

Interactive comment on “Accounting for the effect of horizontal gradients in limb measurements of scattered sunlight” by J. Puķīte et al.

J. Puķīte et al.

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Dear Reviewer #2,

Thank you for the kind comments that are leading towards improvement of the article.

General comments:

This is an interesting paper dealing with the effect of neglecting horizontal inhomogeneity in atmospheric minor constituent profiles on the retrieval of these constituents from satellite-based limb scatter measurements. This is an important aspect of all atmospheric remote sensing applications using limb viewing satellite instruments, and investigations in this direction will be very useful. I recommend the paper be published after the authors have given consideration to the comments (mainly minor and requests for more explanations/discussions) listed below.

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We are happy about the value given to our work.

I have several general comments:

1) I'm a little puzzled that the term "tomography" does not appear in the paper, because the basic approach is tomography, right? I suggest mentioning this in the appropriate sections of the paper. You may also mention that the IR channels of OSIRIS are specifically designed for tomographic retrievals (having a vertical FOV), although they are mainly used for measurements of terrestrial airglow emissions and not limb-scattered radiation. A relevant citation would be:

Volume Emission Rate Tomography From a Satellite Platform, Douglas A. Degenstein, Edward J. Llewellyn and Nicholas D. Lloyd, Applied Optics, 42, 8, 1441-1450, 2003. and/or the references cited in this paper.

The reviewer is right: the algorithm is based on tomographical approach. Therefore we include the term "tomography" in the article as suggested.

In the abstract we change "We introduce a method to correct for this effect..." to "We introduce a tomographic method to correct for this effect..." In the introduction: "The aim of this study is to demonstrate a possibility to correct for the horizontal gradient effect from the observations by combining consecutive limb scanning sequences..." we change to "The aim of this study is to demonstrate a possibility to correct for the horizontal gradient effect from the observations themselves applying a tomographic approach by combining consecutive limb scanning sequences..."

Besides the MIPAS inversion algorithm in the IR we now mention also the algorithm applied for OSIRIS as suggested by the reviewer.

In the conclusions the sentence "The application ... is demonstrated for the first time" is now changed to: "This tomographic application for SCIAMACHY UV/VIS measurements is demonstrated for the first time".

2) Another general aspect is related to the previous point. Your method allows reduc-

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ing the retrieval errors if horizontal gradients are present in flight/viewing (roughly the same for SCIAMACHY) direction. Still, you have to assume horizontal homogeneity across flight direction. In terms of emissions the measurements are only affected by air masses within the FOV. However, this is not the case for limb-scatter measurements. An extreme case would be a solar azimuth angle close to 90 degrees, and low sun elevation, i.e., the sunlight has a fairly long light path through the atmosphere before reaching the LOS. If inhomogeneities in the absorber concentrations are present across flight/viewing direction, this will also lead to retrieval errors, because they are not accounted for in your retrieval. They don't have to - and cannot - be accounted for in your retrieval of course, but I think it would be worth mentioning this aspect.

Yes, our algorithm is only 2-dimensional as mentioned often in the paper. Therefore it can correct for inhomogeneities along the flight/viewing direction only. However the effect of gradients across flight direction can not gain its maximum possible importance for SCIAMACHY measurements. It is because for measurement places where SZA approaches 90 degrees, the relative solar azimuth angle (SAA) is less than 90 degrees (approx. 50 degrees in January).

As suggested by the referee we point out this fact:

In the abstract we modify “In this study the effect of horizontally inhomogeneous distributions of trace gases on the retrieval of profiles from limb measurements of scattered UV/VIS light is investigated.” to “In this study the effect of horizontally inhomogeneous distributions of trace gases along flight/viewing direction on the retrieval of profiles is investigated.”

In Sect. 4.1. (in the new manuscript) We change “Algorithms assuming homogeneous horizontal distributions do not take into account that the LOS is crossing regions with concentrations different to those that appear around tangent point.” to “Algorithms which assume homogeneous horizontal distributions do not take into account that the LOS and hence the light, which contributes to measurement before and after being

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scattered into the LOS, crosses regions with concentrations different to those appearing around TP.”

We start Sect. 4.2. (new manuscript) we changed 1st sentence adding “along flight/viewing direction”: “In order to account for possible gradients of the considered trace gas along flight/viewing direction...”

At the end of Sect. 6 (new manuscript) where we discuss possible improvements in the algorithm we add:

“ Also, in additional studies the effect of possible gradients across flight/viewing direction should be investigated because the 2-D approach presented here did not account for it.”

In Conclusions after “It allows the correction for cases with large horizontal gradients” we add “along flight/viewing direction”.

3) I also think - in line with the other referee - that the paper should also include a more detailed description of the retrieval scheme, because (a) The Pukite et al. [2006] proceeding paper is not easily accessible, and (b) the paper would then be a more stand-alone document

We improved the algorithm description in the article, now particularly describing our algorithm for calculation of box AMFs (being the key point for development of 2-D algorithm), and also providing more details on the retrieval in general (see also reply to comment below). We included an Appendix in the manuscript: “Calculation of box air mass factors by RTM Tracy-II”. We added a reference at the end of the 2nd chapter and also encourage interested reader to read the article by Kühl et al. (2007) which became available online just after the manuscript submission to ACPD. This article is describing all algorithm aspects in detail.

4) I suggest using “tangent height” or “tangent altitude” rather than “elevation”, because the latter is not really used in the limb-scatter community, and may lead to confusion.

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Changed as suggested.

5) This comment is related to the applicability of the 2D retrieval scheme to current limb-scatter instruments. You mainly discuss the first few SCIAMACHY limb states at high northern latitudes, where no nadir measurements are performed (and these few states will not necessarily coincide in space with the vortex). Little is said about the applicability of the method for the remaining parts of the orbit (2D-retrievals at lower latitudes are presented, but the issue of under sampling is not properly addressed in my opinion). Perhaps you can specify a minimum horizontal distance between two consecutive limb measurements required for the 2D retrievals to be appropriate.

From our best knowledge there is no limb-scatter satellite instrument (including also SCIAMACHY) especially designed for limb scatter tomography. However we realized that the most northern limb scanning sequences of SCIAMACHY orbit are providing a possibility for tomographic applications (besides existing emission IR applications of MIPAS and OSIRIS). As can be seen from the results the applicability is good for these most northern scanning sequences. The interest is enhanced by the polar vortex appearing at the latitudes where these sequences are taken. However for other scanning sequences where nadir measurements are performed in between the correction of the retrieves profiles is small, if any for studied gradients. This requires further investigations and improvements of the algorithm. The current algorithm can be successfully applied for the most northern scanning sequences only, where no nadir scanning sequence is performed in between.

In order to point out this in the article we add the arguments at the end of Results section: "For other parts of the SCIAMACHY orbit, where nadir scanning sequences are performed between limb scanning sequences, the presented algorithm can not be applied in its current form. It is giving only unconvincing improvement because of a poor overlap of the sensitivity areas of consecutive limb scanning sequences. This requires further studies and improvements in the algorithm."

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6) *The paper contains typos and grammatical errors (e.g., missing articles and use of present continuous). I suggest the manuscript be thoroughly proof-read by a native speaker or by one of the senior co-authors.*

Corrected as suggested.

Specific comments:

7) *Page 16156, line 24 - 26: The statement, that nadir observations provide only total column information is not correct. Both GOME and SBUV nadir measurement were/are used to retrieve vertical profiles of ozone. This method only works with really strong absorbers and has a fairly poor vertical resolution (about 10 km), but it provides vertical profile information. Relevant publications are, e.g.:*

Hoogen, R., V.V. Rozanov and J.P. Burrows, Ozone profiles from GOME satellite data: Algorithm description and first validation, J. Geophys. Res., 104, 8263-8280, 1999.

Bhartia, P. K., R. D. McPeters, C. L. Mateer, L. E. Flynn, and C. Wellemeyer, Algorithm for the estimation of vertical ozone profiles from the backscattered ultraviolet technique, J. Geophys. Res., 101, 18,793-18,806, 1996.

We corrected the sentence mentioned by the Reviewer as follows: “While nadir observations (i.e. space borne instruments looking perpendicularly to the surface of the Earth) provide knowledge of the total column density and low vertical resolution information (about 10 km) about profiles for strong absorbers only...” We added here also the suggested references.

8) *Page 16157, line 16: “ .. and consists of 4 pixels.” This is not the case for all wavelength ranges and latitudes, I think. Perhaps you can add “for the spectral range used here” or something like that.*

Yes, although the 4 pixels are the value for most part of orbit it is right that at specific parts also 2 or 1 pixel at a tangent height is the case. We corrected it in text as “of up to 4 pixels for the UV/VIS spectral range”.

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9) Page 16157, lines 23 - 25: *I think von Savigny et al. [2005] (The ozone hole breakup in September 2002 as seen by SCIAMACHY on ENVISAT, J. Atmosph. Sci., 62(3), 721 - 734, 2005) must be added to the list, because it also includes a description of retrieval codes for O3, NO2, and BrO from SCIAMACHY limb measurements.*

Done as suggested.

10) Page 16158, line 17: *“For our retrieval we apply a two step method”. I suggest adding a brief explanation what the two steps are (or omit this statement, because the two steps are explained in the next section). This is not obvious to people outside the limb-scatter community.*

We add a short comment for the relevant sentence: “...where Differential Optical Absorption Spectroscopy (DOAS) and profile acquisition by applying radiative transfer modelling are performed in two separate steps.”

11) Page 16159, section 2: *I suggest adding more details on the retrieval algorithm, since the Pukite et al. [2006] paper is not that easily accessible. What reference tangent height was used?*

Besides pointing to Köhl et al., 2007 (see comment above) some most important details regarding our DOAS algorithm are given: “For OCIO the fit-window ranges from 363.5 to 391 nm and for NO₂ from 420 to 450 nm. As reference spectrum we use a measurement at a tangent height where the absorption of the considered trace gas is small (~36 km for OCIO and ~42 km for NO₂). The small abundances of the considered absorbers which appear at the tangent height of the reference spectrum are estimated by a latitude dependent a-priori and their impact is added to the retrieved SCDs”

We also want to point out that the Pukite et al. [2006] paper is easily accessible. Since the web-address to this (free) article is given there should be no problem to access it.

12) Page 16159, line 26: *“Also, because of the slantness of the limb observations the*

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measurements are practically insensitive to the atmosphere below the tangent height."

18217;m not sure this statement is correct. If you refer to the raw limb radiances, when speaking of "measurements" this statement is wrong, because the surface albedo and clouds affect the limb radiances significantly (up to a factor of 2, roughly). I suggest specifying the meaning of this statement.

Regarding this point, please see also the reply to a similar comment of reviewer #1.

Meant here are the (indirect) measurements of slant column densities, as the correspondent to the measurement space (or data space), being later by inversion converted to the vertical column densities/concentrations or model space. Although (directly) measured intensities in fact are sensitive to the atmosphere below, most of the information about a considered trace gas is coming from the atmosphere above the tangent height because of the slantness of line of sight. We replace the word "measurement" with "SCDs derived from measured spectra" in order to make this more clear.

13) Page 16160, line 13: "In general the instrument exhibits higher sensitivity to air masses closer to instrument since the light contributing to the measurement integrates along the line of sight."

This statement can be (and should be) specified further. For high tangent heights - with a negligible line of sight optical depth - the difference in sensitivity between the near and the far side is also negligible (as you point out a few lines below). The asymmetry increases with increasing optical depth along the LOS and therefore with decreasing tangent height. Also, the main reason for the asymmetry is the extinction along the LOS, and not directly the "integration of light along the LOS", I think.

We think there was a misunderstanding. We want to say that for high tangent heights there is symmetry for the scattering event distribution (of light contributing to measurement), but not for the sensitivity.

The light after being scattered into the line of sight moves towards the instrument. (see

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Fig. 2 in the article)

Thus, also photons that are scattered at the far side of limb are traversing the near limb side (but not opposite!). This is what we are calling integrating along the LOS.

Therefore in fact we want to say that besides the extinction along the LOS also the integration of light along the LOS is a principal factor.

In sum, the sensitivity for the near limb side is larger.

The sensitivity along the LOS depends on both the total number of photons being already scattered into the LOS at a particular place along the LOS and the slantness of the LOS at this particular place. We want to explain this aspect in a simple gedanken experiment.

If one would have only 2 scattering events the sensitivity would be 0 for the line of sight segment behind the remotest one scattering event i.e. at the opposite side of this scattering event to the instrument. The sensitivity of 1 (in terms of intensity) would appear for the LOS segment between both events. The sensitivity will be 2 for the segment between the nearest event and the instrument. In terms of SCDs and thus in terms of this article it is a bit complicated because the slantness also should be considered.

To deal with the problem more in detail we change the relevant text:

“Along the LOS the instrument has different sensitivities for different locations in the atmosphere. In general, the instrument exhibits a higher sensitivity to air masses closer to the instrument since the light contributing to the measurement integrates along the LOS (see schematic view in Fig. 2). On average, one will get gradually increasing sensitivity for the LOS towards the instrument.

Another factor is that the asymmetry of the sensitivity region increases for rising optical depths and therefore with decreasing tangent height (mainly due to scattering on air molecules). Also absorption (especially ozone) and scattering by aerosols, clouds and

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reflection at the ground modify the measured light intensity.

For high altitudes, where the atmosphere is optically transparent, a nearly symmetrical distribution across the TP of photons being scattered into the LOS is observed by the model: Nearly one half of all photons contributing to the measurement are scattered into LOS between TP and instrument (near limb side), the other half from behind the TP (far limb side).

For the retrieval at low altitudes a limiting factor is the large probability for Rayleigh scattering i.e. the atmosphere is optically thick. Furthermore, usually clouds are present along the LOS at low altitudes, also preventing sensitivity for low atmospheric layers.

Therefore at low tangent heights with an optically dense atmosphere, more photons contribute from volumes of the side between TP and instrument. Thus, besides the low sensitivity for altitudes below 12-15 km, a larger shift of the sensitivity towards the near limb side occurs.”

At the caption of Fig.2 we add: “note that the impact of the considered absorber on the detected slant column density increases according to the number of light paths.”

14) Page 16160, line 16: “with different elevations”. I suggest replacing “elevation” by “tangent height” here and throughout the paper. My first interpretation of “elevation” in this sentence was “solar elevation”

Changed as suggested by the reviewer.

15) Page 16162, lines 10 - 15: “Also the spatial distance ... without nadir observations between them”.

This is of course only a very limited part of the SCIAMACHY orbit, and the latitudinal spacing between the following states is about a factor of 2 larger. Are the 2 conditions you mentioned above also fulfilled for these, i.e. for the majority of the SCIA limb measurements? I think this should be discussed more in the paper. The ‘problem’ of alternating limb-nadir measurements does not occur with OSIRIS, so this instrument

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would be better suited for a 2D retrieval approach.

Please refer to Point 5 for the discussion of the first condition for 2D retrieval, the spatial overlapping. Regarding the second condition, we realized slightly increased rates of SZA change for the measurement time of January when moving southward. However this could be still enough to combine limb scanning sequences in one inversion if the spatial distance between them would be small enough.

We add in article the SZA change per one minute also for other part of SCIAMACHY orbit:

“The SZA change per minute during a SCIAMACHY orbit increases until the equator is reached, with its maximum of 0.25 (in January) and then decreases again southwards.”

And we say (see discussion above) that the algorithm is not giving reasonable improvement because one of criteria (spatial overlap) is not fulfilled.

16) Page 16166, line 25: *I don't understand the phrase “.. in one inversion constraint”. You mean “in one inversion” or “simultaneous inversion” ?*

We mean one, both in altitude and latitude resolved inversion constraint inverting SCDs of all scanning sequences simultaneously. We correct the relevant place accordingly.

17) References, Deutschmann, 2007: *Is there a website, where the manual and the model can be downloaded from?*

The program and the manual are usually available in the internet but now the web server is not available but we hope to repair the server until the final acceptance of the manuscript.

18) Fig. 2: *Again, I suggest replacing “elevation” by “tangent height”*

Replaced as suggested.

19) Fig. 4: *I think the figure would be easier to read with reversed axes.*

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Changed as suggested.

20) Fig. 6: “elevation” should be “tangent height”

Changed as suggested.

Typos etc. (there are more, I didn't track all of them):

Page 16156, line 11: “full spherical” should read “fully spherical”

Page 16156, line 16: “ofhorizontal” should be “of horizontalLY”

Page 16156, line 24: add space after “observations”

Page 16156, line 24: remove “is”

Page 16158, line 20: “full” should be “fully”

Page 16159, line 14: “full” should be “fully”

Page 16166, line 12: “photochemical” should read “photochemically”

Page 16166, line 21: “horizontal” should be “horizontally”

References, Bovensmann 1999: “Nöel” should be “Noël”

References, Butz 2006, Dorf 2006, and Sioris 2006: “Boesch” should be “Bösch”

Caption, Fig. 4, 2nd line: “The Illustration IS” or “The Illustrations ARE”

Corrected as suggested.

Again, we would like to thank a lot the Reviewer #2 for the very constructive comments.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 16155, 2007.

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