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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 #1

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General Comments: The manuscript combines observations of lightning NO2 from SCIAMACHY with coincident lightning observations from the World-Wide Lightning Location Network (WWLLN). The results indicate that lightning NOx production can vary strongly between different thunderstorms and in some cases where significant lightning activity is indicated by the WWLLN, no enhancement in NO2 is evident from the satellite as would be expected based on literature estimates of production per flash. The analysis is thorough and well thought out, although subject to a number of uncertainties and limitations that are discussed by the authors. The article should be of great interest to the ACP community and I believe publication is warranted though several areas could benefit from further investigation or explanation. The events investigated are limited regionally by 1) the detection efficiency of the WWLLN, which seems to be low over oceans, and 2) the presence of air pollution which could contaminate the

NO2 signal from lightning. Though the authors have used all available data, because of these limitations it is not truly a global study and I think that could be stated more clearly in the introduction and conclusion. It seems to me that a major issue is the use of climatological detection efficiencies applied to specific storms. As the authors note, lightning is a highly variable process and it is possible that DE varies strongly between different events in the same locations. The authors investigate this using LIS overpasses and find large differences, but this is difficult because LIS overpasses are so short in duration and lightning activity can vary strongly over the lifetime of a storm. I wonder if it would be possible to compare with ground-based networks which are available in the US and Europe. I understand that for most of these locations, the levels of pollution are too high to be considered for the PE analysis, but it may give some information into how reliable the climatological DE estimates are.

Specific comments:

p. 18259, L13 – Add a sentence defining what an air mass factor is. I think this would be unclear to readers without a remote sensing background.

p. 18261, L10-11 – I think more detail on the determination of the stratospheric fraction is necessary. TSCDs are likely to be strongly dependent on the estimate of the stratospheric component and small errors could have a considerable impact on the calculations. What type of uncertainty does this introduce into TSCDs?

p. 18261, L24-26 – Values for the sensitivity, E, are calculated based on high resolution model simulations of events during the TOGA COARE/CEPEX period. The authors note that E is insensitive to the cloud optical thickness. Could E vary in different regions or different meteorological conditions? If not, why not?

p.18264, L13-15 – I think it would be useful to move some of the material, including plots, from Appendix A to this section to help the continuity and to show the regions where the analysis is possible.

p. 18265, Line 6 – How is the pollution mask defined? There are already a large number of figures so I hesitate to encourage adding another, but I think it would be helpful to show the mask, possibly overlaid on a global plot of NO2 column densities, since it ultimately limits what cases can be included in the analysis.

p.18271, Lines 17-22 – Why is this? Is it because there is more potential for pollution or aged LNOx contamination when larger regions are considered? Since the authors note that the size of the area considered is important in estimating PE, I think some more explanation could be helpful. What does PE look like if intermediate sized areas of 5x5 SCIAMACHY pixels are considered?

p.18273, L15 – The authors mention that NO2 profiles modified by convection are used. Where do these profiles come from and how strongly do they affect the calculated TSCDs? How variable might these profiles be between storms or at different points during the lifetime of a single storm? I think it would be helpful to add some of these details to Section 2.1.

p.18275, Paragraph 3 – This description is very vague. These differences in DE are quite large and while they may be offset by increases in production per flash, it would be good to try to estimate the potential magnitudes of these effects. Would it be possible to include a table similar to Table 2 for a few cases where some detailed estimates of the changes in PE and FRD are given?

p. 18280, L28 – Isn't the PE around the US likely influenced by pollution outflow or aged LNOx? If so, I'm not sure that a difference between subtropical and tropical lightning has been demonstrated.

Technical comments:

p. 18256, L3 – Change 'high' to 'large' or 'strong' to distinguish magnitude from altitude.

p. 18257, L18 – Change 'came up' to 'have become available'

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