

Interactive comment on “Stratospheric methane profiles from SCIAMACHY solar occultation measurements derived with onion peeling DOAS” by S. Noël et al.

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Reply to referee 2

We thank the referee for the helpful comments and will consider them in the revised version of the paper.

Answers to comments:

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1. *p 4804, l 8-10: Please provide a few more lines on the differences between the onion peeling retrieval methods applied here and in (Noel et al., 2010).*

The main differences to the previous method are the use of weighting functions instead of optical depths and the different (a-posteriori) saturation correction. We will add this in the paper.

2. *p 4807, l 25: convolved instead of convoluted?*

Will be changed.

3. *p 4809, l 25: how has the error threshold of 1 ppmv been determined? This seems quite a lot, certainly more than 100% relative error. Are there criteria available from independent quantities like χ^2 , or convergence criteria?*

The (indeed very high) value of 1 ppmv for the threshold is used to sort out major outliers, typically resulting from very specific measurement conditions, like the presence of very strong polar stratospheric clouds which block the solar signal. In these cases the calculated errors (which in fact are based on the fit residuals and are therefore related to χ^2) become very high, usually much larger than 100%. With a choice of 1 ppmv as a threshold these data are correctly filtered out. Note that – as mentioned in the paper – this filtering is not really relevant for the results as only two out of more than 33000 measurements are affected by this.

4. *p 4810, l 2: Why has the retrieval been set up on a constant 1-km grid, and not on a tangent height grid, as it would be natural for onion peeling approach? This would avoid the necessity to smooth in a post-processing step. Has it been verified that the mass (number of molecules) along the slant path is conserved by the smoothing step?*

The 1 km retrieval grid is used because otherwise it would be required to calculate the weighting functions individually for each measurement (as each measurement has a different tangent altitude grid) with the radiative transfer model

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(which is computationally quite expensive).

Mass conservation has not been considered explicitly in the smoothing process; a simple boxcar smoothing has been applied. As this is similar to an averaging procedure, the total mass should be conserved.

5. *p 4813, l 10: please clarify what the 'mean error of SCIAMACHY VMRs' is. The standard error of the mean? Or the averaged error of the single SCIAMACHY profiles?*

It is the averaged error of the single profiles. We will clarify this in the paper.

6. *p 4813, l 22 ff.: Since comparisons are presented in vmr, what contribution to the differences could come from differences in pressure and temperature between ACE-FTS and SCIAMACHY? Have you compared the pressure-temperature profiles of the correlative measurements? Further, which systematic error contribution could come from spectroscopic uncertainties in case of SCIAMACHY?*

For the calculation of SCIAMACHY VMRs pressure and temperature have been taken from ECMWF data. In order to estimate the impact of uncertainties resulting from pressure and temperature we compared number densities from SCIAMACHY with number densities from ACE-FTS, calculated using ACE-FTS pressure and temperature. The results are in fact very similar to the VMR comparisons.

Spectroscopic errors are difficult to quantify. In the radiative transfer calculation we use the latest HITRAN 2008 data, so we expect this error contribution to be small. These types of systematic errors have already been discussed in Noël et al., 2010. We will add a reference to this in the paper.

7. *p 4815, l 13: It is not necessarily mesospheric air which is observed in the stratosphere during polar winter (these vmrs should be much smaller), but just subsidence of the stratospheric air masses.*

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Agreed. We will change this in the paper.

8. *p 4815, l 15: Some more discussion on the variations during polar winter, particularly the sharp peaks which occur most pronouncedly at 30 and 35 km, would be appropriate.*

The sharp peaks are probably related to a sampling issue. At the days where the peaks occur (e.g. 11 December 2002, 2 December 2003, 22 November 2005, 18 February 2007) only very few measurements were available (only 3–4 profiles), and these happen to be outside the vortex, which is why the average concentration is high. We will add this information in the paper.

9. *p 4816, l 4: What do you mean with 'tropospheric influence'? Please specify.*

'Tropospheric influence' means here, that the seasonality observed in the lower altitudes is mainly influenced by the seasonality of the tropopause height. This will be clarified in the paper.

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