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Interactive comment

Interactive comment on "Seasonal characteristics, formation mechanisms and source origins of PM$_{2.5}$ in two megacities in Sichuan Basin, China" *by* Huanbo Wang et al.

Anonymous Referee #3

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The manuscript is based on the observation conducted in four selected months in two cities in the Sichuan Basin, China. It represents the results of PM2.5 and the chemical components. The seasonal variations are shown and the difference in terms of the formation mechanisms and geographical influence between the two cities is discussed. The content of this manuscript fits the scope of ACP and the data is interesting to be studied. However, I found this manuscript is only a report of the results in a rarely investigated region in China but without in-depth analysis. No novel point has been raised and discussed in this manuscript. I would not recommend it to be published in ACP in the current stage.

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General comments: 1. The sampling campaign in four selected months may not be enough to provide sufficient data to answer the questions (objectives) which are supposed to be studied in this work. The two sampling sites seems not ideal to understand the characteristics of PM2.5 in two basined cities with typical geographical features. Regional sites without direct emissions are better in my opinion. In order to discover and reveal the formation mechanisms of secondary aerosols, more data and analysis are necessary. 2. The data are not well presented in this manuscript. The readers can hardly find the sufficient information to know and understand the results. For example, how many samplers were collected in the campaign? How many samples were taken and how about the variations of data in clear days, moderate polluted days and heavy polluted days? Were there some different pollution episodes? 3. The analysis and discussion are superficial and full of speculation. No solid evidence can be provided to support the conclusions, which makes the significance and implication ungrounded. For example, to support their hypothesis, the diurnal variations of monitored gases are presented and discussed. However, the data of PM2.5 and their chemical components are on daily basis, which weaken the analysis and leads to vague conclusions.

More specific comments are shown as follows: Specific comments: 1. As I suggested above, are the two sampling sites and the data representative for this investigation on the characteristics of aerosol in the two basined cities? Obviously they are both highly affected by the traffic emissions which may bias the analysis. The topography of the two sites and the influence should be discussed. 2. Line 78: Please provide the details of the sampler. 2. Three samplers were used in this campaign. The comparison of the three samplers should be provided to show the accuracy and consistency of the data. 3. How many samples were collected? How the blank filters (lab blank and field blank) were collected? 4. Line 111-113: There were only 5 elements detected by XRF. Normally I would expect more elements could be measured by the XRF technique. Why? 5. Line 121: Please provide the details of the weather station. 6. Line 178-179: The authors pointed out that higher sulfate concentrations were found in summer. In Table 1, I found that lower sulfate average was in summer than that in winter. Please check

the data. 7. Line 178-185: The discussion on sulfate, nitrate, chloride and potassium seems superficial and arbitrary. The analysis should be based on the data from this campaign and be made with in-depth study instead of guesses. 8. Line 188-190: The high SOC content was observed in winter. In this work, the estimation of SOC mainly depends on the seasonal minimum of OC/EC. However, it should not be surprise to see high OC in winter because organic aerosols may not necessarily be only formed by secondary reaction but also by direct emissions (e.g. biomass burning). 9. Section. 3.3 discusses the difference of data between the two sites. As it known to all, the difference can be due to many possible factors (emissions, atmospheric reactivity, meteorological conditions, the surrounding terrains). It is really hard to synthesize significant information from the comparison. Therefore, more in-depth studies are necessary. 10. Line 227-238: More information should be provided for the pollution episodes. For example, how many polluted days and in which seasons were captured? How many pollution episodes were observed? 11. Line 254-256: The distinct characteristics in the urban area in the Sichuan Basin should be further investigated and discussed. How may the topography and meteorological conditions influence on the characteristics? 12. Line 271-272: "Both CO and EC concentrations increased on polluted days, suggesting the important role the meteorological condition played on PM2.5 accumulation."Why? I cannot see any link. The occurrence of CO and EC in the troposphere should be influenced by the emissions, removal mechanisms and other factors (including meteorological conditions but not exclusively). 13. Line 274-275: "CO can be considered as a reference pollutant species whose temporal variations were mainly from the impact of meteorological conditions."Why? See the comment 11. Also, I think the CO-scaling method should be further explained with more details and with references. 14. Section 3.4.2: The diurnal trends of monitored gases could not give any solid evidence to support their hypothesis. In this case, especially when the formation mechanisms of secondary aerosols are discussed, high resolution data are necessary. We should not rely on the unsolid speculation.

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