**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 #2

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This manuscript reports aldehyde measurements based on 24 hr samples performed before, during and after the Olympic Games in Beijing. The authors report very high values for formaldehyde, acetaldehyde, and acrolein. Based on correlation analysis they conclude that local direct emissions had a larger impact on acetaldehyde than on formaldehyde and acrolein.

The paper has some major deficiencies. In the current form I cannot recommend this manuscript for a potential publication in ACP.

Major concerns:

What was the reason not to take samples on rainy days? This would potentially lead to a bias in the data set. How was a rainy day defined (i.e. any short-term rain event within

a 24 hr period)? What was the fraction of the number of days which were considered rainy days vs days without rain?

Results for HCHO measurements as shown in Table 1 are about a factor 2-3 times higher than those reported in Li et al. (2010) for the same periods. The authors should discuss the potential reason for this.

According to the Li et al (2010) paper HCHO mixing ratios were lower during Olympic Games than before the Olympic Games which contradicts the statements made in this paper.

Oxidation of isoprene is an important secondary source for formaldehyde. This source depends on solar radiation and temperature. This may have partly been the reason for the temperature dependence of formaldeyhde as described by the authors. The authors completely missed to include a corresponding discussion.

Fig. 2: I am assuming this presentation is based on 24hr averages

(1) I really doubt the value of 24 hr average values for wind data

(2) The authors have not put units to the scale (0-0.5?). If it is m/s, then averaging has been done in a wrong way, since the threshold of the RM Young wind sensor is about 1 m/s. I am not sure, if the authors included any "calm" readings which eventually may have produced 0-0.5 values.

(3) Contrary to the authors' statement this plot does not reflect the data in the Wang et al. (2010) paper. The Wang et al paper does report wind coming from other directions, as does the Li et al (2010) paper.

I am confused about the discussion in lines 3-11 on page 19748. On the one hand the authors suggest secondary sources to be dominant for formaldehyde and acrolein, and primary traffic emissions to be important for acetaldehyde. On the other hand the authors suggest the presence of common sources for the three aldehydes.

I am confused about figure 5. The dots are all over the place and I do not see any relationship, not even for the correlation between formaldehyde and the daily maximum photooxidant (O3+NO2) as suggested by the authors.

Minor concerns:

Page 19739, lines 19-25: It would be helpful to mention some of the HCHO mixing ratios which were reported in these exposure studies.

Page 19741, line 4: You want to remove "....pedestrian, and bicycle traffic."

Page 19747, lines 19-23: not sure, if it makes sense to report changes in [%] for the temperature.

Page 19749, lines 1-3: Why would this only be important for acetaldehyde and not for the other aldehydes?

Table 2: I would recommend to include median values, too.

Table 3: I guess all data is based on 24 hr measurements apart from the ones which are indicated by (2). I recommend to spell this out explicitly.

Table 4: Also report the measuring interval of these studies, if available.

Fig 3: scales for meteorological parameters are missing

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