

This work evaluates the performance of a low-cost thermal dryer for the OPC-R1, in comparison to an OPC-R1 without a dryer and the Palas Fidas 200, which has an automated drying system. Two separate scenarios which can lead to erroneously high PM readings at elevated relative humidity (RH) were investigated in a laboratory setting. (1) Fog conditions, where water droplets suspended in the air are incorrectly detected as particles, and (2) hygroscopic growth of particles, leading to elevated size and mass measurement relative to a measurement at lower RH. To evaluate the performance of the R1 with dryer against the other two instruments, both the time series and “drying efficiency” are presented. The authors conclude that the low-cost dryer was able to mitigate positive artifacts due to the two effects outlined above, but raise concerns over the comparability of their “fog” conditions to the real-world scenario. They state the need for further work to characterise the temperature profile and optimise the power requirements of their drying system.

This work is a useful addition to the literature on low-cost particulate matter sensors. The literature review is thorough and highlights that drying systems are both under-studied and potentially simpler to implement than post-calibration of measurements for RH. The two scenarios being investigated are clearly distinguished and explained.

I would like to highlight two main aspects of the paper which I think should be clarified. I think this is good work; my concerns relate to how the results are presented. I think the results could be more clearly placed within the context of standard reference measurements of PM, and certain aspects of the method are unclear.

It is not clear to me what the authors are trying to achieve through use of their low-cost dryer. Multiple comparisons are made which all suggest a different target. Are they trying to (1) completely remove all water, (2) produce “reference-equivalent” PM readings to supplement regulatory monitors, or (3) simply reduce the positive artifact due to the presence of water? I think this needs to be stated from the outset as it affects which evaluation method is most appropriate.

Comparing the R1 + dryer to the R1 without a dryer proves only aim (3), that water has been removed by heating. The reading is closer to dry particles, but we don’t know how close.

Comparing the R1 to the Palas may prove (1) or (2). My uncertainty here is due to the lack of information on how the Palas PM measurements were processed. The Palas can output (1) raw particle number size distribution, (2) PM “classic” and (3) PM “ambient”. The authors could have obtained PM mass from (1) by applying their own density and penetration factors, (2) is calculated by the Palas using a default assumed density $\rho = 1.3 \text{ g cm}^{-3}$, whilst (3) has an additional empirical correction factor applied to make the readings “reference-equivalent” (Di Antonio 2020).

Firstly, I would be unable to reproduce these findings without knowing which of the three methods the authors have used.

Secondly, if the PM “ambient” readings have been used, I think it is worth pointing out that (under default IADS settings) the Palas is not *trying to fully dry the particles* because it is aiming to be reference-equivalent. Hence this is relevant to aim (2). The EU reference standard specifies conditioning for 24-hr at 20°C, 50% RH (see CEN standard 12341), ie not completely dry. Given this, the Palas can be expected to potentially measure PM which still contains bound water and not remove all fog droplets as it may not sufficiently lower the RH.

Thirdly, when the authors manually set the IADS to 70°C, they were overriding these reference-equivalent settings and were likely removing nearly all the water. This comparison is most appropriate to aim (1).

A short explanation of what each of the authors’ comparisons (R1 without dryer, Palas automated settings, Palas at 70°C) actually means in the context of how much of the water is being removed and the relation to regulatory PM monitoring would make this paper more impactful.

A more precise description of how the Palas PM values were obtained is essential to make this work replicable by other authors. There should also be a more detailed description of how the R1 PM values were derived- were they the firmware-output values and what density and refractive index have been used? I would also point out that some discrepancy between R1 and Palas PM readings is to be expected because the R1 has a larger minimum detectable diameter.

My second point of feedback is that the meaning of the T_{OPC} quantity should be clarified. In particular, the authors should specify what this temperature corresponds to- is the sensor situated (1) on the OPC circuit board, or (2) within the sample air flow? The authors should then be clear about what they are preventing from overheating by including an upper temperature limit.

I suspect case (1) applies, from my own work with the OPC-N3, a similar model of OPC. This makes T_{OPC} the temperature of the OPC box/components rather than the sample. Therefore the upper temperature limit of 35 °C would be preventing the OPC from overheating and is not an appropriate control measure to prevent excessive sample heating.

As the authors state in the penultimate line, it is a shame that no information has been provided on the temperature (and RH) that the dryer conditions the sample to. This would be an important parameter in assessing which of the above three aims the dryer is best suited to. From the fact that the IADS had to be set to 70 °C to achieve comparable PM readings, I suspect that the sample is being heated much higher than the 35 °C limit on T_{OPC} . This information would also be useful to assess potential loss of semi-volatile components.

I have a few remaining minor points, which I shall outline more briefly below.

Figure 1 presents the averaged particle number and mass distributions during a fog event. I assume these have been averaged over some time period corresponding to the fog event. The authors should indicate (1) whether the data have been averaged and (2) over what time period.

Some improvements could be made to the Methods section. Near the end of the paper, line 220 gives typical flow rate and power requirements for the self-developed and Palas drying systems. I think these would be better placed earlier, in the Methods section (say around line 125). Please also give the dimensions of the Palas' heated inlet for comparison. The authors should specify the orientation of the R1's inlet/dryer- is it pointed upwards as pictured in Figure 3A (similarly to the Palas inlet)? Within Methods, the authors say they used "sodium chloride, potassium chloride, ammonium sulphate, and ammonium nitrate" (line 111) with the aerosol generator, then go on to simply say how they used ammonium sulfate on lines 192/193. The information on lines 192/193 would be best moved to Methods, and I am not sure why these other salts have been included with no further discussion.

There are a few remaining points to clarify within the results sections. On line 158, the authors state the humidifier was turned off after reaching $300 \mu\text{g m}^{-3}$. Given the differing RH sensitivity of the three instruments, the authors should state which of the three instruments was used to determine when this threshold had been reached.

Wet towels were added before switching the humidifier off in experiment 1 (fig 4A) but after in experiment 2 (fig 4B). It is not clear to me why this difference is present. Additionally, the analysis from line 157 would read more clearly if the authors actually name (before further discussion) which of the two experiments is being described (IADS set to automatic or 70°C). The authors simply use "Fig. 4a", "Fig. 4b" to indicate which experiment they are referring to, but I think this could be clearer.

Please give errors for the values of η_r and η_s . Additionally, for the experiment with IADS at 70°C, only η_s is given, please also specify η_r . The Palas settings have changed so it would be informative to compare the η_r values from each experiment.

Do the authors know the typical temperature of the IADS during the automated setting experiment? It would be informative to compare this to the 70°C set temperature in the latter experiment.

In line 196, the authors discuss the lack of a "sudden decrease" in the Palas $\text{PM}_{2.5}$. I think it would be worth recognising that there is still a decrease, just slower than the R1 for the reasons discussed in the paper. The fact that the Palas $\text{PM}_{2.5}$ is less than the R1 without any dryer shows that some sample drying must be occurring, as the pre-experiment calibration should have largely removed other sources of disagreement.

In line 214, the author suggest the drops and jumps in $PM_{2.5}$ measured by the OPC R1 with dryer may be due to loss of semi-volatile components during heating. However the sample is ammonium sulfate (non-volatile), so where have the semi-volatile components come from? If unfiltered air has been used with the aerosol generator, this should be specified, particularly as this could also limit the extent of water uptake by the ammonium sulfate.

Finally, a few suggestions regarding the wording/readability in certain places.

- Line 98: “made from greenhouse glass”
- Line 106: “consisting of a”
- Line 124: “brass tube 50 cm in length”
- Line 200: “much more slowly”
- Line 203: “the decrease could have other causes”

Overall, good work! I found it very interesting to read.

References

Di Antonio, A. (2020). Development of novel methodologies for utilising low-cost sensors for ambient Particulate Matter measurement (Doctoral thesis).