Atmos. Meas. Tech. Discuss., 3, C1–C5, 2010
www.atmos-meas-tech-discuss.net/3/C1/2010/

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.

J.-L. Bertaux (Referee)

bertaux@latmos.ipsl.fr

Received and published: 30 October 2010

General comments:

This paper proposes a new way to measure water vapor in the Upper troposphere and Lower stratosphere (UTLS), by analyzing the spectrum of solar scattered radiation at the limb in the near IR, region 1353-1410 nm. There is a large interest in the precise monitoring of the evolution of H2O in this region, since contradictory results have been reported in the recent past. An increase of H2O in the lower stratosphere could have a negative impact on the ozone balance budget. The only way to get a global picture is from space; but there are difficulties to measure H2O in this region with already used

methods. Therefore, any new method is more than welcome in this respect. In this paper, the method is described, simulations of retrieval are performed, and some results obtained from SCIAMACHY are compared to balloon borne in situ measurements for validation purposes. This is an important work and I recommend publication. I have still a number of remarks or clarifications that would need to be addressed.

Specific comments:

1. The method relies on the fact that the observed spectral region is quite opaque to radiation which comes from the ground, because of the strong absorption by H2O in the band considered. However, when there is a high altitude cloud below the tangent point, it may change the spectrum in an unforeseen manner. This is a limitation of the method, which is addressed in the main text, but not at all in the conclusions. It should be mentioned in the Conclusions, and perhaps discussed a little more in the main text.
2. Generally speaking, the "sun normalized intensity" must be more precisely defined. The concept of "radiance factor" Rf is useful in this context, and I would recommend to use it. The radiance factor is the ratio of the observed radiance (intensity) to the radiance B0 that would be observed when observing a plane surface with an albedo of 1, scattering as a Lambert law, and illuminated perpendicularly by the sun. In the such a case, the intensity emitted by the surface is isotropic, and is: B0=F/pi where F is the solar flux (say, in watt/cm2 nm s, or photons/cm2 nm s) The radiance factor when observing a surface with albedo A is Rf=A. Whatever is the target observed (surface or thin atmosphere) the radiance factor is defined, for a target providing a brightness B (or intensity, or radiance, these are all the same), is: B=Rf * F/pi. Therefore, Rf=B/B0. So, my question is: what does mean the "sun normalized intensity" for the authors? I hope that it is the radiance factor as it is defined above, and in this case it should be mentioned; otherwise, I would recommend to adopt the radiance factor as the unit of "sun normalized intensity". Note that some authors are using the expression (I/F) for the radiance factor, while I say it is pi*I/F. These authors mean the same thing as me, but they take out the pi factor, probably a remnant of the time when astrophysicists

where calling the solar flux pi*F ! It is time to stop the confusion, and use the concept of radiance factor. This is relevant to the text and to Figures 1, 2, 3.

3. It may appear a little paradoxical to be able to retrieve the H2O profile at altitudes which are not observed (below the lowermost tangent point of 12 km) and to retrieve H2O at more altitude points than the 5 measurement points at altitudes 12,15.3,18.9,21.9 and 25.2 km. The author could comment on this aspect. In fact, nadir observations are also providing H2O values (as well as ozone), with only one direction of sight. Observations at the limb of solar backscattered radiation provide information elsewhere, in contrast to occultations, where the number of retrieved points cannot exceed the number of measurements.

Detailed comments: p.4012, lines 7 and 8: . . . the emitted infra-red radiation. . . as written, it suggests that it is the IR thermal emission of the ground, or the gas that is observed. I think that it is rather the solar backscattered radiation in the middle infra red. Can you check?

p.4013, line 26: Signal to noise ratio of 400 to 700: is it per spectral element? (spectel) or for the whole band, and what is the associated spectral sample and time of integration?

P.4018: lines 16, 17. How is determined the polynomial fit? I presume it is the one giving the best fit to the logarithmic spectrum. It should be mentioned.

P.4019, paragraph lines 15 to 19: imposing a positive value will produce a bias when the quantities to be retrieved are quite small, compared to the error bar.

P.4022, line 7. It is not clear to which spectrum the parameter Csol applies. I assume that it must be the solar spectrum as measured each day by SCIAMACHY. It should be mentioned here.

p.4025, line 22. . . . shown by the blue line. I assume that this simulation is with an albedo 0.5. It should be written here in this sentence, as well as on Figure 3, after

“single scattering”, and also in the figure caption for clarity.

p.4025-4026, figure 3. The red line seems to be absent from the figure 3. The caption for figure 3 suggests that it is almost identical to the cyan line, and therefore cannot be seen on the figure. I do not understand such a similarity. Both calculations are with no aerosols (only Rayleigh scattering), and only multiple scattering. The only difference is the albedo: 0 and 0.5. Let B0 the radiance observed in the case A=0. An optically thin estimate of the radiance is: B0= F tau/ 4 pi, where F is the solar flux, and tau the Rayleigh optical thickness along the line of sight (neglecting the phase function). With an albedo A=0.5, and a solar zenith angle of 69°, the brightness B coming from below is: B= A F cos(sza)/pi. (Lambert law) The flux received by one gas molecule (or one aerosol particles) coming from below may be computed in integrating B over the whole downward hemisphere (2 pi steradian). It is therefore 2 AF cos(sza), which is F cos(sza)=0.36 F for A=0.5. Therefore, changing the albedo from 0 to 0.5 will increase the observed intensity by 36%. Possibly the authors have normalized, for this not by F, as they should have done, but by F+ 2 AF cos(sza). Or it means that no light at all come from the ground, because of strong absorption by tropospheric water vapor? In this case, the same simulations done outside the band, say around 1300nm, would show a larger sensitivity to surface albedo? And what happens when there is a cloud with high albedo at high altitude, like 8 km?

Technical corrections: P.4019, line 6: what is the value of L, the number of spectral points?

P.4020, line 4: . . ..height. . .

p.4024, line 12 / what the acronym ESFT stands for?

p.4031, line 16: . . .which decrease the amount of light traveled along. . .

p.4033, line3: . . .at the following. . .

Caption of Figure 4: add: The curves for the four cases considered on Figure 3 are very similar. (I assume that the blue and cyan lines are indeed on the figure?)

p.4052, figure 10: the yellow color used for the case albedo=0 is unvisible oin both figures.May be you may use a color already used, but double thickness?

p.4053, figure 11: upper left figure: the longitude is indicated E, while it is W on other plots.

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