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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Vertical profiles of lightning-produced NO_2 enhancements in the upper troposphere observed by OSIRIS" *by* C. E. Sioris et al.

Anonymous Referee #1

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The paper shows interesting observations of NO2 enhancements in the upper troposphere based on remote sensing results using the OSIRIS sensor on the ODIN satellite together with model results and further data. This is the first application of satellite based limb scattering to study upper tropospheric NO2. The OSIRIS data allow identifying also weak NO2 enhancements in the upper troposphere, which are difficult to detect with other existing nadir instruments. The study quantifies the mean altitude of lightning-induced NOx (LNOx) emission from lightning in thunderstorms and shows new findings in the horizontal distribution of LNOx. The study suggests that the LNOx source rate in the upper troposphere is larger than in the model used, which may be a consequence of the difficulty to represent the vertical emission profile or in the absolute amount of LNOx emitted globally. These results should be published soon.

This reviewer is not an expert in remote sensing of trace gases. Hence, I cannot comment on these aspects in detail. However, the results look generally consistent and the paper appears to be well done.

However, before this study gets published, I recommend various minor changes:

1. My main concern is the interpretation of the data with limited view to lightning only. I agree that the model supports their view, but the model contains only an approximate representation of surface emissions and convective transport and hence may overestimate the lightning contribution. - I think, the authors should not fully exclude that some of the NO2 enhancements come for surface sources transported upwards within deep convective events.

2. For example, the fact, that fewer upper tropospheric NO2 enhancements were found in the Pacific and Indian Oceans indicate that part of the NO2 enhancements are caused by surface emissions. Also, the authors note large enhancements, possibly from lightning, in the outflow into the western north Atlantic in early August (page 5026, l 10) which they cannot fully explain. I think, the authors cannot exclude that large parts of these enhancements come for the surface. I ask that this possibility is stated more clearly in the text including the conclusions and the abstract.

3. My second concern has to do with the analysis of the observations and the conversion of observed NO2 values into values at other times. The sensor observes the sunlight scattered within the atmosphere towards the sensor. I expect that the signal is strongly sensitive to the presence of clouds and to the assumed NO2 and O3 profiles. I also wonder how important the diurnal gradients are that exist between the near and far sides of the limb near twilight. In particular, I wonder whether the scaling with the photochemical box model form 6:00 LT to 10:30 LT is really sufficiently insensitive to the cloud albedo.

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4. On top of page 5020, the authors discuss the impact of errors in ignoring cloud albedo. For their statements they just cite previous studies. I am not convinced about these statements. It would be better to show explicitly that the results do not depend on cloud albedo. The authors may consider showing a case study with albedo values varying within reasonable limits.

5. The authors compare observations for the period May 2003-May 2005 with simulations driven with assimilated meteorology for the year 2000. This makes a direct comparison of observed and modelled results less certain. Better would be simulations performed for the time period of the observations. At least this problem should be discussed.

6. On page 5020, lines 15 ff, the authors discuss NO2 enhancements found in the Saharan desert region over Libya, Egypt and Chad (Fig. 3). I suggest that the authors use a trajectory analysis to identify from where these enhancements could be from.

7. Page 5027, line 13: I suggest inserting “large” before “gaps”.

8. In discussing Fig. 3, the authors note that “many upper tropospheric (UT) NO2 enhancements lie in topical Africa”. It would be good to see a plot of occurrence frequency versus latitude to quantify the “many”.

9. Figure 6: As an interesting check on the model’s capability to simulate lightning induced NOx enhancements, I would be interesting to see a similar distribution based on the GOES model results? If the model is good in reproducing this distribution it would be a strong support for the model’s accuracy. If not (what I expect), the result would show the readers the still existing limitations of such model analysis.

10. Figure 7a. The geographic coordinates are not well readable. I have difficulties to find the coordinates (at 23.936_ S, 72.074_ E) of the lightning spot mentioned in the text. (By the way, these coordinates are given with too many digits.) At present, the axis notation uses letters which are far too small and difficult to find because of the

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dark background.

11. I would have preferred to have used SI units instead of the non-SI units “pptv” etc. You hardly measure volume ratios (rather molecular density ratios), and the term “billion” has different meanings in the English and American languages. Also the unit “Tg N/year” does not conform with SI standards. This standard recommends to use Tg/a or Tg a-1, and the reference to Nitrogen mass should be part of the text. The term pptv is explained in the text, but ppbv (legend of Fig. 3) is not.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 5013, 2007.

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