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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Gaseous pollutants in Beijing urban area during the heating period 2007–2008: variability, sources, meteorological and chemical impacts" *by* W. Lin et al.

Anonymous Referee #2

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General Comments: It is an interesting and well structured article and I suggest publication of the article after taking into account the comments listed below.

Major comments:

The authors state that high wind speed concurs usually with lower humidity and hence this is a possible reason for the positive correlation of RH with all pollutants except ozone. However, they should also consider that RH is negatively correlated with temperature (as warmer air can hold more humid air). It may be that the negative correlation of ozone with RH is linked with the positive correlation between ozone and temperature as a consequence of the temperature versus RH anti-correlation.

Are the slopes of daytime regression lines in Table 2 significantly different from the nighttime slopes? I would suggest that the authors should also consider the errors of the slopes in order to give an answer for my above mentioned question.

I think that the fact that wind speed correlates positively with ozone and negatively with all other pollutants is not adequately emphasized. There is some discussion that physical processes such as transport of ozone from above or the clean sector is the reason for the different behaviour for ozone versus wind speed correlation but more elaboration is needed and possibly relevant references for this effect.

In page 6930 the authors claim that collinearity between CO and SO2 is not significant. However this statement sounds strange since Table 1 indicates a correlation of 0.808 between these two pollutants. Please clarify and explain the statistical measures VIF and condition index.

In page 6931 the authors state that the coefficients a and b were applied to the calculation of relative contribution of mobile and points sources. Please clarify which are these calculations. Furthermore it is found that the relative contrinution from mobile sources has a maximum at 13:00. Is that sensible? Shouldn't be the maximum of mobile sources at early morning with the highest traffic? Please clarify this issue.

In page 6932 it is anticipated that the period with the highest ozone (24.2 ppbv) is linked with cold and dry air rapidly descending to the site. The RH of the air masses is as low as 20%. Such dry air masses are often linked to stratospheric air descending to lower troposphere. Have the authors explored this possibility. Deep stratosphere to troposphere transport events down to the surface are rare but may happen. There are a number of such cases explored in the literature (Stohl et al., Atmospheric Environment, 34:1323–1354, 2000; Gerasopoulos et al., Atmospheric Environment, 35:6347–6360, 2006; Akritidis et al, Meteorology and Atmospheric Physics, 109:9-18 , 2010).

A common index to estimate the ozone production in polluted areas as well as the clean free troposphere, is the OPE. It seems that the OPE calculation in Figure 11 is based

on daily means which is not scientifically meaningful. By definition OPE is meaningful if you follow an air mass and see the number of O3 molecules chemically produced per molecule of NOx oxidized to NOz within this air mass. Considering that you are in a station that receives for a few hours air masses of similar origin you may assume that OPE calculation is also meaningful from a scatter of Î§3 versus NOz within a few hours. Hence this means that OPE should be based on hourly data of a specific day. Then someone can explore for many different days the range of OPE calculations.

Minor comments: “dynamic” in line 3 of page 6928 should rather read “dynamical” or “physical”.

The word “valleys” in line 26 of page 6929 does not sound as the most appropriate word.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 6919, 2011.