Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-308-RC2, 2017


Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Evaluation of a low-cost optical particle counter (Alphasense OPC-N2) for ambient air monitoring" *by* Leigh R. Crilley et al.

**Anonymous Referee #2**

Received and published: 4 October 2017

This manuscript describes the evaluation of a low cost optical particle sensor with respect to ambient PM monitoring. The advent of such low cost sensors is an important development in the PM monitoring field which will be important for future spatial distribution measurements and hence epidemiological health studies. The topic is well within the scope of AMT, and could be useful to community in understanding the advantages and limitations of such technology. However, the manuscript is not entirely well written, suffering at times from lack of clarity, and incomplete information. The issues are described further below. If these issues can be addressed then I believe this manuscript could be publishable in AMT and provide useful information.

Overall, the manuscript is too qualitative with respect to understanding how accurate and precise these sensors may be. On too many occasions the authors use the termi-



nology "reasonable" to describe the agreement or precision etc.. Such terminology is far too subjective. What is considered "reasonable"? The authors should strive to be more quantitative in this respect, as many people will want to use such sensors and their recommendation may carry some weight within the community.

In my opinion, such technology has a long way to go before it can be a useful in determining the spatial distribution of PM and hence be used in health studies. One could argue that the accuracy is less important than the inter-instrument variability in this regard. However, a CV between sensors varying from 0.2 to 0.8 does not inspire confidence (ie. fig 3). The authors seem to think that such a CV is adequate, however if that is the case they must justify why they think that to be "reasonable". On pg 9, line 20 the author's state that the CV is "perhaps not unreasonable". This is entirely speculative, and depends upon the application. For most applications I doubt this is reasonable. The authors overall seem to be saying that this is a good sensor for deployment for spatial/health studies, when in reality the data they show indicate that is not really the case. I suggest this technology remains quite far from easily being used in such studies, especially because of the variability between instruments, the need for corrections on individual instruments, and the poor accuracy. These limitations need to be front and center in this manuscript to avoid confusion.

The comparison of the sensor with a TEOM needs to be justified more concretely. It is not clear how they can be comparing "apples-to-apples" with a TEOM which by their own admission uses a nafion dryer to dry particles first (while the OPC does not). The authors should explain exactly what the TEOM they are using is providing and how it can be compared to the OPC sensor. Are they truly comparing the same quantity? At first glance it does not seem like they are, but not enough information is provided to determine this. For that matter, why are they comparing with a TEOM at all, if they have just finished assessing the accuracy with a TSI/GRIMM. By doing so, they are adding another uncertain variable into the assessment which may not be needed.

The description of the OPC sensor that is being investigated is highly lacking informa-

tion. The authors need to improve their description of the sensor significantly. Although it may have been described in other work (which they have not even cited), it should be in part described here as well. Reading this short paragraph description I am left wondering: How does it sample? With a pump? Passively? How does the data collection work? What data is collected exactly? Does it only provide a mass concentration value? Does it provide number concentrations as well? What is the time resolution? What does the manufacturer say it should do? All these things and likely more need to be described in the methods section.

If the GRIMM instrument is noted to always be 20% higher than the TSI, then which one is the standard? I am assuming that the TSI is the so-called "gold standard", as it is calibrated with a known stream of particles at some point or another. Is that the case? The authors make it sound as if they realize that the GRIMM is consistently incorrect. If so, then why are they using the GRIMM as a comparison at all? If they are trying to assess the accuracy of the OPC then they should determine which standard is truly accurate, and only compare to one of them. It does not make sense to me to be assessing accuracy with an instrument which is not providing the correct values. It seems the true measure of accuracy is using the TSI, so why not simply use that?

Since the reference instruments and the OPC are essentially coarse particle instruments, the inlet fabrication and geometry are critical in transmitting the largest particles into any of these instruments. Any slight bends and differing bends between instruments will highly impact the large particles that enter the instruments. How is this mitigated? Are they the same between standards and the OPSs? If not, then I don't see how any real analysis of accuracy can be made, since some large particles being lost preferentially can severely affect the PM10 mass. The authors could potentially calculate the losses as a function of size and inlet bends etc. . .using on-line calculators at the very least, to be sure they are at least consistent between instruments. This is less of a concern for the precision determination.

While I do not doubt that the OPC has an artefact associated with RH, I also notice



in many of the figures that the inaccuracy seems to be worse at higher PM loading. Is it possible that the high RH may also be correlating with high mass? In that case which one is more important? Is it truly the RH or is it the mass that is causing the artefact? By their own admission, the authors note that there are other factors at play. Can these factors be determined? It would seem that rather than a correction based only on Kholer theory, additional corrections are needed. It might be possible to make a multivariate empirical correlation between the OPC/TSI ratio and the RH, mass, and/or others. Can this be done? A multivariate analysis may help to determine what factors are truly responsible for the discrepancy and to what degree.

It remains unclear why RH should cause an artefact. I do not dispute that one exists, but the authors should attempt to explain why fundamentally the RH should make any difference to the OPC. In principle the OPC is determining if a particle scatters or not. If it does, then it is counted. So even if RH affects scattering (which it will), then I do not see how it will stop the scattering all together such that a particle is not counted. The authors need to provide a plausible hypothesis at least to explain this issue.

What does the manufacturer say the specifications should be for the OPC sensor? It seems like no attempt was made to contact the manufacturer to get an idea of how the mass is calculated. Given they are assessing their instrument; one would think they would be agreeable to helping them out. How do these results compare with what the manufacturer says it should do in terms of accuracy and precision?

There are many studies where mobile measurements of PM were made in urban and suburban areas. By looking at the spatial variation of the PM in those studies, one can get an idea of what kind of inter-instrument variability is required for this to be a useful instrument. Some attempt at this should be done, at least qualitatively.

Minor issues:

Pg 2, line 2: the term "reasonable" is used here and not justified.

Pg 2, line 30: this line is awkwardly written. Remove the “are” and use “companies” or “manufacturers” but not both.

Pg 3 , line 20: define “PUWP” and “dylos”

Pg 3, line 19: add “the” before “dylos” (if I am reading this correctly)

Pg 3, line 21: remove the “s” from “environments”

Pg 3, line 22: add “they” after “however”

Pg 3, line 29: “sites” to “site”

Pg 4, line 11: replace “were” with “used”

Pg 4, line 15-17: awkwardly written. Please improve. And remove “s” from “systems”

Pg 5, line 17-18: it is not clear what this is supposed to be used for in this paper.

Pg 5, line 29: briefly describe what the point of the “filter dynamic system” is.

Pg 6, line 6: add an “s” to “OPC”

Pg 8, line 15: awkwardly written. Please improve.

Pg 9, line 20: far too speculative without backing it up.

Pg 10, line 5: define what “consistent” means to you. Fig 3 indicates it is not at all consistent. . .

Pg 10, line 7: again, “reasonable” is too subjective.

Pg 10, line 23: again, the use of “reasonable”. . ..what does this mean?

Pg 11, line 5: it should not agree with the GRIMM as you have already stated it is 20% off to begin with.

Pg 15: how is the volatile fraction determined? (briefly). What does “gravimetrically corrected” mean in this context?



Table 2: units of slope? Or unitless?

Pg 17, line 1: is this the median of all OPCs or all them individually?

Pg 21, lines 7-8: this has no bearing on the current study.

Pg 22, line 15: what is “knock on”??

Pg 22, line 20: remove “while”

Pg 22, line 23: “suitable” is not what the reader gets from this paper. See my comments above.

Figure 1: difficult to see as there are too many lines. Perhaps shorten the time scale and zoom in. Perhaps a log scale would help too.

Figure 5: too small to see anything other than the peak. Perhaps use a log scale to better see what is going on.

Figure 6: Too small to see anything. I suggest you split the y-axis and zoom in to where the majority of data is.

---

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-308, 2017.