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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozone mixing ratios inside tropical deep convective clouds from OMI satellite measurements" *by* J. R. Ziemke et al.

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General Comments

This paper describes some interesting new remotely-sensed measurements of ozone mixing ratios in the tops of tropical convective towers using a novel and clever radiative transfer retrieval algorithm. The science addressed in the paper is highly relevant to important atmospheric science issues which concern the composition of the tropical tropopause layer and the dynamical relationship between the troposphere and stratosphere. The interesting retrieval technique employed in this work extracts valuable additional information content from satellite measurements and is definitely worthy of future use, subject to further consideration of the residual method considered here. The paper is generally well written and figures are of a good technical quality with con-

tent well within the bounds of the ACP journal. Therefore it is my recommendation that this paper should be accepted for publication. My specific comments below suggest some improvements for the benefit of future readers as well as some concerns over the validity and use of the "residual method", which needs further consideration.

Specific Comments (All page numbers refer to the print version of the paper)

Abstract: There is a slight confusion in the abstract (and later in the paper) which it might be good to clear up for the benefit of the reader. The authors describe increased ozone concentrations in continental tropical convective clouds, relative to maritime clouds, and attribute the difference to the presence of ozone precursors from biomass burning and lightning. It would appear that the authors are suggesting that lightning in the body of the cloud affects ozone concentration either directly or indirectly through a link with biomass burning hydrocarbon chemistry. Which of these are the authors inferring? Note that lightning is ubiquitous in tropical deep convective towers, although greater lightning frequency is noted in cloud over land. Further confusion arises from the statement at the end of the paper (P. 16395, Line 3), that elevated ozone concentrations in these same continental convective clouds are comparable to those observed in clear sky conditions and are not perturbed by the presence of deep convection. The authors appear to have contradicted their explanation here. A clearer suggestion for the possible causes of the maritime/continental contrast could be made and what role in-cloud processes could play in both cases.

P. 16383, Introduction: The authors state (from Vasilkov et al., 2008) that the OCCP is several hundred hPa within tropical convective clouds due to the clouds' lesser optical depth at cloud top. This is also illustrated in Fig. 1. It would appear that this idealized optical depth profile refers to the convective tower only and not to the majority of the cloud as seen from above - the cirrus outflow. How is the retrieval affected if a cirrus deck is assumed?

P. 16385. Line 2: The pixel size of OMI is 13 x 24 km. Therefore many isolated deep

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convective clouds may only fill a few pixels. How might the retrieved ozone mixing ratio be affected by horizontal inhomogeneity in cloud optical thickness, especially at the edges of clouds where a pixel may contain clear air?

P. 16388. Line 6. The residual method described in this section (Section 4) could be rather dangerous. The potential for compounding systematic errors from two independent satellite measurements could bias the results. It is nice to see multiple satellites used in this way, but extreme caution is required when combining results, especially for point (pixel) measurements.

Also, in this paragraph, the authors say that this method can be used to retrieve a mixing ratio for every pixel, but only large spatial and temporal averages are considered in Section 6, with additional smoothing applied. Have the authors considered how accurate the residual method would be for individual pixels; and furthermore, how potential systematic errors could bias the wider-averaged spatial scale in Figures 8 and 9? A brief mention of such errors is given in the paragraph starting on P.19392, line 26, but I think this is somewhat underplayed. It is good to see satellite data being used in this way, but I think a stronger and clearer caveat needs to be made here.

Furthermore, it is stated that this method could be applied in clear air. If so, a convincing test would be to compare the retrieved upper tropospheric column using this method, with existing ozonesonde measurements. Perhaps a few suitable satellite overpasses with SHADOZ ozonesondes could be found and compared?

Technical points:

P. 16382, Line 13: change "aboard" to "onboard".

P. 16394, Line 13, Summary: The first mention of the radiative transfer model (LIDORT) used for this study is made in the summary. This model should be first described and referenced in the introduction or method sections.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16381, 2008.