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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Nitrogen dioxide stratospheric column at the subtropical NDACC station of Izaña from DOAS, FTIR and satellite instrumentation" *by* Cristina Robles-Gonzalez et al.

**Anonymous Referee #2**

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This paper presents a 13-year (2000-2012) comparison study of the stratospheric NO2 vertical column density derived from ground-based DOAS and FTIR as well as satellite nadir (OMI, SCIAMACHY) observations over the NDACC station of Izana (Canary island). The differences between the instruments in terms of spatial representativeness and vertical sensitivity are discussed and taken into account in the comparisons. The difference in measurement time, which can have a significant impact in the comparison results in the case of a rapidly photolysing species such as NO2, is corrected by using box-model simulations. The paper shows the importance of considering the effective solar zenith angle of the DOAS observations when comparing them to direct-sun FTIR



and satellite nadir measurements. A first trend analysis performed on all time-series shows an increase of the stratospheric NO2 vertical column but larger trend values are obtained for ground-based instruments than for satellites. Possible reasons for positive trend values and discrepancy between instruments are discussed.

The paper of Robles-Gonzalez et al. is clearly structured and the method and results are generally presented and discussed in an appropriate and balanced way. Therefore I recommend the paper for publication in AMT after addressing the following specific comments:

Specific comments:

Trend analysis: To my opinion, there are several issues with the trend analysis. A first point is that there were instrument changes during the 2000-2012 period for both FTIR and zenith-sky DOAS. Did you investigate the possibility to have a bias in the corresponding NO2 vertical column time-series due to these instrument changes ? If not, this should be done and if there is a bias for one or both techniques, then its impact on the trend analysis should be assessed. A second point is that applying a linear regression for the trend analysis is maybe too simplistic for a time-period of 13 years. I think it would be useful to include the solar cycle and QBO in the calculation. This would also help to compare the derived values with other published studies since the latter take usually these effects into account.

Comparison DOAS/FTIR: it is found that AM values compare better than PM ones. A possible reason for that would be the contamination of afternoon FTIR measurements by the upwelling of high NO2 boundary layer airmasses. Maybe this effect could be quantified for some selected days.

The maximum of NO2 vertical column is observed in June for satellite and in July for ground-based instruments. Did you investigate the impact of the temperature dependence of the NO2 cross sections on these results ? Do you obtain similar results without applying any photochemical correction on the different data sets ?

Technical corrections:

The overall quality of the English is poor throughout the manuscript. Maybe the authors should think to polish the text with the help of an English native colleague.

Some other technical corrections:

*Page 2, lines 70-71: ‘GB DOAS’ instead of ‘DOAS GB’ *Page 2, line 74-75: you should add units to the difference values (molec/cm2) *Page 4, line 138-140: ‘The air mass factor (AMF) used for the conversion of NO2 slant columns to vertical columns are the NDACC NO2 standard AMF available on the NDACC UV-vis web page (http://ndacc-uvvis-wg.aeronomie.be/tools.php) and based on the Lambert et al. (1999) harmonic climatology of NO2 profiles’ *Page 7, line 248: ‘the DOAS effective airmass’ -> ‘the location of the DOAS effective airmass’ *Page 8, section7: replace ‘diagonal’ by ‘1:1 line’. *Page 17, Table 1: ‘BIRA-IASB NO2 AMF LUTs’ -> ‘NDACC NO2 AMF LUTs’ *Page 18, Figure 1: units for NO2 concentration should be molec/cm3 and not molec/cm2.

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