

***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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This paper builds on an earlier analysis of the GASP aircraft data by the same authors (Schnadt Poberaj et al., ACP, 2007). It extends the analysis to examine changes in ozone in the UTLS. The objectives of this paper are (1) to examine changes in ozone between the GASP data for 1975–97 and the MOZAIC data for 1994–2001, and (2) to determine if these changes are consistent with those derived from ozonesondes, the only other data set covering this time span in the UTLS. I suggest this is stated clearly in the abstract

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This analysis of the GASP/MOZAIC changes is particularly valuable, as all prior analyses of trends in ozone above the surface since the 1970s have had to rely on sonde data. Given this fact, the comparisons of the changes derived from GASP/MOZAIC data to those derived from sondes are essential. To understand why the sondes and the aircraft data give different changes in ozone, the authors have to go into some detail about the idiosyncrasies of the sonde data, and the results from the various intercomparisons. However, they need to be more succinct about it, as pointed out below. In some cases they are repeating information in their 2007 paper, and in the literature, rather than briefly summarizing it.

Overall, I like this paper and recommend publication in ACP after revision. The authors should do everything they can to shorten it, particularly when they repeat information that is in their 2007 paper. The paper is so wordy, that it is easy for the reader to lose interest, and the paper contains interesting results.

Since this review is written after those of D. Parrish and O. Cooper, it will be clear that sometimes I endorse their comments, but not always.

Overall comments.

1. Cooper is concerned that there is a fair weather bias in the sonde data. This is based on his own analysis of data from Wallops Island. However, Wallops does not adhere to the policy followed by most sonde stations (including those in Canada, Europe and Japan), which launch their sondes on Wednesday, the geophysical day for weekly measurements. The long-term European stations obtain data on Monday, Wednesday, and Friday if they launch 3 a week, and on two of these days when launching 2 a week. So there should not be a fair weather bias in these data.

2. I concur with both reviews that the paper should focus on the regional results more than the results for the individual boxes, but retaining Figure 1 (showing the trends for boxes) is fine with me. There are inconsistencies between the trends in boxes and the regional trends and the latter should be more robust. Figure 1 should retain the

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indication of significant changes. This is the only way the reader can tell if there was a significant change in ozone (the hypothesis being tested by the significance test). With no cross hatching the reader then knows that the data imply no change in ozone. The figure shows that the data cannot be used to infer a significant change less of than 10% on the 10x10 grid.

The text should refer only to regional trends, and only to significant trends, when giving a value for the change in ozone. The text comments in places on insignificant trends as if they are significant, instead of saying there is no evidence for a change in ozone. Section 4, the summary, should be rewritten entirely, with the focus on either significant changes in ozone, or on no change in ozone. For example, the present text (p. 2472) discusses 'moderate changes in ozone' in autumn and winter (for the Middle East), while Figure 2 shows no significant change in ozone in those seasons. The authors must check all their comments on regional changes against their results in Figure 2, and make the text and figure self-consistent.

3. Like Cooper, I am concerned about the filtering of the UT data (p. 2446). The text should address whether the results change if the filtering is not done.

3. Add the boxes used for the regions to one of the maps in Figure 1, or give the latitudes/longitudes in a table. State how the regional averages were formed.

4. It is not necessary to compare the UT trends in detail with surface trends in a given region, as they will not necessarily be the same, as noted by Parrish.

5. Of the results, the increase in the Middle East region are particularly interesting, as there is no prior data for this region; also the increase over India is of great interest. The only previous data for India were from sondes, and early Indian sondes were outliers in the intercomparisons.

6. In comparing the aircraft and sonde changes in ozone, the regions should be chosen to match the locations of the sonde data. In Europe, the sonde stations are at 47-51

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N, so the region should not be 35-55 N but rather something like 42-57 N.

Figure 2, 7 and 9 must be enlarged, as it is impossible to read the numbers on Figure 7 and difficult on Figure 9.

Other comments.

1. I do not have a problem with the concept of different trends in different regions in the troposphere. (p 2438). Indeed, Figure 2 shows this is the case for the GASP/MOZAIC data. It is not necessarily ungeophysical.
2. 2439, l. 4. Harris et al. (2008) did not discover that Pinatubo caused low ozone in 92/93. Either cite the original papers from the mid-90s, or WMO, 1999.
3. 2433, l. 18-2444, l. 7. You do not need to state everything that was done to the Uccle data to homogenise it, just refer to the paper that discusses it.
4. 2444-5. There is no need to repeat the detailed discussion of why you shifted the sonde data by 150 m (or 250 m) as this is in the 2007 paper. Just state why you shifted the data and by how much, and refer to the earlier paper, and explain what you did to the Uccle data. This needs 3-4 sentences at most.
5. 2457, l. 6. Schultz et al. gives trends for biomass burning in SE Asia + Indonesia, and it's not clear if biomass burning has increased significantly in SE Asia. In Indonesia, biomass burning is very dependent on El Nino and the Indian Ocean Dipole. Note that these trends are based on a model.
7. 2464, line 22. By 'confined' I think you mean 'limited'. This same problem occurs elsewhere.
8. 2465, Section B, first paragraph. Give the conclusion of the Tarasick paper that is relevant to your study in a couple of sentences. Similarly in Section C just give the results, don't elaborate on what they did in detail.
9. 2469, l. 4. Replace 'has been discussed controversially' with 'is controversial'.

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