

Interactive comment on “Measurement report: Chemical characteristics of PM<sub>2.5</sub> during typical biomass burning season at an agricultural site of the North China Plain” by Linlin Liang.

This study reports a measurement research on the characteristics of the chemical components of PM<sub>2.5</sub> during 15 October to 30 November at the agricultural site of the NCP. The authors linked their results to the BB emission and claimed the importance of softwood burning to the air quality in NCP during the heating season. Overall, this is a nice piece of paper with clear objectives and methods and will provide valuable results. Before considering publication in ACP, major revisions should be made. Some comments and suggestions are listed as follows:

General comment:

Although it is a measurement report, which should present substantial new results from measurements of atmospheric properties and processes, the scientific goal should be improved well through focusing on the innovation in measurement or data analysis methods. The current results are no longer new compared with that reported in 2013 of Beijing by Cheng et al. (2013). What is the current data in this rural site of NCP may bring us to a new knowledge of chemical characteristics, especially in atmospheric properties and processes? Is there any difference between this study with that reported previously, e.g., a faster conversion rate, a new emission type due to the emission control by the government, etc. Besides, the logicity of this paper should be improved. For example, “the LG/MN ratios from crop residue

burning, i.e., rice straw, wheat straw, and other straws, were similar and characterized by high values, yet overlapped with those from hard wood and leaf burning ( $>10.0$ ), while soft wood characterized by relatively lower LG/MN ratios ( $< 5.0$ )". The ratio of LG/MN in this study is around 20, which the authors claim that the air quality was influenced by softwood emission. This conclusion is obviously inconsistent with their previous analysis.

Specific comments:

1. P4, L107. The abbreviation LG and MN should be spelled out first time. Similar with that in P7, L189, "Elemental carbon and primary organic components" , which has been used as EC or POC before. The abbreviation through out the manuscript should be checked carefully to unified.
2. P8, L202. "Moreover, such an enhancement in secondary transformations during daytime is more evident in terms of the mass contributions of secondary inorganic ions to PM<sub>2.5</sub>-cal, that the contributions of SO<sub>4</sub><sup>2-</sup>, NO<sub>3</sub><sup>-</sup> and NH<sub>4</sub><sup>+</sup> to PM<sub>2.5</sub>-cal decreased from daytime (9.9%, 14.5% and 10.0%) to nighttime (6.5%, 9.6% and 7.1%) (Figure 3)." The conversion rate of SOR, NOR should be useful here.
3. P8, L214. The BB episodes section. The detailed description of this episode 31 Otc is helpful to readers for understanding, such as the meteorological conditions, wind rose plot. Besides, the PMF or model

simulation should be made to conclude how much the BB contribute to the PM2.5.

4. P9, L230. “The central heating systems in North China cities were operated during period IV, and the ambient level of LG was observed at  $0.96 \pm 0.63 \mu\text{g m}^{-3}$ , which was slightly higher than that in period III.” Is this statement tell us the central heating systems used in NCP will emitted more LG. As we know, the heating system was changed since 2016 over NCP from coal to gas at least in the main cities of NCP. The rest area of NCP are substituted by the electric power system such as air conditioner. Does that means the LG may originated from gas or other fuels?
5. Conclusion section. The local soft wood contributed to high concentrations of PM2.5 in NCP during heating season should be more considered.
6. Language improvement should be made by a native speaker.