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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hydrocarbon-like and oxygenated organic aerosols in Pittsburgh: insights into sources and processes of organic aerosols" *by* Q. Zhang et al.

Anonymous Referee #1

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Review of 'Hydrocarbon-like and oxygenated organic aerosols in Pittsburgh: insights into sources and processes of organic aerosols' by Q. Zhang, D. R. Worsnop, M. R. Canagaratna, and J.-L. Jimenez

Overview: Overall, this is an extremely well written manuscript. There are minimal errors and the paper is extremely well laid out and clear. The content of this study is timely as the roll of organic material in atmospheric aerosol is currently of interest in atmospheric science. The paper will make a good addition to the literature and is appropriate for ACPD.

Comments:

1. AMS aerosol characterization is laid out in this paper up to 1000 nm. I notice from previous work that the AMS is increasingly inefficient above (numbers vary) about 600 nm. Why is 1000 nm used? A cryptic statement is made in the Experimental about inlet size differences and references to other papers. 2. It appears many of the figures (e.g., 1 and 13, 2 and 3, 5 and A1, etc.) hold exactly the same information plotted in either identical or very similar ways. As a consequence this results in a much longer paper than it need be. 3. It is my understanding that the AMS does not efficiently measure EC. Although the topic is mentioned in passing throughout the paper I can find no single description of how HOA is related to POA is related to EC and why it is expected that the AMS would be used to determine the quantity of this species. Conversely, it is expected that OOA is SOA which is accessible to the AMS. Specifically, there is a comparison of the POA/SOA to EC/OC ratios made but if, as is my understanding, the AMS can not quantify EC then why is it expected that this comparison can be made? 4. It would be useful for the authors to comment on the need for yet another AMS publication from Pittsburg (from the references there appear to be at least two others plus a fourth paper on the HOA / OOA method). It is not clear why all this work is not presented in a comprehensive work from Pittsburg or, otherwise, why is the HOA / OOA method not described for all recent AMS work in a single comprehensive manuscript (a reference is made to a paper (Dzepina 2005) that would appear to be this same paper but using data from Mexico). To be clear this is an excellent paper but this group of authors needs to begin to self impose a restriction on the shear number of recent papers from AMS instruments that are appearing in the literature. A quick scan of recent ACPD papers illustrates this point as well as anything. It will be highly disappointing if this paper is but the first of many individual papers on HOA vs. OOA from a multitude of field locations. If this is the authors plan I would HIGHLY recommend that they remove this paper and a SINGLE comprehensive manuscript be written.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 8421, 2005.

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Interactive Comment