

Interactive comment on “A semi-automated system for quantifying the oxidative potential of ambient particles in aqueous extracts using the dithiothreitol (DTT) assay: results from the Southeastern Center for Air Pollution and Epidemiology (SCAPE)” by T. Fang et al.

Anonymous Referee #1

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General comment: In this work, the authors describe the development and the measurements of oxidative potential of PM by a semi-automated system compared to routine approach using DTT assay. Furthermore relationships between this oxidative potential and season as well to PM mass are then investigated. The routine and practicable measurement of oxidative potential of PM is of great importance because of the possible roles in both aerosol ageing and health impacts. In consequence this work is

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clearly important and within the scope of AMT. My main concerns with this manuscript are mostly related to the focus of the manuscript. In my opinion the authors should give more detailed information on the semi-automated system compared to the routine system (e.g. saving time and costs, handling, as well applicability in routine analysis). Furthermore, we appreciate the experiments conducted in order to determine precision and accuracy of the system, but the uncertainty of the entire system is still not quantified, neither is the uncertainty of the experimental protocol performed manually. Finally, the correlation analysis to mass and a discussion to health relevance should be reduced/rephrased as long as no input of the correlations of the oxidative potential to chemical composition and health endpoints are given.

Specific comments: - line 39: Is the CV for your standard really higher than for ambient samples? This is unusual and I suggest thinking about an alternative standard and name it here positive control! -line 45, 46: If the DTT activity is well correlated to PM_{2.5} mass why should I measure with DTT assay? And why are you suggesting that regional sources and not long term transport might be the reason for the variance? In the following sentence this is indirect suggested by the seasonality. Please be more specific and clarify this sentence/your statement. - line 87: What is missing in this section is ROS generation after inhalation the main exposure pathway and e.g. activation of immune system like macrophage response finally also maybe leading to ROS. I would suggest including a short passage on this. - line 97: This a very general comment can you be a bit more precise and give example for time needed for analysis please? - line 107: You mentioned a manual protocol, is this lab internal or is it based on published literature, if so please refer to it. - line 189: Please rephrase, the wording is a bit misleading → 3.5 mL with a concentration of 40 μg/mL represent a total PM mass of 120 μg. . . . - line 205: If possible please provide a map and maybe also a picture of the trailer in the supplement, that would makes it easier for the reader to get an impression of the investigation/area - line 217-231: Although the extraction efficiency of the water-soluble substances is the relevant/crucial factor within your system and we assume it to be fairly good, we propose to give some estimate on the total extraction efficiency (includ-

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ing that of particles) of the applied approach. - line 226: Please specify the sonication procedure! - line 252: Insert space in front of Kumagi et al. - line 265: So please state, LOD is 20 $\mu\text{g}/\text{mL}$ correct? - line 252: Insert space in front of mL - line 282: Please give full name for abbreviation of PQN - line 287: Please give the CVs - Line 154: Is there any DTT left in the system after self-cleaning? We recommend mentioning that the carry-over has been quantified and attach the data in the supplement.

- line 305: In abstract the CV for standard is given with 12% here 15%? - line 322: Please check/insert space in front of the authors - line 330: I agree with the authors but this sentence does not provide any extra information an is/should be in the conclusion sector. - line335: Please give some information on the statistical analysis normal distributed, no heteroscedasticity, post hoc..used? - 370: I fully agree but of certain interest would be here also to have some chemical component information. Maybe just briefly you could provide some information at least to some elements or group of elements etc. that later will be published in Verma et al. as you mentioned. - line 372: Can you please explain exactly why do you think the southeast activity is related to regional and not local/single sources? - line 377: This is a very unspecific statement and the listed publication are of course fine but providing some more detailed information of PM related Oxidative stress and subsequent endpoints like oxidative DNA damage, increase f inflammatory markers etc. at least listing some review articles ore even books would be nice here. Otherwise it becomes not clear how the DTT reactivity is related to health effect endpoints. - line 406-408: Please give also here reasons why this stands for regional sources? What about e.g. with longterm transport?

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