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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison between five acellular oxidative potential measurement assays performed with detailed chemistry on PM_{10} samples from the city of Chamonix (France)" *by* Aude Calas et al.

Anonymous Referee #1

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This paper reports on comparisons of a number of acellular assays during one year of non-continuous sampling at a single site. This type of study is crucial to help guide selection of assays for future use in health studies and to help interpret existing health studied involving measurement of OP. However, this paper is lacking in many ways. First, why was PM10 used when in most studies reporting on aerosol OP the focus is on PM2.5, as it is in most health studies. There are a large number of citations that are not considered in this work, with some result published work reporting completely opposite to the findings reported here and which the authors seem unaware. This is

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a major issue that must be corrected prior to publication, specifically a more complete Introduction and more complete discussion of results in the context of published work. It should also be made clear that when comparisons are being made to other studies, that those studies also were reporting findings based on PM10 (more on this below). Mixing results from PM10 and PM2.5 studies leads to confusion.

Specific comments:

The Introduction is missing many key citations. This includes: Page 2, line 11. There are more acellular assays then listed. What about those measuring OH production, eg, [Charrier and Anastasio, 2011; Vidrio et al., 2009]?

Page 2 lines 18-23: A whole series of papers is not discussed for data collected over various seasons that compares two assays (DTT and AA) [Fang et al., 2016] and compares the assays to chemical species or sources [Verma et al., 2012; Verma et al., 2015a; Verma et al., 2015b; Verma et al., 2014].

Page 2 lines 33, there are more health studies than cited by the author, which are useful to cite in this journal since many readers will not be familiar with the health journals where much of this work is published. Ie, [Abrams et al., 2017; Atkinson et al., 2016; Bates et al., 2015; Strak et al., 2012; Weichenthal et al., 2016a; Weichenthal et al., 2016b]

In my view it is unfortunate that the authors choose to measure and compare PM10 since the composition of PM2.5 vs PM10 is often very different and PM10 less often used in health studies – which is the whole point of this work. It would be helpful to know specifically why PM10 was studied if the goal is to develop insights on assays used in health studies, and how, if any, can these results be applied to PM2.5, which is what the vast majority of OP measurements report. For example, it would be very useful to state what was used (PM10 or PM2.5) in the cited OP-health studies.

When comparing the results of this study involving PM10 to others, which may or may

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not be measurements of PM10, or even referring to other studies, it must be clear what is being compared. For example, page 11 line 6 and line 9-10, page 12 line 10 and line 16, and page 16 line 2; the results of this study are claimed to be in agreement with Janssen et al, 2014, Yang et al., 2014, etc, but Janssen measured PM10 and PM2.5; are the results being compared to just the Janssen PM10 results? Yang did only PM2.5, so how can these results be directly compared to Yang without noting this important difference? Much more care must be considered given that this work is only PM10.

Page 2 line 12, It is understood that doing a bulk analysis integrates over size, but the way it is stated (Oxidative potential tests for airborne particles integrate several of biologically relevant properties (e.g. size, and chemical composition) likely to drive PM toxicity.) makes it sound like a positive attribute of the assay, it is not. The advantage of these assays is they integrate chemical species in the particles that may contribute to OP, a unifying property potentially linked to the particles toxicity through oxidative stress. Integrating over size is likely not advantageous as in the case of PM10 it mixes very different aerosol sources (chemical components).

How exactly were the PM10 filter samples collected, eg, what type of filter sampling system at what flow rate.

Page 3 to 6. It would be valuable to know if the assays are performed in a manor exactly consistent with given protocols for each assay. This is given to some extent, but more explicit statements on this would clarify things. For example, does the DTT assay follow the protocol of Cho et al, [Cho et al., 2005], etc?

Page 9 line 2, what does more dispersed data mean? Higher standard deviation?

Page 11, line 6, It states, that : OP DTTv and OP AAv are in agreement with another study (Janssen et al., 2014). However this was not found in another study, [Fang et al., 2016].

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Univariate analysis of OP with metals. (page 12). Given that total elemental metal concentrations were measured, not water-soluble or speciated metal ions, how can one link these metals to OP assays through redox activity since: 1) Total (elemental) metal concentration is not necessarily correlated with the soluble metal concentration. 2) Only the soluble metal is involved in the redox reactions. The total metals can only be used to identify sources (assuming source profiles were based on total metals).

Page 15, line 10, what does “PM participating to the background”. . . mean

Page 15. Line 14, some other studies show antagonistic effects on OP that one may wish to consider, see [Wang et al., 2017; Xiong et al., 2017].

Page 15 line 28: What exactly does this line mean? “Additionally, PM extractions were realized only for the DTT and the AA assays which can lead to a difficulty in the results comparison.”âĂĺ

Page 16, Line 2, this conclusion is opposite of Fang et al [Fang et al., 2016], which should be noted and possibly discuss possible reasons why.

Page 15 and 16, I would say the biggest limitation is the use of PM10 for this study instead of PM2.5. This would be a good place in the paper to discuss this (see comments above).

Fang et al, [Fang et al., 2016] did exactly what the last line of the main text states, and it is never discussed, although the paper is cited.

Page 18, line 21 and 22, a source apportionment like that which has already been done, but never cited? See [Bates et al., 2015; Fang et al., 2017; Verma et al., 2014]

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