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Atmospheric Chemistry and Physics Discussions

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

J. Pukite

Janis.Pukite@mpch-mainz.mpg.de

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Dear Reviewer #1,

GENERAL COMMENT: The paper is very well written and informative. It is very well suited to be published, with some additions (see below). The authors should be commanded for such a clear expose and nicely presented results. This paper should be very well received by the Limb Scatter community, as it gives insight on how to deal with inhomogeneity effects.

We appreciate the very constructive comments by the Reviewer #1. Thank you much for the positive and encouraging feedback given to the article.

Three main points: (1) This paper does assume that the reader has a good background

in retrieval method and relies somewhat heavily on past references, mainly from the lead author. In view of the fact that the whole scheme depends on the AMF factors, I would ask for a better definition of these factors. Please define them better, in 1D, write down the equation. In the text, it is only defined as ratio of slant path column density to vertical column density. As such, it appears it is only geometrical (which it is in the optically thin limit), but in fact it does incorporate absorption along the slant path. If you thoroughly explain it in 1D, then you do not need any more explaination for the 2D case. I assume that the VCD within a cell is only the product of the density within that cell and the height of the cell. If that is all it is, please state it, or explain further.

The box AMF formula given in the article has been the so called classical AMF definition, which in our case are a good approximation for weak absorption. Although these would be valid for NO_2 and OClO in the considered wavelength regions, we decided to recalculate the results using as box AMFs a more general definition: Now the differential ($dSCD_g/dVCD_b$) is calculated, which describes how the SCD at a certain tangent height (or certain geometry g) is changing when the VCD (at some altitude, or in the box b) changes. Please note that the results for box AMFs are almost the same (in the range of statistical error) and the findings of the paper are not changed.

We agree with the referee that a more detailed description of the algorithm should be provided and as suggested we include exact explanation of how our RTM calculates the box AMFs. Also, the definition of the VCD as product of number density and cell height is made clearer.

(2)Again on the airmass factor, since all your method relies on them. You need to tell the reader how they are computed. (even though you have described it in earlier paper). You cannot have no explanation at all, you need to give a few sentences on how you use your Monte Carlo code to evaluate them.

We follow the suggestion of the reviewer and describe the AMF calculation by TRACY-II in a few sentences (see reply to comment above).

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In addition, a more detailed description of the main principles of RTM by Tracy II (weighting of generated light trajectories, calculation of intensity and relation of box AMF with it) is provided in the appendix of the article.

And why do you need a Monte Carlo code? Could not you derive from a 2D RT code?

The advantages of Tracy-II for this study are that it is fully spherical also for multiple scattering (and at large SZAs), and is a 3D model and provides output (i.e. box AMFs) in 2 dim (or even 3 dim) as stated in the article. Other RTMs that properly account for these points will also be suitable for 2 dim retrievals.

Do the air mass factor need to be recomputed for each specific case.

A typical set of AMFs (e.g. from the middle of the month) for a certain period is giving only negligible error, because the solar azimuth angle is changing not so much (<10° for the most northern state) during January. However for the studies of this article, we calculated the box AMFs for each specific case. We would like to keep this detailed information outside of the article because we think it would distract the reader from the main points.

(3)To appreciate the computation required to go from 1D to 2D, please, state the CPU time required to do a sample computation.

A calculation for one scanning sequence on Tracy-II takes approximately 1.5 times more in 2-D mode as in 1-D for the examples given in the article. We add this information to the article.

SPECIFIC COMMENTS (1) 1) Does the paper address relevant scientific questions within the scope of ACP? YES 2) Does the paper present novel concepts, ideas, tools, or data? YES 3) Are substantial conclusions reached? YES 4) Are the scientific methods and assumptions valid and clearly outlined? YES 5) Are the results sufficient to support the interpretations and conclusions? NOT QUITE. See above 6) Is the description of experiments and calculations sufficiently complete and precise to allow

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their reproduction by fellow scientists (traceability of results)? NEED MORE EXPLANATION 7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES 8) Does the title clearly reflect the contents of the paper? YES 9) Does the abstract provide a concise and complete summary? YES. 10) Is the overall presentation well structured and clear? YES 11) Is the language fluent and precise? YES 12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? NO, and that is a problem. SCD and VCD have to be defined with equations 13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? YES. See above 14) Are the number and quality of references appropriate? YES 15) Is the amount and quality of supplementary material appropriate? N/A

Corrected as suggested by the reviewer (see above).

SPECIFIC COMMENTS (2) (a) The authors are using the word "elevation" instead of "Tangent height", which may be confusing when ones knows that in the Limb Scatter community, the word "elevation" is used to refer to up/down scanning. Minor, but irritating.

Changed as suggested by the Reviewer.

(b) I guess that you have multiple scattering. Why is it that (1) Fig. 4 shows no effect on the TH below the TP, and (2) you can state (Page 6 top): the measurements are practically insensitive to atmosphere below? Surely, you can have photons bouncing back from layers below TP.

The confusion maybe occurred because with "measurement" we meant the measurement space variable i.e. SCDs which are inverted in our two step algorithm. Surely backscattering, albedo and possible clouds impact the measured intensity. However the impact on the SCD from a layer or box below tangent height is small in comparison to that above tangent height because of different effective path lengths through them.

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We exchanged “intensity” in the relevant place with “SCDs derived from measured spectra” and tried to make it clear throughout the article that we are quantifying sensitivity in respect to SCDs. See also comments on these issues in the response to the Referee #2.

(c)Page 3: You refer to “the swap across flying direction”. You may mean to write: “the cross track swath”

Changed as suggested.

(d)Page 3, bottom: “for every limb scanning sequence separately”, you may want :“for every limb scanning sequence one profile at a time”

We rephrased this sentence to point out the difference of the described 2D retrieval approach to the conventional 1D retrieval. We add: “i.e. each corresponding profile is retrieved independently from the measurements of the previous or following scanning sequences.”

e)There is sentence on Page 4 which mystifies me: “For that purpose, an overlap between ... is utilized”. What is meant there? Please provide more explanation if this is an important point.

We explain the point more precisely; we add: “...we are taking advantage of the fact that the LOS for one particular scanning sequence crosses atmospheric volumes already probed by previous measuring sequences - under a different geometry.”

(f)Page 5: change “acquisition” for “retrieval”.

Changed as suggested.

COMMENTS ON THE FIGURES None. They all look great. You make the life of the reviewer easy.

At the end we would like to thank the Reviewer #1 for the detailed comments.

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

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Interactive Comment