Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-666-RC2, 2021

Comment on acp-2021-666

Anonymous Referee #2

Referee comment on "Are reactive oxygen species (ROS) a suitable metric to predict toxicity of carbonaceous aerosol particles?" by Zhi-Hui Zhang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-666-RC2, 2021

The paper from Zhang et al. discusses the suitability of ROS to predict the toxicity of carbonaceous particles. Several measures of ROS such as particle-bound ROS, cellular ROS and acellular ROS were used and the results were compared with the cellular toxicity (i.e. viability of the cells). Two SOA precursors, i.e. biogenic and anthropogenic precursors were used and the results essentially revealed that photochemical aging of both SOA increases both the particle-bound ROS as well as the OP, which correlates with the cellular toxicity. This is a nicely written manuscript and shows important results. I recommend its publication in the journal ACP. However, I have some comments as below:

- Page 11, line 303: The authors attributed the differences in the content of organic peroxides vs. total ROS in naphthalene vs. pinene-derived SOA to the oxidation regimes. My 1st question: isn't there much more in the total ROS than simply the organic peroxide? And, if so, is this comparison valid? My 2nd question: if it is attributed to the oxidation conditions, then which regime is more atmospherically relevant (photo-oxidation vs. ozonolysis)?
- Page 12, Line 333: Do you really have to store the filters for 6 months? It is kind of expected that most of the particle-bound ROS will be lost in that time-frame. A more relevant experiment could have been analyzing the filters after couple of days (which is equivalent to ambient filter sampling for days), so that the effect of the integrated filter sampling, could have been better captured.
- Section 3.4: I think the relevant discussion of this section actually starts from line 444. The discussion above that line does not fit under the heading of this section. Some rearrangement is warranted in this section.
- Lines 450: The insignificant toxicity of fresh or aged-soot particles is surprising and inconsistent with the previous studies. I think it is related with water-insolubility of the soot particles. Did the authors make sure that soot particles remained suspended and are not lost?
- The trend of carbon oxidation state vs. ROS content does not match in Table 1 vs.

Figure 3. Figure 3 shows an increase in the ROS content with the carbon oxidation state while Table 1 shows the reverse trend (see top two rows). Can the authors provide an explanation?

- Line 533: Since the authors didn't measure different ROS components (and just hypothesized), I don't think this sentence is well supported from the authors' results.