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Interactive comment on "Effect of aerosol composition on the performance of low-cost optical particle counter correction factors" *by* Leigh R. Crilley et al.

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This manuscript describes the use of data collected with a low cost optical particle counter together with reference grade PM instruments to assess overall accuracy and, especially, the sensitivity to relative humidity. The manuscript is reasonably well written and understandable, though some minor editing would be required prior to publication. There is currently considerable interest in the use and performance of low cost air quality sensors and also considerable need for establishment of best practices for operation and data analysis. As noted in the manuscript, though only an Alphasense OPC-N2 was used for this analysis, the findings can at least qualitatively be extended

to the array of similar low cost PM sensors that rely on particle light scattering for measurement. Perhaps simply because of the advantage of hindsight, there are changes in techniques and instrumentation that could have provided a more easily interpreted dataset. The use of a mixture of reference instruments and the rather narrow range in humidity encountered at each of the study sites somewhat limits confidence and extension of the results. But despite the limitations of the dataset, the results would still be valuable to others using these or similar low-cost sensors and the manuscript should be publishable after the concerns identified below are addressed.

Section 2.2: There needs to be some discussion of the relationship between the reported ambient relative humidity and that in the sensor. Were the OPC-N2's inside some sort of enclosure? And if so, how was its temperature related to that outside? Would solar heating impact the temperature during the daytime? And if the sensor and outdoor temperatures are not always the same, could that help explain the large spread in derived PM2.5 at high RH mentioned on page 7, line 3?

Section 2.2: Details of the sites and reference instruments at each should be provided in a table. As is, the descriptions are structured differently enough that it is difficult for a reader to appreciate the similarities and contrasts among the sites.

Section 3.1: The relationship between RH and composition for both Delhi and Beijing should be discussed and possibly graphed. The assertions that "there is clear influence of RH on the measurements performed in Delhi. . ." and that the "stepwise increase in the derived particle mass between a RH of 40-50% RH may point to deliquescence. . ." in Beijing should be made only after quantitatively describing any role of RH-dependent variation in composition (due to connections with things such as wind patterns and photochemical production).

Figure 1: It would help to include curves representing the mean or median PM2.5 vs. RH

Figure 2: There is a brief mention in the text that the OPC-N2 tends to underestimate

PM2.5 at low RH, which partially explains the «1 OPC/TEOM ratios in Beijing (it is hard to say for the Birmingham sites because of the wide y-axis range used). But I'd like to know whether the authors have an explanation for the apparently strong dependence of the ratio on RH for RH < 40% where hygroscopic growth is probably not significant enough to be responsible. Could this be related to a confounding relationship between RH and composition?

Figure 2: Clarify in the caption that the k fits are shown in color.

Page 11, line 14: It is not necessarily true that any enhancement at RH < 79% is not due to ammonium sulfate because the particles may be in a metastable state.

Figure 3b: The sulfate content should be presented as a fraction and not as an absolute concentration. It is not surprising that sulfate mass concentration increases with increasing total (TEOM) concentration.

But that is unimportant for the OPC-N2 vs. TEOM comparison and only the fractional contribution matters.

Section 3.3: The manuscript suggests that the OPC-N2-based PM2.5 is higher than the reference and that using the measured density of 1.28 rather than the assumed density of 1.65 would not explain the discrepancy. Why not? It seems this change would improve agreement, whereas the manuscript seems to suggest it would not.

Page 13, line 9: What does "lose structure" mean here? The amplitudes of the daily peaks are lower than earlier in the measurement period, but why should that indicate there is a problem? This is especially important because it is the basis for the suggestion that the lifetime of the sensor in Delhi is only a month. And furthermore, even if the sensitivity of the OPC-N2 decreased due to dirty optics, the authors should not attempt to extrapolate to all low cost sensors as they do here.

Page 14, line 11: Does "...though within a large range" refer to 0.1 – 0.12? If so, I don't think of that as a large range. And if not, reword this so it is clearer.

Section 3.4, first paragraph: Rather than relying on trajectories, it seems statistical tools or machine learning could provide at least some improvement in correction accuracy using things like weather observations, day of the week, time of day...

Page 16: Use a word other than global, which implies something based on more than three urban background sites during only relatively short periods of the year.