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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 #2

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This interesting paper searches for upper tropospheric lightning NO2 enhancements and indeed finds them. The search technique is discussed in detail, and appears to be sound. Combination of the OSIRIS observations with other data sets provides some useful indications of difficulties in the current understanding of lightning NOx production. But the paper does not provide new insights in the global lightning NOx source strength, because an in-depth investigation of model results and model difficulties (e.g. vertical distribution, source strength, IC:CG ratio) is lacking. The value of the paper is therefore mainly the observational evidence that lightning NO2 can be detected by OSIRIS in the upper troposphere, with a potential for constraining the vertical distribution of lightning NOx to some extent (namely in the upper troposphere). The case

studies are nice. I am impressed by 6.1 and 6.2, where the authors claim, using additional information as side evidence, that OSIRIS is able to capture the evolution of a lightning plume.

On the other hand, the paper is sloppy in many places. Definitions are often missing. Occasionally, referencing is poor. At some points, the paper is lacking clarity. Especially section 2 (Method) is difficult to read. For instance, on P5017, the bullets 1 and 2 indicate criteria for a first data selection. Then, a 'first step' (P5017, L14) is taken to carry out a cloud presence check, which requires two conditions inconveniently called '1)' and '2)' (P5017, L22 and further). This is followed the authors branching out to interpretation of cloud observation (P5018, L12-20). Only on P5018, L21, we come upon the 'second step' -altitude registration- that is treated in depth. Subsequently, on P5020, we find two new bullets labelled '1' and '2' that are brought up to explain how retrieved profiles are examined for NO2 enhancements. In summary, the authors try to clarify a long chain of retrieval steps by repeatedly (4 times) using the numbers 1 and 2, while at the same time trying to twist in some interpretation of observations (clouds), and all this is not improving clarity or readability.

I am surprised that the authors did not use the differences between the OSIRIS measurements at 06:00 and 18:00 LT. Lightning activity is strongest in the late afternoon over land, and one would expect that the 18:00 LT observations reveal higher NO2 enhancements than at 06:00 LT.

This paper should certainly be published, but I feel that it needs to be cleaned up considerably to improve clarity.

Specific comments

P5015, line 12-13: at least in this study there seems to be little justification for the claim that 'global coverage' is provided by limb scattering techniques. Are other studies available that pose better examples?

ACPD

7, S1457–S1462, 2007

P5016, L7-8: what is meant by “OSIRIS can observe approximately the same volume of air in the summer hemisphere within 12 h”? Is this based on OSIRIS taking measurements at 06:00 and 18:00 LT? It is not clear to me.

P5016, L18-22: these lines are not very interesting. Could they be shortened? As a matter of fact the complete first paragraph (until P5017, line 1) of the Method-section holds little useful information.

P5017, L25: please define ‘radiance scale height’.

P5018, L10: please give a reference to support “where the Junge layer would be detected”.

P5018, L28: please define tangent height as TH before using it.

P 5019, L2: please explain what a pointing offset is, and how it is determined.

P5020, L11-14: How/how much is this different from P5017, L8-12?

P5020, L13: slant column density should be defined.

P5021, L1-8: it would be interesting to mention how often these enhancements occur in Spring. What climatology was used? The sentence “Further analysis ... the troposphere” seems redundant.

P5021, section 3: why do the authors spent so much (a complete section) on measurement biases? As pointed out in the introduction, the Odin orbit is suitable for a lightning NO2 detection, which is what the authors state they are after. My feeling is that measurement biases would be important if the authors wanted to create a LNO2 climatology from the OSIRIS observations.

P5022, L11: why is meteo for the year 2000 used? Surely 2003-2005 is available.

P5022, L13: there seems to be a typo (line break) after “includes a”

P5022, L15-18: please give some more detail on the scaling to reproduce mean flash

ACPD

7, S1457–S1462, 2007

Interactive Comment

rates. Is this done regionally? Is the scaling the same for the two years with different lightning? What is meant by 'the timing of emissions of lightning NOx'? If the timing relates to meteorological variables such as cloud height, how does the scaling affect the fate of the LNO2 (downdrafts, rainout, etc.)?

P5023, L10-12: the Choi et al.-reference does not provide an observed range "of tropospheric NO2 enhancements due to lightning", but only a simulation with and without lightning that suggests the $<$2.5 1015 molec/cm2 enhancement. This should be removed or rephrased.

P5023, L16-17: why does neither problem appear major? Because Figures 4a and 4b look alike?

P5023, L19-21: Figure 4c represents OSIRIS at 10:30 LT, not GEOS-Chem. Should it be 4d-4e as in line 25?

P5023, L26-27: can you explain why Figure 4e shows that at 12 km, most of the NO2 is from lightning? Is it because the spatial patterns of GEOS-Chem LNO2 and OSIRIS NO2 are correlated? If so, please provide quantitative information.

P5024, L10-12: if OSIRIS observations are higher than GEOS-Chem at 12 km, this may also imply that the vertical distribution of LNO2 in the model is incorrect. Could the authors comment on this?

P5026, L5-6: what is meant by "the altitude of 12 km chosen from the model simulations"? This raises the question of how representative the model layer is for the OSIRIS observations shown in Figure 4.

P5026, L8-10: bringing up consistency with Choi et al. for the early spring enhancements isn't very convincing. Choi et al. found 2-3 days with enhanced simulated LNO2 columns over a small part of the Western Atlantic. Either remove or replace 'found' by 'simulated'.

P5026, L25: please define "upper tropospheric column enhancements". Are these the

ACPD
7, S1457–S1462, 2007

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enhancements in one retrieval layer only? Does Figure 6 consist of both 06:00 and 18:00 observations, or do they represent a 10:30 intermediate?

P5026-5027: the statement that 'a large fraction of lightning NO2 enhancements will be difficult to detect with the current generation of satellite nadir instruments' is too pessimistic and therefore misleading. There is evidence in the peer-reviewed literature that GOME and SCIAMACHY are able to observe LNO2. Especially if used in a statistical sense (GOME, SCIAMACHY and especially OMI and GOME-2) the nadir instruments have a tremendous capacity to overcome single-pixel precision limits mentioned by the authors, and in fact their presenting single-day SCIAMACHY and OMI NO2 data in Figures 7 & 8 goes against their statement.

With respect to the latter, I think earlier work done on lightning NO2 observations from nadir instruments receives too little attention in this paper. Similar numbers as given in Fig. 6 are published in the literature, and the manuscript would benefit if the range presented in Fig. 6 is compared to estimates by Beirle et al. (2004, 2006), Boersma et al. (2005).

Furthermore, a comparison to the vertical distribution of LNO2 from the model would be most interesting. There are also aircraft observations of lightning NO2 in the upper troposphere (these are sometimes the same as used in the model). Have the authors looked into these (and whether they extend to above 12 km)?

P5027, L21-22: what is meant by "This trajectory puts ... closer to the line of sight of OSIRIS"?

P5027, L7: there seems to be a misplaced ')' after nadir.

P5027, L10: what is the basis for the factor ˜4? Has this been simulated with GEOS-Chem?

P5029, 26-27: why aren't the LIS-flashes shown here? I think Figure 9 needs to be cleaned up. The HNO3 profile does not add useful information, since no data below

ACPD

7, S1457–S1462, 2007

the 13.8 km maximum altitude is available (so how do you know it is maximum?), and I think it should be removed. This would leave just NO and NO2 profiles at sunset, something that should be mentioned in 6.3, not just in the figure caption. The x-axis suggests really small concentration (in 1e-9 pptv).

P5031, L2-3: the GEOS-Chem low bias of 6-7 ppt seems to be mentioned here for the first time.

P5031, L10-19: I did not get this from the text - do the authors refer to a particular figure? If so, 'bands' are hard to distinguish given the scarcity of useful observations.

P5036, Table 1. How are the first and third entries different?

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