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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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A lot of good work is done in this paper. The authors have made progress in this challenging remote sensing problem relative to previous presentations of this work at symposia. I am going to focus on my criticisms, rather than pointing out the highlights. Obviously water vapour is a strong absorber and thus a likely molecule for retrieval investigations. 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

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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. Other main points are that the authors might discuss is the benefit of using the ~1350nm band rather than ~935 nm or ~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 pathelngths 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?

I question whether the application of the polarization calibration improves the spectral fit quality? Even a reference showing that the limb polarization calibration is working properly for channel 6 would suffice.

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.

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). 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

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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).

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.

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 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).

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. 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.

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.

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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. 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. 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.

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

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

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)

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

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