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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On dithiothreitol (DTT) as a measure of oxidative potential for ambient particles: evidence for the importance of soluble transition metals" *by* J. G. Charrier and C. Anastasio

Anonymous Referee #2

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General Comments

This manuscript describes some nice work with some important new results on the reactions of PM components with DTT. Given the increasing use of the DTT assay as a diagnostic for ROS production from PM samples, this work is relevant and timely. Historically only organics were thought to be reactive in this assay, but the authors make a convincing argument that metals are important and perhaps the dominant species responsible for DTT consumption. The manuscript is well-written and the study has been carefully performed. The work certainly merits publication in ACP in the opinion

of this reviewer.

Specific Comments

1. Figure 5, which shows the relative contributions of PM components to DTT consumption in a hypothetical PM sample, is I think a little misleading given the large variability in the concentrations of these species. The authors do, in fairness, explicitly address this issue in the manuscript, but is there perhaps a better way to represent this graphically? Perhaps a bar chart for the relative contribution for each component at the median concentration with error bars showing the range of ambient concentrations measured?

2. For the calculation of the contribution of copper to the hypothetical PM (and perhaps also for the SJV samples), the copper concentration is in the range where the regression equation seems to be systematically lower than the measurements (Figure 4). This suggests that the copper contribution to the hypothetical particle sample may be higher and that the metals may account for more than 80% of DTT loss in the SJV samples according to this analysis (although see the comments below).

3. While summing the individual contributions to DTT loss in the PM samples is certainly a useful way of looking at the mixtures to a first approximation, it is clearly possible that the chemistry is more complex than this. Have the authors done any measurements with mixtures of metals and/or quinones and metals to see if the effect of each component is truly additive? Clearly there may be many other components present in the ambient PM samples that increase or inhibit the reaction rate of the metals and quinones.

4. Is there anything that can be learned from looking at the redox potentials for the components studied? Is there any relationship between this property and the reactivity of the components in the assay?

5. The inhibition of quinone reactivity by EDTA is a very interesting result. Is it possi-

ble that trace metals not removed by chelex treatment are involved in the reaction of quinones with DTT?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 11317, 2012.