

***Interactive comment on “Technical Note:  
Particulate reactive oxygen species  
concentrations and their association with  
environmental conditions in an urban, subtropical  
climate” by S. S. Khurshid et al.***

**Anonymous Referee #3**

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Overall this is a well written manuscript and the topic is of interest to the readers of ACP. Although the amount of data collated is reasonably large the overall conclusions that the authors draw out of this data is a bit modest. The main conclusion is that the strongest correlation is between the ROS concentration (as measured with the DCF-DA assay) and ozone but also there is a strong correlation with solar radiation. The question is still open to whether the main confounding factor is ozone or solar radiation. This issue has been raised by one of the reviewers as well.

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The other more important issue that I have with this manuscript is in the way the authors report the ROS concentration. To be compatible with the previous measurements using DCFH-DA they have chosen to represent the data in units of nmoles/m<sup>3</sup>. In some of the previous studies with the DCF-DA data on the particle mass was not available or was not taken sufficiently reliable so it was justifiable to present the ROS concentration as normalised per volume of air. In this study the authors have measurements of particle mass so it would have been of benefit if the ROS concentration was presented as normalised per mg of PM. This study is discussion the ROS related to PM and its correlation to meteorological conditions. Additional useful information on ROS coming from the secondary processes versus ROS related to primary emissions (i.e. Traffic) could have been seen if the data was presented as per mass of PM.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 5061, 2014.

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