

## Comments on “Relationship between Chemical Composition and Oxidative Potential of Secondary Organic Aerosol from Polycyclic Aromatic Hydrocarbons”

In this manuscript, the authors assessed the DTT activities of lab-generated SOA using flow tubes from two PAHs (phenanthrene and naphthalene). The mass normalized DTT activities of SOA were compared to those from monoterpenes (limonene and  $\alpha$ -pinene). Peroxide content was also determined to assess the contribution from peroxide to OP. Aging effects from oligomerization, heterogeneous oxidation, and mixing with copper were investigated on the DTT activities of SOA. In my opinion, this is an important work. It provides reference to future studies on the DTT activities of SOA and addresses some current open questions regarding OP. It provides a new angle of looking at chemical composition that contributes to OP and health. The manuscript overall is fairly strong and I recommend acceptance of the manuscript after following small corrections are done.

1. Line 36-37, the authors stated “Oxidative stress has been..., and is often expressed as the oxidative potential (OP)”. This sentence is misleading in that “oxidative stress” is “oxidative potential” while these two terms are different. The authors should make sure the definitions of these two terms are clear.
2. Line 37-38, delete “mass normalized”. OP can be expressed in “volume normalized” as well.
3. Section 2.3, the authors quoted McWhinney et al. papers for the DTT protocols, despite that, detailed protocols should be provided either in method section or the supplement. For example, what is the volume of sample added to each well, what is the concentration of DTT solution (i.e. the initial DTT concentration in the reaction), concentration of DTNB, was DTT consumed more than 50%, and etc.
4. Line 173, the SOA was extracted in Methanol then blown down to complete dryness before re-dissolving in phosphate buffer. Have the authors assessed the vaporization loss of SOA upon complete dryness? Some Methanol-soluble compounds might not be solubilized in phosphate buffer which is mainly DI. The authors could in a way underestimate the OP of SOA due to artifacts from complete dryness.

5. Line 188, should be “Fig. S2(b)”?
6. Line 363, peroxide content from  $\alpha$ -pinene are 40-100% in this study which is larger than other previous studies (Docherty et al., 2005; Epstein et al., 2014; Mertes et al., 2012) where roughly 20-60% of peroxide are found in  $\alpha$ -pinene SOA. Please explain why such large variation in this work and larger values compared to other studies.
7. Line 379, please explain why the number of DTTm of benzoyl peroxide is 38 pmol/min/ug while that in Table 2 is 160 pmol/min/ug.
8. The initial DTT at t=0 (18000 pmol) in various types of peroxides in Figure 3 are different from that in blank controls (~37000 pmol, Figure S2). Please explain why the authors chose different initial DTT amount to begin with between blanks and samples.
9. Figure 6, asterisk missing?
10. Figure 9, can the authors comment on the delta decrease of T2 relaxation larger in the lower  $\delta(^1\text{H})$  range than higher range with Cu added to SOA compared to SOA alone? This is not observed in 1,2-NQN case (Figure 11).

Docherty, K. S.; Wu, W.; Lim, Y. B.; Ziemann, P. J. Contributions of organic peroxides to secondary aerosol formed from reactions of monoterpenes with O<sub>3</sub>. *Environ. Sci. Technol.* 2005, 39(11), 4049–4059.

Epstein, S. A.; Blair, S. L.; Nizkorodov, S. A. Direct photolysis of  $\alpha$ -pinene ozonolysis secondary organic aerosol: effect on particle mass and peroxide content. *Environ. Sci. Technol.* 2014, 48 (19), 11251–11258.

Mertes, P.; Pfaffenberger, L.; Dommen, J.; Kalberer, M.; Baltensperger, U. Development of a sensitive long path absorption photometer to quantify peroxides in aerosol particles (PeroxideLOPAP). *Atmos. Meas. Tech.* 2012, 5 (10), 2339–2348.