

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2022-105-RC2>, 2022
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Comment on acp-2022-105

Anonymous Referee #2

Referee comment on "Mixing state of black carbon at different atmospheres in north and southwest China" by Gang Zhao et al., Atmos. Chem. Phys. Discuss.,
<https://doi.org/10.5194/acp-2022-105-RC2>, 2022

This manuscript conducted DMA-SP2 measurements at three sampling sites in China and investigated the microphysical properties of BC-containing particles, including mixing state and morphology. The valuable and high-quality measurement data presented in the manuscript will help better understand BC properties in the different atmospheres in China, which is of great interest to the scientific community. Therefore, I recommend this manuscript for publication, as long as the following comments are properly addressed.

- Some sentences in the abstract seem to be redundant. Please try to make it concise.
- Line 62, I think this classification is scientifically inappropriate. There are also other types of BC particles, such as fresh fractal-shape aggregates, partially coated, etc.
- In the section of Part 3.1, the authors provide a detailed introduction of the three measurements, including the meteorology, gaseous pollutants, and PM2.5 pollution features. However, such information is not related to the topic of the manuscript, nor any of the discussion in the following parts. It is suggested that this part is simplified and focuses on what is related to the topic.
- Line 209, why in this study a different lag time (1.4 us) was used?
- Line 228, "the CP site is not far from the urban, and thus the fresh BC particles from the traffic contribute a large amount of the total ones." This part confuses me. Why the CP site instead of the urban site (PKU) is dominated by traffic emissions?
- Line 332, is electron microscopy data from this study or the previous study (Moteki et al., 2014)? If it is from the previous study, is the conclusion representative of the situation in this study?
- Figure 4, which sampling site is discussed here?