

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 manuscript presents an interesting assessment of the influence of relative humidity on the performance of one low cost sensor. The results are robust, even if the amount of data presented could be considered scarce. They are useful in general for the scientific community. I would favor publication, but a number of relevant issues should be addressed first:

Comment 1 Title: please modify to “the performance of a low cost sensor”, as the authors mainly analyze one type of sensor and the title is therefore misleading. The few data presented for another 4 sensors do not justify generalizing in the title.

C1

Response 1 We have amended the title as suggested.

Comment 2 Page 1: Line 19: “sensors can accurately report particle mass and number concentrations”, please remove as this is not a conclusion from this work. The authors have not studied the overall performance of sensors.

Response 2 We have removed this text and amended this sentence as follows:

“This study shows that it is important to understand that the results provided by low-cost particle sensors, such as the PMS1003, cannot be used to ascertain if air quality standards are being met”.

Comment 3 Line 30, reference needed.

Response 3 The following references have been inserted: Snyder et al., 2013; Jovasevic-Stojanovic et al., 2015; Rai et al., 2017.

Comment 4 Page 2: References needed in lines 3, 4 and 5. In general, please review references in the introduction, as they are scarce.

Response 4 The following references have been inserted: Jayaratne et al., 2018; Rai et al., 2017; Kelly et al., 2017. Fine et al., 2010; Piedrahita et al., 2014. Holstius et al., 2014; Rai et al., 2017; Crilley et al., 2018; Jayaratne et al., 2018.

Comment 5 Line 7: “the performance of low cost” should be “the performance of one low cost”

Response 5 The sentence has been changed to “. . . the performance of a low-cost particulate matter sensor”.

Comment 6 Page 3: Line 23: “out of each”, sentence unfinished

Response 6 Text changed to “. . . airflow into and out of the device”.

Comment 7 Line 29: what were the results from the intercomparison of the Dusttrak? They could be useful, even if in Supporting Information

C2

Response 7 These results are detailed in our publication Jayaratne et al., 2018. In this paper, we have inserted the following text:

“Prior to the study, the DustTrak was calibrated against a standard TEOM in the laboratory. With dry ambient aerosols, the PM_{2.5} concentrations reported by the two instruments agreed to within 10% (Jayaratne et al., 2018). With normal ambient aerosols, the readings again agreed closely until the relative humidity exceeded about 75% when the DustTrak readings were significantly greater than that of the TEOM”.

Comment 8 Page 4: Line 29: how do the authors know? The Dusttrak concentrations also increased with RH>78%. Is 1.8 the ration between the sensor and the Dusttrak readings? If so, what was the ratio for RH between 60-75%? Please clarify these issues: with the data in figure 1 it is not possible for the reader to extract the conclusions in lines 28-30 on page 4

Response 8 The deviation is from the concentration value at lower RH. We have changed the text to:

“The critical relative humidity beyond which the PM_{2.5} concentration reported by the PMS1003 begins to deviate from the previous ambient value is indicated by the broken line in the figure”.

No. This refers to the increase in the PMS1003 reading from its steady value at low RH values. There is a 80% increase between a RH of 75% and 88%, hence the ratio 1.8. Accordingly, there is no increase between 60% and 75%, so the ratio here will be 1. We have changed the text as follows:

“Beyond this value, the PM_{2.5} readings indicated by the PMS1003 increased steadily from about 9 $\mu\text{g m}^{-3}$ at a relative humidity of 78% to about 16 $\mu\text{g m}^{-3}$ at the maximum relative humidity of 89% achieved in this experiment – an increase of almost 80%”.

Comment 9 Lines 9-14: these are not original results from the authors and should be moved to the introduction. Especially, figure 2 should be removed as it is published

C3

material and in addition it doesn't add relevant information for the paper.

Response 9 The material is relevant as it shows that there are many deliquescent substances in the Brisbane environment which is used to explain our results. Also, this Figure is not directly from the paper cited (Harrison, 2007) but we have plotted it based on data provided in that paper. However, as suggested, we have moved the figure (now labelled 'Fig 1') and the following text to the Introduction:

“The composition of particles in the atmosphere of Brisbane, as derived from Harrison (2007) is shown in Fig. 1. The subtropical, near-coastal environment is characterised by the presence of several hygroscopic salts such as sodium chloride, ammonium sulphate and ammonium nitrate that have deliquescence relative humidities in the range of 70% to 80% (Hu et al., 2010). Many particles in the air in Brisbane contain these salts in varying concentrations. Once the relative humidity exceed the respective deliquescence values, those salts begin to absorb water, resulting in particle growth and the excess water is registered by PM sensors, unless they are removed at the instrument inlets by heating or drying. While more expensive instruments such as the TEOM have built-in drying features at the sample inlets, it is not standard on low-cost sensors and even in many other mid-cost monitors such as the TSI DustTrak”.

Comment 10 Line 31: “illustrating” should be “suggesting”. This comparison is useful, but it can only suggest. Without a collocated CPC it is not possible to conclude firmly that the increase in PNC is an artifact and not that an additional source could be present which by chance correlates with foggy scenarios. However unlikely this is, it can't be ruled out with the data presented by the authors.

Response 10 We agree and have replaced the word “illustrating” with “suggesting”.

Comment 11 Figure 7: same as for figure 2, it should be removed as it is not primary research by the authors (it is already published by other authors). Therefore it should be removed and, if anything, referenced in the introduction

C4

Response 11 As suggested, we have removed Figure 7 from the paper.

Comment 12 Page 7, lines 15-16: why is this an “interesting observation”, if it is “not unexpected” by the authors? It all seems quite expectable (until line 22) page 7, line 33: “sensors are not always fit for purpose”, I believe there is a misconception here: precisely because they are fit for purpose they shouldn’t be used to verify compliance with standards, as this is not the purpose that sensors are designed for. This should be the message to be conveyed, to that they are not fit for purpose.

Response 12 We agree. We have deleted this text and revised the two sentences as follows:

Page 7, lines 15-16: “We also observed that the PNC and PM concentrations reported by the PMS1003 decreased in the presence of rain”.

Page 7, line 33: “What this illustrates is that it should not be presumed that low-cost sensors are suited for regulatory applications”.

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