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Comment

Interactive comment on “NO₂ seasonal evolution in the North Subtropical free troposphere” by M. Gil-Ojeda et al.

Anonymous Referee #1

Received and published: 18 June 2015

This paper presents a three-year data set of NO₂ volume mixing ratios (vmr) derived by applying the Modified Geometrical Approximation (MGA) to MAX-DOAS observations at the high mountain observatory of Izaña (Tenerife Island). The NO₂ vmr at the level of the station (2370m asl) is obtained by dividing the NO₂ differential slant column density (DSCD) measured in the horizontal viewing geometry - using the zenith elevation angle as reference - by the horizontal optical path estimated from the corresponding oxygen collision complex O₄ DSCD. These NO₂ vmr observations are compared to parallel in-situ measurements. The level of agreement between both techniques is seen to strongly depend on the three main wind regimes observed at the station (South wind/Upslope breeze/weak or no breeze). Back-trajectory calculations and a chemistry-climate model are used to interpret these features.

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General comments:

The Gil et al.'s paper presents interesting results and is reasonably well written. The scientific quality of the paper has been improved by adding a Section on the Optimal Estimation Method. However, a lot of questions remain unanswered regarding this method: On which criteria did you decide not to include aerosols in the OEM retrieval ? Did you perform sensitivity tests on this ? What is the impact of the apriori profile on your retrieved profiles, especially on the vertical extension of the NO₂ layer ? A brief description on the main sources of uncertainty of the OEM is still missing. All these points should be discussed in the revised manuscript.

Specific comments:

Page 14479, lines 15-29: The Thalman and Volkamer (2013) O₄ cross-sections are used in the test on the impact of the temperature dependence of the O₄ cross-sections. Since this cross-sections data set is now considered by the DOAS community as the reference O₄ cross-sections, what would be the impact of using them on the MGA approach results instead of Hermans et al. (1999) ?

Page 14482, line 9: a reference should be added for the Gaussian correlation functions. Why did you choose a correlation length of 300m ? What is the impact of this correlation length on the vertical extension of the NO₂ profiles presented in Fig. 3 ?

Technical corrections:

Page 14478, line 5: 'specie' -> 'species'

Page 14500, Fig. 4: The y-axis legends in the three plots should be identical, e.g. 'NO₂ concentration (pptv)'.

Page 14485, line 1: You should refer to Sect. 4 instead of Rodgers et al. (2000).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14473, 2015.

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