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Comment

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

Anonymous Referee #2

Received and published: 22 December 2009

Review of "Ozone profile retrievals from the Ozone Monitoring Instrument" by X. Liu and colleagues.

This is a nice paper that gives a good review of an important new atmospheric composition dataset. I find it highly suitable for ACP and have only minor suggestions for improvement that should be easy to implement.

Substantive comments _____

I have only a small number of substantive comments:

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,

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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?)

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.

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.

Minor comments —————

———— Page 22695

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Line 6: comma needed after 'that'

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)?

—— Page 22696

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

—— 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.

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 as 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'

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

—— 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.

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?

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

—— Page 22703

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

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

—— Page 22704

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

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

Line 22: Insert 'the' before 'AK'

—— Page 22705

Equation 5: Strictly speaking, some ensemble covariance should be used here rather than S_a (e.g., Rodgers 2000, page 49). While most people do what the authors have done, and use S_a instead, some acknowledgment of that simplification would be good.

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

—— 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.

—— 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.

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'?

Line 8: Is this really a middle troposphere enhancement? Could the poor sensitivity

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lower down be pulling you back to a priori while the real atmosphere is also enhanced in the lower troposphere?

—— 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?

—— Page 22715

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

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

—— Figures

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.

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

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