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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Problems regarding the tropospheric O_3 residual method and its interpretation in Fishman et al. (2003)" *by* A. T. J. de Laat and I. Aben

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In this final comment, I want to address some points made by the Anonymous Referee (5 January 2004) and the Interactive Comment of de Laat (6 January 2004).

The Anonymous Referee states that he finds that it is difficult to tell how much of the tropospheric column seasonal change is due to increased ozone concentration versus change in tropospheric column thickness (i.e., tropopause height). De Laat and Aben argue that tropopause height variability is the primary reason for greater TCO amount during the summer. Furthermore, they say in the original paper (p. 5787) that boundary layer concentrations must be 125 ppbv to account for the observed seasonal cycle if tropopause height is constant. The original Fishman et al. (1990) study presented

a detailed comparison of the seasonal cycle of ozone from ozonesonde data with the seasonal cycle derived from the satellite technique. In that study, we (one of the co-authors was Logan) showed how the 500-hPa ozone concentration varied seasonally with ~ 50% enhancement during the summer at both Hohenpeissenberg and Wallops Island. The monthly height of the tropopause is also shown in this depiction (Figure 3) and it is true that the depth of the troposphere also increases during the summer. Both factors contribute to the increased TCO, but as pointed out in our earlier Interactive Comment (19 December 2003), the variability in tropopause height is not the dominant factor controlling TCO amount. The Anonymous Referee points out that the de Laat and Aben contention that satellite data adds little information seems valid. In the original deLaat and Aben paper, however, I feel that this claim needs to be supported through actual calculations and it would be beneficial to the scientific community to present such calculations rather than refer to them in the hand waving manner that they do.

In the Interactive Comment of deLaat and Aben (6 January 2004), they state the realization that validation of the Fishman et al. (2003) may not be possible based on currently available (ozonesonde) data. We agree with this assessment and we have presented as thorough a comparison as possible with the available data (e.g., Fishman et al., 1990; 1996; and Fishman and Balok, 1999; Creilson et al., 2003). They imply that the observed variations described in Fishman et al. (2003) simply fall within the magnitude of errors and should not be considered as real variations.

It is at this point that we strongly disagree. The most striking examples of the validity of this variation are the observations over northern Africa. The decreased amounts of TCO over small regional domains are directly attributable to increased elevation of terrain features in a region that is void of both local anthropogenic emissions or any variability of tropopause height. As stated in the paper, this climatological difference is based on an average of more than 1500 points. With such a robust number of points, it would be extremely unlikely that such a variation is a result of random error. It is

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primarily based on this finding that we feel confident that other regional features are real. However, proving such variability is not possible with existing data.

As we state in Fishman et al. (2003, p. 899) this variability could easily be validated with a pair of ozonesonde stations in northern Africa: One located at one of the high spots above 4000 m and the other at the background elevation, only a few hundred meters above sea level and only ˜1000 km away from the elevated site. Ideally, one could examine several hundred soundings from each site (to establish a statistically significant data base) and then see what the difference is between the TCO at both locations and relate that difference to what is observed in the satellite data. As also stated in Fishman et al. (2003), such a data set would give important insight into how well ultraviolet backscatter techniques see ozone in the planetary boundary layer.

Other measurement validation experiments designed to confirm the existence of regional enhancements such as those seen in northern India, east Asia, or the eastern United States, would be more difficult to conduct in light of the complicated nature of emission patterns that, when combined with potentially complicated meteorology, would produce ozone pollution patterns that would be extremely difficult to interpret and subsequently relate to satellite observations. Furthermore, not extrapolating our findings from northern Africa to speculate on the existence of regional enhancements resulting from widespread pollution without definitive proof would have shut down the opportunity to expand our level of scientific knowledge. Not publishing Fishman et al. (2003) certainly would have curtailed the spirited interactive discussion that has ensued during the past year.

Additional Reference

Fishman, J., V.G. Brackett, E.V. Browell, and W.B. Grant, Tropospheric ozone derived from TOMS/SBUV measurements during TRACE-A, J. Geophys. Res., 101, 24,069-24,082, 1996.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5777, 2003.