**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Ozone profile retrievals from the Ozone Monitoring Instrument" *by* X. Liu et al.

Anonymous Referee #1

Received and published: 17 December 2009

General comments

This Referee Comment relates to the paper from Liu et al. on "Ozone profile retrievals from the Ozone Monitoring Instrument".
The paper used for the review was the one as published on the ACPD website (pp. 22693-22738).

The paper explains with considerable detail the method used to retrieve vertical ozone profiles from OMI, it furthermore explains what wavelength and radiometric calibrations are applied and it also characterizes the retrievals and shows some examples. I think this paper contributes to the understanding of the issues surrounding ozone profile retrieval in relation to the instrument specific issues that OMI seems to have.

One general comment with regards to the text:
The paper is split up in just seven long sections. Please consider the use of named subsections to help the reader's focus.

Specific comments

Page 696 Line 6: "The overall algorithm" refers to the GOME algorithm or one at RNMI

Page 699, line 13-14: Here it is stated that 5 adjacent spectral pixels are co-added for UV1 and 2 for UV2. Hence the spectral resolution of the measurements is significantly reduced in an artificial manner. The reason given is that it is supposed to reduce the measurement noise and that it would speed up the retrievals. Co-adding does reduce the noise for the co-added pixel but the overall error in the retrieved profile due to measurement noise does not change because of co-adding. The reduction of the spectral resolution can result in loss of information. Please quantify the difference in terms of DFS with and without co-adding of spectral pixels. Also, I would expect that the same number of forward calculations is needed with or without co-adding. Please explain why it speeds up the retrievals.

Page 699 Line 19: You use a channel dependent slit function but there also may be a cross track dependency of this slit function. Did the authors take this into account?

Page 700: You mention that the MLS ozone profile goes from TOA down to 215 hPa, which corresponds to UV photons with a wavelength typically lower than 300 nm. However, in Figure 1 you show spectral biases compared with OMI for wavelengths across the full spectral range used in the retrieval. I assume this simulated spectrum is the 'first iteration' of the forward model run without any fitting taking place. Please clarify how you account for parameters such as surface albedo, in order to make judgements on systematic biases in the OMI spectrum.

Page 702 Line 22: You state that you use NCEP temperature profiles but not the associated surface pressure. Given that the surface pressure at sea level typically varies

between 1030–970 hPa, this is a difference of 5

Page 702 Line 29: You adjust your model layering to tropopause height. Please indicate how sensitive the overall ozone profile is to this adjustment.

Page 702: Perhaps related to the co-adding of spectral pixels is the statement on page 702 lines 10-12, where it is mentioned that the ozone absorption cross sections are convoluted with the fitted OMI slit functions weighted with a high-resolution solar spectrum. If there is significant variation of the ozone absorption cross section within the spectral region covered by the slit function, say 1 FWHM, this might lead to significant errors in the retrieval. For weak absorption this procedure is correct (and often used in DOAS), but for strong absorption this procedure is incorrect. The authors seem to be aware of that as they mention on page 710 line 28 "simulation of radiances at a higher spectral resolution before convolution with instrumental slit functions" as a possible improvement of the algorithm. As the forward calculations need to be accurate to about 0.1

Page 703 Line 9: With the introduction of spectral fitting parameters there is a considerable risk of cross-correlation. Especially the wavelength shifts of rad/irrad and the cross sections may be correlated. Was this considered?

Page 703 Line 14: Here it is mentioned that scaling parameters for mean fitting residuals (1 parameter for each channel) are included in the state vector. I assume that this refers to first-order correction mentioned on page 701 line 10. Please make that clear in the text. The "mean fitting residuals" have not been mentioned and should be explained.

Page 703: Many parameters are fitted. More detailed information is needed. Please add a table listing the parameters that are fitted, a-priori value and error, and typical values obtained during the retrieval. Please indicate any significant correlations between these parameters (see comment above). Please indicate the effect of including individual fit parameters on the ozone profiles so the reader can assess its importance.

Page 705, around line 25: Here it is mentioned that errors in the forward model and errors in the assumptions made for parameter values are not discussed in this manuscript. The argument given is that these errors were investigated using the GOME retrieval algorithm and were found to be generally small compared to the smoothing error. However, the retrieval algorithm used here is substantially different from the algorithm used for GOME, as is explained on pages 698 and 699. The spectral range 307 - 325 nm was not used in the GOME algorithm. Please clarify why the authors think that a re-investigation of the forward model errors and errors due to forward model parameter assumptions is not needed here. For example, the treatment of polarization correction, under sampling correction and the use of O2-O2 for cloud characterization differs from the GOME algorithm.

Page 706 Line 20: You refer to Fig 4. which introduces the interferences line to the reader. This line is not explained here, nor adequately in the figure caption (see also later comment on interferences). Please make a forward looking statement to a named subsection (of sect 4) where you discuss this line.

Page 706, around line 26: For me it seems more logical to normalize the averaging kernel by the ozone profile, not the ozone variability. The averaging kernel operates on the profile. Please explain why the ozone variability was chosen to normalize the averaging kernel.

Page 709: Retrieval Interferences. The terminology 'interference' is unclear. Scene dependent physical properties such as aerosol and surface albedo are part of almost any retrieval in the VIS spectral range, unless you demand pure clear sky cases. In my opinion, scene dependent effects can hardly be classified as an interference. I suggest that the authors rephrase the interferences to something like "wavelength dependent surface albedo correction" or something similar that describes what is actually fitted. Please also update the caption and legend of Fig 4 and 5 accordingly.

Page 710 Line 3-5: The authors use the surface albedo correction as a way to com-

pensate for unknown surface pressure while skipping over the NCEP surface pressure. If you can, please indicate the effect on the ozone profile of using the actual NCEP compared to the surface pressure correction in the wavelength dependent albedo component. E.g.: some scenes over sea.

Section 4, first paragraph: What is the order of this new wavelength dependent albedo parametrization you fit?

Section 4, first paragraph: this seems to be part of an algorithm description. Please consider moving relevant parts to section 2.

Page 710 Line 25: The authors suggest to "use other auxiliary information as accurately as possible" without specifics. Please clarify: what kinds of information?

Page 711 Line 26: GOME may have a different Signal/Noise ratio than OMI. How do the authors take this into account in these comparisons?

Page 712 Line 21: The authors mention "the ozone trend in the operational algorithm". Please clarify: what trend?

Page 716. The a priori error for the columns is plotted in Fig 7 (black) which shows that the a-priori error for the individual layers in the troposphere can be as small as 0.5 - 4 DU. Further a correlation length of 6 km is used, which means that the a-priori error for the tropospheric column is not large, perhaps 2 - 6 DU (estimated). On page 716, line 15 a solution error of 3-5 DU is mentioned for the total ozone column, the stratospheric ozone column, and the tropospheric column. And there it is mentioned that these columns can accurately be retrieved. I fail see that the tropospheric column can accurately be retrieved, because my estimate (see above) of the a-priori error of the tropospheric column is about as large as the final solution error. Please add the a-priori errors for the columns and layers to Tables 1, 2, 3, and 4. Depending on the outcome of the calculations, you might need to re-consider the statement that the tropospheric column can accurately be retrieved.

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Page 716 Line 29: The authors introduce two new instruments (HIRDLS, TES) which have not been discussed in the paper before. New information does not belong in the summary. If really relevant, move the discussion to an earlier section.

Fig 10: You mention in the text that the model has 25 layers and that there is a limited vertical resolution in the retrieval. However, the plots in Fig 10 (and others) are vertically interpolated and look very smooth. This gives an impression of high vertical sampling which I think is not justified. Have the authors considered using polygons or other plotting methods that indicate more clearly the vertical sampling/extent of the layers?

Technical corrections

Textual:
Page 710 line 17: fore –> for
Page 704 L 14: one of the TOZs should be TOC.
Page 707 L 15: "will be retrieved at a broad altitude range". Retrieved at or smoothed to?
Page 715 L 25: priori –> prior

Figures:
Fig 4: Please increase the line thickness for clear distinction between the two lines in each set, or consider a different line style for one of the lines.
Fig 4: suggestion to put the numbers 1,2,3 also in this plot for reference.
Fig 8: The white contours if tropopause height is sometimes hard to see. Please increase line thickness.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 22693, 2009.