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Interactive comment on "An Automated On-line Instrument to Quantify Aerosol-Bound Reactive Oxygen Species (ROS) for Ambient Measurement and Health Relevant Aerosol Studies" *by* Francis P. H. Wragg et al.

Anonymous Referee #2

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This manuscript presents an innovative and practically promising analytic technique to detect aerosol-bound ROS in ambient environment. The work integrates the well-known DCFH ROS detection assay into a portable instrument, which executes a complete analysis including sampling, sample preparation, and detection. The technique is promising to be applied in practice to detect aerosol-bound ROS in the real atmosphere and the authors also demonstrated this with an urban roadside site. The result of the filed study is intriguing in the observed correlation between ROS concentration and the PM2.5 measurement. I take this paper a good match to the theme of AMT journal. Therefore I recommend the publication of this paper. However, there are still

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Discussion paper

some scientific questions that the author should consider revising.

In this work, H2O2/HPR assay was used to calibrate the instrument, and the active ROS here is H2O2. The same HPR assay is also used for aerosol particle bounded ROS detection. Does this mean only H2O2 species can be detected? The authors are encouraged to elucidate what ROS species can be detected by this technique.

The authors suggest 12 minutes a “suitable value” as a time resolution. A more thorough discussion of this conclusion is expected. It is hard to read the 12 minutes from Figure 6. From my understanding, 12 min is claimed to be the time interval of the reaction to reach a steady state, the instrument at which is used to calculate the ROS concentration. This time length should also be validated for the case of H2O2, which is used a standard solution for calibration.

The authors found different ROS/aerosol ratio with different UV lamp power. Although a “probable” explanation is provided, the authors are encouraged to add more work to test their hypothesis if possible. Other than the consideration of compounds’ volatility suggested by the authors, other reasons remain, e.g., the different reactivity with HPR in the two cases.

Beside the issues above, some sentences in the manuscript should be more quantitative. A few specific comments are as follow:

L40-45;L115: The authors mentioned the disadvantage of traditional techniques at multiple positions through the paper, without explicitly introduce what are these techniques. A clear identification of them helps readers to understand why they have these disadvantages. L161: what are the suitable time and temperature? L281: how quick is “quick-reacting”? L380-385: It is a good strategy to compare results of real atmosphere measurement by this work with existing studies. However, there are more studies existing with similar data reported. Some of them are cited in Wang’s work (Wang et al., 2011). The authors can compare their results with more work. L420 Figure 4: What’s the meaning to use a nonlinear regression for the calibration curve?

Technical corrections: L268: the unit after 0.25-0.5 is blurred. It should be uM according to Figure 4.

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