

## ***Interactive comment on “Enhanced toxicity of aerosol in fog conditions in the Po Valley, Italy” by Stefano Decesari et al.***

### **Anonymous Referee #3**

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Review of Decesari et al.

The paper from Decesari et al., measures the capability of aerosols to generate reactive oxygen species during the fog conditions in Po, Valley, Italy. The authors use a suitable assay, which is biologically relevant to indirectly interpret the toxicity of ambient aerosols. The study is well conducted although the sample size is small ( $n \sim 6$ ). However, the reviewer can understand the intricacies involved in collecting the samples with enough mass for conducting all the toxicity and chemical analysis, particularly in the ambient conditions as in the study. The measurements are novel and this is probably the first time, ROS activity of the fog has been measured. The manuscript is very well written and the results are interesting and highly useful from the perspective of policy intervention, particularly in controlling the trans boundary movement of the pollutants. I support the publication of this manuscript. However, I have few comments below,

which can help the authors to further improve upon their work.

Page 1, Line 34: There have been many epidemiological evidences showing the links between traffic pollution and adverse health effects (e.g. Brunekreef et al., *Journal of Exposure Science and Environmental Epidemiology* (2007) 17, S61–S65; doi:10.1038/sj.jes.7500628). Janssen et al., 2011 is not the first evidence.

Page 2, Line 1: Bates and Fang et al., 2015 were not the toxicological studies. Bates et al., 2015 should be considered as epidemiological study and Fang et al., 2015 didn't report any direct linkages with the health impacts.

Page 3, Line 15-16: The conversion of  $\mu\text{g}/\text{mL}$  to  $\mu\text{g}/\text{m}^3$  needs more description. What was the sampling flow rate, etc?

Page 4, Line 14: The unfiltered fraction was directly assayed for the ROS activity but this fraction would be containing a lot of quartz fibers as well. These fibers are also shown to be toxic to the cells. How did the authors make sure that their results are well controlled in this environment?

Page 4, Line 22-23: What was the level of blank? And how much was the typical response from the sample in comparison to blanks?

Page 6, Line 34: The secondary ionic species. ... like what? Are these secondary ionic species shown to be toxic or ROS- active?

Page 7, Line 14-16: Although, it seems possible that the high correlation of inorganic species such as  $\text{SO}_4^{2-}$ ,  $\text{NO}_3^-$ , etc. could be due to their co-linearity with the WSOC, however, recent studies (*Environmental Science & Technology* 51 (5), 2611-2620; *Environ Sci Technol.* 2012, 46(12):6637-44) have shown that  $\text{SO}_4^{2-}$  enhanced acidity of the aerosol can solubilize the metals (such as Fe and Cu), which are known to generate ROS. Do the authors think to include such possibility in their work?

Page 8, Line 14: See my earlier comment, could the toxicity of Fe, Ni and Cu be due  $\text{SO}_4^{2-}$  enhanced acidity and the solubility of these metals?

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Page 9, Line 4: There should be “with” between “water” and “respect”

Page 10, Line 2: What do you mean by scavenging rate of ROS? I think this sentence needs to be either further cleared or modified.

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-118, 2017.

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