Atmos. Chem. Phys. Discuss., 10, C13930–C13947, 2011
www.atmos-chem-phys-discuss.net/10/C13930/2011/

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.

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Received and published: 21 February 2011

- We thank Reviewer #2 for the helpful comments to improve our manuscript.

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, O3, NOx, 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 NOx 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-NOx 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.

- BL/UT air contribution: Figure 17 is the most convincing figure, however it is not possible to present this figure earlier in the paper. In Sects. 3.2-3.3 and 4.3 we therefore now included more cross references to the detailed description in Sect. 5 (incl. Fig. 17).

2. Part of the analysis is focused on quantifying the amount of NOx 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.

- The equation and its description in the Appendix have now been moved to Sect. 4. We also moved Sect. 4.1 (LINET stroke observations) to Sect. 3 (AMMA observations). One paragraph of Sect. 4.5 (details on the estimate of the vertical wind shear) was moved to the Appendix.

3. The calculation of the production of NOx from lightning gives values (2500 g N/flash = 180 moles NOx/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 NOx/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 NOx/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.

- It is correct, that our results are located at the lower end of Fig. 28 in the review article by Schumann and Huntrieser (2007). However, Fig. 28 is mainly based on model results. We believe that the main reason for these differences between model studies and our studies is that the model studies are based on midlatitude storm observations and our studies focus on thunderstorms in tropical regions. We have now added some comments on this to Sect. 7 (last paragraph). In Sect. 7 we discuss in large detail, why tropical thunderstorms may produce less LNOx in comparison to other regions. We point out the importance of the vertical extent of the ice charged cloud region and that it would be important to also simulate the cloud microphysical processes in the models. A recent study by Beirle et al. (2010), based on NO2 column densities from SCIAMACHY measurements, also indicate distinctly lower LNOx production rates (<1 Tg(N) a-1) compared to model studies. The large uncertainty range in the estimate of global LNOx production rate therefore still remains. From our point of view, we need more observations of LNOx in different types of regions. These regional estimates of LNOx production rate per flash may then be used in model studies to improve the global estimates. Our general impression, based on all our field campaigns up to now, is that LNOx production rates per flash are lower in tropical regions compared to subtropical and midlatitude regions.

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.

- Since the paper is already rather long (comment Referees and Editor), an additional figure showing the region of the study will not be included. The region of interest (with latitude and longitude included) is visible in the satellite figures (1, 2, and 6). These figures will be enlarged in the final version. In these figures, the surrounding capital cities (e.g. the AMMA sites Quagadougou and Niamey) are indicated. We now also added the countries (Mali, Burkina Faso and Niger) to the legend.

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 O3 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 O3 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.

- We agree that the cold point tropopause was located between 15-16 km according to observations with the Geophysica aircraft from other AMMA flights (not available for 6 and 15 August 2006) and as indicated by the new isothermes added to Figs. 4 and 8. However, the cold point tropopause is not the upper limit of the main convective outflow (needed here). According to Geophysica measurements, the ozone tropopause was, as expected for the TTL, located lower 13-14 km (Fig. 13). The investigated storms on 6 and 15 August were in a decaying stage and according to the satellite images (Fig. 1, 2, and 6) only reached cloud top heights around 13 km (as already mentioned in Sects. 3.2-3.3). In Sect. 4.4 we described how we estimated the upper level of the main convective outflow according to the equivalent potential temperature profiles (Figs. 5 and 9) based on ideas by Highwood and Hoskins (1998) and Folkins et al. (2000). For the 15 August case, it was necessary to extrapolate the equivalent potential temperature profile from the Falcon to 12.5 km, as already described in Sect. 4.4.

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.

- We think that these figures are important because the vertical cross sections of the wind velocity give additional information on the location of the AEJ and TEJ, essential for the generation of MCS. This latitudinal information complements the single vertical profiles from the Falcon. Furthermore, we can compare the wind velocities from the Falcon with ECMWF data (Sect. 3.2-3.3). It is also important to demonstrate that the ambient wind conditions (AEJ and TEJ) were rather different for the two cases presented in this study. We have now added isothermes to Figs. 4 and 8 to indicate the cold point tropopause.

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?

- In Sect. 3.2 we suggested that the air injected into the MCS originates from the top of the BL (1.4-1.5 km altitude) coinciding with the bottom of the SAL ($\sim$1.5 km). From the available temperature and humidity data from the Falcon flight on 6 August 2006, we decided to calculate the temperature at the lifting condensation level (Bolton, 1980) which gives information about the cloud base height directly over Quagadougou. Mean

values for the lowest 100 m layer were used (301 K and 66% relative humidity) resulting in a lifting condensation level temperature of 292.5 K corresponding to an altitude of 1.4-1.5 km. This new information has been added to Sect. 3.2.

- Trajectory analyses based on Lagranto are available. The 24-h backtrajectories show a path from northwestern Nigeria to Quagadougou, as also roughly seen in the MSG time series in Fig. 1. However, from this type of trajectory analyses it is not possible to gain information on the vertical ascent in mesoscale features as MCS.

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)

- No, the aircraft was flying too fast to sample exactly the same airmasses downwind (segments 6-7) as near the core (segments 3-5). Furthermore, the aircraft ascended from 10.1-11.3 km near the core to 11.7 km further downwind.

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?

- Here we use the term “aged air” for emissions that were released a few hours ago (<12 h) and were advected a few hundreds km (<500 km) from the convective core.

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.

- On P. 22784 (L9) it was mentioned that the HCHO BL mixing ratios were in the range of 0.8-1.2 nmol mol-1. In the MCS outflow, mixing ratios in the range of 0.6-0.7 nmol mol-1 were observed (L24). It was mentioned that the decrease in HCHO mixing ratios between the BL and outflow region may be due to the short lifetime and high reactivity of HCHO (in comparison, mean CO mixing ratios remained constant). We have now also added the possibility of scavenging of HCHO.

8. P. 22786, L10-12. It is difficult to believe the explanation of why O3 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.

- The mentioned dip in HCHO is due to mixture of the outflow with background air. It is correct that the CO mixing ratio increases to 148 ppbv in this part of the time series, however the values listed in Table 2b are mean values for the period when the aircraft penetrated the outflow. For a shorter time period (59033-59087 s) than segment 6, including only the CO peak period, the mean CO and O3 mixing ratios are 141 and 46 ppbv, respectively. The time series in Fig. 7c show that outside the convective core (after segments 3-5), the O3 mixing ratio starts to increase earlier (before 59000 s) in comparison to the CO mixing ratio (after 59000 s), perhaps also due to the rapid altitude change (O3 vertical gradient more pronounced). We assume that this is the reason why in segment 6 the mean O3 mixing ratio increases while CO does not yet change.

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.

- HCHO data have been added to Figs. 5 and 9.

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.

- We have now reorganised this section and start with the equations. The LINET observations (Sect. 4.1) were moved to Sect. 3.4. We have kept the order of the Figs. 10 and 11. To our opinion it is better to see the differences between the two cases (6 and 15 August) by showing the distributions side by side.

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

- Here we only compared the AMMA LINET observations to other LINET observations during TROCCINOX and SCOUT-O3. It is not possible to compare LINET stroke rates to measurements during EULINOX and STERAO where completely different lightning detection systems were used.

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.

- Since the paper is already rather long (comment Referees and Editor), an additional figure showing the vertical distribution of LINET strokes will not be included. To our opinion this distribution is not essential for the paper. Mean height and standard deviation of the IC strokes (8.2ïĆś2.9 km) are already given in the text.

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?

- In Table 3 we only selected the flight segments which are located within the convective core where we assume that the mixing with the ambient air was not yet very prominent. Further evidence are discussed later in Sect. 5 and shown in Fig. 17. On 6 August, the cloud base height coincides with the BL height (1.4-1.5 km), which has been added to Sect. 3.2.

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 NOx 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.

- Since the paper is already rather long (comment Referees and Editor), additional figures showing the ozonesondes from Niamey and Cotonou will not be included. To our opinion, these profiles can not add additional information to the Geophysica profiles, which are available for the area of our interest (Quagadougou). The Geophysica measurements show that the main deep convective outflow is in general located around 12 km, coinciding with minimum O3 mixing ratios (Fig. 13). For our calculations, we are

interested in the top of the main convective outflow and not the maximum height of the convective outflow. In Sect. 4.4 we described how we estimated the upper level of the main convective outflow according to the equivalent potential temperature profiles from the actual flights (Figs. 5 and 9) based on ideas by Highwood and Hoskins (1998) and Folkins et al. (2000). To our opinion, this is the most accurate estimate based on local conditions. At the time when the MCS were investigated by the aircraft, they were in a decaying stage and cloud tops were not as high as during the mature stage earlier. According to satellite images from 6 and 15 August (Fig. 1, 2, and 6) cloud top heights were in the range of $\sim$13 km (as already mentioned in Sects. 3.2-3.3).

- Lines 1-4 on P. 22795: (a) From the Geophysica aircraft, CO measurements are available from the 7 August 2006 (one day later). The vertical profile (data available up to 19.5 km) shows slightly enhanced CO mixing ratios in a layer between 11-13 km (90-100 ppbv). The peak in CO mixing ratios (105 ppbv) was observed in 11.5 km. For the 6 August case we do not expect to see a strong C-shaped profile, because we have the impression that the upward transport of polluted BL air from lower layers was inhibited by the SAL, as explained earlier. (b) Was now cut as explanation. We added that the CO mixing ratios are slightly enhanced above 9 km and reach values in the range of 100-110 nmol mol-1, similar to the values at the top of the BL (1.5 km) (Fig. 5a).

- Law et al. (2010): used the definition by Gettelman et al. (JGR, 2004) for the level of main deep convective outflow. It is located where the potential temperature reaches 350 K (pressure 200 hPa), which has now been added to Sect. 4.4. Law et al. (2010) estimated this level to 12.5 km for AMMA, which correlates well with their observations of cloud presence (Fig. 2).

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?

- We do not assume that the LINET detection efficiency is 100%, which it of course not is. In Sect. 2.3 we mentioned that the detection efficiency of strokes with low peak currents (<10 kA) decreases with increasing distance from the LINET detection centre. However, in our earlier studies (Huntrieser et al., 2008), by comparison with other lightning detection systems, we found that LINET strokes with higher peak currents >10 kA are detected with the same efficiency in the whole LINET area. Therefore, the relationship between LINET strokes (>10 kA) and LIS flashes should be constant within the LINET area.

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?

- For the calculation of the annual global lightning-NOx production rate (most commonly used number concerning LNOx), it is common practice to scale the observations from single thunderstorms to the global scale. In this manner, it can easily be compared which contribution would result based on different types of thunderstorms. MCS belong to the most intense thunderstorms on Earth (Zipser et al., 2006) and therefore impact global-LNOx essentially. Our findings based on results from our tropical field campaigns, range 1-8 Tg(N) a-1, fit rather well within the range 5ïĆś3 Tg(N) a-1 given by Schumann and Huntrieser (2007). However, on the whole our mean value ~3 Tg(N) a-1 is located at the lower end of most other results on LNOx published up to now.

17. The authors do a really nice job of comparing the AMMA results to other storms in other regions. The GLNOx 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 NOx 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.

- It is correct, that our results are located at the lower end of Fig. 28 in Schumann and Huntrieser (2007). However, Fig. 28 is mainly based on model results. We believe that the main reason for these differences between model studies and our studies is that the model studies are based on midlatitude storm observations and our studies focus on thunderstorms in tropical regions. In Sect. 7 we discuss in large detail, why tropical thunderstorms may produce less LNOx in comparison to other regions. We point out the importance of the vertical extent of the ice charged cloud region and that it would be important to also simulate the cloud microphysical processes in the models. A recent study by Beirle et al. (2010), based on NO2 column densities from SCIAMACHY measurements, also indicate distinctly lower LNOx production rates (<1 Tg(N) a-1) compared to model studies. The large uncertainty range in the estimate of global LNOx production rate therefore still remains.

18. P. 22801-22802. Could you explain why using the CO to O3 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 O3 ratios?

- The vertical O3 profiles (Fig. 13) indicate that the region above 8 km is affected by the convective outflow (O3 mixing ratios decrease) and therefore not suitable as typical background. This has now been added to the text.

- Differences in CO/O3 due to morning/afternoon contrast: In Fig. 15 we show results from many other flights in addition to the 060806 flight, as listed in the header (0108b, 0408, 0708, 1108, 1508a, 1808a). During all these flights (both morning and afternoon flights close to Quagadougou) the conditions were very similar. We therefore do not expect that the morning/afternoon contrast (flight 060806 and 150806b) can be an explanation for our findings.

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").

- As far as we know, it is not possible to resolve this small scale mixing of the convective outflow with the ambient air with FLEXPART. Instead cloud-resolving models are needed. In Sect. 4.1 we stated: For several TROCCINOX, SCOUT-O3/ACTIVE and AMMA cases cloud-resolving model simulations are in preparation, but not ready yet (Huntemann et al., 2010; K. Pickering, NASA Goddard, personal communication, 2010). The results from these simulations will show if our assumptions are correct or not. As mentioned at the beginning (recommendation from Editor and Referees), we should try not to extend our paper further.

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

- To our opinion, it is also important to show the horizontal distribution of the CO and O3 mixing ratios. We have now added some text to this section to point out the specific features (regions with strong O3 increase) in this figure.

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.

- In Sect. 4.3 we now added that more details on this can be found later in Sect. 5. It is correct, that entrainment takes place as the air is lifted from the BL to the UT. However, within the convective core the trace gas mixing ratios are more impacted by the direct transport from the BL than from the entrainment.

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?

- The results in Fig. 18 are partly also shown in Fig. 15 and 17. To reduce the length of the paper (comment Referees and Editor), we decided to cut this figure.

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

- In this paper, compared to our TROCCINOX and SCOUT-O3 papers, we also wanted to focus more on O3. Adding analyses of the lightning flash lengths and their relation to wind shear would again increase the length of the paper. Furthermore, from 6 August the LINET data set was too small for this kind of analyses.

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.

- We have now added this reference to the discussion in Sect. 7.

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.

- The IC stroke height was already mentioned in Sect. 4.1. We now also added these results to Sect. 4.4 (focusing on the depth of the outflow).

- Further evidence: Above we discussed in several replies why our observations give no indications that the main convective outflow (parameter needed for our calculations) extends above 13 km.

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

- We now added this comment also to Sect. 6.

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).

- The AMMA results discussed in Sect. 7 were also added to this section now.

Technical Details.

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

- The latitude -1.5°E is correct.

2. P. 22771, L24. TLL –> TTL

- Has been corrected.

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

- HCHO has been added to Table 1 and a reference on the performance has been added (Andrés-Hernández et al., 2010).

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

- Has been corrected to relative humidity.

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

- Has been corrected.

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

- Has been corrected.

7. P. 22777, L28. Remove “also"

- Has been corrected.

8. P. 22778, L27. Remove “however"

- Has been corrected.

9. P. 22782, L4. dryer –> drier

- Has been corrected.

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.

- The mean value 15 m s-1 for the layer 3-6 km has been added.

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

- Has been corrected.

12. P. 22787, L3. dryer –> drier

- Has been corrected.

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

- Has been corrected.

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

- Has been corrected.

15. P. 22791, L8. Remove “first"

- Has been corrected.

16. P. 22792, L26. Remove “mainly"

- Has been corrected.

17. P. 22793, L8. Remove “mainly"

- Has been corrected.

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

- Has been corrected.

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

- Has been corrected.

20. P. 22794, L16. was –> were

- Has been corrected.

21. P. 22796, L10. Remove “also"

- Has been corrected.

22. P. 22796, L13. Remove “in Table 3"

- Has been corrected.

23. PLNOx [molecules NO/LIS flash] calculation. Is it correct? What I get is: 2500 [g N/ LIS flash] / 14 [g N/mol] x 6.022x1023 [molec/mol] = 10.75x1025 [molec/LIS flash]. This is about twice of what is listed in Table 4.

- Has been corrected in Table 4 and in the text.

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

- No, the Pickering et al. study is for the tropics. It has been added to the text that these other studies are for the tropics and midlatitudes.

25. P. 22804, L27. expect –> except

- Has been corrected.

26. P. 22805, L7. Remove “also”

- Has been corrected.

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

- Has been corrected.

28. P. 22809, L5. –> hydrometeor

- Has been corrected.

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

- Has been corrected and improved.

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 NOx into Table 3 and for the production of lightning NOx into Table 4.

- Since some different parameters are listed in Table 3 and 4, it is difficult to merge the tables. The tables will be enlarged in the ACP version in comparison to the present ACPD version.

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

- More details have been added to the legend.

- Finally we have added some new references on LNOx: Beirle et al. (2010) to Sect. 1, Barthe et al. (2010) and Yair et al. (2010) to Sect. 7.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 22765, 2010.