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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ambient concentrations of aldehydes in relation to Beijing Olympic air pollution control measures" *by* J. C. Gong et al.

Anonymous Referee #1

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When I started to read this paper I was very enthusiastic about the results I was expecting to see. However, I soon became very disenchanted with many of the statements and conclusions that are not supported by the data, by conventional knowledge of photochemistry, and by other studies. For the reasons detailed below I don't support the publication of this paper.

1. Most if not all studies of aldehydes from urban sources are given in ppb and not μg/m3 and this needs to be changed when comparing with past studies.

2. This study employs C18 cartridges without any discussion regarding the use of an ozone scrubber and/or the effects of ozone as a significant positive artifact on the formaldehyde measurements. The JGR study by Gilpin et al. (vol 102, D17, 21,161-21,188, 1997) documents this interference. The Gilpin et al. study also points to the

importance of adding formaldehyde standards directly to the inlet, even when employing techniques that are calibrated by liquid phase standards, such as the present study. Unfortunately, the present study does not give any details regarding the specifics of the measurement calibration, the effects of ozone, zeroing, etc. This leads me to question the accuracy of the present formaldehyde results, particularly when ozone is changing. Can some of the present results be due to this positive artifact from ozone, and can changing ozone mask any trends the authors are trying to see due to enactment of control strategies? What is the explanation of the 115.5% collection efficiency for formaldehyde?

3. Can the authors really claim statistically significant reduction in acetaldehyde concentration between sub-periods 1 and 2 when the standard deviations are so high and overlap? What are the median concentrations?

4. There needs to be further explanation of the Spearman correlation coefficients on page 19745.

5. On this same page of 19745 the statement that "formaldehyde and acetaldehyde concentrations in the summer time of Beijing were substantially higher than those reported for other cities during photochemical seasons" cannot be substantiated. Airborne formaldehyde concentrations in Houston, Texas, for example, attained instantaneous values greater than 30 ppb, which are higher than the average 28 ppb values reported here.

6. The biggest problem I have with this paper is the inconsistency regarding the trends in the 3 aldehydes and the resulting explanations. Formaldehyde and acetaldehyde are known to both be emitted by vehicle traffic, with formaldehyde > acetaldehyde emission factors, and both are produced photochemically. Studies by Calvert in 1981 and by Sigsby in 1987 show CH2O/CH3CHO molar emission ratios in the 1.2 to 3.1 range for motor vehicles. Thus it is hard to understand how a reduction in traffic would yield a reduction in CH3CHO but not CH2O. The inconsistency in the trends is not

adequately explained. If as the authors claim that Fig 4 shows common sources of the 3 aldehydes (large scatter and no regression fits), than one would expect that such a common direct source would yield the same temporal trends. Its hard to imagine that photochemistry would dominate CH2O and not the CH3CHO trends as well, when in fact both are produced from both sources. This inconsistency is compounded by the fact that nothing was given in this paper regarding the effects of boundary layer height on the concentrations. It is also very hard for me to comprehend that low correlations were observed between CH2O and CO, when every field study I have been on shows very high correlations between these two in the boundary layer. The statement on page 19748 that O3 is critical for the formation of CH2O is not quite correct. In general, O3 is produced from CH2O, which is produced from OH with various VOCs, and not the other way around. The authors note on page 19748 that VOCs play an important role in the formation of aldehydes but no measurements of VOCs were given other than the references cited and this complicates any data interpretation. The specific VOCs will have a differential affect on formaldehyde relative to acetaldehyde but nothing was given in this paper to further assess this.

7. The final conclusion that the air pollution control measures adopted during the Olympics appeared to be associated with reductions in acetaldehyde but not formaldehyde and acrolein is not consistent with our understanding of these gases from both direct and photochemical sources. Since this is the fundamental topic of this paper and this inconsistency was not adequately explained by any detailed data analysis, I do not support the publication of this paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19737, 2010.