

## ***Interactive comment on “Measurement report: Dual-carbon isotopic characterization of carbonaceous aerosol in Beijing and Xi’an: distinctions in primary versus secondary sources” by Haiyan Ni et al.***

### **Anonymous Referee #2**

Received and published: 20 July 2020

The overall quality

The article by Ni et al entitled “Measurement report: Dual-carbon isotopic characterization of carbonaceous aerosol in Beijing and Xi’an: distinctions in primary versus secondary sources” is presenting application of natural carbon isotopes to characterise carbonaceous aerosol composition in Beijing and Xi’an (two Chinese mega-cities). The title of the paper does not seem to fully reflect the subject of the article. In fact the spectrum of issues discussed in the paper is much wider than “distinctions in primary versus secondary sources”. While we are observing an increased interest in the topics related

Printer-friendly version

Discussion paper



to the study of air quality and its impact on the health of citizens, the development of various methods dedicated for identification the pollution sources is a very important topic that fits into the scope of the journal. Carbonaceous aerosols constitute an important fraction of air pollution observed in mega-cities. Application of carbon isotopes allows to identify the share and its temporal and spatial dynamics of different emission sources. In this context the paper address relevant scientific problem and demonstrates a possible solution based on comprehensive use of isotopic tracers applied for both elemental carbon (EC) and organic carbon (OC) fractions of carbonaceous pollutants. The presented methodology was applied for the short term measurement campaigns performed in two Chinese mega-cities having different emission sources structure. Authors demonstrated usefulness of this methodology for identifying share of the pollutants having different origin and presented an interesting data increasing the understanding of the differences between at first glance similar urban environments. The paper is well structured, the description of methodology is clear and in my opinion complete. Abstract contains a clear message of the paper. The results are correctly presented and in most parts well discussed. The quality and number of figures is correct. Presented results are discussed in relation to other studies appropriate referenced in the text. I recommend to publish the paper after a minor revisions.

### Specific comments

The authors discuss in the paper a wide spectrum of issues including different contribution of coal, liquid fossil fuel and biomass combustion to elemental and organic fraction of carbonaceous aerosols, temporal (day-time vs. night time and haze events vs. clean periods) variability as well as spatial (location specific differences between Beijing and Xi'an cities) variability. In addition an issues related to primary and secondary organic aerosols are discussed. In such wide range of discussed aspects it is difficult to keep the description clear. Maybe a short summary following each part of the discussion would help reader to keep on track of the analysis.

The Methodology section is very detailed and well referenced but I found no informa-

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

tion concerning possible mineral contamination (carbonates) of the collected samples is present. Were there any corrections to the mineral fraction contamination in the samples applied?

More detailed description of study sites (location, topography, typical emissions) would help to understand the differences in presented results between two cities.

Did the authors considered the admixture of bio-fuels into liquid fossil component in the context of F14Cliq.fossil parameter? Is it a case of Chinese liquid fuels market?

I don't see any clear reason for introducing equations 14 to 16.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-455>, 2020.

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