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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An apportionment method for the Oxydative Potential to the atmospheric PM sources: application to a one-year study in Chamonix, France" *by* Samuël Weber et al.

Anonymous Referee #2

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The manuscript of Weber et al. represents the OP results obtained by analyzing a series of filter PM10 samples collected during a year-long period at an urban location in France, using two different assays, namely the dithiothreitol assay (DTT) and the ascorbic acid assay (AA). Combining results obtained by different analyses of the collected filters, including soluble ions, metals, PAHs and combining these results with PMF and linear regressions analyses for the identification of different sources and the subsequent attribution of redox-activity to different PM sources. It occurs that a large part of the observed OP is linked to biomass burning and vehicular sources for both

assays.

The paper is well written and easy to follow, though there are some issues and more thorough discussion should be made in specific sections. A very interesting point of the study is that the used assays appear to be sensitive to different ROS. Other than that the paper can be recommended for publication after addressing the issues listed below.

Specific comments:

1) Samples consist of PM10 while PM2.5 is most commonly used as being able to penetrate inside the respiratory system. Although the used range (PM10) surely covers the totality of the OP distribution, the difference of acidity between fine and coarse fraction surely plays a key role in the aerosol OP, influencing the solubility of metals (e.g. Fang et al. 2017). Authors should comment on this.

2) It is stated that the current study uses simulated lung fluid (SLF) solution, complicating the direct comparison with other studies. It should be clearly stated in the abstract and conlcusions section that a method different than the standard DTT protocol is used in order to avoid confusion. Furthermore, as seen in Calas et al. (2017), the OPDTT measured in Milli-Q water and three different SLF extracts does not present statistically significant differences. Authors should comment on the choice of extract. Finally, in the extraction phase (P5,L13) is different extraction volume used for different samples or only a different area of the used filter? This is not clear.

3) There is no mention of the LOD for the specific assays using the SLF, nor blank/ blank corrections.

4) When presenting the concentrations of the PMF sources, emphasis is only given for the correlation of OP solely with biomass burning and vehicular sources, even though it appears that "nitrate rich" source could also be correlated, as during winter enhanced nitrate concentrations are usually associated with biomass burning. Although men-

tioned further on (P12, L10) it should also be mentioned and commented on, here.

5) A more thorough discussion should be made in the Intrinsic OP section, namely a comparison with other values found in the literature (even though the majority concerns PM2.5) and the use or not of an intercept in the linear regression model. Furthermore, it is stated that other studies also highlight the importance of the vehicular source to explain the OP. In Verma et al. (2015) even though HOA (representing traffic) correlates significantly with OP at some sites, the generated linear regression models do never include HOA, though in some cases the linear regression model include copper. It is known that copper may originate from brake wearing, but also it can be linked to other anthropogenic activities, such as industry and/or coal burning.

Technical corrections:

Title: “Oxydative” should be corrected to “Oxidative”

Abstract, L1: “. . .induces cellular oxidative stress in vivo, leading to adverse. . .”

P6, L19: “. . .DTTv shows larger values. . .” (delete “has”)

P7, L7: “. . .sources appear to be strongly correlated. . .”

P14, L18: “...biomass burning and vehicular sources. . .”

References

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