

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

Anonymous Referee #2

Received and published: 16 December 2008

The paper "Sensitivity of satellite observations for freshly produced lightning NO_x" by S. Beirle et al. reports on a study investigating the sensitivity of UV/vis satellite measurements of NO₂ over thunderstorms to lightning produced NO_x. For their study, they use results from a cloud resolving model including chemistry in combination with a radiative transfer model to simulate the complete system from NO_x injection to observed NO₂ slant column for a large number of individual 2 x 2 km² pixels. Their main result is that the sensitivity of the satellite measurements is smaller than assumed in previous studies and is surprisingly constant considering the large variability in cloud optical properties and NO_x concentrations modelled.

The paper reports on an interesting and detailed sensitivity study which is very relevant for attempts to reduce the large uncertainties on current lightning NO_x estimates using

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



satellite observations. The manuscript is well written, clearly structured and methodically sound. I therefore recommend it for publication in ACP subject to minor revisions as suggested below.

1) Notation

In the notation of the formulas in the first part of the chapter, I feel that all quantities related to NO₂ or NO_x should have the superscript NO₂/NO_x. This has been done for most quantities but not for the vertical profile p which in the different formulas has different meanings.

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).

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 E^L are very specific to the modelled pixels and much less general as the separated effects.

2) Methodology:

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 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 don't have any direct validation for the vertical distribution of NO_x in thunderstorms, model results should be considered to be uncertain.

I was also surprised by the use of approximation 14 in the RTM simulations. The big

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



advantage of this study is the availability of detailed information on cloud properties for the radiative transfer calculations, and I don't understand why such a simple (and necessarily rough) approximation is used.

For the calculation of box-AMFs, only scenes are used with more than 50 flashes. In 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). I'd therefore suggest using only a NO₂ column threshold but no flash criterion.

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.

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.

3) Technical corrections

Abstract, line 2: why i.e.?

Abstract, line 15 However instead of But

Section 2.1. SCD and VCD has already been introduced before

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

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

page 18118, line 8: practice, not practise

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

page 18125: inverted C-shape

page 18125 and elsewhere: why Regimes, not regimes?

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.

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

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

page 18134: typo SCIAMCHY

page 18134: replace "min" by "minutes"

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

