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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Increasing surface ozone concentrations in the background atmosphere of southern China, 1994–2007" *by* T. Wang et al.

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This review is by Owen Cooper, co-editor of this manuscript. I am posting my comments now to stimulate the discussion of this paper. My comments are made without the benefit of having read the reviews of the anonymous referees and their assessments will have a major influence on my decision as to whether the paper will be published in ACP. The anonymous referees are free to disagree with any of my comments when they write their reports.

Overall I find the findings to be very interesting and the paper is generally well written and organized. Several issues need to be addressed and/or corrected as outlined in my review below. My main concern is that the p values for seasonal ozone and annual CO rates of increase are fairly high (> 0.05) but the results seem to be treated as highly

significant. Clarification of which rates of increase are significant needs to be made. In these cases, analysis of variance tests can be used to determine if ozone or CO increased significantly between 1994-2000 and 2001-2007 (as discussed below).

Main concerns:

Throughout the manuscript the word trend needs to be replaced with something like "rate of change" or "increase". Fourteen years is not really long enough to establish a true trend, but is long enough to talk about changes in ozone.

Some of the references in the Introduction are not correctly summarized. For example on page 10432, lines 23-26, previous studies are given credit for attributing past changes in ozone above western North America to Asian emissions. However, Jacob [1999] doesn't look at ozone observations, he just used a model to predict future changes of ozone. Jaffe and Ray [2007] speculate that rising Asian emissions could be one of several reasons why ozone is increasing, but they make it clear that they weren't able to identify the source of the ozone increase. The text also implies that Collins et al. [2003] have shown that past changes in ozone in the mid- and upper troposphere are linked to climate change and enhanced transport from the stratosphere. But this paper only compares 1990-94 to 2090-94 and only talks about possible ozone changes 100 years in the future.

Recent papers relevant to the Introduction are: Increasing ozone in marine boundary layer inflow at the west coasts of North America and Europe, D. D. Parrish, D. B. Millet, and A. H. Goldstein, Atmos. Chem. Phys., 9, 1303-1323, 2009

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

Please provide a little more information on the GOME and SCHIMACHY data. Which "level" of data was downloaded? Did the authors conduct any processing of the retrievals, or apply any cloud screening. Or was this all done by TEMIS?

Figure 3 provides the rate of change of ozone for all four seasons. While the average of these 4 rates is similar to the annual rate in Figure 1, the statistical significance drops from $p<0.01$ for the annual analysis to $p=0.07$-0.18 for the seasonal analysis. None of the seasons are significant at the $p=0.05$ level which is generally considered to be the maximum p-level to indicate a robust and significant rate of increase. This point needs to made clear in the text. Why does p increase when the data are examined seasonally? Is it because the sample size is reduced by a factor of 4 when compared to the annual analysis in Figure 1? What do you get if you take a season and split the data into two groups, 1994-2000 and 2001-2007, and conduct an analysis of variance test? Is the later group significantly greater than the earlier group? It may be that there is a highly significant increase in ozone between 1994-2000 and 2001-2007 (as could be shown by the analysis of variance test), it's just that using a linear fit to describe the increase is not as significant.

The revised CO rate of increase is 3.5 ppbv/year with a p value of 0.15. Such a high p value does not give strong evidence that CO is increasing significantly. This analysis includes marine air from the south which isn't expected to have a strong increase in CO. What do you get when you filter the CO by source region or by season? Is the rate of increase more significant when transport is from East China or during autumn? Is it possible that as China's economy becomes more modern with more efficient power plants that CO emissions are increasing at a lower rate than NOx emissions? Also, what about shipping? Do the satellite data show an increase in NO2 along the major shipping lanes of southeast China? Several recent papers show that ships account for at least 13% of global anthropogenic NOx emissions but produce very little CO.

Page 10441 line 25 It's not clear to me how it is determined that the increase in background ozone accounted for 70% of the increase in the total ozone in Hong Kong. If local NOx emissions are decreasing in an effort to control ozone, then shouldn't locally produced ozone be decreasing? This would imply that all (not 70%) of the total ozone increase in Hong Kong is due to background ozone.

In all figures the font size of the text along the axes as well as the text listing the rate of increase and p values needs to be increased.

Minor comments. If no explanation is given please replace the text in the manuscript with the suggested text.

page 10430, line 7 increased at an average rate

page 10431 line 22 wild fires

page 10432 line 29 the major types of air masses influencing the site, and the ozone

page 10433 Please make it clear that Hok Tsui is separated from the main urban are of Hong Kong by a ridge

page 10434 line 18 against a NIST

page 10436 line 26 The 14 years of data give

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 10429, 2009.