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Interactive comment on "Simultaneous Measurement of Urban and Rural Particles in Beijing, Part II: Case Studies of Haze Events and Regional Transport" *by* Yang Chen et al.

Anonymous Referee #1

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Online single-particle chemical composition analysis was used as a tracer system to investigate the impact of heating activities and the formation of haze events in two parallel field studies at both urban and rural sites in Beijing. This manuscript focuses on case studies. One of the key points of this manuscript is that there is a pattern of transportation and accumulation of particles in both the urban and rural areas. The input of regional particles was a consequence of weakening atmospheric circulations, resulting in the stagnation of the air which provided favorable conditions for the accumulation of pollutants, ultimately leading to severe haze events. In the rural area, the heavy haze was mainly controlled by air stagnation and local emissions, but regional transport was also observed before the event.

This work represents a potentially substantial contribution to understanding the heavy haze formation in Beijing. However, I do have several concerns mostly related to this point. I will support the publication of this manuscript if the authors can properly address my following comments.

The hypothesis of regional transport can trigger a high pollution event is interesting. The evidence provided in this manuscript, however, can only suggest that there is some possibility of this happening at best. The evolution of particle compositions and fractions were not consistent or repeatable in the high PM events. The transition of particle fraction was not there for most of the cases. At least they are not obvious from the time series. The higher wind speed was used as another evidence for regional transport, which is far from robust. The authors did not discuss other possible causes, such as boundary layer height. The argument of why higher PM causes stagnation of atmosphere is also lacking. I wonder if the authors can provide some reasoning from the meteorological perspective. In order to support the current conclusion in the manuscript, the authors need to better illustrate those points with more than time series and address the reproducibility in all the high PM events. Otherwise, the authors can tighten their language and only provide this as one of the probable theories.

Minor Comments:

1. Line 110: Adding the two sites on this map would be helpful. 2. Line 138: Another Table 1? And it is unclear to me what these correlations are. 3. Line 273-274: " These results are consistent with the analysis of particle categories." Please expand and support this argument.

Editorial Comments: It seems that the manuscript has many typoes and I am only listing the ones I caught. Please proofread intensively before considering resubmission.

1. Line 22-33: I strongly recommend NOT to use abbreviations for particles types like EC-Nit, EC-Nit-Sul, ECOC-Nit-Sul, Nak-Nit, and OC-SulECOC-Nit in the abstract. PKU and PG have not been introduced either. Please fix. 2. Line 56-57: "Sun et al.

(2014); Sun et al.(2013a)" Format of citation needs correction. 3. Line 113: " Particle types, their ratios at both sites," Do you mean fractions instead of ratios? 4. Line 175: " control emissions from household emissions" fix typo. 5. Line 292: "such provinces as Hebei, Henan," Check grammar.

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