Review of “Ozonesonde climatology between 1995 and 2009: description, evaluation and applications” by S. Tilmes et al., submitted to APCD.

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December 21, 2011.

Tilmes et al. present an analysis of ozonesonde data for 1995 to 2009, and discuss their results in the context of regional averages, some comparisons to MOZAIC and surface data, plots of time series that include earlier data (but no serious trend analysis), and some limited applications to model evaluation. I would concur with Reviewers 1 and 2 that this is a disorganized and poorly written manuscript. The other serious problem with this paper is that it contains little new science, and does not properly acknowledge prior work on the topics covered. This was pointed out by the other reviewers, but here I will give more specifics, as much of this work on the data mimics my own earlier analyses.

I presented an analysis of ozonesonde data in Logan et al. (1999a, 1999b, hereinafter L99a and L99b). In those papers, I used data for 1980-1993 for most stations, 1980-1995 for the Japanese stations, and all available data for tropical stations. The data were analyzed on pressure levels, and relative to the tropopause height on a grid of geometric altitude. Monthly means, standard deviations, and number of soundings per month were provided to the community for each station, in both coordinate systems. In that paper, I also examined the variability in ozone as a function of height, showed cumulative probability distributions, showed how the seasonality in ozone changed relative to the tropopause, as well as compared ozone in different geographic regions but similar latitudes on pressure levels, etc etc. I also did my best to show how my work built on earlier work in the literature. With colleagues at NASA/Goddard, I collaborated on developing a zonal mean climatology, averaging the sonde station data in 10 degree latitude bands for 1988-2002 (McPeters et al., 2007). With modeling colleagues, I collaborated on evaluation of the vertical gradient in ozone across the tropopause using my updated analysis of the results in L99a and L99b for 1985-2000 for extra-tropical stations, and all available tropical data (Considine et al., 2008). In that work, we used the sonde analysis on pressure levels and relative to the tropopause height to evaluate ozone profiles across the tropopause in a model with coupled tropospheric and stratospheric chemistry. Currently, I routinely evaluate versions of the GEOS-Chem and GMI models with the sonde station means updated to 1990-2005, as well as with means for the MOZAIC airports since 1994, an in-house exercise that one does not publish. A simple updating of monthly means for ozonesonde data at various stations is straightforward, if tedious. I routinely provide my updated climatology to anyone who asks, including new stations as appropriate. Furthermore, I told the lead author on this paper that I had updated the sonde files when we discussed her poster at the Workshop on Tropospheric Ozone Changes in Toulouse in April 2011, 6 months before submission of this paper.

Tilmes et al. seem to have duplicated my earlier analysis in L99a and L99b for 1995-2009. From reading this paper you would not know that my earlier work analyzed ozone relative to the tropopause height (or that anyone had), or that I presented cumulative distribution functions, etc etc. I found myself wondering if the lead author had actually read my papers. The authors also do not seem aware of Considine et al. (2008). Many other papers analyzing sonde data focus on the seasonal, vertical, and spatial distribution of ozone, relative variability in the ozone profile versus altitude, trends, etc, for example those by Oltmans, Thompson (SHADOZ data), Newchurch, Tarasick, Staehelin's group, Bill Randel, and for high latitude stations, Kivi et al. (2007, JGR), not to mention the WMO/UNEP Assessments of Ozone Depletion that updates trends every 4 years – and I am sure I am missing many others in this extensive literature. There are numerous papers using sonde data to evaluate 3-d models, both individual models and intercomparisons, over the past almost 15 years. It would be had to tell this from the paper by Tilmes et al.

It is a challenge to write a good paper about such a well understood topic as the distribution of ozone in the troposphere and stratosphere based on sonde data, and the use of such data in model evaluation. First the authors must acknowledge what others have done that they are building on, and then they must focus on what is new in their study. They must also explain clearly exactly what they have done. As a prerequisite, they must also know what the main focus of their work is. In my opinion, the paper submitted by Tilmes et al. is deficient in all of these areas. In my review, I suggest areas they could refocus their efforts on a new paper or papers.

In advising any authors on how to improve their paper, I need to know what their main focus is, and this is hard to ascertain the present submission. Right now, the text is as follows: (1) an introduction; (2) some text about the sonde data (without key details about the data analysis), some information about grouping stations regionally, as well as a little bit about tropical circulation; (3) discussion about the variability of ozone using the Hellinger distance to compare distributions, how the tropopause was defined, discussion of ozone distributions above and below the tropopause; (4) some comparisons of sonde with MOZAIC and surface data using seasonal means and regressions only; (5) some time series plots and differences between mean ozone in 2000-2009 and 1990-1999; (6) altitude distributions on the pressure grid and relative to the tropopause; (7) comparisons to mean models, and some results using these Hellinger distances.

What is new in this paper, apart from using monthly means and medians for sonde data for 1995-2009? There is some focus on comparing cumulative probability distributions (CDFs) in regions using the statistical concept of a Hellinger distance, for both station to station comparisons in a region with respect to the tropopause height (Section 3, 2 figures), and with models (Section 7, 3 figures) . Sonde data are grouped within regions, some of which are appropriate, some of which are not (more below). There is a comparison by region of sonde and surface data by way of correlations of long-term seasonal means (in 500 m increments), where a high value for r merely indicates that the two sets of data have the same seasonal cycle, an overly simplistic approach (Section 4).

In terms of what is not new in this paper, there is a section (5) on the time evolution of ozone that shows time series of seasonal means (interspersed with comments about the seasonality of ozone, features that have been well known since the mid-1980s), and comparisons of median profiles for 1990-1999 and 2000-2009. This is an uncritical analysis, offers no new insights into the time evolution of ozone, and makes almost no citations to the extensive literature on the topic (from the WMO Assessments to the many individual papers cited therein). It is not my job as a reviewer to provide a list of literature on the time evolution and trends of ozone in the troposphere and stratosphere. My paper in review at J.Geophys. Res. gives an example of the type of critical analysis of ozone time series needed in a region (Europe in this case), and I collaborated with the data originators. The consensus of the Tropospheric Ozone Workshop in April (attended by Tilmes) was that this type of analysis is needed for other regions, and working groups were set up. For the stratosphere, such an analysis must involve satellite data, and there are on-going efforts following a Workshop on Past Changes in the Vertical Distribution of Ozone held in January 2011 in Geneva.

Section 6 of this paper presents median seasonal profiles for sonde stations grouped by region, in similar manner to figures in L99a and L99b for monthly mean profiles (and in other papers), with the addition of more recent sites. In this paper the half width of the distribution is used to show variability at each level, rather than the standard deviation as commonly used, and some information is given on year to year variability of seasonal means. Except for the use of different statistics and a few more stations, we do not learn much more about the vertical distribution of ozone as given in L99a, L99b, papers on SHADOZ by Thompson and by others. It is well known that ozone is most variable (in relative terms) around the tropopause in the extratropics, as shown in Figure 9 (in L99a and papers by other authors). I did not learn anything in Section 6 that I did not already know from the extensive literature on ozone, which I try to keep up with.

I will now focus my review on the new material in this paper. Section 3 shows (Figure 2) probability distribution functions (PDFs) for seven sonde sites in Europe above and below the tropopause, and compares them to the regional means using the Hellinger distance plotted vs. the percentage difference between the regional mean and the station value. The PDFs are very similar to each other. (In L99a, I showed CDFs for one station as an example, below, at, and above the tropopause.) Figure 3 shows the results for other regions, using only the plot with the Hellinger distance, not PDFs. Ozone distributions are very similar for northern high latitude regions. However there are large differences among distributions for sites grouped together for Japan, the tropics, and southern mid-latitudes. This is hardly surprising. The Japanese stations are located from the mid-latitudes (Sapporo, at 43 N) to essentially the sub-tropics (Kagoshima, 30 N), with very different tropopause characteristics, and a large gradient in ozone between the stations (see L99a, Figures 12-14). These three sites should not be grouped together. Because of the large difference in the altitude of the tropopause between Kagoshima and Sapporo in winter (~7 km) and in spring and late autumn (~4 km), the layer that is 3-5 km above the tropopause will be in the lowermost stratosphere at Sapporo (as defined by the tropopause to the 380 K surface), and above the tropical tropopause (or 380 K) above Kagoshima. Thus one can explain the large spread shown in Figure 3 in these seasons. I expect that similar arguments hold for the southern mid-latitude regions, where the latitudes range from 38 S to 54 S. The use of Hellinger distance may be useful for comparing CDFs, but first the authors must first think through the characteristics of the locations they are grouping together, and recognize that they may be comparing completely different parts of the atmosphere when analyzing data relative to the tropopause height. I was able to work out why the results for Japan looked the way they did, but this is the job of the authors, not the reviewer.

Similarly, there are large spatial gradients in tropical tropospheric ozone (the so-called wave-one). It makes no sense to expect the sites to have similar ozone distributions in the upper troposphere, relative to the thermal tropopause (which is essentially the same height throughout the inner tropics, see review by Seidel et al. JGR, 2001). One only has to look at monthly mean profiles and variability for Watulosek and Ascension to figure that the distribution of ozone a few km below the tropopause will be very different at these two sites (and to have read the SHADOZ papers). There are numerous papers the wave-one, and also about the uniformity of stratospheric ozone in the tropics.

Section 7 of the paper is about model evaluation using the climatology for 1995-2009. First the authors show the ozone seasonal cycle at 800, 400, and 200 hPa, and the vertical seasonal profile for four northern extra-tropical regions, compared to the mean of the models that participated in the HTAP comparison. This is a standard approach used for well over a decade using single sonde sites and single models around the globe. It has also been used in model intercomparisons, showing the individual model results (e.g., Stevenson et al., JGR, 2005). The use of a multi-model mean is an inappropriate construct, as it tells you nothing about how well individual model simulate ozone, or what the spread is. And using a correlation coefficient for the seasonal cycle merely tells you how well the model matches the seasonal cycle – a very weak test, and a simplistic use of statistics. A paragraph cannot do justice to evaluating the HTAP models, and the authors seem unaware of the published papers on the HTAP models (e.g, Jonson et al., ACP, 2010, Fiore et al., JGR, 2009, and likely others).

The authors then show the same sort of model-mean comparisons (for 200, 100, and 50 hPa) for the CCMVal models, many of which treated only stratospheric chemistry. They also show the results relative to the tropopause height, the approach used by Considine et al. (2008). A much more detailed analysis of the CCMVal models is given by Hegglin et al. (2010) who show individual models, and use MLS ozone data, and by Strahan et al. (JGR, 2011). Both those studies offer vastly more insight into the performance of the CCMVal models (and reasons for discrepancies) than given here. I urge the lead author to read those papers, and think about what she is doing that is new.

Perhaps the only new approach shown here is to compare the PDFs of the various models for one region (West Europe) above and below the tropopause, showing that a few have very different distributions from others. The Hellinger distance is used for various regions, again with only a description of the plots, and no insight. Given the papers I mentioned above, the authors need to decide what added value their work has, and so far I see very little, in terms of evaluating the CCMVal or HTAP models. In terms of using PDFs to evaluate ozone in a stratospheric model relative to the tropopause they should take a look at the paper by Rood, Douglass et al. (JGR, 2000).

Summary.

This paper is completely unacceptable for publication. There is very little new science in it. In terms of making the updated files for the ozonesonde data for 1995-2009 available to the community, a simple Technical Note (like Hassler et al., ACP 2008) might be the best way to go. If the authors wish to write a new paper on the temporal evolution of ozone in either the stratosphere or the troposphere, they have a lot more work to do. If they want to write a paper about what can be learned from the PDFs of ozone data and models, they have a lot more work to do. In either of these areas, they must first acquaint themselves with the existing literature, and see what is new that they can add from their work.

In terms of how to craft a paper, this one is sorely lacking. I shouldn't have to say this, but the convention is to include a section that summarizes which data you use, and how you treat it. In the present paper, this topic is scattered, with information in the Introduction (with details of web-sites), in Sections 2 and 4, Appendix A, and the Supplement. Other Reviewers have given detailed comments on some of this. I would only add that you must explain how you deal with double tropopauses, common especially in the sub-tropics (see paper by Bill Randel). I presume that the tropopauses are computed on geometric heights?

I could easily spend another day (or two) criticizing this paper sentence by sentence, but that should be the job of the mentors or co-authors of the lead author.

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