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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chemical oxidative potential of secondary organic aerosol (SOA) generated from the photooxidation of biogenic and anthropogenic volatile organic compounds" *by* Wing Y. Tuet et al.

Anonymous Referee #1

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Summary: This study uses molecular probe DTT to measure oxidative potential of SOA coming from different precursors and generated under different conditions (dry, humid, high NOx). The results show that despite differences in SOA chemical composition and ageing conditions, for most precursors there is no substantial difference in terms of their DTT activity, except for naphthalene SOA. The manuscript is well written and clear and appropriate for publishing in ACP. The study is addressing an important issue of the health effects of PM. SOA is an important (and frequently dominating) part of urban PM and yet there haven't been many studies addressing health effects of SOA. I recommend a few smaller changes and addressing the following comments:

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Major comments:

1. Different cell-free assays have been used to measure oxidative potential (OP) of PM (DTT, DCFH, AA, BPEAnit); however, none of them has yet been recognized as “the best” for this purpose. Moreover, there are a few studies comparing OP trends of two or more assays and their responses towards different PM components (e.g. organic content, metals) [1-4]. These studies have generally found low to moderate correlation between different assays and suggested use of more than one assay to get more comprehensive picture on the OP of PM. This means that if this study had used a different acellular assay that the results and conclusions of it could have been different. The authors should acknowledge the existence of other assays, including their differences in rresponses towards different PM components and justify their choice (DTT). 2. Prior to measuring OP the authors used sonication (1 hr) to extract particles collected on filters. Sonication of water is known to produce OH radicals and it has been used to study degradation of organic compounds in wastewater treatment [5 and references therein]. Therefore, it is quite possible that 1 hr sonication resulted in change of SOA. If possible, it would be good if authors could collect SOA from at least a couple of precursors on filter(s) and attempt to quantify the difference in DTT response between 1 hr sonication and some other, less invasive extraction technique (e.g. shaking).

Minor comments:

Page 3, line 50: in addition to producing ROS, PM also can contain ROS.

Page 5, line 83: Could it be that the dose was different? If yes, it should be mentioned as a possible reason for different findings.

Page 6, line 120: instead of ft, meters should be used.

Page 6, line 121: can you please indicate manufacturer and model of black lights and fluorescent lamps? Page 8, line 151:What was the zero air flowrate?

Page 8, line 168: how long was the PM collected for, what was the flowrate

Page 9, line 170: why wasn't the mass determined by weighing by difference? Was the mass too low? What densities were used to convert from volume to mass concentration?

Page 9, line 181: how were the extracts stored between extraction and analysis?

Page 12, line 247: What was different in the method used by Kramer et al?

Page 13, line 271: Pi bonds instead of pie bonds.

Page 13, line 276 – naphthalene PM was also more active under humid conditions

Page 13, line 280 – how much different? It is visible from the figure, but the figure is in the Supplement, so it would be good to get that information without necessarily going to the Supplement.

Page 14, line 290: in this section the difference in the OP of naphthalene SOA formed under dry and humid conditions is discussed. As m-xylene and naphthalene both show difference in the OP for dry and humid conditions and the difference in f44 is suggested as a possible reason for that, it would be good to compare f44 of naphthalene SOA for different conditions.

Page 15, line 322: In Fig 3 O:C ratio for each aerosol is presented by one point – does this mean that O:C was not changing during the PM collection period? I am assuming that aerosol was collected at the end of ageing, after the lights were off, however, that is not clear from the experimental section.

Page 16, line 339: different OA subtypes from which ambient data? Reference needed here

Page 16, line 341&342: From figure 2 it looks like there is not much of a difference in the OP of SOA coming from beta-caryophyllene and pentadecane. Page 20, line 442: the sentence should read: " This is consistent with many studies using DTT to show oxidative potential associated with sources related to incomplete combustion.".

Page 24, Fig 3: What is MO-OOA? Can that be thought of as more widely used LV-OOA? Or somewhere between SV-OOA and LV-OOA?

Page S1, Figure S1: Naphthalene's mass spectra is quite different from mass spectra resulting from SOA coming from other precursors. That should be discussed in the main text.

References:

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2. Jean-Jacques Sauvain , Michel J. Rossi & Michael Riediker (2013) Comparison of Three Acellular Tests for Assessing the Oxidation Potential of Nanomaterials, Aerosol Science and Technology, 47:2, 218-227

3. Marco Visentin, Antonella Pagnoni, Elena Sarti, Maria Chiara Pietrogrande, Urban PM2.5 oxidative potential: Importance of chemical species and comparison of two spectrophotometric cell-free assays, Environmental Pollution, Volume 219, December 2016, Pages 72-79

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5. B. Miljevic, F. Hedayat, S. Stevanovic, K. E. Fairfull-Smith, S. E. Bottle & Z. D. Ristovski (2014) To Sonicate or Not to Sonicate PM Filters: Reactive Oxygen Species Generation Upon Ultrasonic Irradiation, Aerosol Science and Technology, 48:12, 1276-1284

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