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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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deLaat has offered a response to our first interactive comment and it is clear from his description of what we have said that there is still some misunderstanding as to what we have done. In the beginning of his comment, he accurately summarizes what we have done in our example, i.e., we have taken two different TOMS values of 308 and 305 DU, respectively. He then imagines a hypothetical case where the actual TOR is 10 DU, despite the fact that the Logan (1999) climatology says that 30 DU is present. It is at this point that it is clear that he has misunderstood our method, when, later in the analysis, he says that TOMS=288 and 285, respectively. The TOMS and SBUV total ozone values cannot be changed in our technique; they are the inputs that drive our

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TOR calculation. Doing so completely violates the whole point of our technique.

What I think deLaat is trying to say is: What happens if the Logan climatology is incorrect? If the Logan climatology is seriously incorrect, then the accuracy of our method is greatly compromised. However, there is no better dataset available at the present time and Logan has incorporated as much data as is humanly possible to derive her climatology; furthermore, she has critically examined the quality of the measurements to ensure as much consistency as possible.

Here is an example of what using the SBUV data can do and why it is used. The amount of ozone in the tropical stratosphere is influenced by the quasi-biennial oscillation. From one year to the next, the average amount of stratospheric ozone at low latitudes may vary on the order of 15-20 DU (Ziemke and Chandra, 1999). Using SBUV data to filter out the large scale interannual variability helps to remove the effect of the QBO. However, what would happen if the tropospheric ozone amount varied by 20 DU? Then our technique would be off by a considerable fraction of that amount. I am reasonably confident that this scenario happened in 1997 when widespread burning greatly enhanced the amount of ozone everywhere in the tropical troposphere. Evidence for this anomaly is provided indirectly by Novelli et al. (2003) through his CO data which shows that the unusually high amount of burning in 1997-1998 significantly perturbed the abundance of CO at low latitudes. Note that CO may be a better tracer of the integrated amount of ozone than surface measurements of ozone at low latitudes because of the decoupling of the seasonal cycle at the surface from what takes place in the free troposphere (see discussion in Fishman et al., 1991). In this particular year, it appears that tropospheric ozone in the tropics, even far removed from the Indonesian fires, was enhanced by as much as 10-20 DU. When background values are enhanced, then the gradients generated by the local hotspots become relatively muted and the actual values are underestimated.

deLaat goes on to state, <=If our analysis is correct, and there is a direct dependence of the TOR product on the Logan climatology, then it should be shown what additional

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information is from the satellite information. <= I cannot think of a more explicit example than what was shown in the two tables presented in my first comment. If that is not a convincing comparison, then I do not know what is! As deLaat states, we are constrained by the Logan climatology to produce a globally averaged value that is consistent with her climatology. Indeed, if our technique produced a globally averaged value that was significantly different, then the technique would be in serious error.

In response to the specific questions raised on page S2182:

1. In our first interactive comment, we agreed that equation (8) is correct. At the points where SBUV and TOMS are identically equal, the calculated TOR defaults to the Logan climatology. This explanation leads into some of the reasoning that will be discussed answering question (3).

2. In reality, equation (11) may be a more accurate representation of the TCO than what we use. However, as pointed out in the above discussion, we do not use the SBUV data to compute the TOR; we use it to define how much ozone is in the stratosphere. We have examined the deLaat premise by computing how much interannual variability in tropospheric ozone would be present if only the interannual variability of tropopause height were considered. For this example, we examined the TOR over northern India for the month of May, which had the greatest range of monthly values (42.4-53.0 DU). Using the Logan climatology and the tropopause height information from the NCEP analysis (which ranges between 98 and 114 hPa), the range of variability was <2 DU (47.1-48.9 DU). Thus, the variability in tropopause height accounts for less than 20% of what is observed.

3. The 5-day average for the SCO fields was used to help identify the transient nature of tropospheric ozone. An example of this transient nature was illustrated in Fishman et al. (1986) over South America. At one particular spot, a variability of 15-20 DU was observed during a 4-day period (11-14 August, 1980). Thus, because of the relatively sparse nature of the SBUV measurements (700-800 daily SBUV measurements

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vis-à-vis 28,800 daily TOMS measurements), it is unlikely that the SBUV total ozone values were as high as the TOMS value for the particular day at the specific spot. Regardless of the variability of the stratospheric component (we never claimed that it was constant), the 5-day average as well as the considerable smoothing and interpolation that has to take place to generate a SCO matrix of 28,800 points (100 by 288), results in considerably more variability being present in the resultant TOR calculation because the daily TOMS are not smoothed. As part of our ongoing research, we are looking at alternate methods of defining the daily stratospheric ozone distribution, including the use of ancillary data sets and ozone profiles from other sources applied through neural networking (Müller et al., 2003).

4. It is not required for us to show how the <=Indian pollution plume<= fits in with our current knowledge of meteorological variability. In deLaat and AbenŠs discussion, the seasonal difference that they demonstrate takes place primarily at latitudes north of 35°N. As we noted in our first interactive discussion, the area we are interested in lies in northeast India and is considerably farther south than the region highlighted in deLaat and Aben. According to their analysis, the seasonal tropopause height difference between DJF and JJA is 1 km or less. Furthermore, the location of the emissions of nonmethane hydrocarbons and nitrogen oxides, the precursors to regional scale ozone production, are extremely consistent with the distribution of ozone in India and China that our satellite technique has revealed (Streets et al., 2003). If there is an influence on the amount of ozone by transport from the stratosphere, it could not be distinguished in our data. It is indeed possible that this kind of transport plays a role, but it is well beyond the scope of our original paper to delineate the source of the ozone observed by satellite.

In closing, I agree with their statement, <= . . . since there is no extensive validation of this dataset, . . . it is thus not possible to objectively determine how accurate the TORs are (and the small scale features). <= The same statement was made in conjunction with our earlier work published more than a decade ago (Fishman et al.,

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1990). The TRACE-A experiment validated the finding of high tropospheric ozone over the South Atlantic, a finding that was totally inconsistent with our knowledge of atmospheric chemistry (and transport) at that time. On the other hand, the enhancements found over northern India, China, and the other regions discussed in Fishman et al. (2003) are not surprising in light of our knowledge of ozone precursor emissions determined from both conventional emissions estimates (Streets et al., 2003) and satellite measurements of nitrogen dioxide (http://www.doas-bremen.de/no2_from_gome.htm).

The dataset published in Fishman et al. (2003) is the culmination of an effort that I have overseen for more than a decade; I am aware of the shortcomings of the methodology and I believe that these have been discussed thoroughly in both Fishman and Balok (1999) and Fishman et al. (2003). However, I am confident that the regional scale enhancements found in this dataset will be validated when the properly well-posed set of measurements becomes available for such validation. Until then, I embrace all other measurements and modeling studies that can be used to support or refute what we have found. Our hypothesis that the enhancements are due to anthropogenic activity has not been refuted by anything said in deLaat and Aben or in any of the subsequent discussion. It is time to move on and this can only be done in the true scientific spirit of diligent measurements and well-posed theoretical studies.

Additional References

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