We thank the reviewers for their time and comments. Below are detailed responses to each comment.

Response to anonymous referee #1 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 responses towards different PM components and justify their choice (DTT).**

   Thank you for your suggestion. We have incorporated this into the manuscript. We did not discuss the sensitivity of different assays to different metals as our study did not involve metal species.

   Line 95: "In the present study, the water-soluble oxidative potential of SOA generated from various precursors under different reaction conditions was measured using the DTT assay (henceforth referred to as $OP^{WS\text{-}DTT}$). While numerous cell-free assays have been developed to measure oxidative potential, the DTT assay is well-suited for the purposes of this study due to its proven sensitivity to organic carbon constituents and correlation with organic carbon (Janssen et al., 2014; Visentin et al., 2016). Furthermore, there are many previous studies reporting the DTT activities of laboratory-generated SOA and ambient samples for comparison purposes (Kramer et al., 2016; Bates et al., 2015; McWhinney et al., 2013a; McWhinney et al., 2013b; Verma et al., 2015; Xu et al., 2015a; Xu et al., 2015b; Fang et al., 2015; Lu et al., 2014). VOCs were chosen to represent the major classes of compounds known to produce SOA upon oxidation by atmospheric oxidants and to include precursors of both anthropogenic and biogenic origins (Table S1)."

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).**

   A study by Gao et al. (in prep) using the same DTT system as that of this study showed that sonication of ambient samples has negligible effects on the DTT activity when compared to shaking. Furthermore, Antinolo et al. (2015) showed that the effect of peroxides generated via sonication on DTT activity was negligible.

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

   We have modified the manuscript accordingly.

   Line 49: "…PM can contain ROS/RNS and generate ROS/RNS via…"

4) **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.**

   Thank you for your suggestion. We have modified the manuscript accordingly.

   Line 83: "Furthermore, results from cellular exposure studies are inconclusive, with some studies finding significant response from SOA exposure and others finding little to no response. The exposure dose also differed from study to study, which may result in inconclusive results. This also highlights a need to consider dose-response relationships as demonstrated recently in Tuet et al. (2016)."

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

   We would prefer to keep the current units as the chamber facility has been described in previous studies as such (Boyd et al., 2015; Nah et al., 2016).

6) **Page 6, line 121: can you please indicate manufacturer and model of black lights and fluorescent lamps?**

   We have modified the manuscript to include this information.

   Line 130: "…black lights (Sylvania 24922) and natural sunlight fluorescent lamps (Sylvania 24477)."

7) **Page 8, line 151: What was the zero air flowrate?**

We have modified the manuscript to include this information.

Line 160: "…passing zero air at 5 L min^{-1} over the solution…"

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

We have modified the manuscript to include this information.

Line 176: "Aerosol samples were collected onto 47 mm Teflon^{TM} filters (0.45 µm pore size, Pall Laboratory) for approximately 1.5 hrs at a flow rate of 28 L min^{-1}."

9) **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?**

Mass loadings were low for isoprene and pentadecane. To be consistent, we choose to determine mass by integrating the SMPS volume concentrations for all SOA systems. An aerosol density of 1 g cm^{-3} was assumed.

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

Extracts were analyzed immediately after extraction. We have modified the manuscript to clarify this.

Line 190: "All filter samples were extracted within 1-2 days of collection and analyzed immediately following extraction"

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

We have modified the manuscript to include specific differences.

Line 257: "…they utilized a different method for measuring DTT consumption (i.e., different extraction solvent, different initial DTT concentration, different method for quantifying DTT activity)…"

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

We have modified the manuscript accordingly.

Line 282: "…and associated pi bonds…"

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

We discussed naphthalene SOA in the next paragraph, beginning line 301, and have noted that naphthalene SOA is more DTT active under humid, $RO_2 + HO_2$ conditions.

14) **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.**

We have modified the manuscript to include this information.

Line 289: "More specifically, *m/z* 44, which serves as an indication of oxidation (O:C ratio) (Ng et al., 2010), is very different for this experiment (dry signal: 0.098 vs. humid signal: 0.15)."

15) **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.**

We observed the same m/z 44 and OP trend for m-xylene and several SOA systems, where higher m/z 44 corresponded to higher OP. However, we observed a different trend for naphthalene where a lower m/z 44 corresponded to a higher OP. We believe naphthalene SOA may be an outlier due to its aromatic ring-retaining products. Furthermore, effects of humidity and correlations with elemental ratios appear to be highly dependent on hydrocarbon precursor.

16) **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.**

All elemental ratios were stable during the filter collection period. We have modified the manuscript to clarify this.

Line 330: "Bulk aerosol elemental ratios (O:C, H:C, and N:C) were also determined for each SOA system as different types of aerosol are known to span a wide range of O:C (Chhabra et al., 2011; Lambe et al., 2011). All elemental ratios were stable during the filter collection period and could thus be represented by a single value."

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

We describe the ambient data previously in line 338 "Ambient data included in Fig. 3 are for different organic aerosol subtypes resolved from PMF analysis of AMS data collected in the southeastern U.S. (Verma et al., 2015; Xu et al., 2015a; Xu et al., 2015b)." We have included references in the mentioned sentence to clarify.

Line 350: "This is true for both the laboratory-generated SOA in this study and the different OA subtypes resolved from ambient data (Verma et al., 2015; Xu et al., 2015a; Xu et al., 2015b)."

18) **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.**

The difference for β-caryophyllene and pentadecane are small and are more noticeable in Fig. 2. We have included a reference to Fig. 2 in the sentence to clarify.

Line 353: "Indeed, for several SOA systems (β-caryophyllene, pentadecane, and *m*-xylene), SOA with higher O:C ratios also had a higher intrinsic $\mathrm{OP}^{\mathrm{WS\text{-}DTT}}$ (Fig. 2, 3)."

19) **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.".**

We have modified the manuscript accordingly.

Line 454: "This is consistent with many studies using DTT to show oxidative potential associated with sources related to incomplete combustion (Bates et al., 2015; Verma et al., 2014; McWhinney et al., 2013b)…"

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

MO-OOA is the same as LV-OOA. Xu et al. (2015b) used MO-OOA instead of LV-OOA as it was found that O:C ratios were not always correlated with aerosol volatility (Xu et al., 2014; Hildebrandt et al., 2010).

21) 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.

We have discussed the key AMS peaks associated with increased $OP^{WS\text{-}DTT}$ *in the paragraph beginning line 301.*

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