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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chemical characteristics and causes of airborne particulate pollution in warm seasons in Wuhan, central China" *by* X. P. Lyu et al.

Anonymous Referee #4

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The paper on chemical composition and source analysis by Lyu et al. presents interesting results for Wuhan, China and would be a good addition to the literature on the topic. The paper discusses chemical composition of PM2.5, possible sources and formation mechanisms for SIA and SOA. However, I have a few questions and suggestions for the authors before the paper is accepted for publication. Some of the sections of the paper present conflicting information, and the introduction section needs to be thoroughly edited (e.g. Lines 39-40, 46, 51-52, 60-62).

Specific Comments Lines 204-205: Please clarify- "... related to variations in number of construction sites.. " Is there a considerable variation in the number of active construction sites? It is surprising that the contribution from carbonaceous aerosols to

total PM is very low. It is somewhat counterintuitive that OC did not increase during the pollution episodes (Lines 328-338), since the increase is being attributed to biomass burning. In autumn, however, an increase in OC is associated with biomass burning. This is not coherent. Lines 268-273: This estimation can help interpret if the ions are neutralized. Consider expanding this section to include a discussion on whether or not the aerosols were neutralized during autumn season. Were the correlations between different metals analysed? Some of the K can also come from sources other than biomass burning (especially crustal material), and it would be useful to understand if that is the case. This is particularly relevant with reference to the earlier comment about construction sites. Also, was K found to be correlated with OC/EC? PMF: The authors do not provide any details about the outputs, and the process used for determining the factor number, or the stability of the factors. How was the uncertainty estimated? In the current analysis, the fourth factor (biomass burning) seems to include SIA, SOA and the typical biomass burning tracers. Is it possible to tease out secondary aerosol factors if 5-7 factor solutions are used? What was the total sample size used for PMF analysis? Is traffic not a source for PM in the sampling region? Technical comments Please proofread the manuscript carefully, and edit for language. Line 24: Contribution for PM10 or PM2.5? Methods: Were these hourly measurements? Which method was used for OC/EC analysis? Please include 1-2 lines about the custom element analyser. Table 1: For some cities (e.g. Beijing), is the reported value the arithmetic mean, or some other statistic? Please clarify. Also, change Tai Wan to Taiwan. Lines 60-62: Please rephrase Lines 194-195: Either specify when these statistical parameters are reported in Table 1, or remove this sentence. Lines 202-207: How strong was the correlation between the two fractions of PM? Was there a difference in this relationship during the episode compared to non-episode days? Lines 223-224: What does this sentence mean- "...Due to the fact that the chemical, optical and toxic properties tend to be more apparent in smaller particles.."

Lines 233-234: PM2.5 isn't entirely composed of secondary particles. Please edit the statement.

Line 295: p-value is missing Secondary inorganic aerosol is typically referred to as SIA in the literature. Consider using the same nomenclature. Section 3.4.1: What is the correlation coefficient for the observed/modelled ozone? Figure 11: For reporting PMF profiles, it would be easier if the authors split the profiles by episode-non-episode. The current plots are difficult to interpret since there is a lot of information on the same plot.

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