

## ***Interactive comment on “Characterization of black carbon-containing fine particles in Beijing during wintertime” by Junfeng Wang et al.***

### **Anonymous Referee #1**

Received and published: 1 November 2018

Characterization of black carbon-containing fine particles in Beijing during wintertime  
Wang et al.,

BC also called as soot is an important aerosol from incomplete combustion of fossil fuels and biomass burning. Understanding the soot mixing state in polluted air of Beijing, it is quite important issue to evaluate their potential optical, hygroscopic, and human health. The authors used one SP-AMS to determine mixing state of soot particles collected in Beijing during the wintertime. They found coating/BC ratio was at 5.0, much smaller than highly aged soot in other places. Also, they studied coating chemical species and their possible formation mechanism. The scope of this study is suitable for ACP. However, the paper need to one substantial revision before it can be published. I list several concerns about the conclusions

(1) L28-29 deleted very (2) L30 only (3) L33-34 how do the result indicate dominant contributions from primary emissions? You can say that these particles might source from local emissions instead of long-range transport particles. Am I right? (4) L35-36, 38-40, seemly for me, the conclusion is constrast. One you mentioned primary emission. Other one you want to mention the secondary species. (5) L41 at-during (6) L44-45, I don't think the conclusion is from your solid result. Most you speculate these results. (7) L44-47 the conclusion cover all the possible. I would ask the author revise it carefully. What is your conclusions during the sampling period. If these solid conclusion are not from this study, you need to remove it. Seemly, I like to see what you find on BC particles not for haze formation. (8) L56 Morphology of BC might be altered greatly. These citations don't supply any morphology of BC particles. You need find others from electron microscopies. (9) L77, I don't agree with the claim. For example, Wu et al., 2017. Size distribution and source of black carbon aerosol in urban Beijing during winter haze episodes. Atmos. Chem. Phys. 17 (12), 7965-7975. The study seemly, give the online BC-containing particles in Beijing. (10) L161 Discussion, deleted s (11) L260-261, L284-285, L331-332 L347-348, all the parts discussed the aqueous reactions for nitrate and SOA formation during the nighttime. I take a look at the data from the study. It is too simply to get such conclusion. I might ask the authors cite more related references here. For example, Wu et al., 2018. Environmental Science & Technology Letters 5 (3), 160-166; Sun et al., 2018. Journal of Geophysical Research: Atmospheres 123 (2), 1234-1243. Kuang et al., 2016. Geophysical Research Letters 43 (16), 8744-8750. (12) L278-279 I don't understand the sentence. Why was the large decrease of organics coating concentration? (13) L292 at-during (14) L307 This can be expected for urban aerosols. I don't understand it. Why? (15) L328 of-at (16) L269 miss comma after ws (17) L317 at two polluted episoes (18) For section 3.5.2 Seemly, the authors found different coating species on soot particles. FE, the author found large SOA; SE the author proposed large POA instead of SOA. Do the authors answer how POA associated with BC? If these particles were emitted from sources, these mixing should occur in all the time, not just SE. Were there different sources in

[Printer-friendly version](#)[Discussion paper](#)

SE and FE? Seemly, the author didn't supply any wind and backtrajectories here. I would ask the authors carefully check the data. Make sure the differences in FE and SE are large. Here the authors only compared the organics. What about the sulfate and nitrate are in the coating of BC there. I am certainly struggling on the part.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-800>, 2018.

Printer-friendly version

Discussion paper

