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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization and comparison of $PM_{2.5}$ oxidative potential assessed by two acellular assays" *by* Dong Gao et al.

Anonymous Referee #2

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In this work, the authors compared the results from 3 different acellular assays of oxidative potential in 2 different media. OP has recently become a popular topic of research due to its potential to represent PM's ability to drive oxidative stress and explain PM health effects. Understanding the assays used to measure OP is an important topic for atmospheric chemists, because they will provide insights into sources and/or compounds that may be particularly toxic. The authors found different level of sensitivities of these assays to different components, such as copper, iron, and organic compounds. These relationships were investigated by association, using multilinear regression models. Overall the results are a valuable contribution and are complementary to those currently in the literature. I just one major point of concern, and I hope the authors will consider it while revising the manuscript. I recommend publication in ACP

after considering these questions/comments.

My major issue with this work is the use of a per-air volume measure of OP (extrinsic OP) rather than a per-PM mass measure. All the comparisons made here are chemical, with the attempt to associate a particular fraction of PM to its contribution to OP. In that case, I would argue that the OP should be an intrinsic measure (i.e. oxidant depletion rate per PM mass). Otherwise the variability could be driven by total PM mass. I understand that the assays were performed on a per filter basis (which is equivalent to a per-volume basis), and it might be difficult to fix the amount of PM mass used to analyze OP. At the very least, there needs to be a discussion examining whether or not the variability in OP (and therefore the reported associations shown here) is driven by the PM mass, rather than its composition.

Other minor comments: 1. Does RTLF composition change with different regions in the lung? Given the sensitivity of the assay results to the relative concentrations of AA and GSH, this may be important. (This may seem like an obvious question to medical researchers or toxicologist, but an atmospheric audience for ACP might not understand.)

2. Samples are collected on a daily basis. Would that bias against sources that vary on shorter timescales (i.e. traffic-related emissions of metals)? If so, that should be stated as a limitation of this study.

3. Should we really expect a difference between summer and winter, given that the climate in Atlanta is similar between the seasons? What are the known differences in the sources between summer and winter this area? This type of comparison can be somewhat misleading and is likely not generalizable to other regions, because every city might have its own characteristic summer/winter sources. Just seeing "summer" and "winter" in analysis, one could jump to the wrong conclusions.

3. It would be useful to state in the Methods section the concentration of PM during these assays. PM concentrations should be much lower than those of the antioxidants

to ensure one is looking at the catalytic redox cycling.

4. Limits of detection and quantification for all of the assays should be reported.

5. The discussion around BrC comparison needs to be better motivated. It is not clear why that comparison was made in the first place, other than that measurement was available and it was convenient to make that comparison. BrC from biomass burning, for example, can be derived from nitrophenols, and is not exclusively HULIS. Unlike the other chemical species, BrC is not chemically defined, but rather an optically defined group of compounds, so their contribution to OP might not be straightforward.

6. Why is EC not included in the multilinear regression analysis? It seems to have a reasonable Pearson's r from Table 1.

Technical/formatting comments:

Line 164: typo after GR Line 169: typo in 2-vinylpyridine; not sure if the abbreviation 2-VP is needed if it is not used again Line 185: replace "required" with "performed" Line 257: If UA is not studied here, it might be better not to include UA in this comparison Line 266: "consistent lower" should be "consistently lower" Line 365: "shown the strongest estimated effect" is a strange word choice. Perhaps "estimated to have the strongest effect"? Line 388-390: The sentence here is stylistically awkward and grammatically incorrect. Table 2: the number of digits in the exponent are not consistent (some are E-3, and some are E-05)

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