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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 #2

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General remarks:

The submitted manuscript details measurements of lightning, chemical species (CO, O3, NO, NOy), and meteorological parameters obtained during the Scout-O3/ACTIVE campaign of 2005 with a focus on the Hector thunderstorm observed on 19 November. Analysis of several other thunderstorms is also included. The authors use a variety of airborne observations to estimate production per lightning flash in these storms and then use extrapolation to estimate the global lightning NOx source per year. In addition, several theories regarding differences in production per flash in different thunderstorms are presented. The analysis presented is well done and the topic very important and relevant to ACP readers. However, the paper is very long and needs to be more cohesively organized. At times the analysis can be difficult to follow in one reading, although

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after a second reading, most of the methods and ideas are sound. For this reason, I recommend publishing with revisions. I have outlined some suggestions below which could be used to shorten the paper – the authors need not follow these precisely as long as a shorter and clearer presentation is achieved in the revision process.

Specific remarks:

1. Section 1 (Introduction) is reasonably concise. I would recommend significantly shortening Sections 2 and 3 (on Hector and other field projects conducted in the area) and adding these to the Introduction. While a large portion of the background on Hector is relevant to the later LNOx analysis, much of Section 3 could be removed with only the most relevant results highlighted. If the results of some of these campaigns are particularly relevant to the results, they could be introduced in later sections as necessary.

2. Section 4 is well written and fairly concise. I think it would be useful to include a few more details on the LINET system in Section 4.3 on p. 14373. Some relevant material is contained in Section 7.1 which would be more helpful if introduced earlier in the paper. Also, the method used to estimate LNOx (Section 4.4) needs to be moved so that it introduces the section on these estimates (Section 6).

3. Much of Section 5.1 (General meteorological situation) could be removed including Figure 4. Section 5.2 (which provides a lengthy summary of the 19 November flight) could also be reduced in length. For instance, I'm not sure that Figure 6 is necessary because the flight locations and proximity to lightning are more strongly related to the analysis and are given in Figure 9. Some of the sequencing of figures is also difficult to follow. For example, figures 8 and 9 are introduced and discussed before NO and NOy mixing ratios presented in Figure 7. Though interesting, I would suggest cutting the paragraph on CN on p. 14381-2.

4. Section 6.3 on the contribution of BL-NOx to anvil NOx could likely be reduced in length, especially since the BL was found to fairly clean, and therefore, a minor factor

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in determining anvil mixing ratios.

5. The estimate of anvil outflow depth (Section 6.4) is somewhat hard to follow and very long. The authors seem to use two terms – main anvil outflow and anvil outflow which have different definitions. This can be unclear and I am not sure how precisely the narrow range of main anvil outflow altitudes can be identified using these methods. I would suggest using a single range and term to describe it. For example, on p. 14390, the range of 10.4-11.8 km for anvil outflow is used on line 14, and on line 16, the range for main anvil outflow of 10.5-11 km is used. Section 6.5 is also hard to follow and I wonder if it is necessary or could be incorporated into other sections. Sections 6.6 and 6.7 are comparatively concise and easy to follow.

6. Lastly, the theories presented in Section 7 are very interesting but I wonder if they might be better explored in a separate short paper. Some of these concepts could be introduced briefly and qualitatively in Section 8 (Summary and conclusions) but investigated in more depth, possibly with the addition of some model simulations, in another manuscript.

Paragraphs 1 and 2, p. 14388 – How is the value of 0.13 nmol/mol BL-NOx calculated? Over what altitudes is this applicable (BL, anvil, etc.)? I think the justification given in this section that the BL-NOx contribution to anvil NOx should be fairly small is convincing, but a sentence of two added to describe this calculation in more detail would be helpful.

Lines 6-10, p. 14390 – Why are the Geophysica wind measurements discussed instead of measurements taken from the Falcon? Is it because these data better indicate the outflow level? I notice a similar vertical pattern evident in the Falcon data though the peak at 10.5 km is much smaller. It might be good to mention this.

Figure 13c and d. Its hard to see the changes in gradient noted. Would it be possible to make this more evident, either by changing the scale on the plots or by indicating where on the profiles these changes occur?

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Technical corrections and comments:

Throughout manuscript - Change the phrase ‘in large detail’ to ‘in detail’

Throughout manuscript – Remove italics. The points emphasized by italics have generally been made in the text and the italics are not necessary.

Line 23, p. 14364 – Change disproportional to disproportionately

Line 3, p. 14365 – Add a few words: ‘. . .(TROCCINOX) conducted during. . .’

Line 8, p. 14370 – This needs to be clarified. In previous sections there is discussion of production of LNOx per flash being weaker in the tropics than in the midlatitude storms due to differences in flash length and shear. I believe that the production indicated in this sentence is due to the concentration of flash rates – is that correct? If so, perhaps the sentence could be changed to ‘. . .estimate LNOx production rates in the tropics, where lightning flashes occur most frequently’ or something of this nature.

Lines 22-23, p. 14375 – I think it would be best to remove the sentence beginning ‘Up to now. . .’ Though the method laid out is certainly reasonable, it is still impossible to know these parameters exactly.

Table 3, Table 4 – I think there is an inconsistency between the data given for flight segment 191105_1a_I. In Table 3, the mean stroke rate is given as 0.091 while in Table 4, it is 0.114.

Line 13-14, p. 14387 – Change ‘. . .was similar low or even lower. . .’ to ‘. . . was as low or even lower than on 16 November . . .’

Lines 3-4, p. 14390 – Reaction with fresh lightning NO emissions could also reduce ozone mixing ratios – may want to mention that this could be contributing factor.

Line 17, p. 14399 – Insert space between ‘more’ and ‘PLNOx’

Line 24, p. 14399 – Change ‘closer area’ to ‘small area’

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