**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 2'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 Substantive comments

S.1. First, on page 22696 the authors describe a total of four planned papers for this new product. This seems like a large number to me, and not typical for the field. Getting one or two 'measurement science' papers published in a mainstream journal (ACP, JGR etc.) is hard enough in today's environment. Four seems very ambitious. Have they considered fusing and/or condensing some of these? What is the status of the companion papers? Are they planned, submitted, in press? Unless these are well advanced (i.e., submitted at least), might I suggest the authors avoid making confident predictions for papers that might not eventually materialize. Will any of these papers describe comparisons with other OMI/MLS-based tropospheric ozone products (e.g., the work of Ziemke or Schoeberl?)

Answer: For satellite retrievals and validation, we think that it is not unusual to write several papers as we have done in the past for our GOME retrievals. We have considered condensing/combining them into one large paper, but this would become too long and would miss many of the important details. In addition to this paper, another paper that validates OMI retrievals with MLS data has also been published on ACPD (ACPD, 9, 24913-24943, 2009). We are working on the third paper to validate OMI ozone profiles against ozonesonde observations and OMI/MLS tropospheric ozone columns; we have done all the comparisons and just need to finish the writing. Since we have not submitted these papers yet, we changed "four" to "several" in the revision. We cited the second paper published on ACPD and then mentioned "In separate papers, we will validate our retrievals against ozonesonde observations and OMI/MLS tropospheric ozone columns (Schoeberl et al., 2007) as well as operational total ozone products."

S.2. Second, on page 22700, and in figure 1, the authors show the average difference between OMI radiances and those fitted assuming MLS/climatology. They state that the pattern is repeatable from day to day, but do not quantify by how much. Similarly,

while the mean is important, the correction is only really valid so long as the standard deviation about that mean (e.g., around the orbit) is small (e.g., compared to the radiance noise and/or the mean itself). I didn't see a discussion of that standard deviation. Apologies if I missed it. I know this is kind of touched on in lines 14/15 of 22701, but a little more detail would be welcome.

Answer: We removed the sentence "The overall features do not vary from day to day" and added more details at the end of the paragraph: "The mean differences can vary by up to a few percent from day to day especially in the wavelength range 300-315 nm, but the overall features (i.e., wavelength and cross-track dependencies) do not change much." The reason we did not show/discuss the standard deviations of the mean differences is that we use zonal mean MLS data and zonal mean climatology (instead of spatiotemporally collocated observations). We added: "The standard deviations of the mean differences, not shown here, vary from 1-2% at wavelengths shorter than 295 nm (except around strong solar fraunhofer lines) and longer than 320 nm to 6-10% between 300-315 nm. They are mainly determined by the zonal variability of ozone due to the use of zonal mean MLS and climatological ozone profiles."

S.3. Third, I found figure 10 and the accompanying discussion rather unclear and sweeping. It was hard to match the text to features in the figure. A better description of what features are being described would be very helpful here (specific locations and dates). Perhaps circling or annotating features of interest in the figure itself would help. The progression of the 'plume' beyond the second day is rather unclear to my eye. Also, in the cross sections, all the enhancements seem to be well connected to higher O3 values above, so I'm not sure by what basis (at least from OMI data alone) the authors can draw the conclusions they do about some being pollution transport, others being stratospheric influx. A bit more clarity here would be helpful, but not essential to the overall thrust of the paper.

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Answer: We agree that it is very difficult to separate high ozone due to pollution transport and stratospheric intrusion from OMI data alone. That is why we specifically mentioned on page 22713 line 23-28 "Due to limited vertical resolution and the fact that pollution plumes from continental outflows often mix with stratospheric air masses, it is difficult to identify the origins of these high ozone features from OMI retrievals alone. It is necessary to use other in-situ observations, model simulations, and meteorological fields to assist with the interpretation of OMI retrievals." Since this event has been studied in detail in Zhang et al. (2008) by combining satellite and aircraft observations of ozone and CO, and GEOS-Chem simulation, our point in referring to this paper is to show that OMI ozone shows the features due to pollution transport as seen from AIRS and GEOS-Chem CO, and stratospheric intrusions (enhancement well connected to high O3 values above) simultaneously occur even in regions of pollution transport as indicated by AIRS and GEOS-chem CO (suggesting it is difficult to separate pollution ozone from stratospheric transported ozone).

We have modified the first few sentences of the last paragraph of Sect. 6 to: "The progression of this transport event can be seen clearly from Atmospheric Infrared Sounder (AIRS) observations and GEOS-Chem simulations of CO (Fig. 7 of Zhang et al. (2008)). OMI retrievals (Fig. 10a) show features very similar to the spatiotemporal distribution of CO except for some high ozone likely due to stratospheric intrusions. For example, the high-ozone stream over the west coast of the US where there is low CO is not caused by the transport of Asian pollution, but by a stratospheric folding event, the spatiotemporal evolution of which can also be clearly seen from Figs. 10a and 10b. The cross sections also clearly indicate significant stratospheric influences, even in regions of pollution transport, as many high ozone features are well connected to high ozone above."

2 Minor comments

Line 16: I question the need to capitalize the 'Z' in OZone in this context. By extension, TOZ and TOC are very similar (indeed the author himself confuses them at a later point in the manuscript). Why not change TOZ to simply OZ, or even OC (matches better with SOC/TOC)?

Answer: There is only one typo that shows two TOZs. We changed TOZ to OC.

Page 22696 Line 27: Suggest 'Sect.' -> 'Section'

Answer: This actually follows ACP practice.

Page 22698 Line 4: Would be good to define 'under sampling' and the correction in a little more detail (a sentence or two). As it is, nothing is conveyed to the uninitiated reader.

Answer: This has been described in our GOME paper (Liu et al., 2005). We added "Because GOME spectra are not Nyquist-sampled, errors occur when interpolating solar irradiance to the radiance wavelength grid." and cited two references "(Chance et al., 2006; Liu et al, 2005)"

P22695, Line 6: comma needed after 'that'

P22698, Line 11: suggest 'and' -> 'while' (3rd word from end of line).

Line 21: Delete 'Since' at end of line, capitalize 'The' at start of line 22.

Line 26: Period after '2006).' then 'they' -> 'These factors'

Page 22699, Lines 4-6: This sentence leaves the reader wanting to know more detail about the polarization approach. It turns out this is answered in the next paragraph, but

the sentence at the end of the current paragraph interrupts the flow. Some rewording would make this clear. Perhaps it is a simple as deleting the last sentence and adding the clause 'as described in the next paragraph' at the end of this sentence.

Lines 18-19: 'except that ... adjusted ... distances' -> '(adjusted ... distances)

Line 28: swap 'has' and 'only'

Page 22700, Line 17: 'fore' -> 'for'

Answer: we made the above changes

Line 22: 'do not vary from day to day' - it would be better to be quantitative. Also see,

general comment above.

Answer: We changed that to "The mean differences can vary by up to a few percent from day to day, especially in the wavelength range 300-315 nm, but the overall features (i.e., wavelength and cross-track dependences) do not change much."

Page 22702 Line 4: The authors might want to describe the frequency resolution of this albedo here (I think it's described later on). On instinctively takes 'frequency dependent' to mean 'channel by channel' which is clearly not the case as retrievals would be all but impossible that way.

Answer: To be clearer, we changed "the use of wavelength-dependent surface albedo" to "the fitting of wavelength-dependent surface albedo in UV2 with a first-order polynomial"

Line 16: Under what circumstances is the cloud top pressure not available? Is it just

that the algorithm has not been run for some periods, or is it some fundamental limitation

of the algorithm?

Answer: This is due to quality flag control. We added "due to quality flag control" at the end of the sentence.

Line 29: 'modified' is very unclear here. Is everything scaled by the surface pressure? Is the tropopause added in as a specific surface?

Answer: We changed "by the surface pressure and the daily NCEP thermal tropopause pressure" to ": daily NCEP thermal tropopause pressure is used to replace the level closest to it, and layers between the surface and tropopause are distributed equally with logarithmic pressure"

Page 22703, Lines 13-15: For completeness, some description on the constraint of the other state vector parameters would be good here.

Answer: Also at the request of reviewer 1, we added the list of a priori values and their a priori constraints in a table.

Page 22703, Line 7/8: I suggest 'accurate' -> 'different'?

Page 22704 Line 14: One of the TOZs should be a TOC.

Line 22: Insert 'the' before 'AK'

Answer: We made the changes.

Line 20: Might the TOZ here also be supposed to be TOC?

Answer: No. It refers to total ozone column.

Page 22705 Equation 5: Strictly speaking, some ensemble covariance should be used

here rather than Sa (e.g., Rodgers 2000, page 49). While most people do what the authors have done, and use Sa instead, some acknowledgment of that simplification would be good.

Answer: We added "Strictly speaking, the full covariance matrix of a real ensemble of states should be used to estimate **Ss** (Rodgers, 2000). Due to the unavailability of the full covariance matrix, we use **Sa**, only the diagonal components of which are derived from an ensemble of states. Comparisons with ozonesonde and MLS data show that the derived smoothing errors by assuming ozonesonde/MLS as truth are generally consistent with our approximate estimates of smoothing errors using Eq. 5"

Equation 6: 'I' should be in bold upright font.

Answer: Ic is a vector here.

Page 22707 Line 3: Is this overestimate the same one as mentioned earlier in the text (page 22703, line 23)? If so, some cross reference would be good.

Answer: We added ", which was mentioned in Sect. 2.5," after "OMI level 1b data"

Page 22708, Lines 10-20: I think more discussion of the scaling of the averaging kernel is needed here (line 26 of 22707). For example, assuming it still applies, one cannot appreciate the magnitude of the 'cross terms' (influence of stratospheric O3 on tropospheric retrieval etc.) in figure 6 without knowing the magnitude of the scaling.

Answer: Note that scaling is not applied in Figure 6. The magnitude of the "cross terms" is shown in Figure 7. We added a paragraph to describe the reasons for scaling with a priori errors: " Averaging kernels for some layers show large oscillations at high altitudes (values can be outside the range [-1,1]), suggesting that retrieved ozone values in those layers are very sensitive to ozone changes at high altitude. However, actual retrievals are barely affected by these large values, because overall changes

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in ozone from a priori values at high altitudes are very small (< 0.05 DU). To better represent the actual retrieval sensitivity and dampen these large oscillations, the AKs in Fig. 5 have been normalized by the a priori errors. Since averaging kernels operate on the differences between true and a priori profiles, which are statistically represented by a priori errors, this normalization is equivalent to plotting the averaging kernels for retrieving a modified state vector that has a priori error of 1 at each layer."

Page 22708, Line 23: 'an' -> 'and'

Page 22709 Line 5: suggest 'low' -> 'weak' or 'tenuous'

Page 22711 Line 6: Second 'from' -> 'using'

Line 9: 'likely necessary to improve' -> 'potential avenues for improving'

Page 22712 Line 6: 'stratosphere and upper stratosphere'? Was one of these supposed to be troposphere'?

Line 18: 'larger' -> 'poorer'

Page 22713 Line 1: 'white' -> 'black'?

Answer: We removed "stratosphere and" from "stratosphere and upper stratosphere" and made the other changes.

Line 8: Is this really a middle troposphere enhancement? Could the poor sensitivity lower down be pulling you back to a priori while the real atmosphere is also enhanced in the lower troposphere?

Answer: Based on the retrieval sensitivity, I think that the enhancement is greater in the middle troposphere, but there is also some enhancement in the lower troposphere. Take the retrievals at lon=0, lat=10.5S for example, the DFS at layer 1 (1013-704 hPa), 2 (704-489 hPa), and 3 (489-340 hPa) are 0.19, 0.38, 0.24, respectively; retrieved

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ozone values are 51, 80, 82 ppbv, respectively and a priori values are 33, 45, 50 ppbv, respectively. The ozone increase from a priori is significantly larger at layer 3 than layer 1 while the DFS is only slightly larger. Ozonesonde observations at Ascension Island also show this middle-tropospheric enhancement (Thompson et al., JGR, v108, D28241, 2003).

Page 22714 Line 5: How do we know it's not just a plume of O3 rich air from the stratosphere below some tropospheric air? Just because low values are higher up, that can't only mean lofting surely?

Answer: The animation of daily maps shows the transport of tropical marine air. Also these values (30-40 ppbv at 200 hPa) are much lower than typical background values for these altitudes (a priori ozone value is about 80 ppbv). We modify "Figure 9b shows the transport of low ozone tropical air to the upper troposphere at 120w" to "Figure 9b shows low ozone values of 30-40 ppbv in the upper troposphere at 125W, which is likely due to the transport of tropical marine air based on the animation of daily maps."

Page 22715, Line 8/9: 'quite consistent' - could you be a bit more quantitative?

Answer: I misread the x-axis scales from Zhang et al. (2008) due to having 2 scales (one for CO and one for O3). The ozone from aircraft observation is 25-45 ppbv over 0-2.5 km and 60-80 ppbv over 3.5-10 km. Our retrievals show about 70 ppbv over 3.5-10 km, consistent with aircraft observations, but much higher values of 55 ppbv over 0-2.5 km. We changed "quite consistent with aircraft observations of ozone" to "show ozone values of 70 ppbv over 2.5-10 km, consistent with aircraft observations, but show much higher ozone of 55 ppbv over 0-2.5 km."

Line 17: 'dissimilar' -> 'improved upon GOME'?

Answer: Not all the features are better than GOME. So we did not make this change.

Figure 5: Having symbols at every point on every line is just clutter, and actually unhelpful. Why not just have one symbol per line, at the surface which the kernel is supposed to be describing - hopefully (but not always as the authors note) the peak. Figures 6/7: There is really no need for the symbols here

Figure 10: As described above, some annotations would be helpful. Also, the dates are too small and in a code that is unclear to most readers. A longitude scale on the maps would be helpful.

Answer: We made changes as suggested.