in the peroxide data?

The hydroperoxyde measurements were stopped before the landing in Cotonou so the impact of the city plume cannot be discussed on this species. It is not a major problem as hydrogen peroxide data are mainly useful in this work to distinguish the role of convection from other transport pathways. Type III air masses have not been studied during convective episodes.

P 15963 Line 10 The three flights listed are not given a type? Is that why they don't appear in Table 1?

We thank the reviewer for this comment. In table 2, we only provide positions of air masses observed during the flights with long meridional cross-sections where the air

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mass origin is not obvious. For the MCS outflow exploration, the 4 flights all correspond to recent convective transport and there is no need a table to identify the position of these air masses. But we agree that the dynamical processes behind these air masses are different from the ones listed in table 2 (recent outflow of the MCS, focus on convection in the latitude band 10-15°N). Thus it is better to name the MCS outflow air masses, type VI. This is justified by the fact that, for these air masses, there are specific transport pathways that need to be discussed. Section 4.1.2 was rewritten to clarify this point.

Page 15964. Missing references in ROOH discussion, check Prather reference.

Indeed the work of Prather showing the important role of ROOH in the HOx budget in the tropics is relevant to this paper and it has been now properly acknowledged (l. 595-597)

Page 15966. Discuss the southern hemisphere influence on the NH CO in view of prior findings that NH helps support SH CO or ozone, eg papers by Jonquieres (TROPOZ), Edwards et al, 2003. This topic deserves amplification.

This point is now discussed in the new introduction with several references to previous work including Jonquieres and Edwards, 2003. We have also stress in section 4.2 the similarity of the inter-hemispheric transport previously observed during the NH dry season with the transport pathway discussed in our work that is relevant for the NH wet season (l. 664).

Page 15967 Lines 20-27. See comments at the beginning about a diagram or map displaying the various air mass types and synthesizing the paper. The results here are too detailed.

The conclusion has been modified as explained previously. We agree that there were too many details making rather difficult the identification of the main points.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 15941, 2008.