

Interactive comment on “Direct observations of organic aerosols in common wintertime hazes in North China: insights into their size, shape, mixing state, and source” by S. R. Chen et al.

Anonymous Referee #1

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This paper has presented a descriptive scheme of individual organic aerosol particles for the light and moderate hazes which were often seen in Northern China Plain. The mixing states and size distribution of the organic aerosol particles were discussed, and it is proposed that most of the organic aerosol particles were sourced from the uncontrolled domestic coal combustion. A number of the-state-of-the-art techniques were used in the characterization of individual particles, including TEM, nanoSIMS, and AFM. The results obtained are interesting and helpful for us to understand the forming mechanisms of the wintertime light and moderated hazes. I agree this paper to be accepted for publication after moderate revision.

My detailed comments on current manuscript are as follows:

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1. Selection of sampling sites needs to be introduced in more details, especially the importance and significance of the three sites. I understand that the Jinan city is regarded as the representative of uncontrolled coal combustion site, but in fact the urban area of this megacity might not rely on coal for domestic energy anymore and instead, the petrol and gas might be its major energy sources. This manuscript is designed to explain domestic coal combustion in the vast area of countryside, but no this type of sampling sites were selected.
2. “1 Introduction” part: the introduction needs to be succinct and should be more concentrated on the organic aerosol particles and related hazes. The current text in the introduction part is too complicated and not well focused on the main aims.
3. The classification of the six types of organic particles needs to be careful. For example, the type 4 particle (doom-like) looks more like a mixture of possible organic and other materials such as ammonia or nitrates, and the type 5 particle (dispersed) may be the results of the shrinkage of organic-coated particle.
4. Line 30: “(Tai, S2)” might be “(Mt. Tai, S2),”
5. Line 33: I suggest to change “OM-coating” into “coating OM” for the type 6 particle.
6. Line 71: “the various air pollution levels” may be changed into “the various air quality levels”
7. Line 74: the definition of “Haze as a weather phenomenon is defined by visibility \leq 10 km and RH \leq 95%” requires references.
8. Line 117 and also throughout the whole manuscript, acronyms and abbreviations must be explained at first occurrence. For example, the first appeared “BrC” should have a full word phrase.
9. Line 129-133: the methods mentioned here are repetitive of the “2 Experimental Methods” part.

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10. “2.1 Sampling sites and particle collection”: In Line 145, the authors mentioned that “aerosol particles collected at S1 mainly reflect local, ground-based urban and industrial emissions”. This means the S1 site can’t represent the potential uncontrolled coal combustion source?

11. Line 151: “During the winter monsoon season, S3 is the downwind of the Jing-Jin-Ji area . . .and Shandong province.” This looks not correct. From the map, S3 is located in the east of the JJJ area, and how can we regard it is the downwind of S1 and S2? Furthermore, S3 is not located upwind area, it may not appropriate to serve as a background (clean) site.

12. “2.3 NanoSIMS analysis” part: It is good to see that the NanoSIMS gives the ions $^{12}\text{C}^-$ and $^{12}\text{C}^{14}\text{N}^-$ which could represent the organic matter in individual particles. However, to the study of this manuscript, how many of the individual particles were analyzed? Were all particles measured by NanoSIMS?

13. Line 198: “with 20, 25, and 13 individual particles analyzed by this method for each of the three sampling sites.” Which sites exactly these numbers correspond to?

14. Line 229-230: Soot may also be the C-dominated particles?

15. Line 254: Please check if it is “. . .ratio of width and height..” or “. . .ratio of height and width. . .”.

16. 275-276: The category of “soot, mineral, metal, fly ash, and sulfate particles” is not same as that of the line 229-230?

17. Line 278: The OM-fly ash might be the overlapped particles during sampling and not necessarily the mixed particle in the air?

18. Line 314, “For example, Moffet et al. (2013) suggestedBased on these comparisons, we conclude that those type 1-3 OM particles were not emitted by vehicular emissions in the NCP”. However, all these data for comparisons were from North America and Japan, which I don’t think can exclude the type 1-3 OM particles present from

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vehicular emissions in urban areas of Chinese Cities.

19. Line 325, the authors didn’t analyze the emissions from heavy industries or coal-fired power plants, so I don’t think they can obtain the conclusion that “the type 1-3 OM particles were not emitted from heavy industries or coal-fired power plants” and that “they were from coal combustion or biomass burning for household heating and cooking in wintertime”. More evidence needs to be provided.

20. Line 413: Please check if the 1-3 OM occupy 70% of aerosol particles or 70% of the organic particles?

21. “5. Conclusions and atmospheric implications” needs to be simplified, and what are major conclusions?

22. “Acknowledgments”: There are some repetitive words between line 424 and line 425.

23. Table 1: The decimal number should keep consistent.

24. English of the text needs to be polished by a native English speaker.

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