

Interactive comment on “The role of traffic emissions in particulate organics and nitrate at a downwind site in the periphery of Guangzhou, China” by Yi Ming Qin et al.

Anonymous Referee #1

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Qin et al describe measurements of PM composition at a suburban site in the Pearl River Delta, China. The paper is topically relevant to ACP. The data collection methods, analysis, and interpretation all seem appropriate. However, the authors do not clearly articulate what new knowledge is generated by this manuscript, aside from a relatively small (2 month) dataset collected in a city without previously published AMS data. A revised manuscript should place more emphasis on the unique contribution of this work.

I have several comments on Section 2.3. Overall, this section seems more like Results than Methods, since much of the section is dedicated to comparing the various constrained versus unconstrained PMF solutions. Specific comments for this section

C1

include:

1. The main goal of the text from ~line 131-189 is buried. My interpretation is that the authors are describing their efforts to separate an HOA factor from a cooking (COA) factor, and that these two factors do not cleanly "fall out" from unconstrained PMF (e.g., moving from 3 to 4 to 5 unconstrained factors starts splitting OOA factors rather than splitting HOA and COA).
2. I am confused by Lines 167-175. I think the authors performed a separate unconstrained PMF analysis of high concentration events, assumed that the high concentration peaks are dominated by HOA, and used the resulting HOA factor from these high events to constrain the more general HOA mass spectrum. If this is the case, the description is extremely confusing (I had trouble determining if lines 167-175 describe a single PMF solution approach or multiple), and the utility of this approach is not described well. Also, the authors do not describe what evidence they used to attribute the high concentration events to traffic/HOW.
3. Lines 190-201 describe an "optimum" solution (the authors should be careful with this word because their source apportionment was not formally optimized) and compare 10 runs of that optimum solution. What is changing between these 10 runs? It is not explained why the 10 runs might result in different answers.
4. Does the final PMF solution incorporate the high concentration events, or exclude them? E.g., does the pie chart in Figure 5 include the high concentration events?

Comments on Section 3.3 - Formation of SOA

1. The main idea of this section seems to be that SOA formation is dominated by gas-phase oxidation chemistry (hence a strong SOA-to-Ox relationship) rather than heterogeneous or aqueous oxidation pathways. This should be stated more clearly.
2. The SOA/Ox slope is higher than other locations (Table 2). The humid tropical climate is given as the reason. Can this claim be substantiated more quantitatively?

C2

Another possible explanation is that emissions are different in the PRD than the North American cities listed in Table 2. The authors should address possible explanations besides climate.

3. One could argue that Figure 7b shows that SOA formation was essentially independent of temperature for this city over the temperature range sampled in November and December.

Other comments

1. Line 91 - Please provide details for the manufacturer of the MARGA
2. Line 89 - what method does the Grimm 180 use for determining PM mass?
3. Line 92-93 - Please provide more detail on how the Grimm 180 data were corrected with quartz filter data. Did the correction use bare quartz filters only?
4. Section 2.2 needs to be broken into multiple paragraphs.
5. Line 118 - Need citation for the Middlebrook paper
6. Figure 2b - label says NH₃, should be NH₄
7. Line 343 - it is not clear what "this method" and Method 1 refer to, specifically.
8. Figure 6 - In December it seems that the mean HOA concentration is often outside of the 75th percentile. Are the diurnal patterns in this month skewed by a few very high concentration days?
9. Figure 6 - It looks like the average LVOOA is about 1 $\mu\text{g}/\text{m}^3$ higher in December than November. Given that there is less photochemical activity, does this seem physically reasonable? Or could it be an artifact of the source apportionment?
10. Fig 4 - C₃H₃O does not seem like a great COA tracer. Is there a scatter plot showing the relationship of COA with this tracer ion?

C3

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C4