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Atmospheric Measurement Techniques Discussions

Interactive comment on "The Influence of Humidity on the Performance of Low-Cost Air Particle Mass Sensors and the Effect of Atmospheric Fog" *by* Rohan Jayaratne et al.

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Overall Comments This paper has the possibility of being a good paper, with some considerable extra additional text and analysis. The science area being discussed is very topical and the authors seek to show a particle-bound water effect on the DustTrak instrument measurements compared to the dried TEOM data.

Some specific areas which I think could improve the manuscript:

Comment 1 1.Referencing The authors only cite 15 papers, despite there being a significant body of work now in this area. For example, Kingham et

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al. https://www.sciencedirect.com/science/article/pii/S1352231005008885 who Specifically compare TEOMs with dusttrak and other instruments (And cites other measurement which have shown the differences and over reading of the instruments which do not dry the particles. I could have chosen several other papers.

Response 1 We have cited the following two additional papers in the Introduction:

Kingham, S., Durand, M., Aberkane, T., Harrison, J., Wilson, J.G., Epton, M., Winter comparison of TEOM, MiniVol and DustTrak PM10 monitors in a woodsmoke environment, Atmospheric Environment, 40(2), 338-347, 2006.

Alexandrova, O.A., Boyer, D.L., Anderson, J.R., Fernando, H.J.S., The influence of thermally driven circulation on PM10 concentration in the Salt Lake Valley, Atmospheric Environment, 37(3), 421-437, 2003.

Including these, seven new papers have been cited in this paper now.

Comment 2 The authors should do a more detailed literature review and clearly define the reasons why their new study adds to the wider knowledge

Response 2 We have done a more detailed literature review that has added seven more citations into this paper. The contribution of this paper to wider knowledge is now reflected in the following text:

At the end of the Introduction:

“While several studies have drawn attention to a possible effect of humidity on the performance of low cost sensors, no study has reliably quantified the effect. This study was carried out to investigate and to assess the magnitude of the effect of relative humidity on the performance of a low-cost particle sensor and to understand the mechanisms involved”.

The last paragraph in the Discussion section:

“Since they generally do not have drying facilities at their sample inlets, low-cost par-

ticle sensors measure what is actually present in the air, including both the solid and liquid phases of the particles. This is a real observation and not an artefact of the instrument as suggested by Crilley et al. (2018). This is an important aspect to be kept in mind when using low-cost sensors to assess the pollution levels in the atmosphere. What this illustrates is that it should not be presumed that low-cost sensors are suited for regulatory applications. For example, while it is reasonable to use low-cost sensors to measure the actual particle mass concentrations that are present in the air; such observations should not be used to verify if the air quality meets the stipulated guidelines or standards for particle pollution".

Comment 3 2. Quality assurance of laboratory experiment The description of the lab results is short and not detailed enough to have confidence in the work. The chamber at 1 m3 is rather small and probably has significant wall effects from particle deposition/emission. No description of how this is checked for is reported - the citation to the previous study is not enough for a reader to have confidence in the methodology. Only one set of experimental results are shown. Ideally at least 5 repeats with similar PM loading should be shown with an uncertainty analysis/error bars etc. In addition a blank run to show there is no effect of background in the chamber. Did the authors do any runs with laboratory derived aerosol?

Response 3 The focus of this paper is not on lab studies but on the field studies because many air quality stations use standard instruments such as the TEOM and BAM that have drying facilities to remove any water in the sample. This is appropriate as air quality standards are based on the solid fraction of the aerosols only. The main conclusion of this study is that low cost sensors that are being increasingly used in field studies today, do not have a sample drying facility and the PM values reported will include any liquid fraction and therefore should not be used for regulatory purposes. The laboratory experiments, including those carried out with five different low cost sensors, four different aerosols, at a range of relative humidity and temperature, are described in detail in our earlier publication (Jayaratne et al., 2018; Plos One) and anyone who

is interested in the details can read it in there. In this paper, we merely mention the laboratory experiments to show that the reported PM2.5 values are affected at relative humidities above about 75%. This observation leads to the more detailed field studies under real world humid conditions and episodes of fog that we focus on in this paper.

Comment 4 3. Field study. Despite running the experiment for a month nearly the authors only show the one day (when there was fog). In the results they talk about several different periods but it is hard to have a clear overview as no dates are mentioned. It would be better to show the full dataset along with the associated local meteorology. Discuss that larger dataset and then focus in on the fog events. Did the RH get close to 100 in the absence of fog? Did the over reading occur then?

Response 4 We have now presented the data from the entire period of observation (new Fig 3). The dates on the x-axis are shown from 21st July to 14th August 2017. The respective dates are also included in the captions of the other figures. The figure includes the time series of the relative humidity.

A description and discussion of the larger dataset is provided in the following text that has been inserted on page 6:

“Fig 3 shows the time series of the PM2.5 concentrations reported by the PMS1003 and the standard TEOM during the entire duration of the study. Also shown is the relative humidity during this period. The relative humidity exhibited a daily cycle with a minimum in the early afternoon and a maximum at night. Note that the peak PM2.5 concentrations indicated by both instruments generally coincided with the time when the relative humidity reached its maximum value near dawn each day. The maximum value often coincided with episodes of fog, although its value did not reach 100%. It is likely that this was a consequence of a limitation of the instrument. At such times, the PMS1003 reading was generally higher than the TEOM. However, from Fig 3, it is observed that on many days, the readings of both instruments increased during times when the relative humidity was high, suggestive that the TEOM did not remove all of

the liquid portion of the aerosols. In the afternoon, the TEOM reading was often higher than the PMS1003. This is probably because most of the aerosols in the atmosphere at this time were ultrafine particles from motor vehicle emissions. The size of these particles are below the minimum detectable size limit of the PMS1003 which is 0.3 μm".

Comment 5 The authors do not mention the systematic under reading by the dustrak cf the TEOM in the non fog part of the graphs shown. No summary statistics of the comparisons between the instruments over the intercomparison period shown - Which I think is essential to understand the bias and offsets. It would be good to see the statistics of different RH bands.

Response 5 We assume that the Reviewer is referring to Fig 2, as this is the only graph that shows data from the DustTrak particle monitor. In general, the readings of the PMS1003 sensors differed between the individual units. The differences depended on the type of aerosol and the concentration being measured. At the low concentrations found in the ambient environment of Brisbane, the coefficient of variation between 'identical' PMS1003 sensor units was about 0.07 and this is reported in detail in Jayaratne et al. (2018).

As observed in Fig 2, the PM2.5 readings of the particular PMS1003 sensor used in this experiment were about 20% higher than the readings on the DustTrak. In the present study, the absolute magnitudes of the PM2.5 concentrations are not considered to be important since the paper is focussed on the differences in PM2.5 concentrations reported by the DustTrak and the PMS1003 sensors as the relative humidity varies. We show that the concentration remains relatively constant up to a relative humidity of about 75% and then rises sharply as it is increased further. It is the difference in the reported concentrations that is important, not their actual magnitudes.

However, we have inserted the following text in the description of the graph in Fig 2 in Section 3.1:

"As observed in the figure, the PM2.5 readings of the particular PMS1003 sensor used in this experiment were consistently higher than the readings on the DustTrak. In general, the readings of the PMS1003 sensors differed between the individual units. The differences depended on the type of aerosol and the concentration being measured. At the low concentrations found in the ambient environment of Brisbane, the coefficient of variation between 'identical' PMS1003 sensor units was about 0.07, as reported in detail in Jayaratne et al. (2018)".

Comment 6 4. Fog vs high RH: Though ther is some discussion about the process of fog formation and particle activation, the fog effect is mixed up with deliquescence. What the DustTrak observes is aqueous particles, and the TEOMs will have a mixture of effloresced and supersaturated aqueous particles. The growth curve either from a dry or supersaturated aqueous particle is similar. This is completely different to fog formation which is cloud particle activation which occurs when RH>100%. The author should clearly separate these two regimes and these separate processes, both in the results and discussion.

Though I am reasonably sure the observation of fog and rain with the different measurements by both instruments are correct, and are somewhat explained by the authors, there is a more scientific discussion required to make sense of the results. It would also be useful to see a clear extrapolation to more a general discussion about what type of fog it was (see https://www.atmos-them-phys.net/14/10517/2014/acp-14-10517-2014.pdf for definitions) and how typical the short period of observation they have is (I.e. how would it affect annual data capture, could you write a met station interface to remove data with fog?

Response 6 We agree with the reviewer's comments. However, the mechanism of water uptake by particles, the formation of fog in the atmosphere and the physics behind the removal of fog by rain are beyond the scope of this paper. Here, we focus on the effect of wet aerosols and fog droplets in the air on the readings reported by low cost particle sensors and other particle sensors with no drying facilities at the inlet. We did

not have any means of determining whether there was fog at RH<100%. The reports of fog in this study are all based on visual observations and may not be exact when the RH is near 100%. We have inserted the word “visually” before “observed” in at least three sentences in Section 3.2:

“Fog was visually observed at the site during the early morning hours”.

“In Fig 5, we show the number concentration of particles larger than 1.0 μm against the corresponding number in the lowest size bin, 0.3 to 0.5 μm on the 31st August when there was an episode of fog visually observed during the early morning”.

“Fig. 6(a) shows the results on the 24th July when the relative humidity did not exceed 80% and there were no visual reports of fog”.

Regarding removing data with fog, our results show that it is impossible to predict the formation of fog based on the relative humidity alone. This is discussed in detail in the following paragraph in the Results and Discussion Section:

“An obvious question that arises from this work is whether it is possible to derive a correction factor for the particle number and mass concentrations reported by the low-cost sensors in the presence of high humidity and fog. Our results show that, once the deliquescence point is exceeded, the particle number and mass concentrations begin to increase and are not directly related to the absolute value of the relative humidity. Once the ambient temperature reaches the dew point temperature, the conditions become suitable for the formation of fog droplets in the air and, since a significant fraction of these water droplets fall within the detection size of the PMS1003 (Fig 7), they are detected as particles. We also observed that the PNC and PM concentrations reported by the PMS1003 decreased in the presence of rain. This is not unexpected as it is known that rain washes out a fraction of airborne particles. More interestingly, our results show that the decrease in PNC and PM concentrations reported by the PMS1003 due to rain were significantly greater when there was an episode of fog than when there was no fog. While a significant number of fog droplets fall within the detection size range of

the PMS1003, almost all the rain drops are larger than the maximum detection size of particles. We hypothesize that the raindrops were washing out the fog droplets in the air, resulting in an overall decrease in the reported PNC and PM concentrations reported by the low-cost particle sensors that have no drying facilities at their sample inlets. Moreover, the relative humidity of the atmosphere increased during rain, often approaching 100%. Raindrops are too large to be detected by most particle sensors and, as such, they do not show an increase in concentration during rain. For these reasons, we find that there is no direct relationship between the relative humidity in the atmosphere and the PNC and PM concentrations reported by a sensor or monitor with no drying facility at its inlet and, as such, it is not possible to derive any appropriate correction factors for this effect".

Comment 7 Figure 2 and 7 are not needed as these are well know in the literature, and only meet mentioning in the text.

Response 7 Also, in response to Reviewer 1, we have moved Fig 2 to the Introduction (as Fig 1 now) and, as suggested, we have removed Fig 7 from the paper.

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