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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of satellite observations for freshly produced lightning NO_x" *by* S. Beirle et al.

S. Beirle et al.

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We thank the reviewer for the positive feedback, constructive remarks and suggestions. We reply to the raised issues point by point:

1) Notation a) In the notation of the formulas in the first part of the chapter, I feel that all quantities related to NO_2 or NO_x should have the superscript NO_2/NO_x. This has been done for most quantities but not for the vertical profile p which in the different formulas has different meanings.

Reply: We tried to find a compromise of using superscripts indicating relations to NO_x or NO_2 without having an overkill of indices. Thus, in some cases, where the relation should be clear from the context (e.g. V^0 in eq. 10) we omitted NO_2/NO_x to maintain readability. The profile p, however, always refers to NO_x as noted in line 11 on page

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18116 (except for the introducing equations 2 and 3, where it refers to a “trace gas” in general), and p^L refers to LNO_x accordingly.

b) Also, no difference is made between definitions (e.g. equation 1 in my opinion is the definition of A := S / V or eq. 8 or eq. 11) and equations. I suggest to use the notation := for all definitions (as is already the case in eqs. 5 and 6).

Reply: We adopted the notation := for all definitions.

c) In eq. 12, quantities for lightning NO_x are defined. While I understand the motivation to do this, I think it is an artificial mix of two partly independent effects - the radiative effect of the cloud and the change in vertical distribution of NO_x resulting from lightning. For the measurements, the combined effect is relevant but for individual cases, the two components can change independently. As a result, the factors EL are very specific to the modelled pixels and much less general as the separated effects.

Reply: We agree that for individual cases the effects of lightning on NO_x concentration (due to LNO_x production), profile (due to convection), and visibility (due to clouds) can result in various responds to the satellite measurements. However, if satellite measurements shall be used to estimate the amount of produced LNO_x, one has to separate these effects. It is shown in Appendix A, that the response in the slant NO_2 column (ΔS^{NO_2}) is approx. the slant column of lightning NO_2 (S^{LNO_2}), IF background NO_x can be neglected (eq. A4). As noted in the last paragraph on page 18118, our resulting sensitivities are thus only appropriate for lightning over “clean” regions. In cases where background NO_x can NOT be neglected, the response to ΔS^{NO_2} can even be negative (eq. A3).

2) Methodology:

a) The authors include a detailed discussion of the uncertainties of their estimates, but the general conclusion is that their numbers for the sensitivity should be representative and surprisingly constant. Considering the large degree of parameterisation for

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flashes, their vertical distribution and the NO_x injection, I would be more sceptical about how representative the modelled NO_x profile really is. As long as we don8217;t have any direct validation for the vertical distribution of NO_x in thunderstorms, model results should be considered to be uncertain.

Reply: Models reduce the complexity of reality to (simple) parametrisations that are naturally uncertain to some extent. In particular, the frequency as well as LNO_x production efficiency of IC and CG flashes is still quite uncertain (see also reply to Reviewer 1).

However, we tried to assess the dominating uncertainties of the model and their implications on our study, and found our resulting sensitivities to be quite robust as a consequence of compensating effects (a greater lofting of LNO_x would increase the AMF, but decrease the NO_2/NO_x, so that the change in sensitivity is dampened). Of course, our study discusses only one thunderstorm simulation run and will be extended to simulations of other thunderstorms, probably also involving different CRMs.

We revised the discussion of model uncertainties (section 4.1.1).

b) I was also surprised by the use of approximation 14 in the RTM simulations. The big advantage of this study is the availability of detailed information on cloud properties for the radiative transfer calculations, and I don8217;t understand why such a simple (and necessarily rough) approximation is used.

Reply: Unfortunately, extinction coefficients were not stored during the CSRMC run. Re-running the 3-D model would have required several weeks of computation time on a 16 processor Linux cluster. The parametrization in eq. 14 proved to be a simple and robust method to derive extinction coefficients. For future studies, we will take care to get extinction coefficients directly from the model. However, as checked by modifying the extinction coefficients by a factor of 0.5/2, our results are robust (see table 1).

c) For the calculation of box-AMFs, only scenes are used with more than 50 flashes. In

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my opinion, this artificially limits the scenes to instantaneous observations of lightning NO_x while with the life time of NO_x, even scenes with no flashes in the last hour will contain enhanced lightning NO_x (and therefore contribute to satellite observations). I8217;d therefore suggest using only a NO_2 column threshold but no flash criterion.

Reply: We agree that it would be interesting to investigate the sensitivity for "aged" LNO_x. However, due to the limited model domain of 238x238 km^2 and wind speeds of about 5 m/s up to 30 m/s in the free troposphere, it is not possible to track the aged LNO_x over several hours.

d) Most of the data evaluation in the manuscript is concerned with AMFs or sensitivities. However, the relevant quantity for data interpretation is the vertical NO_x column. Therefore, it is necessary to test if the sensitivities E are independent of NO_x column to ensure that there is no correlation which could lead to a bias. For this, a figure similar to Fig. 6 should be shown with NO_x columns as x-axis.

Reply: We adopt the reviewers suggestion and add a second panel to fig. 6, showing a scatterplot of E versus $VLNO_x$. Both quantities are not correlated (see 18126, line 16). The scatterplot indicates a minimum of sensitivity for moderate LNO_x levels (5e14 molec/cm^2), corresponding to the minimum at medium COTs (at about 20).

e) To make the study more relevant to applications, one would be interested to know what the spread in sensitivities for e.g. SCIAMACHY observations is. Therefore, it would be nice to have the analogue of Fig. 5 but on a resolution of 30x30 km2.

Reply: We considered this suggestion, and took a closer look on the frequency distribution (FD) of sensitivities on 30x30 km^2 resolution. However, for applications, where spatially integrated flash counts are related to spatial means of NO_2 columns, the "total", i.e. spatial mean, sensitivity as defined in Eq. 15 is appropriate. Hence, we would also have to add (and discuss) the FD of total sensitivities to Fig. 5. Furthermore, to make things even more complex, we should then also add the FD of total sensitivities from the sensitivities on SCIAMACHY resolution...

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After checking the different FDs, we decided not to add them to Fig. 5, since they are a) all quite similar, b) provide no real new insights, and c) might confuse more than help. Instead, we give the numbers for mean and std for all the different sensitivity sets in the manuscript, which sufficiently indicate the small differences of the respective FDs.

3) Technical corrections

a) Abstract, line 2: why i.e.?

Reply: We revised this sentence into “This is a particular challenge due to the complex and highly variable conditions of meteorology, (photo-) chemistry, and radiative transfer in and around cumulonimbus clouds.”

b) Abstract, line 15 However instead of But

Reply: done.

c) Section 2.1. SCD and VCD has already been introduced before

Reply: Section 2.1 is meant to develop the complete formalism used in this study. We think that some redundancy in this chapter could be helpful for fluent reading.

d) Section 2.1. vertical variation of sensitivity also depends on absorption, at least if strong absorbers are present in the wavelength region

Reply: We added the aspect of absorption to the general paragraph in section 2.1. However, for the wavelength considered in this study (440 nm), there are no strong absorbers present in the atmosphere.

e) Section 2.1. proportionality between partial columns and concentration only holds if pressure is assumed to be constant in layer

Reply: We revised this sentence to “Note that the partial column of a layer can directly be calculated from the respective mean layer concentration multiplied by the layer height, and the respective profiles are proportional to each other for equidistant

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layers."

f) page 18118, line 8: practice, not practise

Reply: done.

g) page 18120, line 20: "is an improved version of" instead of "is an advancement"

Reply: We changed "advancement" into "further development".

h) page 18125: inverted C-shape

Reply: done.

i) page 18125 and elsewhere: why Regimes, not regimes?

Reply: all "Regimes" are changed to "regimes".

j) page 18128, line 5: it should be noted that this intensity weighting approximation is not strictly correct as slant columns are computed from the logarithm of the intensities and can not simply be weighted by intensity.

Reply: The reviewer is right, but since ln(1-x) can be approximated by -x for x«1, and since the optical thickness of atmospheric NO_2 is generally small, the SCD can be regarded to be linear with intensity reduction.

k) page 18130, line 8: the advertisement pitch in the description of McArtim seems a bit out of place here.

Reply: We would like to point out that, despite of the large number of RTMs, not many of them would have been suited for out study. McArtim provides box-AMFs, and is very flexibel with respect to the modelling of RT in and around clouds - even more as soon as 3D clouds are implemented.

l) page 18130 "scattered light" instead of "stray light"

Reply: done.

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m) page 18134: typo SCIAMCHY

Reply: corrected.

n) page 18134: replace “min” by “minutes”

Reply: done.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18111, 2008.

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