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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mesoscale convective systems observed during AMMA and their impact on the NO_x and O_3 budget over West Africa" *by* H. Huntrieser et al.

Anonymous Referee #2

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The article by Huntrieser et al. presents an investigation of the chemical composition of the outflow regions of two mesoscale convective systems (MCS), one south and one north of the ITCZ, that were sampled by the DLR Falcon aircraft during the African Monsoon Multidisciplinary Analysis (AMMA). The paper describes the CO, O_3, NO_x, and HCHO mixing ratios in the MCS outflow regions and analyzes the contribution of direct transport from the boundary layer as well as mixing with ambient upper troposphere (UT) air. The production of NO_x from lightning is calculated based on the above analysis and other DLR measurements.

The topic of the paper is appropriate for Atmospheric Chemistry and Physics. The

paper is well written, but is rather long. Some points are repeated, but mostly there is a lot of detail for the reader to sift through. My major issues with the paper concern presenting convincing evidence to support a conclusion and discussing results in terms of current modeling approaches for lightning-NO_x parameterizations. The paper needs to address these issues before publication.

Major Points

1. There are a couple of places (contribution of boundary layer (BL) or above boundary layer air, and contribution of mixing in UT) that the analysis did not convince me of the authors' conclusion. For the BL study, I found that Fig. 17 was more convincing than what was presented in earlier parts of the paper.

2. Part of the analysis is focused on quantifying the amount of NO_x produced by lightning. I suggest that the text in the appendix be placed in the main body of the paper, while much of the detail of how each variable was determined should be placed in the appendix (or supplemental material). Without having the equations guiding the reader, it is easy to lose sight of the goal of Section 4. Further, by having this structure, the focus is on the science question instead of the details of the methodology.

3. The calculation of the production of NO_x from lightning gives values (2500 g N/flash = 180 moles NO_x/flash) that are then compared with results from other tropical measurements. However, there is a tendency in the modeling community to use a much higher number (500 moles NO_x/flash) than that calculated in this study. The calculated number for AMMA is also much lower than that recommended by the 2007 review article (330 moles NO_x/flash). In this paper, there is no discussion comparing the current calculation with these modeling studies (or review study), nor is there any recommendation of what production rate modelers should use. I would like to see this kind of discussion included.

Specific Points or Questions

1. It would very much help the reader to have an initial figure showing the region of study, noting the various countries and AMMA observation sites.

2. Sections 3.2 and 3.3: Vertical distribution of convective outflow composition. What is the composition above the aircraft flight levels? I am not convinced that the Falcon sampled the complete vertical range of convective outflow. If one uses the tropopause as an upper limit for convective transport, then radio-sonde data (or ECMWF analysis) would aid the analysis. The Niamey radio-sonde data shows for July-August 2006 that the tropopause is 16.5 km (Fig. 9, Cairo et al., ACP, 2010) and Niamey ozonesondes for 26 July to 25 August show a tropopause at 15-16 km altitude (Fig. 15, Cairo et al., 2010). Indeed, one of the ozonesondes in mid August clearly shows convective transport of low O_3 from 10-15 km altitude. While Niamey is north of the region studied in this paper, it is not too far away to have drastically different conditions. In addition, the Geophysica data (Figure 13 of current study) show an O_3 tropopause of 14 km or higher. I think it would help to indicate the location of the maximum height of detrainment (i.e. tropopause) for the 6 and 15 August MCS days via radio-sonde data and/or ECMWF analysis.

3. As written, I do not find Figures 4 and 8 to be very informative. However, if the tropopause were overlaid then I think these figures would be worth keeping.

4. P. 22782, first paragraph. While the SAL may indeed act as a barrier to polluted layers below, there is not enough evidence given to convince me that the air comes from the bottom of the SAL. Since the 6 August MCS traveled over Niamey, it seems that the DOE ARM data would be useful in terms of determining cloud base height in relation to the BL height. Is it possible to conduct any trajectory analysis?

5. P. 22783, first paragraph. Are flight segments 6 and 7 Lagrangian downwind of segments 3, 4, 5? (that is, the airplane sampled the same air near the cores and then

downwind of the cores)

6. When the term “aged air" is used, how does this translate to distance from the convective cores, and to amount of time that the air has been in the UT?

7. P. 22784, L23-26. The HCHO measurements are interesting, especially comparing 6 August and 15 August observations. Besides noting the enhancement of HCHO compared to background UT values, the authors should also compare the HCHO mixing ratios to the BL values. I see a decrease in HCHO indicating scavenging by the cloud.

8. P. 22786, L10-12. It is difficult to believe the explanation of why O_3 increases while CO does not change (i.e., mixing with background UT air). First, what I see in Fig. 7 is that CO also increases from 140 ppbv to 148 ppbv. HCHO shows a dip just before 59000 s (background air?) followed by a small increase to 0.6 ppbv. It is also hard to believe that there are no measurements of the UT background air to contrast its composition with the convective outflow. Perhaps the changes seen in segment 6 are not important enough to make the statement that mixing with background air caused the changes. The changes at the 12 km altitude seem to be much stronger.

9. P. 22787, first paragraph. Why is the HCHO vertical profile not included in Figure 9? It would be a very interesting addition to this investigation.

10. Section 4. This is the section I suggest reorganizing so that the reader can start with the equations and have less detail on the method of determining each factor. This detail can go in the appendix. There is quite a bit of jumping back and forth from the 6 August to the 15 August results, and it is challenging to remember what was found in the previous sub-sections. I see merit in retaining the presentation of the LINET observations. It may be more appropriate to put this text in section 3 where observations are presented. Further, it would be good to combine sections 4.1 and 4.2 as they closely relate (and there is repeating of text in section 4.2). Lastly, please

consider rearranging Figures 10 and 11 so that August 6 results are in Figure 10 and August 15 results are in Figure 11.

11. P.22789, last paragraph of 4.1.1. How do the results compare to the mid-latitudes (EULINOX, STERAO results)?

12. P. 22791, L18-21. It would be nice to see the vertical distributions of the lightning strokes as supporting evidence for the mean height of the IC strokes.

13. Section 4.3. This section describes an important calculation, but makes at least one strong assumption about entrainment. Why is entrainment not included, or, why not include calculations assuming a small (or large) amount of entrainment? In addition, where is cloud base relative to the BL height?

14. Section 4.4. In determining the depth of the anvil outflow, it seems that ozonesondes would help. It would be great to see the Niamey and Cotonou ozone vertical profiles along with the temperature and dewpoint data to get more information on the structure and composition of the UT. As stated earlier, I find it difficult to believe that the top of the convective outflow is 12.5 km or even 13-14 km altitude with such a strong MCS occurring. I think there is a difference between the altitude of maximum outflow and the depth of the outflow region. Lastly, Geophysica data show the outflow region likely extends to 14 km (Figure 13) although these data are a compilation of the entire field campaign. Lines 1-4 on P. 22795 are troublesome because (a) it is quite possible the Falcon measurements do not show the C-shaped profile because the aircraft did not fly high enough, and (b) mixing with ambient air is used as a process to explain measurements here, but was assumed to not occur when determining the BL NO_x mixing ratio.

In the text, the methods of Law et al. (2010) need to be briefly described. Another source of information is the anvil cloud top height – can this be determined from satellite data? In summary, other resources (satellite data, ozonesondes) should be used to

help determine convective outflow depth.

15. Section 4.6. When determining the LINET to LIS ratio of lightning flash rates, it appears that there is an assumption that LINET has a 100% detection efficiency because there is no adjustment for the LINET detection efficiency. Is that correct?

16. P. 22799, lines 20-21. It is difficult to imagine that the 2 AMMA MCS thunderstorms sampled are representative for the globe. Perhaps instead, it is that MCS dominate the lightning flash rates on a probability distribution chart. Despite this poor assumption, the results fit nicely in the current range of estimates. Is that because the range is so large that most any answer will reside within the range?

17. The authors do a really nice job of comparing the AMMA results to other storms in other regions. The G_{LNOx} can be placed on Figure 28 of Schumann and Huntrieser (2007). The range 1.4 to 3.5 Tg(N)/year determined from the AMMA data is much lower than other recent estimates. This range translates to 180 moles NO_x per flash which is much lower than the 500 moles/flash suggested by other recent papers (Hudman et al., JGR, 2007; Jourdain et al., ACP, 2010; Ott et al., JGR, 2010). It would be nice to see a discussion about this in the paper with explanations as to why these (and other tropical and subtropical measurements) are different from the modeling study analyses.

18. P. 22801-22802. Could you explain why using the CO to O_3 ratio at 7 km is appropriate for comparing with convective outflow at 10 km? Is there not UT background measurements at 10 km altitude? Further, what is the effect on aging convective outflow when analyzing measurements from a morning flight (or storm) to an afternoon flight (or storm). Could the morning/afternoon contrast contribute to differences seen in the CO to O_3 ratios?

19. P. 22802, L23. Can a stronger conclusion about mixing be made by conducting further analysis with Flexpart simulations? It seems that there must be a way to show the importance of mixing (and not just “speculate").

20. P. 22803. I don't think Figure 16 is needed as the same results are shown in Figure 17.

21. P. 22803, L18-23. The data presented in Figure 17 show more convincing evidence that the convective outflow air is from the top of the BL (than the explanation in Section 4.3). However, entrainment could still play a role.

22. P. 22803-22804. There is little to no interpretation of Figure 18. What does it mean that TROCCINOX results are similar (different) than AMMA results?

23. P. 22809, lines 17-22. Why were lightning flash lengths and their relation to wind shear not analyzed for the AMMA data?

24. P. 22810, first paragraph. To add to the literature, Barthe et al. JGR 2010 (in press) have conducted cloud resolving model simulations to examine the ability of CRMs to predict lightning flash rate from various storm parameters.

25. P. 22811, lines 5-9 should be stated earlier in the paper. Although I would like to see further evidence that the depth of the outflow is not higher than 13 km.

26. P. 22812, lines 1-3 should be stated earlier in the paper as well.

27. P. 22812, 23-27. It would be good to see discussion on the contrast of the HECTOR results with the AMMA results (and not just report what was found for HECTOR).

Technical Details.

1. P. 22770, L13. Is it -1.5°E or 1.5°E?

2. P. 22771, L24. TLL –> TTL

3. P. 22772, L8. HCHO is not listed in Table 1. It should be included along with some text on its performance.

4. P. 22773, L19. partly humidity –> relative humidity (or dewpoint temperature, whichever is more correct)

5. P.22774, L25. was –> were (LIS data . . . were compared. . .)

6. P. 22775, L14. Insert "the" before "southwest"

7. P. 22777, L28. Remove "also"

8. P. 22778, L27. Remove "however"

9. P. 22782, L4. dryer –> drier

10. P. 22783, L14. It's a bit misleading to say "up to 24 m/s". It would be better to give a range, e.g. 16-24 m/s. Also, the word "even" is not needed.

11. P. 22783, L29. –> indicative of stronger pollution transport

12. P. 22787, L3. dryer –> drier

13. P. 22787, L13. promoted –> promote

14. P. 22789, L25. structured –> structure

15. P. 22791, L8. Remove "first"

16. P. 22792, L26. Remove "mainly"

17. P. 22793, L8. Remove "mainly"

18. P. 22793, L12. Remove “For the selected MCS flight segments of 6 and 15 August listed in Table 3,”

19. P. 22794, L14. Remove “in Table 3"

20. P. 22794, L16. was –> were

21. P. 22796, L10. Remove “also"

22. P. 22796, L113. Remove “in Table 3"

23. PLNOx [molecules NO/LIS flash] calculation. Is it correct? What I get is: 2500 [g N/ LIS flash] / 14 [g N/mol] x 6.022x10^{23} [molec/mol] = 10.75x10^{25} [molec/LIS flash]. This is about twice of what is listed in Table 4.

24. P. 22801, L1-3. Note that these other studies are for the midlatitudes.

25. P. 22804, L27. expect –> except

26. P. 22805, L7. Remove “also”

27. P. 22806, L28. preferable –> preferably

28. P. 22809, L5. –> hydrometeor

29. P. 22812, L11. Remove “respectively" (proofread that sentence)

30. Tables 3 and 4 could be combined. Also consider transposing the tables because the font gets really small with ACP formatting. Another idea is to split the tables differently by putting the information for the flux of NO_x into Table 3 and for the production of lightning NO_x into Table 4.

31. Please define the tropics and subtropics in Table 4.

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