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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global distribution of tropospheric ozone from satellite measurements using the empirically corrected tropospheric ozone residual technique: Identification of the regional aspects of air pollution" *by* J. Fishman et al.

J. Fishman et al.

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We thank all of the contributors who participated in this interactive discussion. These comments improved the manuscript to provide a clearer understanding of the empirical correction method and its subsequent identification of the regional aspects of tropospheric ozone.

We have addressed most of the comments by providing an updated manuscript that includes the addition of two appendices: ŞDescription of Empirically Corrected Modified Residual MethodŤ (Appendix A) and ŞError Analysis of Empirical Correction MethodŤ

(Appendix B). These two additions respond to the primary concerns of Dr. de Laat by showing that the empirical method should not yield the Logan climatology in the troposphere and they follow the suggestion of Dr. Clerbaux to include an error analysis. The two appendices and their relevant figures have not formally appeared in the open discussion, however they are included in the revised manuscript.

The comments of Dr. Kim have not been addressed directly in the revision. His comments actually highlight many of the relevant findings alluded to in the text. The main point of this paper is to use as much TOMS data as possible because of the robustness of the data set. The details observed using TOMS/SAGE are limited relative to those seen in the TOMS/SBUV data because the data density is much greater using our current methodology. This is a new data set and we are seeing some interesting features. Some of the findings are explainable and confirmed with other measurements, but, as Dr. Kim states, some of them seem to be somewhat inconsistent with some other observations.

In particular, Dr. Kim raises the point concerning not seeing an enhancement over northern India in the TOMS/SAGE TOR data. The sampling frequency of TOMS/SAGE would yield approximately one measurement per month over this particular area compared with more than 7000 data points that comprise a particular monthly average for the current technique. This disparity in sampling frequency is the driving factor that makes the older TOMS/SAGE data appear so smooth. We have examined ozonesonde data from three Indian sites and have not been able to corroborate out satellite findings, but we have also been cautioned by several other respected colleagues that the Indian ozonesonde measurements may have some quality control issues. With respect to comparing surface data with the satellite measurements, Fishman et al. (1991) showed that different seasonal cycles can be present when comparing surface measurements with free tropospheric measurements. This may again be the case in northern India since the TOR is an integral of information throughout the troposphere and the few Indian data sets at the surface are influenced by local and regional effects. We are

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aware of these inconsistencies and mention them on pages 1466-1467. However, the surface data sets in the U.S., which represent a much larger data base than is available in India, do support the satellite findings. As stated in the text (and as presented at the IGAC Conference), our group is conducting studies to provide additional insight into the regional findings described in this study, but we believe it would be remiss to wait until the completion of these studies before the current work is published. We appreciate all the examples Dr. Kim has brought forth and agree that every one of them is worthy of further study. We hope that this paperŠs publication can be used as a linchpin to provide insight into future specific case studies and intercomparisons of these data with other satellite techniques and in situ measurements.

Responses to Dr. Clerbaux (General Comments): 1) Appendix B addresses the error budget. 2) We have not attempted to compare with GOME data because this would be a huge effort beyond the scope of the current paper (and we do not currently have the GOME data in hand). Again, we look at this effort as one of those "to do" in the future. 3) Once TES/AURA and IASI/METEOP start producing data, we would certainly hope to compare our residual methodology (proposed for OMI) with the direct tropospheric ozone measurements from these infrared nadir-viewing instruments.

Responses to Dr. Clerbaux (Specific Comments): 1. Introduction: We did not use TOMS/Meteor 3 because there were no aerosol correction information available when we started this study. 2. Section 2.1: We used Level 3; the term "gridded data" (from the Introduction, p. 1455, line 6) for TOMS implicitly means "Level 3." 3. Section 3.2: the 4 DU is bias error (i.e., lower than the ozonesondes); we insert the word "bias" in the corrected text. 4. Section 3.2: We cannot explain why there is no correlation between population in Jakarta and the TOR. 5. Section 4: reference to TES/AURA has been added. 6. The technical (typographical and other minor) errors have been corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1453, 2003.