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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of water vapor vertical distributions in the upper troposphere and the lower stratosphere from SCIAMACHY limb measurements" *by* A. Rozanov et al.

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Response to reviewer comments (C. Sioris)

Dear Chris,

thank you very much for your comprehensive review. We hope to address all your comments to your satisfaction. Please find our detailed replies to your comments below.

However, the limb scattering technique is poorly suited to measuring water *vapour because of its abundance in the lower troposphere, and secondly because it absorbs strongly at long wavelengths where the light source is scattering by aerosols, rather than by molecules (Rayleigh). This second point is not addressed adequately in this paper. The concentration of fine aerosol in the upper troposphere can vary from the assumed LOWTRAN background aerosol model assumed by the authors and the impact on the photon pathlength distribution is a function of the particle size and the viewing geometry.*

We have implemented an aerosol correction algorithm in accordance with the suggestions you made in your comments and investigated the influence of the stratospheric aerosol type and loading upon the retrieved water vapor profiles.

Other main points are that the authors might discuss is the benefit of using the $\sim$1350 nm band rather than $\sim$935 nm or $\sim$725 nm since the high spectral resolution of SCIAMACHY at shorter wavelengths may offer some advantage relative to the former, as well as being more predictable in terms of photon pathlengths since Rayleigh scattering is a stronger contributor and air density is well known. Without a discussion of these shorter wavelength bands, I question the statement on p.4011 that "SCIAMACHY is the first space-borne instrument providing the possibility to retrieve vertical distributions of water vapor from observations of the scattered solar light performed in limb viewing geometry." How about OSIRIS for example?

We have now included the shorter wavelengths in Fig. 1. It is clear now that neither 935 nm nor 725 nm band matches our requirements of minimum stratospheric a maximum tropospheric influence. Concerning OSIRIS, I have never heard about any water vapor retrieval from this instrument. If you know any publication demonstrating that these retrievals are possible please let me know.

I question whether the application of the polarization calibration improves the spectral fit quality? Even a reference showing that the limb polarization calibra-

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tion is working properly for channel 6 would suffice.

I am not aware of such a reference. Our investigations have shown that the polarization correction do not have any noticeable influence upon the retrieved profiles. However, the radiometric calibration (option 7) was found to do a bad job in some cases. Therefore, in the updated retrieval version the calibration steps 6 and 7 have been switched off and the text has been changed appropriately.

I assume the signal level is insufficient to detect water vapour with ∼20% precision using only one of the four azimuthal “columns” of radiances? The authors should state this. Otherwise, it seems a natural choice to use only one azimuthal “column” of radiances, particularly for the upper troposphere where there is spatial variability (i.e. gradients) in cirrus and water vapour.

It is quite unclear to me how you come to the conclusion that the signal level is insufficient. It is never stated in the text that the azimuthal measurements are averaged. Surely, we use only one azimuthal “column” of radiances for the retrieval. This is why you see 4 SCIAMACHY profiles for each FPH measurement in the comparison plots.

According to the text (Section 3.1), Figure 1 is supposed to show that “the number of photons multiply scattered within the troposphere and then entering the instrument filed of view is decreased compared to weaker bands”. This is not clear. Shorter wavelengths look more favourable than ∼1350 nm (e.g. ∼1200 nm).

No they do not because of a much stronger influence of the troposphere. The response to the doubling of the tropospheric concentration is a measure for the portion of photons which come from the troposphere. The weaker the response the less photons come from the troposphere.

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Also relating to Figure 1, independent doublings of the concentration above and below 10 km are used to show that the retrieval is weakly sensitive to the column of water vapour below 10 km. However, at mid-latitudes, where the tropopause is at ∼10 km, lower stratospheric concentrations rarely double, while tropospheric columns may change by more than a factor of two over very short spatial and temporal scales (e.g. change in weather from a cold summer day [20°C at ground] to a warm one [32°C at ground] with the same relative humidity).

Figure 1 illustrates that in the selected spectral range the sensitivity to the tropospheric water vapor is weakest compared to other absorption bands. This statement is not affected by a possibly large tropospheric variability in any way. It is not claimed that the sensitivity is weak in the absolute sense. We also do not discuss here the sensitivity to the troposphere in comparison to the stratosphere.

The surface albedo a priori uncertainty of 0.1 seems too small. If the surface albedo is going to artificially account for bright cloud decks, it would seem that an priori uncertainty of 0.5 would be more appropriate. Please discuss this selection.

In the iterative process the a priori value for the surface albedo is replaced by the result obtained at the previous iteration. Thus, the final result might be arbitrary far form a priori. This is also clarified in the text.

Did the authors consider the use of a high TH reference, which is known to have many advantages over a solar Fraunhofer reference including avoiding Doppler shifts, instrumental effects and the approximate cancellation of reflected light from below the FOV?

The solar reference is preferred because of a poor quality of limb spectra at high tangent heights. This statement is also added in the text.

The use of a solar occultation Fraunhofer reference raises a large question about the effect of cloud below the field of view (FOV), which is admittedly "neglected" by the authors (see p. 4024).

A subsection on the effect of clouds below the field of view is included.

The authors do not discuss their algorithm for the detection of clouds in the FOV. My impression is that the cloud filtering is done by hand.

The discussion is now included in the manuscript.

Also, I disagree with the idea of using different across-track swaths as a function of tangent height. If a spectrum at a certain azimuth is flagged as cloudy, I feel it is more appropriate to discard the spectra taken at underlying tropospheric tangent heights for the same azimuthal position as well. This suggestion is most relevant in the tropics where thin cirrus forms at the tropical tropopause (~17 km), but their may be a couple of other underlying tropospheric tangent heights used in the retrieval (e.g. 11 and 14 km). Their method may lead to retrieval instability and oscillating retrieved profiles in the upper troposphere. The authors could use their large sample size of coincidences to compare their approach with the one I suggest. Hopefully the azimuths for odd and even tangent heights are lining up better than they did years ago for SCIAMACHY when I looked at twilight OClO.

There should be some misunderstanding. Unfortunately we can not identify any statement in the manuscript that could lead to this conclusion. Please let us now how you came to it. We always use one azimuthal measurement for the retrieval. If a high cloud is detected the complete measurement is discarded. To my opinion it is said by "In the current study only measurements with no clouds detected above 10 km are considered." at the end of Sect. 3.2.1. Do not

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you think so? There is still some misalignment (about 50 km) between odd and even tangent heights.

Many of the illustrative examples are shown for a SZA of 60-70°. The authors should investigate the sensitivity at high sun (SZA=30°), which SCIAMACHY faces every orbit.

There is no significant difference in the behavior of the radiances for high sun. The corresponding statement is added to the text in Sect. 4.1.

In a revised version of this paper, I would expect to be provided with the azimuth difference angle ('d_phi') in the caption of Figures 3-4.

The relative azimuth angle is provided.

I would also need to see figures/results analogous to Figure 4 for a range of realistic 'd_phi' values (30, 90, and 150° for SCIAMACHY), because it is difficult to understand how the aerosols do not change the photon pathlengths more significantly.

As above, there is no significant difference in the behavior of the radiances for different azimuth angles. The corresponding statement is added to the text in Sect. 4.1.

Perhaps a tangent height of 11 km would be more revealing since the local aerosol extinction is more than double the value at 15 km for your assumed aerosol profile.

Figures 3-5 are intended to illustrate the origin of the signal measured by the instrument rather then to analyze the influence of the aerosols. It is already clear that the signal is dominated by the aerosol scattering. Thus, we see no benefit form changing the tangent height from 15 to 11 km.

The time period should be provided for Figures 13-15.

The time period is provided now.

I am curious about the retrieval accuracy in the face of a sharply peaked volcanic plume such as occurred following the eruption of Kasatochi in August 2008. It may be instructive for the authors to consider a case with a sharper aerosol peak at 17 km rather than the weak Junge layer at 23 km in their assumed aerosol profile. Then they could examine the accuracy of the water vapour retrieval at 12 km. I believe the retrieval will have very large errors because the light will be assumed to have originated mostly from the tangent layer (12 km) and from an atmosphere with background aerosols whereas, in reality, the light source vertical profile is not a smooth function of altitude with strong contribution from above the tangent layer. My overall suggestion for the retrieval would be to match the radiance profile for one or two non-absorbing wavelength near 1350 nm (e.g. on either side of the water vapour band) with an iterative retrieval of the aerosol number density (guessing a size distribution, spherical particles and a refractive index for sulfate) and then to use the retrieved aerosol number density as an input into the water vapour profile retrieval. The authors have concerned themselves with surface elevation, but have skipped the major issue of background aerosol in the field of view. Thus, I do not accept the statement that the algorithm has a weak sensitivity to major atmospheric parameters.

We have followed your suggestions and included a subsection on stratospheric aerosols.

A case with a dry lower troposphere and the surface at sea level would be interesting.

A dry troposphere affects the retrieval in a very similar vary as raising the surface layer.

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The authors should realize that multiple scattering within clouds may be an issue since water vapour concentrations can be higher inside of clouds (particularly cumulo-type) than outside of clouds, due to convection. For other gases, the difference between a bright surface and a cloud deck below the FOV may not be significant, but especially for water vapour, the authors should carefully study this.

The influence of clouds is analyzed.

The number of k-values per spectral bin should be given in the correlated-k description. The spectral bin size should also be provided.

The requested information is provided.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 4009, 2010.