Atmos. Chem. Phys. Discuss., 7, S5591–S5595, 2007
www.atmos-chem-phys-discuss.net/7/S5591/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozone profile retrieval from limb scatter measurements in the HARTLEY bands: methodology, algorithm description, sensitivity studies, and validation" *by* G. J. Rohen et al.

Anonymous Referee #2

Received and published: 2 October 2007

General Comments

Aptly described by its title, this paper is review of the methodology and sensitivity of the upper stratospheric/mesospheric ozone profile retrieval from SCIAMACHY limb scatter measurements. It also includes a brief set of comparisons with coincident measurements from other instruments.

I have some rather serious concerns with this paper. It is unclear to me that this work represents a significant advance beyond the previously published work on this topic.

The methodology section has been presented in an extremely similar form in both JGR (Rohen et al., 2005) and ASR (Rohen et al., 2006). The sensitivity (error budget) has also already been presented in both of these papers with very similar tabulated results. The investigation into the sensitivity to each of the terms is explained with a bit more detail in the current paper with some new figures; however, the explanations are brief and the studies are not expanded in a significant way. The stray light section is new, although the conclusion is made that the effect on the retrieved ozone is small and cannot explain the discrepancies that arise in coincident comparisons with other measurements.

The third section of work in this paper, entitled "Validation", does include some new results. Comparisons are made with MIPAS, HALOE, MLS, and a ground based radiometer. The main conclusion that is reached from these comparisons is that the agreement is generally within about 10% at the lower altitudes and that a systematic overestimation exists in the SCIAMACHY product at the upper altitudes. Again, a similar comparison with MIPAS data was already published in Rohen et al., 2006, that concludes agreement to within roughly 10%, although the overestimation at upper altitudes is not clear in this result. It is not clear to me why the overestimation consistently shows up in the comparisons presented here, and not in the result published in 2006. Has the SCIAMACHY retrieval changed in a significant way? The authors should at least mention the previous MIPAS comparison and the different result that they have now obtained at higher altitudes. There is an attempt in the current work to diagnose the overestimation through comparisons within specific latitudes regions and at ranges of solar zenith angle; however, no obvious conclusions can be reached.

For publication, I would suggest that the focus of this paper be placed on the comparisons with the other instruments, which should be expanded, and an effort be made to diagnose the discrepancies that arise. This will likely involve changes and improvements to the existing inversion (see below). The methodology and error analysis that has already been published should not be repeated. A brief summary and reference is

ACPD
7, S5591–S5595, 2007

EGU

all that is required.

Finally, the composition of the paper is in need of editing. The phrasing is cumbersome and confusing at times, and the grammar is incorrect in a large number of places. If this paper is to be published, it requires an extensive edit.

Specific Comments

I am uncomfortable with the authors' claim (in the abstract) that this retrieval constitutes the highest possible ozone retrieval using the backscatter technique as it is shown in the sensitivity studies that the error becomes very large near 60 km and in the comparisons that there are unexplainable systematics at the upper altitudes. On a side note, the authors' use of the term "backscatter" when they mean "limb scatter" is confusing. To me, backscatter (180 degrees) implies a nadir geometry (like SBUV) or lidar.

I am confused about why the effect of the stray light is so small. Although it is not clear from the text, I do not believe that a stray light correction is performed as part of the retrieval. As Rohen et al. (2005) state, "the method to retrieve mesospheric ozone concentrations is essentially based on the shape of the limb radiance profiles". Therefore an effect like stray light should have a rather large impact on the inversion. I believe that the authors are alluding to the fact that the actual stray light signal in SCIAMACHY measurements is small below 60 km and that is why the impact on the ozone inversion is small. Also, it seems to be the case that the stray light is far worse in the 310 nm channel than in the other channels. Because the kernel at this wavelength peaks at the lower altitudes, the stray light at the upper altitudes may not have a large effect. If this is the case it should be mentioned. I notice that above ˜70 km the effect of the stray light is certainly significant and that it produces an overestimation. Is it possible that the magnitude of the stray light signal has been underestimated in this study?

On a related note, the residuals shown in Fig 7 seem quite large and have troubling

ACPD

7, S5591–S5595, 2007

features such as a consistent positive bias between ˜40 and 50 km in the longer wavelengths and systematic offsets (264 nm). I would like to know what the residuals are before the inversion. Also, it would be very helpful if the reference altitudes for each wavelength were stated explicitly.

I would strongly suggest that the authors consider a measurement vector that uses wavelength pairs. This is not really a relevant issue in this review because, as I have mentioned, this SCIAMACHY retrieval has been published in this form already. However, the authors are using a monthly climatology for the neutral air density and I am surprised that they find this sufficient using the current technique. The small ($<$2%) error they show due to uncertainty in the neutral density is based on shifting the entire profile (I believe this is the case; it is not clear in the text) and the upper reference measurement does indeed help. However, the inversion is likely much more sensitive to local perturbations in the neutral density and this should be investigated. The authors are using the TRUE method for tangent height correction. They mention this method uses a tropical ozone climatology; at the very least they should attempt to quantify the latitudinal dependence of the correction before they speculate on possible impact on the ozone retrieval. Surely it is possible to use a more appropriate assumed ozone profile for the measurement latitude? Can the TRUE correction and the ozone retrieval be iterated?

The variation in solar zenith angle across the instrument field of view is stated as one of the largest sources of error in this work. Can this not be included in the model for the inversion? Since only single scatter radiative transfer is considered, it should be quite easy and practical to more accurately model the measurement by incorporating multiple lines of sight across the instrument field of view and dramatically decrease the impact of this effect.

Technical Corrections

The text states that Fig 12 shows the sensitivity to surface reflectance; however, the

ACPD

7, S5591–S5595, 2007

caption states sensitivity to aerosol.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12097, 2007.