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Atmospheric Chemistry and Physics Discussions

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

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Response to referee 1's comments

We would like to thank the referee for very constructive comments on our paper. We have addressed them as follows and made changes in the revised manuscript.

1 General comments

G.1. 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.

Answer: We added subsections for the two long sections: Sections 2 (five subsections) and 3 (four subsections). The five subsections in Sect. 2 are: Inversion technique, adaptation of GOME algorithm to OMI, OMI measurements and calibration, radiative transfer calculation, and retrieval scheme. The four subsections in Sect. 3 are: concepts for retrieval characterization, retrievals to show retrieval characterization, retrieval sensitivity, and retrieval errors. The other sections are quite short, so no subsections are added.

1.1 Specific comments

S.1. P 696 Line 6: "The overall algorithm" refers to GOME algorithm or one at RNMI

Answer: It refers to the KNMI algorithm. We changed "The overall algorithm is similar to our algorithm" to "The KNMI algorithm is similar to our algorithm overall"

S.2. 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.

Answer: We agree that coadding does not reduce the overall measurement noise. The reason that it speeds up the retrievals is because we perform radiative transfer calculations at OMI wavelengths with effective cross sections (convolved with slit function), as mentioned in p22702, line 10-13. Thus, coadding reduces the number of radiative transfer calculations. We removed " and reduce the measurement noise" and added " (because radiance is simulated at each OMI wavelength with effective cross sections)" after "to speed up retrievals"

S.3. 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"

Answer: Yes, the slit function is derived for each channel and each cross-track position. We added "and each cross-track position" after "each channel"

S.4. 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.

Answer: Yes. It is the "first iteration" of forward model simulation except for using MLS and climatological ozone profiles. Surface albedo is taken from an updated TOMS surface albedo climatology, cloud-top pressure is taken from OMI O2-O2 cloud products and cloud fraction is derived from a non-ozone absorbing wavelength at 347 nm. To clarify this, we changed "In the simulation, we use zonal mean..." to "The radiance simulation is the same as that for the first iteration of retrievals before fitting (to be described in Sect. 2.4) except that we use zonal mean..."

S.5. 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

Answer: The reason for not using the NCEP surface pressure is that the relatively coarser spatial resolution (2.5 x 2.5) causes some small (but still noticeable) retrieval anomalies over regions of large terrain gradients (where errors in surface pressure can be greater than 100 hPa). We have compared current retrievals to those with NCEP surface pressure at several ozonesonde stations, and found that the comparisons with ozonesonde observations are similar, so we decided to use surface pressure based on 1 atm and terrain height, which is also used in the OMTO3 algorithm. The similarities with ozone sonde data when using different surface pressure choices arise because the first-order surface albedo polynomial used in our retrievals partly accounts for errors in surface pressure. We tested the effects of changing surface pressure on retrievals for one pixel over the ocean. With surface pressure varying from 1013 hPa to 930 hPa, the retrieved total, stratospheric, and tropospheric ozone columns are all within 1 DU.

S.6. Page 702 Line 29: You adjust your model layering to tropopause height. Please indicate how sensitive the overall ozone profile is to this adjustment.

Answer: The main purpose of adjusting the model layering to tropopause height is to derive tropospheric and stratospheric ozone columns; the actual value of tropopause pressure has negligible effect on the retrieved total ozone and ozone values at layers not affected by the layer adjustment. We added the above sentence in the revision.

S.7. 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

Answer: The weighting of ozone cross-sections by solar irradiances when applying the slit function can reduce radiance errors greatly in regions of large ozone absorption variation (e.g., Hartley bands). We have recently implemented the radiative transfer calculation at high spectral resolutions and evaluated the radiance errors due to the use of effective cross sections: the root mean square (RMS) of the radiance errors are about 0.15% for 270-310 nm for different viewing geometry; the RMS errors in 310-330 nm depend on viewing geometry, about 0.07% for solar zenith angle (SZA)=5 and viewing zenith angle (VZA)=5, about 0.1% for SZA=45, VZA=30, 0.39% for SZA=75, VZA=60. It should also be noted that the systematic part of radiance errors are partly corrected using the soft calibration in Fig. 1, that is why our average fitting residuals in 310-330 nm for one orbit of retrievals are very small about 0.07%.

S.7. 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?

Answer: Yes. The selection of retrieval parameters is based on the evaluation of averaging fitting residuals as well as the comparison of tropospheric ozone at several ozonesonde stations. The cross-correlations between rad/irrad and the cross sections are very small, generally less than 0.1 due to distinct high frequency structures.

S.8. 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.

Answer: The mean fitting residuals do not refer to the first-order correction, but "the final fitting residuals derived from one orbit of retrievals." We added "derived from one orbit of retrievals using all the other parameters" after "mean fitting residuals"

S.9. 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.

Answer: We added a table to show the list of fitting variables, their a priori values and a priori errors. We also added a long paragraph to discuss the cross-correlation between different variables and their importance to ozone profile retrievals as follows:

"In our retrievals, the selection of these parameters is based on their impact on fitting residuals, their cross-correlation with ozone variables, and comparisons with ozonesonde observations. We want to include variables that can reduce fitting residuals and improve comparison with ozonesonde observations, and at the same time to avoid variables strongly correlated with ozone variables. Of all the non-ozone parameters, the first-order wavelength dependent surface albedo term in UV2 has the most noticeable correlation (0.2-0.5) with ozone variables, especially in the troposphere. Despite its correlation with ozone, this parameter is used to account partly for aerosol, clouds, and calibration signatures, and it is very useful to reduce fitting residuals and improve retrievals. Although higher-order polynomials can further reduce fitting residuals, they can adversely impact retrieval accuracy due to overly strong correlation with ozone. In addition, the parameters for radiance/ozone cross-section wavelength shifts can have correlations of 0.2-0.3 with ozone variables. The zero-order shift term for UV-1 has significant values of ˜0.03 nm. We found that these shifts are not due to actual wavelength registration errors in either OMI radiances or ozone cross sections, but instead to some wavelength registration artifacts introduced in our spectral coadding (values are much smaller without spectral coadding). These variables also help to improve retrievals. In general, all the other non-ozone parameters have weak correlations (<0.2) with ozone variables. For non-ozone parameters themselves, there are strong correlations (>0.5) between surface albedo terms and cloud fraction, and between zero-order and first-order radiance/ozone cross-section wavelength shifts in both UV-1 and UV-2."

S.10. 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.

Answer: We have investigated these errors for OMI off-line found that they are generally similar to values from GOME retrievals, and much smaller compared to smoothing errors. The main sources of forward model forward parameter errors are systematic errors in temperature and cloud-top pressure; a systematic 3 K temperature error at all layers leads to about 10% errors for individual tropospheric layers, and an 100-Pa error in cloud-top pressure causes 6-7% errors in the troposphere (Liu et al., 2005).

The errors due to our improved on-line polarization correction (compared to vector calculations) are generally within 1% at each layer. The errors are less than 0.5% at each layer due to exclusion of the undersampling correction. Errors originating from different cloud-top pressures are the same for the same error in cloud-top pressure.

S.11. 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.

Answer: we added "Figs. 4-6 also show results under ideal conditions (to be discussed in Sect. 4)." Before "Table 1 summarizes"

S.12. P706, 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.

Answer: The averaging kernels operate on the differences between true and a priori profiles, which is statistically represented by ozone variability or a priori errors. So normalizing averaging kernels by a priori errors is equivalent to plotting the averaging kernels for retrieving a modified state vector that has a priori error of 1 at each layer. Original averaging kernels at some layers have large oscillations (can be greater than 1 or less than -1) at high altitudes, meaning that the retrieved ozone is relatively very sensitive to ozone changes at these altitudes. However, because the ozone changes at those altitudes are very small (less than 0.05 DU), the impacts on the retrievals are actually negligible. The normalization significantly dampens those large oscillations at high altitudes, better representing the actual retrieval sensitivity visually. We have removed "Because ozone values vary by more than two orders of magnitude ... normalized by the actual ozone variability" and added a paragraph to describe why we normalize averaging kernels and choose ozone variability for normalization.

S.13. 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.

Answer: We rephrased "interferences" to "dependencies" in the text and figures.

S.14. Page 710 Line 3-5: The authors use the surface albedo correction as a way to compensate 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.

Answer: The effect is generally small. We tested the effects of changing surface pressure on retrievals for one pixel over the ocean. With surface pressure varying from 1013 hPa to 930 hPa, the retrieved total, stratospheric, and tropospheric ozone columns are all within 1 DU.

S.15. Section 4, first paragraph: What is the order of this new wavelength dependent albedo parametrization you fit?

Answer: First order for UV-2 (but zero order for UV-1) as mentioned in Sect. 2. For clarity, we added "(i.e., zero order for UV-1, first-order for UV-2)" after "wavelength-dependent surface albedo terms"

S.16. Section 4, first paragraph: this seems to be part of an algorithm description.

Please consider moving relevant parts to section 2.

Answer: We moved "In our OMI retrievals... to account for these effects" to a new paragraph in Sect. 2.4 after "standard sea level pressure of 1 atm"

S.17. Page 710 Line 25: The authors suggest to "use other auxiliary information as accurately as possible" without specifics. Please clarify: what kinds of information?

Answer: We added "(e.g., temperature profiles, aerosols, cloud, surface pressure)"

S.18. 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?

Answer: Because the main purpose is to investigate the effect of using a different spectral range (290-307 nm, 325 nm-340 nm in our GOME algorithm) on the retrievals, we use OMI data (same spectral resolution and signal to noise ratio) except for a different spectral range. We clarified this in the revision by saying "A major difference between our OMI and GOME algorithm is the use of a different spectral range (290-307 nm, 325-340 nm) for the latter (Liu et al., 2005). To investigate how this affects the retrievals, we use OMI data (same spectral resolution and signal to noise ratio) with this modified spectral range" and changed "For GOME retrievals" at the beginning of the next paragraph to "For retrievals with the modified spectral range"

S.19. Page 712 Line 21: The authors mention "the ozone trend in the operational algorithm". Please clarify: what trend?

Answer: It does not mean a specific trend. We rephrased it to "ozone trend from the operational SBUV retrievals"

S.20. 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.

Answer: The a priori errors for the tropospheric ozone column in Fig. 7 are 4.5, 8.5, 13.0 DU, respectively; the use of 6 km correlation lengths (positive correlation) makes these numbers much larger than the root square sums of a priori errors at individual layers (3.5, 4.8, 7.6 DU, respectively). In addition, our statement of solution error of 3-5 DU is confusing, which was clarified in our revision. It should be typically less than 3.5-5 DU (often about 2 DU for tropospheric ozone columns as seen from Fig. 4) instead of from 3 to 5 DU. We added average a priori errors in column and layers in Tables 1 and 2 (values for Tables 3 and 4 are the same as those in Tables 1 and 2 and therefore are not repeated). The average a priori tropospheric ozone column errors for the 3 bins are 9.8, 8,1, and 7.1 DU, respectively. The statement about accurate retrieval of tropospheric ozone column remains unchanged.

S.21. 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.

Answer: We removed the description of these instruments.

S.22. 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?

Answer: The smoothness generally indicates the coarse vertical resolution. Using more layers better represents the profiles but does not really increase the vertical resolution. Some operational products of ozone profiles also provide data at grids much finer than retrieval grids (e.g., 67 layers for TES and 101 layers for AIRS). We have plotted the data in three ways before: polyfill at retrieval grid, contour plot at retrieval grid (e.g., Figs. 2, 3) that is very commonly used, contour plot data at fine layers (Figs. 9, 10). The features to be described are very similar (especially the last two methods). We chose the last plotting method for Fig. 9 and 10 to better represent the profiles in the troposphere (with about 6 layers, instead of the whole atmosphere).

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

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.

Answer: We made the changes, and used the dotted line style (instead of thin) in Fig. 4

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