Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-469-RC2, 2018

Atmospheric Measurement Techniques Discussions

Interactive comment on "Exploring the Applicability and Limitations of Selected Optical Scattering Instruments for PM Mass Measurement" *by* Jie Zhang et al.

Anonymous Referee #2

Received and published: 26 February 2018

The manuscript presents an evaluation of three low-cost, optical-based aerosol sensors by comparing their response to that of several different instruments to laboratory-generated and ambient aerosol. The sensors generally showed good correlations with different reference instruments, but different relationships (slopes), due to differences in sensor properties, calibration, and sampled aerosol properties. The topic is important given the recent interest in using these types of sensors for air quality monitoring, and is appropriate for AMT. In current form, however, the manuscript lacks in a few areas, as described below, and requires significant revision before I can recommend its publication.

General comments:

Considering the paper focuses on an evaluation of several sensors I think much more detail regarding the sensors should be provided. For example, sample flow rates, whether or not a sheath flow is used, lower and upper concentration limits, etc. Additional information on how exactly are calibrations performed by manufacturer should be included as well.

PSL is one of the primary test particle types, but in my experience it can be very difficult generating a pure monodisperse size distribution from PSL without first passing it through a DMA. When PSL is atomized/nebulized it can produce a large mode of fine particles with sizes < 200 nm, which arises from surfactant and other contaminants in the PSL solution. These smaller particles can affect mass measurements depending on the lower detection limit of the sensor. Do the authors have any SMPS measurements showing the quality of the PSL output and confirming that it was limited to just the PSL at the expected diameters?

Tests were only performed on dry aerosol, which is probably necessary to eliminate the complications introduced by water uptake on particles. The manuscript would benefit from some discussion of this however, in that low-cost sensors will need to either account for RH impacts or be actively dried when sampling.

Finally, I think the manuscript is significantly weakened by the lack of a true reference measurement for the ambient measurements, having to rely instead on AMS and SMPS measurements, neither of which is a true mass measurement. The authors' give a reason for this, however I find it somewhat puzzling that the TEOM was unable to provide quality measurements of ambient PM mass.

Finally, there are few places where the wording / grammar is awkward and would benefit from copy editing.

Specific comments:

2, 18: recommend including a statement that the PAS is also an optical particle counter and not a direct mass measurement

2, 20: there are several additional, but quite new references that could be included and compared to here, such as Crilley et al. (2018) and a paper currently in discussion (Feinbeg et al., 2018).

3, 4-5: change to "aerosol mobility diameter" and to "non-refractory chemical composition"

3, 14 & 20: Please provide the scattering angles measured for the DRX and PDR

3, 25: are the specifications more specific? A D50 would be helpful if provided.

4, 2: more appropriate to state that the conversion is done on volume concentrations, not number concentrations

5, 1: please provide typical RH in the chamber for experiments

5, 17: While it may be true that the OPC-N2 does not measure below 380 nm, it would have still been useful to see the level of disagreement. I'm also a little surprised the TEOM could not be used due to the high-frequency noise, since this device is designed for this type of monitoring application. Could a longer averaging period have been used?

7, 21: Clarify that the 0.45 g/L refer to the nebulizer solution concentration. It would be good to state the density of sucrose somewhere (1.59 g / cm3).

7, 20-22: It is strange that the OPC-N2 shows such good agreement for the sucrose in this example. It's stated elsewhere it has a poor detection efficiency, especially below its lower limit (380 nm). Figure S3a shows aerosol volume distributions, which show at least half of the distribution below 400 nm. This would suggest the OPC-N2 misses at least half of the aerosol volume (and therefore mass), yet it looks very similar to the other two sensors, and shows good agreement with the TEOM. How is this possible?

The figure is also not consistent with Table 3, which lists very low ratios, as expected.

Also, Figure S4's legend shows PDR * 3. Does this mean the data for that sensor are multiplied by 3? I don't think this is discussed in the text. More details should be provided justifying this if so.

7, 23: The results suggest good linearity, but I'm not convinced on stability. Were tests repeated after some period of time, and simulated usage/exposure to ambient air? That type of test would be needed to verify stability of the response. If the author's mean stability since original calibration more details on the calibration should be given, including dates, and what was done with sensors between the time of calibration and the testing.

8, 2: Is the adipic acid result normalized against the TEOM or AMS? Not clear here, but earlier it's stated the compound is too volatile to use the TEOM.

9, 3: The OPC-N2 provides binned results though, correct? These would have some dependence on the pulse intensity, which is related to refractive index as well as size, shape.

9, 17: Stating this overlap range explicitly here would be helpful.

9, 22: Is this section comparing number detection efficiency or mass efficiency? Please be clear.

10, 1-2: What is meant by this statement? The sensor response to these compounds due to the limited detection range of the OPC-N2 is poor, but that does not mean the evaluating its performance is complex.

10, 10: Another factor not mentioned is mass from species not measured by AMS, including BC, dust, etc.

10, 11-12: While the SMPS and AMS are consistent, I think the argument that this means the AMS is a good reference instrument for mass is somewhat weak. This is

where the analysis suffers from not including the TEOM, a true PM mass reference measurement method.

10, 14: This seems like a large difference between two instruments both calibrated against the same material (Arizona test dust)

11, 1-2: This statement needs to be supported by more evidence. What constitutes "ambient aerosol" according to TSI? Why is the difference plausible? It seems like the aerosol composition at the test site is not radically different from what would be considered a regional, continental aerosol (as opposed to say, a heavily marine influenced location).

11, 4-12: It seems like the more straight-forward way to evaluate this relationship would be to plot ratios of each sensor to the reference as a function of mass mean diameter measured by the SMPS, as shown in Figure 6. Suggest removing.

11, 16: I suggest swapping the axes to show mean diameter on the x-axis.

12, 26: where do these estimated RI come from? Are they based on the aerosol composition?

Table 1: Suggest adding rough cost of each instrument as I think it is relevant in the context of the manuscript.

Table 3: It would help reader interpret the different groups if you listed the median diameter or similar parameter under the group name. Information in Table 4 could combined here and eliminate a table.

References

Crilley, L. R., et al., Evaluation of a low-cost optical particle counter (Alphasense OPC-N2) for ambient air monitoring, AMT-11-709-2018.

Feinberg, S., et al., Long-term evaluation of air sensor technology under ambient conditions in Denver, Colorado, ATMD-2018-12

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