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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Typical types and formation mechanisms of haze in an eastern Asia megacity, Shanghai" *by* K. Huang et al.

Anonymous Referee #1

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In this study, Huang et al. presented measurements of aerosol and trace gases at a site in Shanghai, China from April to June 2009. Three major haze episodes having distinct characteristics were observed during the experiment, under different meteorological conditions. Using the correlation between gaseous pollutants (CO, SO2, and NOx) and PM2.5, chemical tracers of dust (Al) and biomass burning (K+ and OC, EC), as well as satellite and ground-based lidar data, the authors were able to attribute the three episodes to anthropogenic/industrial pollution, long-range transport of dust from Gobi, and agricultural fires in eastern China. Overall, the manuscript is based on a quite comprehensive, original data set collected from a megacity known to have severe air quality issues. The topic is suitable for the special issue and should also be of interests to the wide science community. The results were presented in an organized fashion. On the other hand, this reviewer found the manuscript lacking quantitative,

in-depth analysis, and missing some important details. Also the quality of the writing needs to be improved: there are numerous grammatical mistakes and typos in the manuscript (too many to list blow), making it difficult to follow at times. Major revisions are recommended before the manuscript can be accepted for publication in ACP.

Specific Comments: In the manuscript, the authors focused on the impact of different sources on the three types of haze episodes. And this brings an interesting question: the sources and transport of dust and biomass burning emissions can well explain the episodic nature of episodes 2 and 3, then how about episode 1? The industrial and traffic emissions are not expected to change dramatically over two months, but why was episode 1 so much more polluted than the "normal period"? This is likely due to change in meteorological conditions, and the authors are encouraged to look into the meteorological mechanisms for the formation of major haze events.

Section 2: a more detailed description should be given for the site – is it urban, sub-urban, or rural? Are there any major local emission sources (point sources, roads), and are they expected to have impact on the data set? And how is this mitigated? Is the site truly representative of a megacity? Also more detailed information is needed for gas analyzers – for example, what kind of calibration standard was used? Is zero check every week sufficient for the CO analyzer, among other instruments?

P21719, line 2, a fixed lidar ratio was used in spite of various aerosol types (industrial, dust, and biomass burning) observed at the site?

P21721, line 18: why made the comparison between two stations that are 32 km apart?

P21722, line 1: are the PM1, PM2.5 and PM10 mean concentrations given above the average of hourly or daily data? 24-h mean should be used to compare to the 24-h standard. P21722, line 15: air mass usually refers to large body of air of similar characteristics (e.g., Arctic air).

P21722, line 17 and Fig. 3: how was mixing height determined? Is it the mean along

the trajectories? How does it compare to lidar data? Lower panel of Fig. 3b shows daily mixing height but the caption indicates it is 3-h average?

P21723, line 18: April should be June?

P21724, line 11: the authors should not assume that the readers all have a good knowledge of the geography of China. Would recommend that the authors mark the important features/locations/regions on the map of Fig. 5.

Section 3.2: overall, the analysis using satellite data is problematic. The authors should demonstrate that the Angstrom exponent from MODIS is a reliable product before using it to distinguish various aerosol types (refer to Levy et al., ACP 2010 for a recent validation study of the product). Otherwise, Angstrom exponent from ground-based sun photometers can be used. There seem to be artifacts in the processing of the satellite data, as Fig. 5a-d look odd with stripes and small data gaps. Also has OMI row anomaly been removed from the HCHO data shown in Fig. 5g?

P21725, line 20: a more quantitative demonstration of the spatial correlation between CO and fire counts would be helpful. Similarly for HCHO and fire counts mentioned in line 11, P21726.

P21726, line 21: why would sea breeze clean aerosol but not CO or HCHO?

P21728, line 28 and Fig. 6: this part is confusing. It appears that the Ca/Al ratio was also small for other days and is quite noisy – does not seem to be a good indicator for the dust source region.

P21729, line 29: what is the ratio between K+ and Cl-? Cl- can also be derived from coal burning.

P21730, line 9-14: from the results of Yamaji et al. (cited above), the OC/EC ratio during MTX2006 was closer to 3 (Zhang et al., 2008) than to the results given here.

P21731, line 8-17: the enrichment of As in agricultural fire emissions is a very important

result – any more data on As emissions from agricultural fires?

Section 3.4: from Fig. 6 one can tell that soil or dust was a sizable part of aerosols on April 6, however this was not reflected in the lidar data?

P21734, line 1, PE3 should be PE1? P21735, line 5: recent studies show that SO2 emissions might have started decreasing in China (refer to Li et al., GRL, 2010 and Lu et al., ACP, 2010).

P21736, line 19: NO3-/SO42- not SO42-/NO3-?

P21737, line 10: there should be updated emission inventory for the region.

P21738, line 27: use of coal should be more or less correlated with utility generation and could be on the rise in China over the past decade or so.

P21739, line 14: there are plenty of minerals as shown in this and previous studies, even if NH3 emissions are reduced.

Section 4: it would really be useful for the authors to give some statistics on different types of haze events: how many inorganic ones, how many biomass burning ones, and how many dust ones? That way, the quality of the paper will be much improved, and the results would be more helpful for the policy makers.

P21741, line 1: a paper on the 2010 dust recently came out in Atmos. Environ. (Wang et al., First detailed observations of long-range transported dust over the northern South China Sea, 2011).

Fig. 1b, may consider using different colors for PM2.5 and PM10. Also it would be helpful if the three episodes can be marked in the figure.

Fig. 2, is gas concentration for standard temperature and pressure?

Fig. 7, the high EF of V for PE1 is interesting, and may deserve some discussion.

Fig. 9, a and c are average profiles for the entire day?

Fig. 10, what do the error bars stand for?

Fig. 12. The CO/PM2.5 ratio seems to be higher in PE1 than in PE3, can you give more discussion on that?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21713, 2011.