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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Using Low Cost Sensors to Measure Ambient Particulate Matter Concentrations and On-Road Emissions Factors" *by* K. K. Johnson et al.

**Anonymous Referee #3**

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General comments:

The subject area of this study, low cost sensors for air quality measurements, is a very hot topic. It is therefore important papers in this area are written with great care to understand what new sensors can or cannot do, and a detailed analysis of the measurement data to check for cross-correlation and challenges. Unfortunately this paper has not realised this. The stated goal of the work was: "to evaluate a variety of lower cost alternatives for generating continuous pollutant measurements". However deploying the home-made box of sensors for between a few days up to a few weeks without replicates or complete data analysis of all the parameters is not particularly useful. I would suggest the authors re-analysis all the data which they have recorded



and undertake some studies, e.g. if you used 50% of the measurement period to calibrate the PM sensor, how well does it do against the other 50% of the dataset etc. Some further quality control experiments in the laboratory should also be done before re-submitting to peer review. The authors need to focus less on correlation plots and spend more time on the actual data, and the physical reasons for them, then more may be learned about how to do low cost measurements well.

Specific comments: Abstract The abstract almost wholly misrepresents the results of the study. Rather than reporting poor correlation of the sensors against the reference instrument, that area is almost completely ignored with a focus on emission factors. That part of the paper used less than two hours of data (with a correlation of 0.18 to a reference instrument) to conclude that emission factors could be measured with $\sim$30% error. The conclusion that the paper's results has showed the potential usefulness of low cost sensors for high concentrations is at best hopeful. The best sensor showed saturation at higher concentrations of PM. I would suggest the abstract be re-written to reflect the results presented.

P3 line 6 Section 2.1 There is a list of aims (i) –(iii). It is not clear having read the whole manuscript if (i) was done at all – the uncalibrated sensors appear to have been installed directly into "a sensor measurement package" (or box). Could the authors specific what (if anything) was done other than directly deploy the sensors and calibrate by running in parallel with other instruments?

Line 12 "The sensors appear promising after initial evaluation" Could this be referenced or detailed.

Line 13 and Table 1: The cost and manufacturer are not really the pertinent details required to evaluate a performance. Please could the authors add to the table summaries of the manufacturer specifications for the sensors . Line13-14 and p6 line 1-7. Calibration: They state in line 30 a Deming regression is used to calibrate. Could they explain why they use this particular regression. The Deming regression assumes the errors for

the two variables are independent (this is reasonable) and normally distributed (this is not known), and the ratio of their variances is known. (it is not known).

The authors are not calibrating the sensors, they are using the reference instrument to calculate a PM from the sensor voltage. And they have assessed the performance by looking at the correlation of the sensor output against the reference TEOM for the same dataset. It would be more standard to calibrating with the TEOM and then comparing it against the TEOM for a different period – which is a reasonable experiment as long as calibration and measurement periods are clearly separated. IF they have done this, they have not communicated it in the manuscript. A flow diagram of their calibration and measurements would help.

P4 line 9: as per previous comment on Table 1, details of the T and RH sensor specifications should be included.

P4 line 17: the authors state that there is a flow of 67 l.m-1. An explanation of why such a high flow rate is used would be useful. What is the response time of the diffusive sampling sensor and then how does that related to the air mass sampled. It would be useful to see a flow diagram with theoretical response time (and empirical from the field measurements). I am slightly concerned that although the sensors are meant to diffusionally sample with the flow rates used, there would be a significant pressure differential on the inlet of the sensor volume and hence they would not be just diffusionally sampling. Is the internal box temperature equal to the external temperature in the field tests?

Line 20 In the multisensor unit the authors add 2 more fans: does this lead to a flow of 201 l.m-1? What effect might that have on the system performance? They note that the configuration means that the PPD42NS is in a different position and hence that may affect the results, but then do not mention it again even though PPD42NS is the worst performing of all the sensors AND (according to the introduction) most widely used. It seems little care was taken when trying to make sure an equivalence was ensured in



the testing.

Line 27 this sentence does not make sense.

Line 29: “the addition of fans may change the size of particles drawn into the sensing volume”: Did it? Were any trials of switching the fan on and off or box with and without lid done either in the lab or field?

P5 Section 2.2: This purpose of this paragraphs is difficult to understand. To paraphrase, the authors are saying: lab experiments have shown that different aerosol types give different results, so we assume they are not representative of field conditions so we decided to do a field experiment. Ambient aerosol is mixed! I am not sure whether the authors know what they are doing in this paragraph apart from imply that the lab experiments were not useful. . P 5 line 23: Was the TEOM inlet co-located with the sensor pack at 4 m. Did the TEOM have a PM10 or other size selecting head on it? If the inlets we not co-located what was the separation? There are strong gradients near roadsides therefore if you are testing one method against another these details should be noted. Similarly for the other two locations: Where is the TEOM or eBAM inlet c.f. the sensor package?

P6 line 9: Laboratory evaluation: The authors should either take this out or describe the experimental results fully. A table of the statistics from the experiment is not sufficient to describe the results. How did the three sensors co-vary with the DustTrak? A plot of the DustTrak concentration and the three sensors during the one hour experiment is the minimum needed for the reader to understand the outcomes. It does matter whether there is an offset, whether there is a response lag or if they all co-vary nicely.

Results section: P6 line 28: inaccuracies are attributed to the TEOM when “using1 hour versus 24 hour averages”. This needs explaining in more detail. The authors are not using 24 hour data. The uncertainties in the TEOM are real and well characterised by many papers more recent than Allen et al., however stating they exist and looking at the dataset being assessed and working out the level of errors likely are two different

things. The particle masses being measured at the roadside are not very small– small is remote background sites where the mass is 0-5ug.m-3, not roadside.

P7: lines 1- 11. Because the authors have not included a plot which shows the variation of T, RH and the PM or CO2 in one figure it is almost impossible as a reviewer to comment on their statistics. They should have a figure with the data in it and then analyse it openly. Again, how close were the two CO2 measurements? Were they exactly co-located?

Line 16: only the data from the PPD60V is shown in Figure 6. It would be useful to see all the data, despite the poor correlations.

Line 21. The authors imply in this sentence that if the RH>70% large errors are to be expected. Given the sensors are being proposed as ambient PM monitors and ambient RH is frequently above 70% RH, does this not imply the sensors are not fit for the purpose intended? The authors do not seem to have an accurate understanding of ambient atmospheric conditions and also do not spend any discussing this rather large potential problem.

Line 30 onwards: The effect of sensor saturation is discussed only in terms of what curve to fit, rather than trying to understand why it is saturating and whether it is quantitative to use the data outside of the linear region. I suspect that you have multiple scatters and absorbers of light leading to a lower recorded concentration.

P8 and Figure 8: A high correlation coefficient is reported for the PPD20Vs at the Indian high PM concentration site. However inspecting the x-y plot, there is a lot of scatter in the <100 ug.m-3 part of the graphs. Again it is a shame that the time series are not shown as only then can you really see the detail. Unfortunately the scatter plot does not tell a reader enough about the performance. The interesting information will be in the details which the authors have not shown or discussed. Again the authors mention the 70% RH threshold as being important, however I would re-iterate that any sensor used in the environment must be able to function across the RH range.



P9 line 4: The statement that the sensors could perform better in future studies in an improved enclosure has no basis (though may possibly be correct). Why do the authors think this? What proposals for improvements are they proposing?

Line 17: Could the authors measure the temperature inside and outside of the box to address the question of the T variation of the electronics?

P10 line 4: Could the LOD as calculated by 2 or 3 SD of the noise on the blank signal be reported. I think this would be a better standard . Also blanks as a function of temperature and RH would be interesting to see. Is there any baseline drift on the sensors over the measurement periods?

Section 3.2: This is one of the most detailed sections of the paper, however in fact the authors only used just over 1 hours data out of 3 days deployment at the kerb-side, ignored periods when CO2 and PM did no correlate and came up with very good sounding numbers with high certainty (compared to all the other measurements in the paper). Unfortunately I do not feel the numbers are robust enough to publish, based on 1 hours worth of data with no replicates.

Section 4 Conclusions: The conclusions section seems to ignore all the poor statistical performances of the sensors, the potential environmental limitations, the issues with lack of correlation with reference instruments, the unknowns about sensor housing performance, the poor performances at <100ug,m-3 and above 200 ug,m-3 and paints a very rosy summary. This section should be re-written to actually reflect the results reported.

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