Atmos. Chem. Phys. Discuss., 9, S651–S655, 2009
www.atmos-chem-phys-discuss.net/9/S651/2009/

Atmospheric Chemistry and Physics Discussions

ACPD

9, S651–S655, 2009

Interactive Comment

Interactive comment on "Long-term changes in UT/LS ozone between the late 1970s and the 1990s deduced from the GASP and MOZAIC aircraft programs and from ozonesondes" *by* C. Schnadt Poberaj et al.

D. Parrish (Referee)

David.D.Parrish@noaa.gov

Received and published: 6 March 2009

General comments:

This paper uses data sets collected in the GASP program in the late 1970s and the MOZAIC program in the 1990s to quantify changes in ozone in the upper troposphere/lower stratosphere over the approximately two decades between the studies. A rather heroic amount of work has gone into analyzing the data as completely and appropriately as possible, and in comparing the aircraft data with sonde data. This paper is a useful addition to the body of literature that attempts to document the temporal evolution of ozone, particularly in the troposphere, over the past few decades.

The major problem with the paper is that it is much longer (64 pages) than can be justified by the analysis and discussion presented. The detailed analysis of the sonde data is secondary to the thrust of the paper, and, while important, will be of interest to only a very small community of specialists, and can be included in an appendix. Overall the paper must be greatly shortened with the major conclusions clearly supported and presented. Specific suggestions in this regard are given below.

Specific comments:

1) Approximately 4 1/2 pages are used to present the summary and conclusions. A more concise presentation will be more useful; in particular:

- pg. 2472, conclusion beginning on line 19 would be more informative if it stated that the "dfferences may underestimate longer-term trends" since (if I understand correctly) ozone levels may have been anomalously high in 1978.

- The 4 bulleted conclusions beginning on pg. 2472, line 22 should be more succinctly stated with just the conclusions established by evidence presented in this paper. Each includes (reasonable) conjecture about the cause of the observed changes, but no evidence has been presented in this paper to support these conjectures. The discussion earlier in the paper is adequate without repeating it in the conclusions.

- The conclusions beginning on page 2474 regarding the aircraft-sonde comparison should be limited to a couple of short sentences, consistent with the suggestions below regarding this entire analysis.

2) One issue should be addressed in the introduction. In the paragraph beginning on pg. 2438, line 9, the authors summarize previous studies of ozone trends. Their summary is that "Long-term tropospheric ozone changes largely differ in magnitude and sign in different regions of the world:" It is true that the derived trends largely

ACPD

9, S651–S655, 2009

Interactive Comment

differ, but this may be due to difficulties in discerning the trends in the face of imperfect measurements and large interannual variability. In fact such difficulties are at the center of a great deal of discussion in the present paper. It should be made clear in the introduction that much of the variability in reported trends may have arise from our inability to adequately quantify trends rather than true variability of ozone trends in the atmosphere.

3) One issue is somewhat subtle, but I believe important. The paper intends to characterize changes in ozone between the 1970s and 1990s. If a quantified change is found to be small enough that it includes zero within the confidence limit that can be placed on the quantification, then the authors term it as insignificant. However, such a finding is certainly significant in the sense a change near zero is important information. From this perspective, the crosshatching in some cells of Figs. 1, 6, 11-13 should be eliminated. All of the change determinations in these figures are significant, and the color scale quantifies these changes. However, it would be useful to indicate the magnitude of the confidence limits on the changes, both for those near zero and those that are relatively large.

4) Figure 1 shows a great deal of variability between adjacent 10 x 10 degree cells that, in many cases, do not seem physically realistic when the rapid zonal flow patterns of the upper troposphere are considered. There is no conceivable mechanism for maintaining such variable ozone gradients under the rapid air transport in the upper troposphere. Quite likely, this variability represents statistical noise. I think that it is useful to retain Fig. 1, but the discussion of regional differences in ozone trends should be based upon the statistically more robust summaries presented in Fig. 2, and the confidence limits presented there must be carefully considered in all discussion.

5) The main thrust of the paper is the comparison between the two aircraft data sets, and the paper should focus nearly exclusively on this comparison. The discussion of ozone sonde data is valuable, but should be very secondary in this paper. I suggest moving Sections 2.2, 3.3, and 3.4 to an appendix, and discuss this comparison only

ACPD
9, S651–S655, 2009

Interactive Comment

briefly within the paper.

6) Section 2.2 requires about 3 1/2 pages to describe the treatment of the ozonesonde data. I am not the right reviewer to judge if the treatment is robust and consistent with the treatment employed in the many other papers in the literature that have worked with these data sets. However, the discussion of "homogenized time series", "corrected by linear scaling with column ozone measurements", "ground calibration factor", "corrections for box temperature, altitude error, SO2 interference, and background current", "pump correction values at every pressure level are adjusted", etc., certainly raises concerns that the analysis may be systematically uncertain to a significant extent. Are there any additional assurances that can be given regarding the lack of importance of such uncertainties? (I am thinking of something like a comparison of results from published analyses with the same parameters extracted from the present data sets.) Such comparisons would help to establish the confidence limits that can be placed on the present sonde results.

7) The term "climatology" is used in several places (e.g. Pg. 2443, line 1; Pg. 2447, lines 8 and 13; and many others). It needs to be carefully defined. The data sets are too limited to really establish a climatology in a general sense. I assume the authors really mean something like an average over all of the available data. Please clarify this term.

8) Section 3.1 uses nearly 2 pages (2449-2451) to compare the UT ozone trends they derive with trends that others have derived from surface measurements in North America and Mace Head, Ireland. This discussion should be greatly shortened, since the connection of regional surface ozone trends, which are representative of the continental boundary layer, with ozone in the upper troposphere is tenuous at best.

9) The paragraph beginning at the bottom of pg. 2451 discusses the possible influence of aircraft emissions, but only in the context of NE USA and the Atlantic regions. If this discussion is included, it should be given from a more global perspective; i.e.

ACPD

9, S651–S655, 2009

does growing air traffic patterns fit with the observed ozone trends on a worldwide perspective?

10) The discussion of the trends over Europe (pg. 2452-2453) is highly speculative. Figure 2 indicates no significant trends, except perhaps marginally in MAM. Yet the discussion starts with the sentence "Over Europe, increases are seen in all seasons except in SON (Fig. 1)." Then the discussion focuses on different trends seen in different sub-regions of Europe, which I suspect are statistical artifacts. I suggest that this discussion be shortened with an emphasis on the apparent disagreement between the present work and the Zugspitze trend.

11) Section 3.1 devotes nearly 4 pages to discussing the trends over the Middle East, Japan, and Southeast Asia. This discussion should be greatly shortened, emphasizing only the most important, most statistically robust findings.

Technical Corrections:

1) Pg. 2437, line 8 - move "both" to after "determined by".

2) Pg. 2437, line 22 - change "possible causes" to "a possible cause"

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2435, 2009.

ACPD

9, S651–S655, 2009

Interactive Comment