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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Direct satellite observation of lightning-produced NO_x" *by* S. Beirle et al.

Anonymous Referee #2

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General Comments: This manuscript contains a very thorough analysis of the production of lightning NOx as estimated from SCIAMACHY tropospheric NO2 columns and lightning data from the World Wide Lightning Location Network (WWLLN). The resulting production per flash values obtained are surprisingly low compared with other estimates in the literature. Much effort is placed in the paper in trying to explain these results. However, the result remains largely inexplicable. The analysis was conducted on very fresh lightning emissions (within one hour prior to SCIAMACHY overpass). These emissions are most often still within the cumulonimbus clouds in which they were produced. Such clouds are very optically thick. My suspicion is that, despite the radiative transfer modeling that indicates fairly good "visibility" for LNOx in the middle portion of a cloud, there is something that we just don't know well enough about radiation behavior in this type of cloud. Therefore, I would recommend that a short sensitivity

study be performed to consider the effect of relaxing the 1 hour criterion to perhaps 2 or 3 hours and rerunning the analysis. This amount of time will still be short enough to minimize chemical loss, but will allow at least some of the LNOx to be transported to regions just outside (clear air) or at the edge of the cloud (partly cloudy conditions) where it might be more visible. This would necessitate consideration of flash counts in pixels upwind of the SCIAMACHY observation pixel being considered. Presumably a comparison of the FRESCO (derived from O2 A-Band observations by SCIAMACHY) cloud top heights with IR cloud cloud tops might also yield some information concerning why the visibility of LNOx appears to be poor. If the FRESCO cloud top is not much below the IR top, then the volume of cloud being seen by the instrument will be small, and the resulting NO2 columns will be small. I recommend doing such a comparison and including it in the paper. These are the only major additions to the paper that are needed. I have a number of more minor comments as listed below.

Specific Comments: p. 18257, line 18: please replace "came up" with "became possible"

p. 18263, line 6: please replace "several" with "$\sim$20-30"

p. 18263, lines 20-21: should note that the WWLLN data prior to 2007 have now been reprocessed with the new algorithm and the DE should now be greater that what is computed here.

p. 18264, Section 2.3: What is not considered here is variability of the WWLLN DE with time of day. I thought that propagation of wavelengths detected by WWLLN was better at night than during the daytime. If so, the DE values used here should be smaller than the diurnally averaged values obtained by comparing WWLLN flash counts with the OTD/LIS climatology. See additional comments regarding the results of the instantaneous DE obtained in Section 4.3.

p. 18265, Section 2.4: I think at least some error in the analysis arises from assuming that all of the LNOx produced during the hour prior to SCIAMACHY overpass remains in the pixel being analyzed. A 30 m/s wind in the upper troposphere yields a transport of 108 km, which is larger than the 30 x 60 km pixel. Use of single pixels likely results in missing some portion of the LNOx that is produced. An analysis was conducted using 10 x 10 pixels, but it was for the event #191 east of Florida which appeared to be contaminated by pollution outflow. I would recommend some more sensitivity analyses of using more than one pixel (maybe much less than 10 x 10), but for pixels not affected by pollution.

p. 18271, line 2: include Ott et al. (2010) P $\sim$ 20 - 42 x 10ˆ25 molec/flash

p. 18275, line 15: The result of a much larger instantaneous DE at 10 AM is surprising. I would have anticipated a value smaller than the climatological DE due to poorer propagation of the VLF signals in the daytime. But maybe the fact that you are considering only relatively large flash rates (and perhaps relatively large peak currents) in the analysis more than compensates for this. Perhaps add some comments on this subject to this section.

p. 18280, line 22: I'm having trouble figuring out what is being said here. Please clarify.

p. 18282, Appendix A1: You could compare your DE in 2008 for the Costa Rica region (mostly ocean) with that ($\sim$22%) of Bucsela et al. (2010) for July/Aug 2007 (after algorithm upgrade). I think the comparison looks pretty good.

p. 18283, lines 15-16: I don't think you know this absolutely for certain. Could this result also suggest that the OTD/LIS climatological flash rates are biased low? There is a tremendous amount of processing that must go into creating these climatologies from very undersampled data. The NASA processing must include consideration of the DE of the OTD and LIS instruments and extrapolate from a very small actual view time. Maybe some comments on the uncertainty of the OTD/LIS climatology might be order somewhere in the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 18255, 2010.