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Atmospheric Measurement Techniques Discussions

Interactive comment on "Water vapour profiles from SCIAMACHY solar occultation measurements derived with an onion peeling approach" *by* S. Noël et al.

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We thank the referee very much for the overall positive judgement and will consider the comments in the revised version of the paper.

Answers to general comments:

Main criticism is that more information about the errors should be provided. We will include a dedicated section in the revised version of the paper in which the errors will be summarised and discussed.

Answers to specific comments:

1. *page 206, l 17-21: The arguments given here for application of the onion-peeling approach are not really convincing. E.g. (multiple) scattering is also not an issue for IR limb observations, nevertheless optimal estimation or similar approaches are widely used for IR retrievals. Further, the amount of data for the occultation observations of SCIAMACHY is limited, i.e. retrieval speed is not a similar severe limitation as for SCIAMACHY limb or nadir observations. A more thorough discussion of the pros and cons of onion-peeling versus optimal estimation for the SCIAMACHY solar occultation observations would be required here.*

   We agree with the referee that multiple scattering is not an issue in the (thermal) IR and that retrieval speed is not as critical for the analysis of SCIAMACHY solar occultation data as for nadir and limb (although for water vapour absorptions in the spectral range considered here line-by-line radiative transfer calculation are really quite time consuming). It is not the intention of this paragraph to state that optimal estimation is not possible for SCIAMACHY water vapour retrieval. In contrast, we are in fact working on an optimal estimation based retrieval, too, and we expect it to be more accurate than the onion peeling method. The point we want to make here that it might not be *necessary* to use optimal estimation and that even a simple approach (like the onion peeling method) can give useful results. We will reformulate the introduction section to clarify this.

2. *page 215, l 6-17: Does this mean that the various correction factors are not linearly independent of each other? Would another choice of correction factors then help to improve the approach?*

   As can be seen from eq. (16) the saturation correction factor for each tangent altitude depends on all other factors above. The correction factors are determined by assuming a certain profile shape and scaling. The choice of a different reference profile would result in different optical depths and also different saturation

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correction. As long as these are used in a consistent way the results do not depend very much on the actual choice of the reference profile. The resulting error is determined by the deviation between the reference profile and the 'true' profile. We tried to keep this error small by using 'real' ECMWF profiles as reference. Nevertheless, as stated in the paper, the saturation correction is considered to be the major source for systematic errors. We will address this issue in the new error section.

3. *page 216, l 5-7: Although I am not really familiar with the work of Sofieva et al., I have some doubts that this is the right explanation for the oscillations in the profiles. I recommend to consult von Clarmann et al., 1991 for a (another?) possible explanation.*

   There may indeed be several explanations for the oscillations. We will add Clarmann et al., 1991, as another possibility.

4. *page 217, l 18-20: I am a bit surprised that linear interpolation is accurate enough for the radiances. Have you tested the behavior of the radiance profiles at different spectral points?*

   Maybe the text is a bit unclear here. We interpolate linearly between the *logarithms* of the measured spectra (we will clarifiy this in the text). We think linear interpolation is appropriate because it is only performed over a very small altitude range (SCIAMACHY measurement data used here are roughly on an 3 km grid). Furthermore, the quality of the absolute radiometric calibration (also of the interpolated data) is not relevant for the retrieval method. In fact, from the retrieval results there is no indication that the linear interpolation is a problem; otherwise there would be a difference in the results depending on the distance between radiative transfer grid point and measurement altitude.

5. *page 220/221 (Section "Validation") and Figs. 7/8: A log-linear plot covering 4 decades of the volume density over altitude is probably not the appropriate way*

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to demonstrate good agreement between various data sets, given the very small variation of water vapor in the stratosphere in terms of vmr. The percentage differences shown in Fig 8c and 8d are far more appropriate.

We agree with the referee that Fig. 8c and 8d are most important to assess the agreement between the data sets. The profiles are mainly shown to illustrate the measured quantities and their variation. In fact, some qualitative results from the intercomparison can be seen in the logarithmic plots, namely that ECMWF profiles are typically lower and that there are larger deviations in the top and bottom altitude range. The quantiative assessment is then shown in subfigures c and d.

6. *page 221, l 14-15: I do not agree with this statement and do not believe that this is the explanation for the low correlation: the percentage variation of water vapor in the 15-20 km range is definitely not smaller than elsewhere, the contrary is true (compare Fig. 6).*

   We agree with the referee that according to Fig. 6 the variation seems to be similar at all altitudes. However, the data set used in the intercomparison is based on collocated data which are not evenly distributed over time. These data show somewhat larger variations at higher altitudes, as can be see from the standard deviations plotted in Fig. 8a. This is what we refer to in the text.

   We also agree with the referee that variations below 20 km are larger and that at least for these altitudes there has to be an additional explanation for the reduced correlations. One possibility is that atmospheric effects like refraction may play a role here. This has to be investigated further. We will mention this in the revised version of the paper.

7. *page 222, l 14/15: This sentence refers to an estimated error of the retrievals, which in fact has not been provided. The sentence refers to Fig. 3b and related text, which is, however, certainly not the only error to be considered. The retrieval*

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error as provided by error bars in Fig. 5c has not been explained. A short chapter which compiles all relevant random and systematic errors of the water vapor profiles should be provided.

Such a chapter will be added in the revised version of the paper.

8. *Fig. 3: From what is shown in the figure and the text on page 215 I guess the figure caption should read: "... (a) Saturation correction factors as function of the water vapor scaling factor for different tangent altitudes. ..."*

   The figure caption will be changed.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 203, 2010.