

# ***Interactive comment on “PM<sub>1</sub> composition and source apportionment at two sites in Delhi, India across multiple seasons” by Ernesto Reyes-Villegas et al.***

## **Anonymous Referee #1**

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This is what I would consider a “standard” AMS and aethalometer study to document air pollution levels in a major urban area in a part of the world where air quality is among the worst; thus, it does fall within the scope of ACP. Its novelty lies in the application of proven methodology in a new location with a sparse amount of spatial and temporal resolution. It is not groundbreaking, but it is important, as the conclusions are relevant for air quality control efforts in what many consider the developing world. The question then becomes, however – could more be done with the data?

On the whole, it is well written and easy to follow (though some care needs to be paid to figure and table numbering), the abstract and title are accurate and the authors

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use appropriate citations. The figures are easily read and understood – especially for members of the community who do this sort of work (diurnal profiles, time series, AMS PMF factor spectra).

My main concern and why I am unable to recommend publication at this time is the lack of evidence that the instruments are actually reporting comparable data – this is based on a lack of comparison of the AMS spectra and on the lack of agreement (approaching 25% difference) in the one set of collocated data shown. See below.

### Specific comments

Line 154, should Figure 1 be called out here? In fact, Figure 1 is not called out at all until Line 226, though figures should be called out sequentially.

Line 165, the C and HR-MS comparison concentrations in S5 do not match the numbers given in the preceding text. In addition, they disagree by  $\sim 6$   $\mu\text{g}/\text{m}^3$  (26 for C, 32 for HR), which is a 23% difference. Does this limit the robustness of the spatial comparison made? I actually think this is the most significant weakness of this manuscript. How can the authors justify comparing data from separate locations/times when two of the instruments do not agree when collocated? Were any comparisons done on the PMF results? Are their spectra similar? Do the PMF results actually indicate that the factors being compared have similar characteristics such as O:C, H:C, etc.? Before I can recommend this paper for publication, I need to be convinced that the data are actually comparable, not just told that they are, even if it is in the supplement (line 254). Simply showing the spectra in Figure 4 is insufficient in my opinion.

Line 179, should this be table S1, not table 1?

Line 201 (and 205 and other places), I would argue that the concentrations observed are lower, not low. 80-200  $\mu\text{g}/\text{m}^3$  are still very large concentrations!

Line 245. I believe most AMS data are presented as LO-OOA and MO-OOA (less and more oxidized) rather than as SV and LV, respectively, in more recent literature.

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Section 4.1. I find the discussion on oPOA to be fairly weak, with no justification of the suppositions made. It makes sense that meteorology impacts the oPOA dynamics, then it is mentioned that it tracks sulfate, then it is stated that oPOA may have a similar volatility as  $\text{NH}_4\text{Cl}$ . No conclusions are made. I suggest this portion of the discussion be removed or strengthened considerably.

Supplement S2. Please clarify how CE was determined. The paper states that the authors used 0.5 based on the ACSM manual. However, in the plots, some show  $\text{CE} = 1$ , and others show  $\text{CE} = 0.5$  (while others show CDCE). In addition, the x-axis in these plots show  $\text{PM}_{2.5}$ , not  $\text{PM}_1$ . Is that reasonable to use?

Supplement S3 and S4. Please check the figure numbering.

Supplement S5. Please correct the caption for S17, as it shows more than Cl and oPOA.

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