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Atmospheric Chemistry and Physics Discussions

Interactive comment on "NO_x production by lightning in Hector: first airborne measurements during SCOUT-O3/ACTIVE" *by* H. Huntrieser et al.

Anonymous Referee #1

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"NOx production by lightning in Hector: First airborne measurements during SCOUT-O3/ACTIVE" by H. Huntrieser et al. [Paper in ACPD, 9, 14361-14451, 2009]

General remarks:

The paper reports and analyses in great detail airborne observations of LNOx (Lightning-produced NOx) made in the north of Australia (Darwin area) during a SCOUT field experiment. Here, the 19 Nov. 2005 case study focuses on the anvil outflow of the "Hector" system which has a daily occurrence over the Tiwi Islands. These observations are compared to those of a MCS sampled in the vicinity of Darwin and to those of a more continental subtropical multi-cell thunderstorm case. The study combines mostly NO, NOy, O3 and CO data collected by the Falcon aircraft of the DLR, a series of lightning strokes recorded by the portable LINET network and radar pictures

to assist the interpretation of the results.

My first impression is that the paper is well-written but too long and contains too much superfluous details (Section 5.2 and 6.4) with lots of reference to figures and tables. This renders the narration of the paper difficult to follow and also dilutes the major interest of the paper which is to refine estimates of high LNOx production rates in tropical thunderstorms (results are well summarized in Table 4). My second remark concerns the choice of the authors to select the observations of a "golden day" in a 1-2 month field campaign (November-December 2005). This is contradictory with the fact that LNOx concentration is highly variable (and poorly predictable) in anvil outflows. So it is frustrating that a larger statistics of LNOx production rate, taken from the whole campaign in the investigated tropical region, is not reported in the study. The third point to outline is the similarity of the manuscript with a previous paper (referred HH08 in the manuscript) about the TROCCINOX campaign in Brazil as for instance, figure 3 and details of the method to get the LNOx production rate are repeated here.

The central discussion of the paper concerns the estimate of the horizontal LNOx flux (F_LNOx in Eq. 1) and the LNOx production rate per stroke (R_LNOx in Eq. 2) from airborne measurements and from LINET data, respectively. The measured excess of NOx concentration (with an averaged \chi_LNOx value per anvil penetration) can be clearly depicted along the Falcon passes in Fig. 7. However the temporal and the vertical aspects of the NOx variability both sides of the penetrations are not well outlined (sections 6.4 and 6.5). The discussion about the estimate of the mean depth of the anvil outflow is also difficult to follow. Why not considering the vertical shear (taken from aircraft and CPOL radar data) as a good indicator of the anvil boundaries? Finally, I don't find the discussion about the role of the wind shear (section 7.2) very relevant because basically the production of LNOx depends on the capability of thunderstorms to become electrified by non-inductive charge separation process, so something which is physically loosely related to the wind shear.

I recommend the manuscript for publication in ACP but with substantial revisions. I suggest the authors to shorten their manuscript, to add results taken from other flights during the whole campaign (if they are available) and to concentrate on the difference between previous estimates of LNOx in tropical areas; e.g. those taken during the TROCCINOX experiment.

Specific questions and remarks:

1. Section 4.3 (pp. 14373-14374): It is difficult to assess the accuracy of the detection of the IC strokes by the LINET network. Is there any indication that this detection was efficient enough at the scale of the network? How high is the IC/CG ratio in the Hector case?

2. Section 6.2 (p. 14384): The references to Skamarock et al. (2003) and Fehr et al.(2004) are not relevant in the context of the present discussion about the dispersion of the LNOx because the model they used contains no explicit lightning flash scheme to produce the LNOx.

3. Section 7.1 (pp. 14399-14400): The way the length of a "flash component" is estimated is obscure. It's difficult to figure out which information taken from the LINET network is used. The authors need to give more details. Moreover I find that a mean flash length of a few kilometers (Fig. 20) is very low compared to the large horizontal extension of the investigated storms. The authors should comment this point.

4. Section 7.2 (pp. 14401-14402): The discussion about the vertical wind shear is not very useful when restricted to the length of the lightning flashes because lightning flashes are very complex end products of tropical convective clouds. The vertical wind shear is a fundamental environmental component in the development of the deep convection itself, without consideration of lightning characteristics. Modifying the vertical wind shear leads to so many changes in the dynamics, in the microphysics and finally in the cloud electrical state that it is not realistic to interpret with geometrical arguments the sensitivity of the flash length to the wind shear.

5. Summary (p14406): Huntemann et al. (2009) should be omitted as it is not a published reference.

6. I couldn't get a good print of Fig. 16 (letters and numbers are missing) but I could visualize the whole pdf file.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14361, 2009.