Atmos. Chem. Phys. Discuss., 4, S936–S937, 2004
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Atmospheric Chemistry and Physics Discussions

ACPD
4, S936–S937, 2004

Interactive Comment

Interactive comment on "Sudden increases in the NO_2 column caused by thunderstorms: a case study in the northern subtropical region" *by* M. Gil et al.

Anonymous Referee #3

Received and published: 16 June 2004

The authors have described a case study relating short-lived increases in the NO2 slant column above Izaña to upwind thunderstorm activity. The arguments supporting this interpretation are not sufficiently compelling to warrant publication in their present form.

Key to the interpretation is the conclusion that the observed NO2 increase occurs in the upper troposphere where lightning-produced NO2 would be carried by the anvil outflow. This conclusion is inferred from comparisons between the solar zenith angle (sza) dependence of the ratio of the slant columns measured during the event to that measured on the previous day, and the ratio of airmass factors calculated assuming a standard NO2 profile with a maximum near 28 km, and (presumably) the same profile

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with an enhanced layer added at lower altitudes. Both the measured and calculated ratios peak near sza=90° if an enhanced layer near 10-12 km is assumed and this information is used to support the main conclusion of the paper. However, the comparison is far from perfect, and the arguments not entirely convincing. The large differences are more readily apparent if the data from Fig. 6 are plotted in Fig. 7 (as they should be). In particular, while the calculations show that the addition of low altitude layer should decrease the total airmass factor, the observations show that the slant column is increased for all sza values. Why? Could this be related to the assumed backgrounds? Are they the same for both days? This information is not provided, but should be discussed. Furthermore, the shape of the measured ratio for szas less than 85° are totally different from the calculations. These differences are not satisfactorily explained, but simply dismissed as an indication that the vertical column must be changing during the earlier measurements. This argument needs additional support. Can the assumption of reasonable changes better reproduce the observations and strengthen the general interpretation? The authors note that the troposphere is much wetter during the ŞeventŤ and devote two figures to this observation, but do not further use this information. Water vapor absorbs strongly at 442 nm or near where the NO2 absorption is (presumably) measured. How does this influence the airmass factors? Can this information be used to produce calculations more relevant to the observations? If this information is truly irrelevant the figures could be deleted. In summary, the agreement between the observations and calculations presented here is not sufficient to convince me of the overall interpretation of the measurements, and certainly not adequate to quantify the amount of the increase due to lightning.

A few corrections in the reference list.

The author list for reference Sarkissian et al. 1995 is incorrect.

The lead author's name is mispelled in Langford et al. 1996

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2263, 2004.

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