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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A UT/LS ozone climatology of the nineteen seventies deduced from the GASP aircraft measurement program" *by* C. Schnadt Poberaj et al.

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This paper provides an analysis of the GASP aircraft data, a program to measure ozone on 5 aircraft that took place in 1975-1979. The program, the measurement technique, and the analysis of the GASP data are described in great detail. I believe that this detail is given to establish the credibility of the data so that they can be used to examine changes in ozone since the late 1970s in a future publication. There are very few papers in the literature analyzing the GASP data, and they do not present a climatological overview, which given in this paper. (I dusted off my files, and extracted a yellowing reprint (Nastrom, 1979) to remind myself of the early work on the GASP

data.)

The authors screened the GASP data, and then analyzed the measurements relative to the tropopause (as defined by PV, from ECMWF), primarily in a potential temperature and equivalent latitude (EL) framework. Comparisons are made to the more recent MOZAIC data, and to sonde data from the 1970's. Clearly a great deal of work has been done, but major downside of the paper is that I learned very little about the ozone distribution that I did not already know from analyses in the literature. One thing that is clear from this paper is that ozone has changed very little from the late 1970s to the 1990s, as evidenced by Figure 4 comparing the GASP data by region with MOZAIC data, except perhaps over India. Yet trends are clearly to be the focus of a follow-up paper, not this one, and this result is hardly discussed.

This paper is very long (65 pages) with little new science. When reviewers recommend that the authors shorten a paper, it usually has no effect. Nevertheless, that is my recommendation. This paper is extremely wordy, and could be cut substantially with the same information content, most likely by a co-author who did not write the submitted version. The authors must make the effort to do so.

What is new in this paper?

1. The credibility of the GASP ozone data seems to be established. GASP employed a method still in use today (a Dasibi UV instrument). I am not the right reviewer to evaluate the calibration methods etc. The data treatment and analysis seems to have been done carefully and thoughtfully.

2. GASP gives more comprehensive data over the northern Pacific Ocean than available from sondes (in Japan, Hawaii, and northern California), aircraft campaigns (e.g., PEM-West A and B, PEM-Tropics A and B), or MOZAIC (no trans-Pacific flights). Figures 6 and 7 provide new information on the spatial and seasonal distribution of ozone across the Pacific in the upper troposphere.

ACPD

7, S1671–S1677, 2007

One comment on this analysis: Figure 6 shows the spatial pattern of ozone over the Pacific gives the number of measurements in each grid box. The number of flights that provided data in each box would give more quantitative information on how representative the data are.

3. The GASP data are used to evaluate the altitude offset in the sonde data, which results from the time lag in the electrochemical measurement as ozone is pumped through the sonde. By comparing to the vertical gradient in the sonde data (all relative to the PV defined tropopause), they find that the estimated offset for the sonde data of ˜150 m is about right.

Other comments:

The referencing is a bit odd, in that is often comes in the discussion, rather than when older data/ideas are first discussed. In the introduction, there should be citations to the recent WMO/UNEP reports (p3453/22), overview papers on the sonde data, e.g., Logan, 1985, 1999 (p3454/2), and the SPARC report that addresses data quality issues (cited by the authors as WMO, 1998, later in their paper) (p3454/8). There were early intercomparison studies that addressed data quality issues, albeit with limitations, but the reader wouldn't know this from p3454.

The GASP papers are not referenced when the program is first mentioned (p3454/26).

p3465. High correlations are found between PV calculated from ERA40 data and GASP ozone in the lower stratosphere, and the authors note that the GASP temperatures and winds were used in the assimilation. The authors then conclude that the tight ozone/PV correlations give a first indication of the quality of the GASP ozone measurements. I doubt that this is true. There are large gradients in ozone and in PV in the lower stratosphere, so the high correlations merely say that the GASP measurements capture a large dynamic range that correlates with PV. It says nothing about the accuracy or precision of the GASP ozone data, it just says it isn't grossly wrong.

ACPD
7, S1671–S1677, 2007

EGU

p3466/8 All data below the PV tropopause were averaged together to form “UT” ozone. Please give the approximate vertical range (in km from the tropopause) of the GASP data in the UT.

p3466/19 “In the UT the ozone distribution is connected to the geographical distribution of ozone precursor emissions”. This is an odd statement, given the long lifetime of ozone in the UT; ozone depends on a lot more than just the location of the source regions. Such a statement is not needed to justify binning the ozone data into large grid boxes to compare to MOZAIC, and I would drop it.

p3467, Section 3.1. The authors should comment that the seasonal cycle seen in GASP data in the lower stratosphere is similar to what is know from sondes and SAGE, with references.

p3468/9. The text is a bit misleading as Fig. 4 shows only GASP data - just say the figure shown the regions used for averaging. No comment is made on the ozone distribution shown in Fig. 4.

p3469/20. A comment is made that over Europe, the NE US, and the Atlantic, summertime ozone is higher in the UT in the older GASP data than in the MOZAIC data, “possibly pointing to a larger photochemical source”. The differences look very small compared to the variability in each data set, and most of the GASP data are said to be from one year, 1978. The sonde data show quite a bit of interannual variability in tropospheric ozone. Given there is only one year of GASP data, and the variability shown in Fig. 5, it is unclear if the GASP/MOZAIC differences are statistically significant, and this needs to be clarified.

p3475/15. I am a bit perplexed by the high mixing ratios on the California coast in the UT being attributed to local, surface, air pollution. Is convection being invoked as a mechanism? I thought the worst pollution in Los Angeles occurred when there was a strong inversion. Are the gradients at the coast really 10 ppb, or is this just an artifact of the color scale?

ACPD

7, S1671–S1677, 2007

p3476, Section 4, Comparison of GASP ozone with early ozone soundings. In this section the authors refer to the early work comparing the GASP data to ozone given by Regener sondes, used in the early 1960s. They refer to personal communications from S. Oltmans and D. De Backer about the problems with these early sondes. It has been known for 30 years that these sondes were highly flawed, and Chatfield and Harrison (J. Geophys. Res., 82, 5965-5968, 1977) presented a comparison of Regener and ECC sonde measurements in the 1960s. Please cite the original literature showing that the ECC sondes measured 50% more tropospheric ozone than the Regener sondes, and drop the lengthy discussion.

p3478-3481. The GASP data, averaged over broad ranges of equivalent latitude (20°), are compared to sonde data from individual stations in Figures 10 and 11. The text should clearly state how they selected the data they are comparing, at the start of this section. Was the selection done only on EL, or was longitude considered? Clearly the data were not matched in time, as there are more days with GASP data than with sonde data. p3479/13 states that the European sonde/GASP differences are within ś10% at all levels and in all seasons in Figure 10. This is not the case at the tropopause, and at 5K above the tropopause, where some differences are 15-20% p3481/3-15. The description of the comparisons of GASP and Canadian sondes as being similar to those with European sondes is misleading. The deviations are much larger in Figure 11 than in Figure 10, both for Resolute (deviations of 20-45%) and for the other stations at the tropopause and at 5K above. Mention is also made of results for SON, but these are not in the figure. The reasons given for the larger discrepancies are that the data are not evenly distributed in equivalent latitude, and that there are large gradients with latitude. Surely the real reason is that ozone is highly variable in the lowermost stratosphere in winter and spring, there are relatively few sonde measurements, and one cannot expect the PV/EL framework to remove all dynamical variability.

p3482, Section 4.2 A comparison is made of concurrent GASP and sonde data in the UT (with the GASP data selected in a large region around the sonde station). The

ACPD
7, S1671–S1677, 2007

EGU

points are referred to as “quasi-monthly”, a bit of a misnomer as there are only 1-3 days of data in most individual months. The large differences, and even the means and the medians, reflect very poor statistics, and the main conclusion is that the inter-comparisons done in the 1970s and 1980s got it right, than ECC sondes of the time measured more tropospheric ozone than the BM sondes, by about 10-25%. This section isn’t necessary, we learn nothing new, and the results could simply be stated in a couple of sentences.

p.3686-3489. The last section is just a summary of what came before, and it serves to emphasize that the GASP data show the same patterns in the ozone distribution seen in other publications, and that there is very little that is new in this paper.

Minor comments: There are some grammatical problems, e.g. p3456/10 “assessed questionable” should be “assessed to be questionable”. I cannot however, copy edit the paper.

p3460/15. “Ěone-minute average values were only considered in at least five minute intervals.” Clarify. I don’t know what is meant. Are you trying to say you took only one 1-minute average value within any given 5 minute period?

p3460/23 Values of a few ppb can occur in the tropical UT as a result of convection as shown by Kley et al. (1997), cited by the authors.

p3461/1 Why choose to exclude values $>$150 ppb in the UT? You are biasing your data to exclude STT events.

p3462/20 Do not start a new paragraph here, as you are still talking about Wallops Island data.

p3463/10 “launched” is a strange choice of word, as MOZAIC is not a satellite program!

p3469/2. “To our knowledge Ě” An example of unnecessary verbiage, drop.

p3469/8. Wrong reference. Logan et al. (1999) is about trends. Logan (1999) and

ACPD

7, S1671–S1677, 2007

Logan (1985) are about climatology, and are the correct references here.

p3469/10. Wrong reference. Logan (1984) is about trends, Logan (1999) shows the climatology relative to the thermal tropopause.

p3469/17. “evolves” seems a strange choice of wording. Do you mean “is found”?

p3473/17 change “on the coasts confining the Pacific Ocean” to “on the Asian coast, Hawaii, and in California.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3451, 2007.