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Discussions

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Interactive comment

Interactive comment on "Effectiveness evaluation of temporary emission control action in 2016 winter in Shijiazhuang, China" *by* Baoshuang Liu et al.

Anonymous Referee #2

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The authors make an assessment of emission controls during the heating season in one of the most polluted cities in China. They performed field studies in Shijiazhuang for two months, analyzed ions and trace elements in PM2.5, and used PMF and HYSPLIT model to investigate the sources and evolution processes of air pollution in and around the city. This paper involves lots of work, and I find some results very interesting, for example, the improvement of air quality due to emission controls is more evident in suburbs than urban area. I can believe the emission control measures might have considerable effectiveness in improving air quality, but I have doubts regarding the method of quantification this paper used, which does not look convincing to me. I don't see much scientific significance in this paper in its current form though it summarized

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Discussion paper

plenty of data and did some analysis. The paper is not well written and needs lots of editing. My major concern is as follows:

1. The authors have found the height of mixed layer and wind speed have a significant influence on air pollutant concentrations, but they use an oversimplified method (Eqs. 8 and 9) to quantify the impact of a single variable (i.e., heating and emission controls) on air pollution, without excluding the effects of meteorological conditions quantitatively. The method does not make sense.

2. The error bars in Fig. 3 are too long, and thus the average values are uncertain. Some of the error bars are even longer than the reduction of concentrations caused by emissions controls (calculated by Eq. 9). For example, the error bar of PM2.5 concentrations during the period of CAHP (blue bar) in the whole city is much larger than the reductions compared to the NCAHP period (pink bar). There are many such cases in Fig. 3, as well as in Fig. 7, which makes the quantification analysis based on these data look not convincing.

3. Some statement in the main text are very misleading. For example, in lines 366–367, "Well known that the NO2 is mainly derived from the vehicle exhaust (Liu et al., 2017b). Therefore, the control effect of motor vehicles was remarkable during the CAHP in Shijiazhuang." And in lines 391–392, the authors say "Furthermore, the effects of control measures for domestic coal might be worse during the CAHP." I don't think the data and analysis this paper presents can lead to such conclusions. The authors should be more precise what they find from their study.

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