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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interannual variability of ozone and carbon monoxide at the Whistler high elevation site: 2002–2006" *by* A. M. Macdonald et al.

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Summary:

This paper presents a high quality data set of 5 years of O_3 and CO measurements plus meteorological parameters from a site that samples relatively undisturbed, lower free troposphere air in southwest British Columbia, Canada. Such data sets are very limited on the North American West Coast, and the paper is thus a valuable addition to the literature.

Strong points of the paper include:

- The authors present a good discussion of their data that places their results in the context of similar analyses that have been performed on other data sets. However, more can be done in this regard, as suggested below.

- The discussion is interesting and appropriate to the issues under investigation.

This paper is well organized and clearly written. Although the analysis presents little that is new or surprising (with one exception), it is suitable for publication after the relatively minor concerns discussed below are addressed.

Important concerns:

1) The authors mention the Mt Bachelor site in central Oregon. However, they do not compare and contrast their measurements with any from that site. Isn't this possible to do? It would be of interest to compare at least the average O_3 and CO seasonal cycles.

2) Exactly how the data are handled is not clear. The authors report standard deviations of the data as a measure of the ambient variability (e.g. Fig. 4), but it is not clear if these refer to standard deviations of 1-minute averages, 1-hour averages, or even daily averages. The averaging period makes a substantial difference in the resulting standard deviations. A clearer explanation of the data treatment is required.

3) A clearer explanation of the uncertainty of the CO measurement is required. Presently (pg. 17630) the information is only: "The detection level was about 19 ppbv and the uncertainty of reported CO concentrations was within $\pm$5 ppbv, taken as one standard deviation of the instrument zero." More details of how these numbers were determined are required. Later (pg. 17630) with regard to the average diurnal cycle of CO, the authors state "This change, however, is close to the sensitivity of the instrument and overall, a diurnal variation is not significant on a monthly average." This statement is not clear to me. If individual hourly averages are uncertain to $\pm$5 ppbv, and measurements are uncorrelated on a one-day or longer time scale, then averag-

ing over many days should reduce the uncertainty of the average. For example, the seasonal average of 5 years of data for one hour in a diurnal cycle should have an uncertainty of only about $\pm$5/(90*5)$^{1/2}$ or 0.24 ppbv. Hence the diurnal variations in Fig. 3 for CO should be highly significant. These issues require full discussion.

4) The details of the CO hourly averages in Fig. 3 are intriguing, and suggest that there may be a small problem in the data reduction. It appears that there is a strong tendency for the even hour data to be significantly higher than the odd hour data. Is this possibly a result of the zeroing process that took place every two hours through much of the measurement period? There are also only 23 hourly data in the diurnal cycle. What happened to the 24th hour? I suggest that the authors carefully review their data reduction process to see if an error has led to odd-even hour differences, and to the data scatter larger than the 0.24 ppbv precision (see preceding point) expected for a 5 year seasonal average of the diurnal cycle.

5) The paper is quite long - 43 pages including 13 figures, most with multiple panels. Every opportunity should be taken to make it more concise. In this regard, I do not see that Fig. 1 is required.

6) On page 17629 the authors state "Very little valley influence is expected in winter." A short explanation/justification for this statement should be provided.

7) On page 17631 the authors note that "The observed ozone cycle at Whistler differs from other high elevation sites on the west coast such as Rocky Mountain and Lassen National Parks (Jaffe and Ray, 2007; Jaffe, 2011). These sites have the spring ozone peak but also have a significant summer peak, sometimes exceeding the springtime maxima. The Whistler ozone data do not show this broad summer maximum...." Of course Rocky Mountain National Park is not on the west coast, but more importantly this interesting finding should be more fully investigated. One interesting question is whether US continental pollution is responsible for the summer peak at the US sites, or if it is a latitudinal difference in the Pacific air transported ashore. This could be

investigated from coastal sonde data. Sondes launched from Trinidad Head California show a broad summer peak at 2 km altitude but not within the marine boundary layer [Parrish et al., 2010]. The 2-km sonde seasonal cycle (presumably representing inflow from the Pacific) is similar to that observed at Lassen National Park. I understand that Environment Canada launches ozone sondes from Kelowna in southern British Columbia. It would be interesting to compare the O_3 seasonal cycle from Whistler with that determined from the Kelowna sonde data at an altitude equivalent to Whistler's elevation.

8) On page 17634 the authors state "In summer, the intensifying Pacific High and weakening Aleutian Low cause a more northwesterly flow over the south coast of British Columbia (Fig. 7b)." I can see no hint of that northwesterly flow in Fig. 7b. This discussion should be clarified.

9) Line 11, pg. 17636 - Can an explanation be given for the large difference seen in Fig. 10a for 2006 compared to the previous 4 years?

10) Line 20, pg. 17636 - The authors state "During the summer biomass burning period (as represented by August), the amount of ozone in the t-P boxes relative to background is approximately constant at 3–5 ppbv over all of the years (Fig. 10c)." This appears to be incorrect as the quantity actually appears to scatter from -2 to +3 ppbv in Fig. 10c. Also the description of the NA enhancement in 2005 appears quantitatively incorrect. The authors should carefully review all of this discussion to ensure accuracy.

11) Line 5, pg. 17637 - The authors mention fires in Alberta as a regional influence. Were the emissions from these fires actually transported to Whistler despite the average prevailing winds shown in Fig. 7. This should be discussed.

12) Lines 4-8, pg. 17638 - This discussion is not clear. Why should ΔO_3(NA) for 2004 compared to ΔO_3(NA) for 2002 provide an estimate for ΔO_3 from anthropogenic sources?

13) The discussion of Fig. 13 beginning on pg. 17639 must be carefully reviewed, revised and clarified. For example, the authors discuss a strong negative correlation between CO and O_3 (slope=-0.21, R^2 =0.85) for the first episode (24 June 2004). However, looking at that day in Fig. 13a, there is no obvious negative correlation, certainly not at the R^2 = 0.85 level. A much clearer, objective discussion is required in regard to the episodes discussed in this figure.

14) Lines 5-6, pg. 17641 - The authors state that "Both O_3 and CO exhibit a seasonal cycle with a spring maximum and summer minimum similar to other background sites throughout the Northern Hemisphere." I am not convinced this is true outside of the marine boundary layer. Elevated sites in Europe exhibit a spring-summer maximum, but that could conceivably reflect regional pollution. A Japanese site (Mt. Happo [Tanimoto, 2009]) does show a strong spring maximum and a summer minimum, but that reflects the seasonal outflow from the East Asian continent, rather than a hemispheric representative feature. Importantly, as noted above, the sondes launched from Trinidad Head show a broad spring-summer maximum above the marine boundary layer. The authors must strongly support their conclusion for the lower free troposphere if they really wish to include this statement. The Canadian ozone sonde record and the Mt. Bachelor data may be useful in this regard.

Technical issues:

1) Figure 3 caption has "ration" rather than "ratio".

2) In Figs. 8 and 9, it would be useful to annotate the curves and symbols with more descriptive labels (e.g. background Pacific, Asian transport, etc.) than the Box number, which requires the reader to refer back to Fig. 6 for an explanation.

3) In Fig. 10, the meaning of the red line should be explained in the caption. Area of what burned and where should be clear.

4) Line 20, pg. 17637 - "co varies" should be "co-varies" or "covaries".

5) Line 8, pg. 17636 - The sentence "In May, there is no significant difference between O_3 in t-P or NA air masses in 2002, 2003, 2005 ...." I think should also include 2004, i.e. 2002-2005.

References:

Parrish, D.D., K.C. Aikin, S.J. Oltmans, B.J. Johnson, M. Ives, and C. Sweeny (2010), Impact of transported background ozone inflow on summertime air quality in a California ozone exceedance area, Atmos. Chem. Phys., 10, 10093–10109, doi:10.5194/acp-10-10093-2010.

Tanimoto, H. (2009), Increase in springtime tropospheric ozone at a mountainous site in Japan for the period 1998-2006, Atmos. Environ., 43, 1358-1363.

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