

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-1007

Anonymous Referee #1

Referee comment on "PM2.5 Source Apportionment using Organic Marker-based CMB Modeling: Influence of Inorganic Markers and Sensitivity to Source Profiles" by Yingze Tian et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-1007-RC1>, 2022

The manuscript investigated the OC and PM2.5 sources with inorganic and organic source profiles, with the dataset of Chengdu of about 64 samples. Considering that the source profiles have been published in other journals, the datasets adopted in this manuscript are only the ambient PM2.5 samples actually. Compared with other similar papers published in ACP, such as the a recent paper of Srivastava et al. (2021, ACP) (two sites, one month for two seasons, with totally about 120 samples, and the filter-based source apportionment results were also compared with those of AMS) of the same group, I feel that this manuscript has a long way to move for possible consideration in ACP, with not only improving the scientific questions answered, the structure, the description, the format, the figures, the references and so on.

Following are the detailed comments.

Title:

how to understand the influence of sensitivity to source profiles? And how to under the the influence of sensitivity to source profiles on PM2.5 source apportionment? What is the meaning of sensitivity? It is quite unclear logically.

Abstract:

Line 20-22, the first and second sentences can be deleted as they are so common. The authors should directly give the main scientific questions existed or unsolved currently. For example, how and why the organic or inorganic source profiles adopted impacting the

CMB results is a good question for this study to answer.

Line 27-28, the authors should referred to other high quality journal paper and should be conscious that listing data is a kind of much low description, which is quite inapposite for a scientific paper especially with high quality.

Line 31-38, I found that no quantitative conclusions were given. I can not accept this. Poor correction, give the r value; higher, lower, overestimate, please use detailed data.

Line 37-38, scientists all know. Do not repeat. Please give the individualized suggestion or implication based on the main conclusions of this study.

Keywords:

These words are so common that they can not reflect the key questions the paper wanted to answer.

Introduction:

- Line 43-47, they can be deleted directly. I suggest that the authors can change them to "To design effective PM2.5 reduction strategies in polluted regions currently, more refined and accurate source apportionment results of PM2.5 are urgently needed".
- Line 57-63, delete them directly. The chemical compositions and formation mechanisms of PM2.5 is not the key problem to be solved of this study. They are so common descriptions which are not suitable for ACP or even a lower quality journal.
- Line 65-74, the organic markers and their adoption in CMB modeling should be better and thoroughly summarized. How and to what extent do they improve the source apportionment results? What are the new findings with organic tracers added compared with no organic tracers? And so on. All these are the base of this study.
- Line 65, many PM2.5 sources do not have a unique composition? If it stands, how can the formers conducting source apportionment studies? Such as cooking? Many sources, Why only cooking was listed? There are papers published on the source markers of cooking emission.
- Line 66, some organic compounds? Which?
- Line 70-71, has been widely used, but the author give no references.
- Line 73, why OM-CMB can not estimate contributions of inorganic ions. I think it is the key problem that the authors should answer with the dataset obtained.
- Line 76-77, are all the papers listed here adopting no local source profiles? I can not believe so.
- Line 97-100, they can be deleted. The air quality, energy consumption, vehicle numbers and so on which impact the air quality should be described clearly.

- Line 102, the sampling map should be given.
- Line 145, extracted for 10 min and repeated for 3 times.
- Line 161, delete it.
- Line 182, why 1.8 was selected, not 1.4, 1.6 or others?
- Line 187-191, so common, delete them directly.
- Line 208, the source profile of domestic coal burning is not given?
- Line 227, the MSR method for calculating SOC has been published in recent years.
- Line 247-249, I believe the source profiles of gasoline in China are abundant. The inorganic ions and elements are not given in Cai et al. (2017), why the authors refer it? I can not understand. Showed to shown.
- Line 255-257, line 260-261, line 268-270, line 272-275, line 291-292, line 295-300, line 398-399, I can not believe that it is a paper written by the authors wo have published many papers already. Do you want to increase the length of the paper? Please give the main rules hided behind the data, not repeat the data.
- Line 281-282, why these sources showed no seasonally variation? It is unbelievable. The combustion condition, the rain, the emission conditions of them in summer and winter are quite different. I believe that it may be related with the limited sampling numbers of this study.
- A biggest problem of the results and discussion is that the author give no quantitative results for any comparison. For example, in Line 302-325, obvious higher contributions during the cold period, emitted more PM, were higher during the dry season, a large percentage, higher fractions during autumn and winter, weaker seasonal variation, high wind strength, strong illumination, less precipitation, high temperature, higher contributions, high precursor concentrations, humidity and PM, the high relative humidity during wintertime, etc. All these data can be obtained by the authors, but no were given. Also no statistic test of the comparison was done.
- Line 316-317, the wet cleaning of them is also higher in summer than that in winter.
- Line 320-321, the recent references should be cited.
- Line 345, in agreement with each other, how to judge?
- Line 347, was more consistent with, how to judge?
- Line 358, very different from other source, how to judge? What are the markers of other sources?
- Line 360, moderately consistent, what is moderately? How to judge?
- Line 362-363, it is sure. What is the main finding of this study. Some differences indicate what extent? Difference is difference, how to understand some differences?
- Line 372, I don not believe it is necessary for such kind of comparison with the replacement of noncatalyst vehicle profile. Of course, the scientists will select the source profiles obtained in China and in recent years. Why the authors selected such two source profiles with obvious difference, with one for USA (Schauer et al., 2002) and one for China (Cai et al., 2017).
- Line 389, with the results using our gasoline vehicle profiles, it is not your source profiles, but cited from formers.
- Line 383, little is how many? How to understand the central city of southern China? Is residential coal not low in non-central city of southern China?
- Line 390-391, Line 407-409, nothing is said. Do not repeat what we already know in the conclusion. Please give the main and specific findings and implications of this study.
- Line 404-405, poor descriptions. What is "the OM-CMB resuspended dust"? It should be the contributions of resuspended dust obtained from the OM-CMB modeling. The comparison is for the source contributions, not for the sources.
- Reference: most of the journals are below the levels of ACP? How can this manuscript published on ACP? Most source apportionment studies published on Nature, ACP, EST, JGR, EI, etc are not cited.
- Figures, the authors should refer to the figure styles of papers on ACP.
- Figure 3, the days are not continuous, so column figures should be used.
- Figure 4, it can be separated and detailedly discussed for each source.
- For all the figures, only day variation, pie figures, etc are given, which indicating that

the detailed analysis of the results are not done and quite necessary. More abundant types of figures are needed. Please see the papers already published.