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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

Anonymous Referee #2

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Global measurements of ozone in the troposphere would be a boon to our understanding of the photochemistry and pollution of that region. Satellite remote sensing is the obvious way to obtain such measurements. Unfortunately, satellites must look through the 90 percent of the ozone that is in the stratosphere to see the 10 percent that is in the troposphere. Fishman invented the first method to use existing satellite measurements to infer the tropospheric content of ozone. He noted that, in the tropics, SAGE made occultation measurements of ozone throughout the stratosphere down to the tropopause. He subtacted the integral of these measurements over the stratosphere from the total column content measurements of ozone from TOMS to get a tropospheric ozone residual. This technique was limited by the restricted sampling of the occultations by SAGE. A number of authors have followed this original idea and

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invented clever alternative methods to remove the stratospheric ozone content from the total column ozone measurements to infer a tropospheric ozone content.

Fishman and coauthors have recently published a number of papers using the profile information from the SBUV nadir-viewing instrument and ozonesondes in an attempt to improve the coverage for the derivation of tropospheric ozone and to extend it to higher latitudes. The difficulty with using SBUV data has been pointed out originally by Fishman. That is, the altitude resolution near the tropopause is not very good. The SBUV retrieves ozone in broad Umkehr-like layers that are labeled from 1 to 12 starting at the surface. These layers are a factor of 2 wide in pressure except for the bottom layer which is a double layer from about 1000 hPa to about 250 hPa. SBUV has information, that is more or less independent of apriori above the density peak. It has little or no information about the profile shape in the lowest layers below the ozone density peak, but does measure an accurate sum for these layers through what is effectively the difference between the total ozone measurements and the upper layer measurements. This fact is used by Fishman and by de Laat and Aden. They use the sum of layers 1 to 3 (1000 hPa to 64 hPa).

De Laat and Aden argue that Fishman's method is returning tropospheric ozone columns that are the result of the information from the ozonesonde climatology, plus terrain height, plus tropopause height. They claim that the satellite data is adding little to the result. Their basic argument seems valid to me. They work through Fishman et al.'s equations to demonstrate their argument. I found it difficult to tell for sure what amount of information might remain from the satellite measurement itself. Their demonstration using the ozonesonde climatology and the model tropopause and terrain heights shows that many of the features derived by Fishman et al. can be obtained without the satellite data. This points out a major difficulty in the interpretation of tropospheric ozone columns outside the tropics. It is difficult to tell the difference between near-surface pollution and a higher tropopause from the column measurements.

I believe that de Laat and Aden's method of using the ozonesonde climatology, terrain

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height, and tropopause height from a meteorological analysis could form the basis of providing a daily reference for tropospheric ozone. This reference could be compared to a set of ozonesonde measurements to determine the rms differences in tropospheric column amount between the reference and the sondes. Analyses using satellite data, such as that by Fishman et al. or others could then be compared to the same sondes to see by how much they reduce the rms difference in tropospheric column ozone amounts. It is my belief that methods that use the satellite data will improve the comparisons to sonde data, but the improvements might not be very large compared to the reference using only the climatology, terrain heights, and tropopause heights. De Laat and Aden could improve their paper by presenting a comparison of their climatology plus ECHAM to a base set of sonde data and giving some statistics.

An issue with the derivation of tropospheric ozone column outside the tropics is the interpretation of the meaning of the column. As de Laat and Aden point out, much of the variability is due to variations in the thickness of the troposphere. Attempts to interpret this as pollution events need a product that is converted to some sort of mean mixing ratio over the layer. I believe if this were done, then many of the features seen in the column would disappear. This would occur even with their suggested improvement in the Fishman et al. method.

Overall, I think that this is a sound paper that raises interesting questions about how well we can derive global tropospheric ozone columns from our existing satellite data. The paper calls into question the conclusions reached by Fishman et al. regarding Asian pollution deduced from TOMS plus SBUV. I agree that these conclusions should be questioned. It is not clear that the proper interprestation supports them even if they turn out to be correct.

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