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Interactive comment on "Validation of SCIAMACHY tropospheric NO_2-columns with AMAXDOAS measurements" *by* K.-P. Heue et al.

Anonymous Referee #1

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The authors present a comparison between SCIAMACHY tropospheric NO_2 column and collocated airborne measurements in the Mediterranean area focusing mainly on the Po-valley. Such kind of work is very important to validate satellite observations and the selected area is one of the most suitable in Europe for the high concentration of NO_2 due to natural and human activity observed there. The work is presented clearly and with appropriated scientific approach. Results show good agreement between measurements and this is encouraging for the use of satellite data in tropospheric monitoring and studies. However the paper would benefit from a more detailed analysis of some issues which are not discussed enough in the text. They are:

-Satellite observations are compared with similar measurements performed by an UV-

Vis spectrometer installed on board the FALCON aircraft and operating in the nadir geometry as well as SCIAMACHY. Thus the observations to be compared are very similar, as stated by the same authors, either in the geometric configuration or in the retrieval method. This means that the results of the comparison would give us indications on the proper functioning of instruments (they must observe similar amounts) but nothing about the accuracy of the measured tropospheric column of NO_2. In fact, as reported by the same authors, the sensitivity to the lowest (closer to the surface) atmospheric layers can be a critical point for such measurements and NO_2 tropospheric column can be underestimated. Is there available any comparison with independent measurements? Has the AMAXDOAS method been validated separately?

-Even if the linear regression between SCIAMACHY and AMAXDOAS collocated observations gives a good correlation (of the order of 0.9, figure 9) data are often scattered a lot with discrepancy of the order of 30% or more (neglecting the measurements performed over mountain regions). On page 7526 authors make the hypothesis that discrepancy may be due to the no-simultaneity between observations but nothing is said about the different field of view (FOV) of both instruments. Which effect the FOV has on the comparison? For example the SCIAMACHY observation around 27x10^{15}molec/cm^2 (figure 9) corresponds to an AMAXDOAS measurements in the range 17 - 27x10^{15}molec/cm^2 (that is very large!). Does this observation match the flying over a city? If this is the case such points must be dealt with care in the linear regression otherwise all the considerations on the correlation coefficient (that is, any possible “overestimation of SCIAMACHY”) and on the offset (that is, “for SCIAMACHY data the stratospheric NO_2 is overestimated”) may have no sense!

-On page 7521 authors fix the parameters for the calculation of the AMF. The NO_2 profile is a critical parameter and authors should motivate the choice made. A discussion also on the variation of AMF as a function of the input NO_2 profile could help the interpretation of results when the conversion from slant column to vertical column is performed.

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Interactive Comment

Furthermore, concerning some more technical aspects:

- On page 7523: the reference “(NERC, 2004)” has no match in the “References” section

- I suggest also an accurate reading of the paper to correct some typing errors.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7513, 2004.