

Interactive comment on “Stratospheric and tropospheric NO₂ variability on the diurnal and annual scale: a combined retrieval from ENVISAT/SCIAMACHY and solar FTIR at the Permanent Ground-Truthing Facility Zugspitze/Garmisch” by R. Sussmann et al.

Anonymous Referee #2

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General comments

The paper deals with the combined use of SCIAMACHY and ground-based FTIR NO₂ column observations to retrieve information on the free tropospheric amount of NO₂ and to validate satellite observations of NO₂ column. Both arguments are of great actual interest for the scientific community because of the few measurements

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of NO₂ in the free troposphere on a daily basis and because of the strong need to validate/integrate satellite observations with auxiliary measurements.

Nevertheless the approach followed by the authors is sometimes not clear and the conclusions on the validation part are actually too poor. It is mentioned in the paper (page 2380, row 13) that the main goal of the work is “validation and synergetic use ...” but actually the authors do not reach any significant results about the validation. In fact they report a large difference in total column comparison (page 2403 row 21) that is in large part due also to the very low FTIR sensitivity to troposphere (as widely discussed by the same authors) so that nothing can be argued on the accuracy of SCIAMACHY (total, in this case) column. The agreement in day to day scatter is not enough to justify the use of “validation” term in the targets of the paper. For the tropospheric background retrieval a comparison is performed only on a range of magnitude basis (paragraph 5.5).

On the other hand the two data set are integrated by using the respective averaging kernels taking thus into consideration the different instrumental sensitivity to the stratosphere and troposphere. This is actually an interesting approach because of the very different sensitivity of the instruments (being SCIAMACHY more sensitive to the troposphere than FTIR).

My suggestion is to neglect the validation part, unless a more accurate analysis on the comparison between homogenous observations is performed. This could be done, for instance, comparing the background tropospheric column retrieved with the new method proposed, with the collocated SCIAMACHY tropospheric column retrieved with the Richter and Burrows (2002) method. In fact the two methods are independent (because now the stratospheric background to be removed is that of FTIR and not that retrieved by SCIAMACHY over the Pacific Ocean) so that useful indications on self-consistency and/or validation of the new approach (/limitations of the old one) can be pointed out.

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Specific comments

-page 2385, row 8: such conclusion can not be pointed out by eye on the plot considering also that actually it is recognizable from the same plot some differences in the slopes. The authors spent a lot of words in the previous paragraphs on the error analysis and I think that here a detailed calculation of the increasing rate of the stratospheric NO₂ considering also error bars is mandatory also because of its importance from a scientific point of view. As stated by the same title of the paper this is one of the main point of the work and I think it deserves more accurate calculations.

-Figure 4 is not necessary.

-page 2390: the procedure explained in the Step 2 has not be discussed from a scientific point of view. Where does the factor 2 come from? Besides at page 2402 row 21 it is stated that this pollution-clearing scheme does not work well (because it keeps info on the PBL NO₂) and few rows below authors come to a conclusion that is exactly the opposite! A clarification is required.

-For the reasons explained above, paragraphs 4 and 5.5 have no-sense in their present form.

-In the text figure 12 is mentioned before figure 11. So please, invert the figure numbers.

-The paragraph 5.3 is maybe the most important of the paper explaining the retrieval of the background tropospheric column from the combined data sets but I think that the scientific approach is not clear enough in the present form. In fact the used parameterization has not been justify (or at least is not clear from the text) neither from a physical nor from a mathematical point of view. A better explanation would improve the comprehension.

-page 2404, row 17-19: it does not make sense. If authors think that an “horizontal map of tropospheric NO₂” is required to explain better some concepts/results they are

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strongly encouraged to add it.

Technical corrections

-page 2395, row 10: the reference (Scheel, 2005) is missing in the References paragraph.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 2377, 2005.

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5, S821–S824, 2005

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