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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Source apportionment and dynamic changes of carbonaceous aerosols during the haze bloom–decay process in China based on radiocarbon and organic molecular tracers" *by* J. Liu et al.

Anonymous Referee #3

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This study showed the measurements of radiocarbon, anhydrosugars, and water-soluble ions in PM2.5 collected in Guangzhou and Beijing, China. The authors found that non-fossil fuel sources make a large contribution to the total carbonaceous aerosols in Chinese megacities. The authors believed that both primary and secondary species are important to the haze formation in Chinese cities. As for me, the results presented in this paper are interesting and will expand our understanding of bad air pollution. This study has a clear logic writing and completely within the scope of ACP. Therefore, I recommend its publication after the following issues are addressed.

Comments: 1. My main concern is the limited period of sampling: only about 12 samples in each site without considering the different seasons or period of years that could influence the CAs emissions and sources.More information about the experiment should be presented in the text, such as reproducibility of the experiment result. 2. Please add the corresponding literature to support Lev/Gal/Man is the biomass burning-specific organic tracers.Line 213: 24-hour or annual standard? 3. Line 245: "the EC" → "EC". In addition, I agree with you that EC suspending over urban areas are dominated by fossil-fuel combustion. What about rural region or remote areas? Does any study focused on this? I think it would be better for readers to understand this paper if the authors can cite same papers conducted in rural/remote. Readers can get a direct comparison results for radiocarbon levels in mind. 4. Line 281: POC during atmospheric transportation may experience aging processes. I suggest the authors adding "gas" before "reactions". 5. Page 34955, Line 23: Both ambient OC and EC can be produced by the combustion activities such as coal and biofuel. I am curious why their conversion factors are slightly different. Specifically, the conversion factors mentioned in this study are 1.10 and 1.06 for EC and OC, respectively. The authors should clarify this. 6. Page 34958, Line 5 – 9: To my knowledge, the ratio of biomass-burning OC to levoglucosan is highly unstable in different emission sources. One of the most possibly most important reasons, in my opinion, is the various biomass types. In particular, Guangzhou is in south China while Beijing is in north China. How the author get the corresponding ratios to calculate the primary biomass-burning OC?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 34949, 2015.