The paper by Reyes-Villegas reports on measurements of aerosol composition and absorption at 2 sites in India (new Delhi and Old Delhi) where ground- based measurements were carried out in different seasons in 2018. Diurnal and seasonal trends in composition and organic aerosol types, resolved by positive matrix factorization, are reported. PM1 composition and concentration differences between the two sites in all seasons are minimal. Organic aerosols (OA) were the dominant component of PM1 in all seasons; higher concentrations of all species were measured in post-monsoon/winter, owing to changes in meteorological conditions. During pre-monsoon, oxygenated OA (LVOOA) was the dominant OA type with peaks appearing during the day, while in post-monsoon and winter periods, hydrocarbon-like OA (HOA) was the dominant aerosol type, with minimum concentrations during the day. Biomass burning OA, oxygenated primary OA, cooking OA, other oxygenated OA (SVOOA) were also identified although not consistently at all sites and all seasons (except for BBOA and COA). Given this is not the first set of measurements in the area (see lines 94-105 for a list of comparable studies) and lack of new scientific insights presented here, I suggest the manuscript be considered as a “Measurement Report”. As such, I still have concerns about the analysis and data quality; some parts of the report also benefit from new reorganization and further clarifications.

Technical concerns:

- What was the sampling protocol/set up at different sites? Was there any cyclone used on the inlet? Was there any control on humidity? If not, how could changes in ambient RH diurnally and between seasons impact the measurements?

- Quantification of different AMSs and Figure S4: what is the difference between pre-flux and flux period? Why is the behavior of the comparisons so different between these periods when using the same CE correction factor (0.5 or CDCE)? For pre-flux period, slope with CE=0.5 and CDCE is very similar. It may be that the composition really dictates CE to be close to 0.5, but better justification/explanation needs to be provided as to why 0.5 is used for the whole study and not CDCE. Comparison is done on online PM1 and PM2.5 filter-based data. How/where are these filters collected? No information is provided, yet the measurements serve as the basis for justifying the selected value of the CE! What is the suspected contribution of 1-2.5 um particles to mass that justifies the comparison?

- Aethalometer data and contribution of BB vs. traffic: it was not explained why AAE_{wB}=2 and AAE_{tr}=0.8 is selected. It seems the basis for selecting these factors is the match between the traces in Fig. S13 for the time period in the box, but it wasn’t explained why is it expected to have equal absorption from wood burning and traffic at two different wavelengths during Diwali period to justify selecting the default AAEs? I also disagree that there was no change in the absorption calculations using different values of AAE; clearly the b_abs_470wb changes in other panels of this graph depending on the AAE value. Why was a different AAE_{tr} used for AE-33 ND-Winter period (Fig. S15)? In general, I’m also not sure the way aethalometer data are presented is useful. What would be more useful could be the fraction of absorption at 470 nm that is due to wb vs. traffic and not the absolute value of absorption due to wb at 470 nm.

- References to PMF factors for oxygenated OA are given in terms of expected volatility (LV/SVOOA). Isn’t it more accurate to call them based on their oxygenation level since the AMS doesn’t measure volatility directly? Factors show up in the last panel of Fig. 4 (SFOA and nHOA) that are not discussed anywhere in the paper. Is it common to look at Q/Qexp for different m/z's? I was under the impression that always an overall Q/Qexp is considered in PMF. What is the acceptable range of this ratio when considering individual m/z's? Values up to 300 are way too high for the overall Q/Qexp. It's concerning that some of the key fragments (43, 55, etc.) have such high values.

- Section 3.1 as written is very confusing. It's mentioned the highest conc. were seen in post-monsoon, but then two periods in post-monsoon are reported. If the point is to discuss the highest concentration, only one number/site/season should be reported. Overall I don't find this beginning paragraph of 3.1 useful as written. If the authors want to describe seasonality, I think the two sites need to be considered separately, so I suggest reorganizing this part.
- L 289: a reference to Chloride deposition is presented here without any discussion of how deposition rate is inferred. Mere reduction in chloride does not mean it's being deposited. Either elaborate or remove this sentence.
- L 363: Aerosol sulfate does not evaporate with typical diurnal changes in temperature whereas ammonium chloride or nitrate do. If oPOA is behaving more like sulfate, I wouldn't call it semi-volatile. Based on the diurnal profiles, I also don't think oPOA has a similar diurnal profile to sulfate! It's more like chloride and nitrate, in which case it's justified to call it semivolatile.

Other concerns:

- Sometime chloride is referred to as Cl and sometimes Cl-. Please use the latter and consistently.
- Sometimes numbers in molecular formulas are not subscripted; please correct.
- This sentence doesn't full make sense: "Comparing these concentrations with the PostM_OD_H2 dataset, HOA concentrations of 30 ug/m3 were identified". Please rephrase.
- Figure S6: data gaps show up with a flat line; please remove those.
- References to figures in SI (PMF section and those after) are not correct; Please correct.
- Fig. S17. Figure caption doesn't match all the panels that are plotted.