

Interactive comment on “Intercomparison of daytime stratospheric NO₂ satellite retrievals and model simulations” by M. Belmonte Rivas et al.

Anonymous Referee #1

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This is an interesting paper that evaluates the agreement between stratospheric NO₂ retrievals from limb sounders (SCIAMACHY, MIPAS, and HIRDLS) and nadir sensors (OMI, SCIAMACHY) across the seasons over the 2005–2007 period. Satellite data are also compared to the 3-D chemical transport models SD-WACCM and TM4. The main conclusions of this work are: (1) A good agreement (better than 10%) is found between limb measurement records, making these data sets suitable for the definition of a reference for stratospheric NO₂ columns. (2) The WACCM model overestimates NO₂ densities in the extratropical lower stratosphere, particularly over northern latitudes by up to 35% relative to limb observations. (3) There are remarkable discrepancies between stratospheric NO₂ column estimates from limb and nadir sensors: it is shown that SCIAMACHY nadir and OMI have overall biases of –20 % and +20 % relative to limb

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observations, respectively. The authors highlighted the fact that these biases in nadir stratospheric columns are not expected to affect tropospheric retrievals significantly, and that they can be attributed to errors related to algorithmic or instrumental effects in the total slant column density retrievals. This study appeals to an assessment of the current Differential Optical Absorption Spectroscopy (DOAS) approaches/settings used in the existing nadir slant column retrievals.

Being well written and clearly structured, I recommend this paper for publication in AMT after addressing the following comments:

Specific comments:

1/An important point is the large bias (30-50%) between OMI (KNMI DOMINO v2.0) and SCIAMACHY nadir (KNMI-BIRA TM4NO2A v2.3) retrievals, especially if both data sets are photochemically converted to the same local solar time (HIRDLS LST), removing by this way the effect of the NO₂ diurnal variation. It is puzzling me because Dirksen et al. (2011) and Hendrick et al. (2012) found a good agreement between OMI, SCIAMACHY nadir and ground-based observations, suggesting the absence of such a large bias between OMI and SCIAMACHY. Can this feature be attributed to the change in the OMI version (DOMINO v0.8 in Dirksen et al. (2011) and DOMINO v2.0 in the present study) ? Also, you mention the fact that the offset in the new OMI NO₂ stratospheric columns is due to wavelength calibration, liquid water, and O₄ contributions issues. I think it would be interesting to illustrate this statement by adding some plots in the manuscript or at least give more details on the sensitivity tests performed.

2/You used only TM4 model results obtained after assimilation of the OMI NO₂ total columns. Since you don't perform any assimilation in the case of the SD-WACCM model, it would be interesting for the reader to add comparisons with TM4 model results obtained without assimilation, in order to determine/quantify the impact of the latter on the agreement with satellite data and to see the real performances of both models.

3/Since aerosols and especially sulphate aerosols can significantly affect the NO₂

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abundance in the stratosphere, it would be interesting to know how aerosols are initialized/implemented in both models. So, please add a few sentences on this in the description of the models (pages 907-908).

4/The SD-WACCM model is used for comparison with satellite data but also for their photochemical conversion to the HIRDLS LST. Did you consider the risk of circular argument by doing this ?

5/Discrepancies (too low and too large NO₂ peaks, especially in the extratropics) are found between models and limb sensors. Is it expected and is it related to the chemistry, transport, or both ? Maybe it would be interesting to have feedback from modellers on these issues.

6/Page 906: Since you show comparison results up to 60°N and S, is the photochemical correction also accurate in case of denoxification ?

7/Page 910, lines 11-12: SCIAMACHY limb is larger than MIPAS by up to 30% around 30hPa in the tropical stratosphere. Any explanation(s) for this feature ?

8/It would be useful to have an overview about the structure of the paper at the end of the Introduction.

9/In Rodgers theory, A is used for the averaging kernels and K for the weighting functions. So, using K for expressing the averaging kernels as you did on page 903 can be confusing. I suggest to replace K by A.

10/Table 3: it would be interesting to have the differences also expressed in %.

Technical corrections

1/'Dirksen 2011' should be replaced by 'Dirksen et al. (2011)'. The same remark applies for almost all references, so please check the entire manuscript.

2/Page 915, line 8: 'Boersma, 2008' should be replaced by 'Boersma et al. (2008)'.

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3/Page 920: (Cook, 2009) is missing in the reference list.

4/Page 923, line 6: the publication years should appear between brackets.

5/Figs. 5, 6, and 7a are still difficult to read for me, especially due to the small fonts for the axis labels.

6/Legend of fig. 13, page 947: 'ff' in 'differences' is not correctly written.

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