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Interactive comment on "Comparisons between SCIAMACHY and ground-based FTIR data for total columns of CO, CH_4, CO_2 and N_2O" *by* B. Dils et al.

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We would like to thank the referee for the review and useful comments and believe that the paper will be considerably strengthened by them.

1. Validation of satellite retrievals such as those from SCIAMACHY needs to be performed with all available independent data to determine the accuracy of the retrievals and determine their limitation. The ground-based FTIR measurements used in this paper can provide a useful validation data set, but I have significant concerns about the method in which it was used. For CO in particular, I do not think the polynomial fit to the FTIR data is a valid extrapolation technique as CO can vary significantly on very short timescales due to the heterogeneity of sources and rapid transport times. This fitting and extrapolation is probably ok for the longer lived species. An example of the

polynomial fit and the original data is only given for CH4 at one site. I would need to see several examples of CO time series with fit to accept that this technique is valid. I think a better approach would be to use the individual FTIR measurements and explore the dependence of the bias on the distance and time from coincidence. For example, try using a criteria of 100 km distance and 12 hours in time, then expand that to 200 km and-or 24 hours, etc.

It is clear that the polynomial fitting procedure is not the ideal solution to the problem (not enough overlapping data points when performing a comparison with strict collocation criteria) but we feel that it is the best at hand. The referee suggests to overcome this problem by increasing the spatial collocation criteria or-and the temporal ones. This would indeed be a more valid option if the increases needed to obtain a sufficiently large set of collocated data remain relatively small. Unfortunately this is not the case. We feel that the spatial collocation criteria as used in the paper are already fairly loose. Furthermore, for short-lived species such as CO, the spatial variability is significant as well. As for increasing the temporal collocation criteria to the extent needed to create sufficient overlap, this will cause problems as well for the same reasons as stated by the referee. However, and this is important, it should be stressed that the 'satellite data scatter' around the polynomial fit includes the natural variability of the species involved and should always be assessed keeping in mind the 'FTIR date scatter' around the same polynomial. We have put more stress on this in the revised version of the paper. We have also included the FTIR scatter values for each product-station individually in tables 5 to 7. Finally we have included figures for the CO polynomial fit as well.

2. From the current analysis I find it difficult to determine on what scale the SCIAMACHY retrievals can be used. If the retrievals are used as monthly averages, would the scatter be reduced and the accuracy improved? Or, is gridded data on some scale (e.g., 5deg x 5deg) better? Would this improve the CH4 comparisons?

As to what scale the SCIAMACHY retrievals can be used, it is clear that the research performed here uncovers some substantial problems and that the criteria for perform-

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ing quantitative studies using individual SCIAMACHY data are not met yet. Scatter however is reduced by increasing the timescale as well as the spatial collocation area and it would be a useful enterprise to undertake a detailed study in order to find out at which time/space criteria-combinations the output becomes relevant and this for various data exploitation purposes, since these require different target precisions as well. However, given the limited dataset and the fact that the algorithms at hand are still developing, such an undertaking would lead us well beyond the scope of this article, which was to identify the strong and weak points of these various retrieval algorithms, in support of the algorithm developers and to inform the scientific community of the actual status.

3. Also, an assessment of the different retrieval methods would be valuable. Fig. 7 gives some indication of the differences resulting from them, but a discussion of why they are so different, particularly the trend in bias with concentration, would be useful.

For a precise characterisation of the different algorithms, and hence their differences, the reader is referred to the various articles concerning these different algorithms. The differences between them are highly variable and complex, involving both algorithm and parameter differences, and a discussion hereof is beyond the scope of this paper. The retrieval algorithm research groups continuously try to find the sources of the differences in the retrieval results, in order to improve their models, but it is a challenging task.

4. Specific Comments p.2680, l.6-8: The 1996 reference doesn't seem appropriate for the current state of MOPITT retrievals - perhaps getting a few words from the MOPITT PIs and citing them as personal communication would be more informative. Or leave out the comment about MOPITT CH4.

We have added a reference to the MOPITT Web site, which states that 'there are no plans to release a methane data product'.

5. Figure 1: This figure is redundant since the locations of all the sites are listed in

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Table 1.

The figure is complementary to the table and gives a far better visualisation of the FTIR network coverage.

6. p. 2682, l.4: It would be nice to include some mention of the cause of the seasonal cycle in CO (and also l.13-15 in the discussion of CH4), i.e., that it is a result of the lower OH concentrations in winter than in summer.

This modification for CO has been made in the revised version of the paper. However the causes of the seasonal variations in the other species are not so evident (various competing causes, namely different seasonalities in the sources and sinks, in dynamics, etc.) Since these causes have no further incidence on the discussions in the paper, we have not gone into a discussion of them.

7. Figure 2: It is extremely difficult to see much in these figures - the data should be averaged or grouped to make the desired point. Just plotting monthly means for each station would help a lot. If the focus is the latitude gradient then some sort of plot versus latitude would be better.

The purpose of the figure is to show the characteristics of the FTIR data: the amount of data and variability in the data, for the different stations individually, as well as the variations from station to station. While I agree that a lot of data is packed into one figure, monthly averages would no longer give the reader an impression of the above parameters.

8. p. 2683, l.16: I was confused here by the statement that there were few coincidences near the poles, since there is a high density of satellite tracks there. It is explained below, but a statement here explaining why there are limited satellite retrievals available in polar regions would help.

This explanation has now been given in the revised version of the paper: 'the amount of SCIAMACHY data points is limited, due to difficulties of cloud filter algorithms to

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distinguish between ice and clouds and to the high solar zenith angles over these regions leading to low signal to noise ratios, and thus larger errors in the retrieved total columns.'

9. p. 2687, l.10-15: Why not use distance in km to determine collocation, instead of degrees? This would take care of the changes in distance with latitude.

This was done for practical purposes since many geophysical phenomena are linked to latitude (solar zenith angle, etc.).

10. Figure 6: I'm not sure what the point of this plot is. If it is really worth showing the seasonal variation in bias, then perhaps that could be shown in a series of plots similar to Fig. 5, with a bias vs latitude plot for each season (DJF, MAM, JJA, SON). Or just show a few pertinent sites for one retrieval algorithm. Or if the only point is that the bias is a function of column amount, then this figure could be skipped, since that is shown in Figure 7.

The figure gives an overview of to what extent the algorithms accurately depict the seasonal variability, any systematic offset from the 0 bias baseline, points to a systematic deviation and thus problem in the algorithm. As such the plot is useful.

11. Figures 9 and 10 also seem like just a catalogue of the results. I find Figure 8 much easier to interpret.

Figures 9 and 10: same comment as for Figure 6

12. Technical Corrections

p. 2683, l.12: 'the dark period of local winter' could be replaced with 'polar night' p. 2683, l. 25: 'some percent' -> 'a few percent' p. 2687, l. 6: do you mean 'possess' instead of 'dispose of'? Tables 1 and 3: replace 'g-b' with 'ground-based' Tables 5,6,7: replace 'A','B','C' with more descriptive names, like 'Bias', 'N', '(scat'. And explain 'R' and 'P'. Figure 5: No need to be in 3-D.

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All suggested technical corrections as listed above are made in the revised version of the paper. However, the 3-D plot shows a better difference between no data available and a near zero bias than a 2-D plot would do.

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