The paper presents the application of a heated inlet to mitigate against the influence of hygroscopic growth of particles on the imputed particle size distribution and mass loading by an OPC, in this case the 'lower-cost' OPC-R1. The paper is well written and the procedures are mostly clear, though considering that this seems to be an ‘open-sourced’ project, it would be helpful to have more specific information on the construction of the heater – more information on electronics is included in the linked Github page, but not sufficient to recreate the design without significant leaps. However, my main concern is that it the interpretation of results is such that it is difficult to know whether such a device will actually improve measurements by low-cost sensors, or introduce other, possibly more difficult to correct errors. Additional experiments that seek to separate ‘drying’ from ‘particle evaporation’ (by varying particle volatility or treatment mechanisms) could do this, but are a substantial extension beyond what is included here.

In general, I share similar concerns to the previous two reviewers, and offer some specific points that should be addressed below.

I am in particular concerned about the influence of and lack of ‘control’ on the temperature of the sample and the fact that it may be as high as 70 degrees C. We don’t really know what the temperature it is, but it appears it may be high as this is when the most consistent behavior with the reference instrument is observed (when the Palas was operated at a fixed temperature). The paper (rightly) includes multiple allusions to the potential for loss of semivolatile material, and this is indeed a major concern for
application of this system for ambient aerosol, especially in urban areas where most low-cost sensors are deployed. Heated tubes of this type are used as ‘thermodenuders’ to remove semivolatile organics, and previous deployments in urban areas find that nearly 50% of organic aerosol will be removed by heating to 70 C (Paciga et al. 2016), though with a longer residence time (I calculate around 8 seconds for your heater geometry vs. 50 seconds for the Paciga et al system). If organic aerosol or ammonium nitrate are substantial components of the sampled aerosol, this heater will remove much of this material and bias any measurements low. The removal of water and semivolatile removal components could have been isolated by running experiments with diffusion or Nafion drivers in parallel with the heated tube. However, in the absence of such data and especially in the absence of information about the actual temperature of heating, it is (and will be) hard to interpret what the heater is actually doing to the aerosol. This is especially tricky because much of fine aerosol number and mass will be in the submicron range, and evaporation will push the entire size distribution out of the range of diameters the OPC(s) can detect.

There is a strong basis for the limitation of inlet temperature to 40 C in ‘reference’ instruments (as noted by WMO/GAW guidelines) due to the influences discussed above. For example, early studies with the ‘TEOM’ found a strong and variable bias due to loss of semivolatiles due to heating (typically to 50 C) (Allen et al. 1997; Charron et al. 2004) and later versions did away with this heating.

The ‘fog’ measurements, don’t appear to really be fog, but are likely residual contaminants from the humidifier. This is evident by the small size (as noted by the authors) relative to actual fog droplet. There’s a wide literature on this, going back decades (e.g. (Rodes et al. 1990)). Therefore, while the application of this heater systems for aerosol measurements in foggy environments may be a goal, it doesn’t appear to be one tested here. Rather, these are similar experiments to the others shown, but with aerosols of unknown composition.

The results in Figure 6 may be helpful to separate ‘drying’ from ‘evaporating’ because they are with a non-volatile aerosol at known RH. However, they are still difficult to interpret. I expect this is because there are interactions between different drying conditions (the low-cost dryer is probably ‘over drying’ relative to reference conditions) and the size cutoff of the two OPCs (the R1 may be ‘missing’ a substantial amount of material between 0.18 and 0.3 microns).
The paper alludes to testing various inorganic aerosols (Line 111) but these data aren’t included. In particular, tests with ammonium nitrate would likely highlight the influence of heating on semivolatile material. This additional data should be included/discussed.

Specific points

L15 – Not clear that comparing average PM2.5 concentrations are appropriate comparisons. Other distribution parameters and comparative statistics are helpful.

L18 – Here and elsewhere there is discussion of ‘accuracy’ of sensors, but nowhere is data from this arrangement compared to a ‘true’ reference measurements, and so this seems a bit of a bold claim.

L31 – Accuracy is one limiting factor, but there are other key concerns: power consumption, durability, ... . What are ‘certain applications’?

Figure 1- These data traces appear to be smoothed. This is not appropriate for discrete data points.

L76 – It’s not clear what ‘quality’ means here. A key considering is that the variable space covered by training and deployment data sets coincide.

L138 – As noted above and by another referee, a lack of specificity about what exactly this temperature refers to is a key question. The key temperature for evaporation of semivolatiles will the air temperature within the heater, as semivolatile materials evaporated will be a function of this temperature.
Eq. 1 – This approach doesn’t seem helpful, as it combines differences in instrument response (e.g. due to different size cuts and calibrations) with ‘drying’ (which is actually drying + evaporation of semivolatile).

L149 – R² isn’t ‘coefficient of correlation’ – this also seems to be a minimal/insufficient way to compare these data sets under the ‘best possible’ conditions. What about slope/offset/bias?

L159 – Given the size distribution measured, it is likely not sedimentation, but rather diffusive loss to chamber surfaces.

L161 – The different size cut of the two OPCs could be a factor here. A minor growth could push particles into the detection window of the Palas. So this is likely not more particles, but growth of the same ones.

L164 – As noted by other reviewer, this is likely because the dryer is going well beyond ‘reference’ conditions, and probably evaporating some of the salts/organics in these particles.

L168 – As noted above, these are not fog droplets, but probably particles of residuals from humidifier.

L189 – Previous work in this area has determined that there is not an ‘optimum’ and recommends the 40°C upper limit. This may be an important need, but as indicated by other reviewer, it needs to be made clear that this is clearly beneficial relative to computational approaches.

L203 – As noted above, sedimentation is likely not an important loss process for particles in this size range (all sub-micron).

L235 – Not clear whether there would be any benefit to drying flow to an electrochemical (or other gas) sensor, as they typically respond to both temperature and RH, and possibly to absolute humidity. Therefore, a ‘dryer’ that doesn’t actually remove water won’t necessarily help (and may make things more complicated when it comes to signal interpretation).
References


