

***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.***

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Reply to comments by David Parrish:

We have updated the manuscript according to the recommendations by all reviewers and the editor. Overall, the paper has significantly been reduced in length. This has been accomplished by creating appendixes (Sects. 2.2 and 3.4), and by shortening the rest. We have not completely moved the analysis of the sonde data to an appendix, as to our minds, this part of the paper is very valuable not only for the community of ozonesonde operators, but also to all people who use the sonde data for data analysis, .e.g. trend analysis, or for global modellers who validate their models using these

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observational data. However, we agree to put the focus more on the part of the aircraft changes and to shorten the comparison of aircraft with sonde data. For this reason, we have significantly shortened the comparison with sondes and shifted Sect. 3.4 to an appendix. The conclusions have been rewritten to become more concise and more clearly presented.

Other significant changes that we have carried out are listed below in a point-to-point reply:

1) The summary and conclusions have greatly been shortened. In particular, we have adopted the recommendation to more succinctly state the bulleted conclusions removing all parts that try to explain the causes of the observed changes. Pg. 2472, line 19 ff. has been changed as recommended. The conclusions regarding the aircraft-sonde comparison have been shortened.

2) Whereas the referee recommends the authors to clarify in the introduction that parts of the regional variability in ozone trends may be due to imperfect measurements and thus difficulties to adequately quantify trends, Jennifer Logan replies that she does not have a problem with different trends in different regions, as they are not necessarily ungeophysical. In fact, in our opinion, both views may be right. On one side, it makes sense that trends differ in different regions because underlying causes for trends differ, such as e.g. regional differences in industrial development and related emissions of pollutants. On the other side, quantification of trends may actually be difficult in some cases due to imperfections of the ozone sensors or their improvements over time, as stated by the referee and discussed in the text. Therefore, we have changed the according part in the introduction including a sentence on potential difficulties in quantifying trends. Furthermore, we have removed the above-cited sentence on differences of long-term ozone changes in different regions of the world. Instead, we introduce the very same paragraph with the following sentence: "The current state of knowledge of tropospheric ozone trends based on ozonesonde measurements can be summarised as follows: ...". The subsequent sentences clearly indicate themselves that trends are

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different in different regions of the world.

3) Concerning the statistical significance discussion in the paper, it is important to distinguish between Fig. 1, where a t-test was used, and Fig. 2, which shows confidence intervals. While the t-test searches for means that are different on a statistical basis, confidence intervals look for the reliability range of an estimate. We agree with the reviewer that in the discussion of Fig. 2, even changes near zero are significant in the sense that this is important information. We have updated the relevant text passages accordingly. However, in Fig. 1, since it is not possible to add confidence intervals to the differences displayed in a geographical distribution graph, we have chosen to use a t-test. The latter, as also Jennifer Logan states, is needed to show the reader where changes are significant in a statistical sense. For the boxes without cross hatching, the differences are statistically not significant.

4) We have rewritten the manuscript now concentrating on the regionally averaged results. Fig. 1 is now only discussed qualitatively, whereas the main focus and the quantitative discussion are laid on Fig. 2.

5) See general comment above.

6) There are actually published analyses on the comparison of ozonesondes against other devices, e.g. the JOSIE 1996 campaign which is mentioned in Appendix B under C.. There, the currently used ozonesondes were tested against a UV photometer in the laboratory (see also Smit and Kley, 1998). However, discussing the confidence limits of ozonesonde measurements in detail goes beyond the scope of this manuscript.

7) We agree with the reviewer that the term "climatology" may not be correct in the sense that an average over three to five years of data may not be termed climatology. Therefore, we have substituted the expression by "multi-annual average" everywhere in the text.

8) As also recommended by Jennifer Logan, we have greatly shortened the compari-

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son with surface measurements. However, since there may be a connection with UT changes (as discussed for East USA), we have not removed the discussion completely, but confined it to a qualitative discussion.

9) In fact, growing aircraft emissions are a worldwide problem and affect UT/LS ozone everywhere where aircraft fly and due to transport also further away from the flight corridors. However, because over the Atlantic, high air traffic density occurs remote from other sources, the effect of aircraft emissions is most likely expected to be measurable within the North Atlantic flight corridor, and this is why we discuss it in the context of the changes seen over the ATL region. We have added a sentence to motivate why we discuss aircraft emissions as potential cause for ozone changes over the Atlantic to Sect. 3.1. In addition, we have removed the numbers for NE USA, because although long-term changes over both regions are similar, it is more likely than over the Atlantic that there may be other processes than aircraft emissions influencing changes there.

10) The reviewer is correct. We have greatly shortened and corrected the discussion for the Europe region (Sect. 3.1).

11) Concerning long-term changes over the Middle East, India and Southeast Asia, we have not followed the recommendation of the reviewer. As also J. Logan states, the increases seen there are particularly interesting, since there are no (reliable) prior data. We have shortened the paragraph about changes in Japan (see Sect. 3.1).

All technical corrections have been considered.

Note that responding to Jennifer Logan's and the editor's concern about the used cutoff values for tropospheric ozone, one side effect was that we found out that the assignment of tropopause information to the MOZAIC data went wrong in some places (when there were more than one aircraft measuring in one minute). To mend the problem, we have recalculated all MOZAIC averages. While there was no major quantitative effect on the MOZAIC averages, still some results have changed marginally and some numbers given in several Figs. have slightly changed (e.g. Fig. 5).

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

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