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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Predominance of Secondary Organic Aerosol to Particle-bound Reactive Oxygen Species Activity in Fine Ambient Aerosol" *by* Jun Zhou et al.

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Review of acp-2019-190

The manuscript by Zhou et al. presents results from real time PB-ROS measurements, conducted with a DCFH probe, both in field as well as in smog chamber studies. The authors show that the OOA is the main contributor to the to the PB-ROS. What is further of interest is that the ROS measurements from chamber studies show a similar level of PB-ROS to the ambient measurements and that the oxidation of anthropogenic gaseous precursors is a significant contributor to the PB-ROS and will dominate the PB-ROS in urban environments. This is an important finding and will significantly con-

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Discussion paper

tribute to our understanding of the PM health effects. Further the manuscript clearly points to a need to use different assays “to better assess relations between PM composition, oxidative potential and possible health effects.”

Overall this is a very well written manuscript and should be accepted after some minor improvements. The only more major comment is that the authors do not really have a long time data set. On the other side taking into account that real time PB-ROS measurements are fairly complex and still in development this is not surprising.

Questions/Suggestions for improvement:

p.4 Instrumentation. Although a detailed description of the on-line PB-ROS instrument was given it would be beneficial to mention few more characteristic of the instrument. For example what was the flow rate of the instrument and what was the time resolution. Also if you have some estimates on the sensitivity of the instrument it would give more confidence in the observed data. I understand that the sensitivity cannot be expressed in units normalized per PM mass but expressing this normalized per volume of air sampled (nmols/m3) would be possible.

p.4, l.139 “Before the aerosol collector, a honeycomb charcoal denuder was installed in a stainless steel tube to remove interfering gas phase compounds.” Do you have any proof of the effectiveness of the denuder in removing the gas phase compounds? It has been shown that the gas phase ROS can significantly contribute to the total ROS (particle + gas phase) therefore if you do not efficiently remove the gas phase your PM-ROS measurements could be biased.

p.5, l.181 VACES. Could the condensation of water on the particles followed by evaporation, that we have in the VACES, have any influence on the PB-ROS concentrations. A comment on this would be beneficial.

p.11, l.386 “Furthermore, in Fig. 3a we show the potential contribution from primary sources by adding one standard deviation to the regression coefficients listed in Table

1 (function 1)." I cant see this in figure 3a. Is this missing from figure 3a or I did not understand the statement?

p.11, l.405 "Our values for ROSPM2.5 (0.07 $\pm$ 0.04)..." what are the units?

P12, l.422 "However, Figure S11 shows that the averaged PB-ROS content in SOA from different emission sources does not significantly depend on these parameters. . ." To which of the 2 parameters, degree of oxygenation and/or the OA loading, are you referring to or both? It has been shown in the past both by Stevanovic et al (2013), Hedayet et al (2016) as well as your measurements Zhou et al (2018) (Figure 4) that the degree of oxidation expressed through f44 has an influence on the PB-ROS. Are you claiming that this is the case only for one type of source i.e. primary diesel or wood combustion? If yes please be more specific and argument this.

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