Characterizing the performance of a POPS miniaturized optical particle counter when operated on a quadcopter drone, by Liu et al.

Response to Associate Editor

We would like to thank the associate editor for his comments on the manuscript. The associate editor raised some interesting and relevant points that we have addressed in our revised manuscript. Our responses are presented in red.

Overall and Specific Comments:

I would like to thank the authors for their efforts in addressing the comments on their originally submitted manuscript that were made by the peer reviewers. I think the manuscript presents a reasonable analysis of the data the authors have collected on the performance of their POPS instrument in both a ground comparison with a SMPS reference instrument during the CLARIFY 2017 campaign and by comparing flight (hover) to non-flight (ground) performance when mounted on a quadcopter UAS.

I would encourage the authors to address a few additional comments prior to publication:

L40: perhaps "operating on the ground and when hovering at 10 m." since both are with the POPS mounted on the UAS.

We modified the text to "We report the root mean square difference (RMSD) and mean absolute difference (MAD) in particle number concentrations (PNCs) when mounted on the UAV and operating on the ground and when hovering at 10m."

L60: "acting as condensation nuclei" -> "to act as cloud condensation nuclei"

Agreed.

L109: "use the" -> "used a" or "included a"

Agreed.

L138: SMPS sample was dried while POPS was not. How close was the POPS temperature to the ambient sampling temperature?

Unfortunately, the POPS doesn't have an inside temperature sensor within the optical cavity. The POPS only has a temperature sensor on the outside on the Beagleboard computer to provide the ambient temperature.

L150: it would be useful for readers if the authors would add some detail to the process used to adjust the sizing (signal -> diameter) for the change in refractive index from PSL to that assumed for biomass burning. Was the Mie calculation (polarized?) carried out for the POPS light collection geometry or more generally? Mie code used for calculation?

The Mie calculations were made for polarized light and for the geometry specific to the POPS. We have added wording to this section (L148-L150) and also added a detailed description of the process as Appendix A. We feel that this will serve as a useful reference document for future operations.

L161: SMPS range is stated as 0.01 μm to 1.00 μm, but in Fig 2 and the description of the overlap size range the upper limit is 0.45 μm.

Apologies – this was a typo – we have now corrected this to "while the SMPS covers diameter ranging from around 0.01 to 0.45μm."

L180 (Table 1): Table 1 as constructed seems unnecessary. The dates are already included in Table 3 along with other important information—perhaps Table 1 could be replaced by Table 3 with an additional column for time.

Agreed. Actually, we have also incorporated the information on the profiles (previous Table 2) into a new Table 1 as this seemed most efficient. Because AMT cannot display tables in colour, we also give up the colour of the test numbers in Table 2, Table 4, and Table 5, but keep the different font.

L209: "contribute to 93% of the scattering" should be just "contribute 93% of the scattering"

Agreed.

L211-: it would be useful to include in the text some of the details that appeared in your response to reviewer 1's question about the CLARIFY PCASP data that are shown in Fig 2: POPS and SMPS were sampling at 330 m while the BAE146 PCASP measurements were 1.9-7.3 km and the potential causes of the divergence at larger Dp.

We added the statement (L205-207): "The POPS and SMPS were sampling at around 330m altitude ASL when at the LASIC site while the PCASP data from the CLARIFY campaign was collected from 1.9-7.3km ASL"

L239: "mass mixing ratio"—Figure indicate volume mixing ratio (ppmv)

Agreed. Amended.

L251->: it seems that the typical (although not universal) increase in PNC between G_R and G_NR could include a contribution from suspension of particulate material by the near-ground prop wash (also coarse more impacted than accumulation?), but the same is not true for the even larger typical increase between FLY and G_NR. What is the proposed mechanism by which higher wind speeds lead to artificially higher PNCs in flight?

See below

L293: "Variations in the orientation of the inlet led to uncertainties in the sample flow rate." Is vague. The POPS flow and uncertainty in the flow are independent of instrument orientation. One might postulate based on the observed increase in the scatter of the reported flow (whether real or perceived) during flight (FLY relative to G_R), that the fast adjustment of the platform to changes in wind produces this variability in the flow. The noise in the flow does not increase appreciably from G_NR to G_R, but there is an observed mean increase in PNC.

See below

L302: How (though what mechanism) do you expect the noise in the reported flow impacted the PNC measurement over time? L299 states that the mean flow rate was unchanged, but the result seems to be a systematic increase in the measured PNC.

The three comments above are rather inter-linked so we deal with all here.

We agree that prop wash could potentially increase the suspension of particles when operating on the ground. It is difficult to establish definitive reasons for the larger PNCs during FLY when compared to G_NR, and they may not be artificial. One reason for FLY registering higher PNCs might be that the surface acts as a sink for particles via dry deposition, the rate of which will primarily dependent upon the friction velocity of the surface which will depend on the details of the fetch, i.e. the direction of the wind (e.g. Pellerin et al., 2017). One recent measurement campaign using a miniaturized OPC (the LOAC) uses a tourist balloon tethered in Paris to examine the PNC within the lowest 50m of the atmosphere (Renard et al., 2020), but the vertical resolution of the measurements that are reported are insufficient for us to determine whether they find similar results over the lowest 10m of the atmosphere. Far more attention has been given to the vertical profile of ozone owing to the direct health impacts of ozone and the impacts of ozone and vegetation damage (Clifton et al., 2020).

However, as suggested by the editor, these impacts may also be artifacts: a potential mechanism could be that strong winds causes the attitude of drone to change, which changes the angle of the inlet relative to the horizontal plane horizontal plane which changes the sample flow rate.

We therefore add a short paragraph at line 396 – in the discussion of the results rather than in the results themselves as this is a more logical place to include the discussion: "While the increase in PNC from G_NR to G_R might be explained by generation or resuspension of aerosols from the surface by the rotors of the UAV, the increase from G_R to FLY is more difficult to attribute. The surface acts as a net sink in aerosols through dry deposition which could lead to an increase in PNC with altitude (e.g. Pellerin et al., 2017), but there are confounding factors from changes in the attitude of the drone and rapid changes in the attitude necessary for stabilizing the position of the UAV during FLY that could also influence the measurements. Indeed, there is evidence that fast adjustments to the attitude of the UAV increase the variability in the flow rate reported by the POPS sensor, particularly at higher windspeeds, where these corrections are larger."

Pellerin, G., Maro, D., Damay, P., Gehin, E., Connan, O., Laguionie, P., Hébert, D., Solier, L., Boulaud, D., Lamaud, E. and Charrier, X., 2017. Aerosol particle dry deposition velocity above natural surfaces: quantification according to the particles diameter. *Journal of Aerosol Science*, *114*, pp.107-117.

Renard, J.B., Michoud, V. and Giacomoni, J., 2020. Vertical profiles of pollution particle concentrations in the boundary layer above Paris (France) from the optical aerosol counter loac onboard a touristic balloon. *Sensors*, *20*(4), p.1111.

Clifton, O.E., Fiore, A.M., Massman, W.J., Baublitz, C.B., Coyle, M., Emberson, L., Fares, S., Farmer, D.K., Gentine, P., Gerosa, G. and Guenther, A.B., 2020. Dry deposition of ozone over land: processes, measurement, and modeling. *Reviews of Geophysics*, *58*(1), p.e2019RG000670.

L309: "differences at sub-micron sizes are less than those at" or "differences in sub-micron aerosol are smaller than those in super-micron aerosol"

We modified the text to: "It also shows that the differences in sub-micron sizes are less than those in super-micron sizes at G_R and FLY."

L312: "summarizes the PSD percentage differences for the two modes"

We modified the text to: "Table 5 summarizes the PSDs percentage differences for two modes at G_R and FLY for each case."

L327: What is meant by POPS "operated in" accumulation mode vs coarse mode? Are these not just different parts of the size distribution (per line 311)?

We modified the text to (L328-L331): "Generally speaking, RMSDs and MADs indicate the impact of rotors and UAV attitude on the performance of the POPS in measuring the accumulation mode is lower than when in measuring the coarse mode, for all cases."

L391: What would the hypothetical mechanism for the rotor induced increase in coarse mode aerosol be?

We now state (L396): "While the increase in PNC from G_NR to G_R might be explained by generation or resuspension of aerosols from the surface by the rotors of the UAV, the increase from G_R to G_FLY is more difficult to attribute”

Figure 3 caption: perhaps “(a) Time series of SMPS and POPS particle concentrations in the diameter range 120 – 450 nm measured during the LASIC/CLARIFY-2017 campaign. (b) Ratio of the POPS to SMPS concentrations shown in (a).” I agree with the spikes in the CO trace from the daily calibrations is distracting. In panel (e), it would be better to not include the lines between gaps in the AOD data.

We change the caption to: “(a) Time series of SMPS and POPS particle concentrations in the diameter range 120 – 450 nm measured during the LASIC/CLARIFY-2017 campaign. (b) Ratio of the POPS to SMPS concentrations shown in (a).”

We have also removed the calibration spikes from the CO plot, and the dashed lines from the AOD plot.

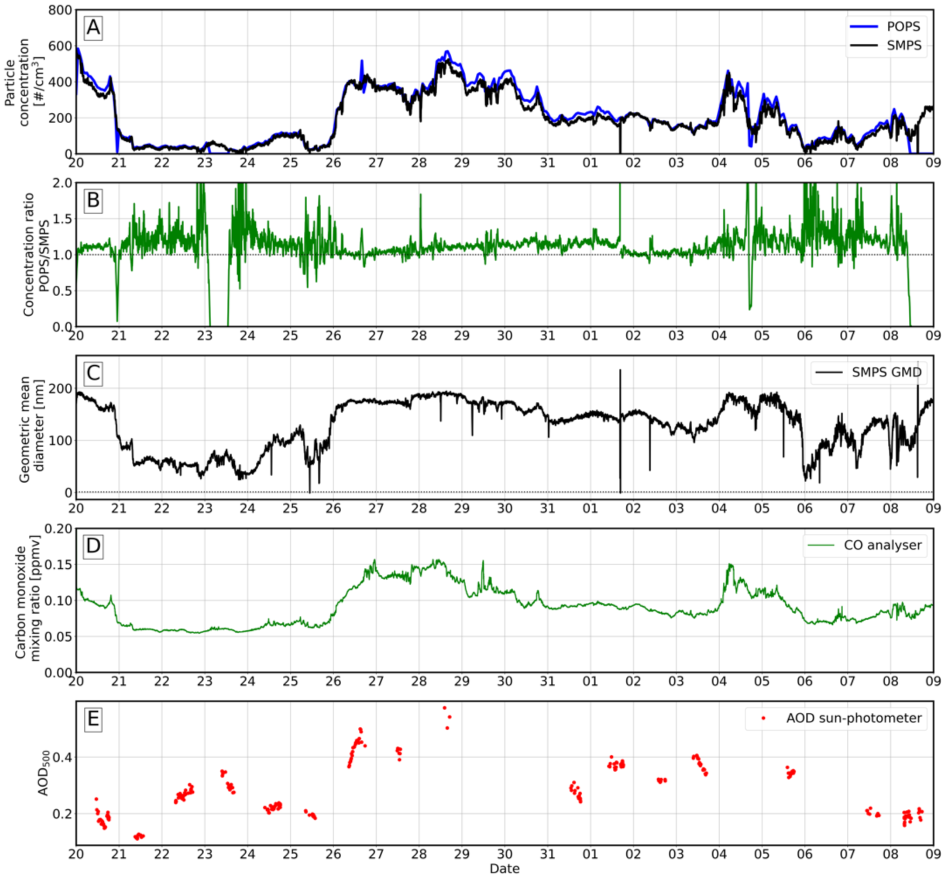

Figure 3. From top to bottom. (a) Time series of SMPS and POPS particle concentrations in the diameter range 120 – 450 nm measured during the LASIC/CLARIFY-2017 campaign. (b) Ratio of the POPS to SMPS concentrations shown in (a). (c) Geometric mean diameter from SMPS. (d)

Carbon monoxide mixing ratio from Los Gatos Research CO analyser, and (e) AOD from Cimel sun- photometer.

Figure 4: it would seem that using a consistent binning across the G_NR, G_R and FLY PDFs of each flight would provide a better comparison of the respective PNC distributions than the variable bin widths that are presented.

Thank you for the suggestion. We have modified the Figure 4 that using a consistent binning across the G_NR, G_R, and FLY PDFs of each flight.

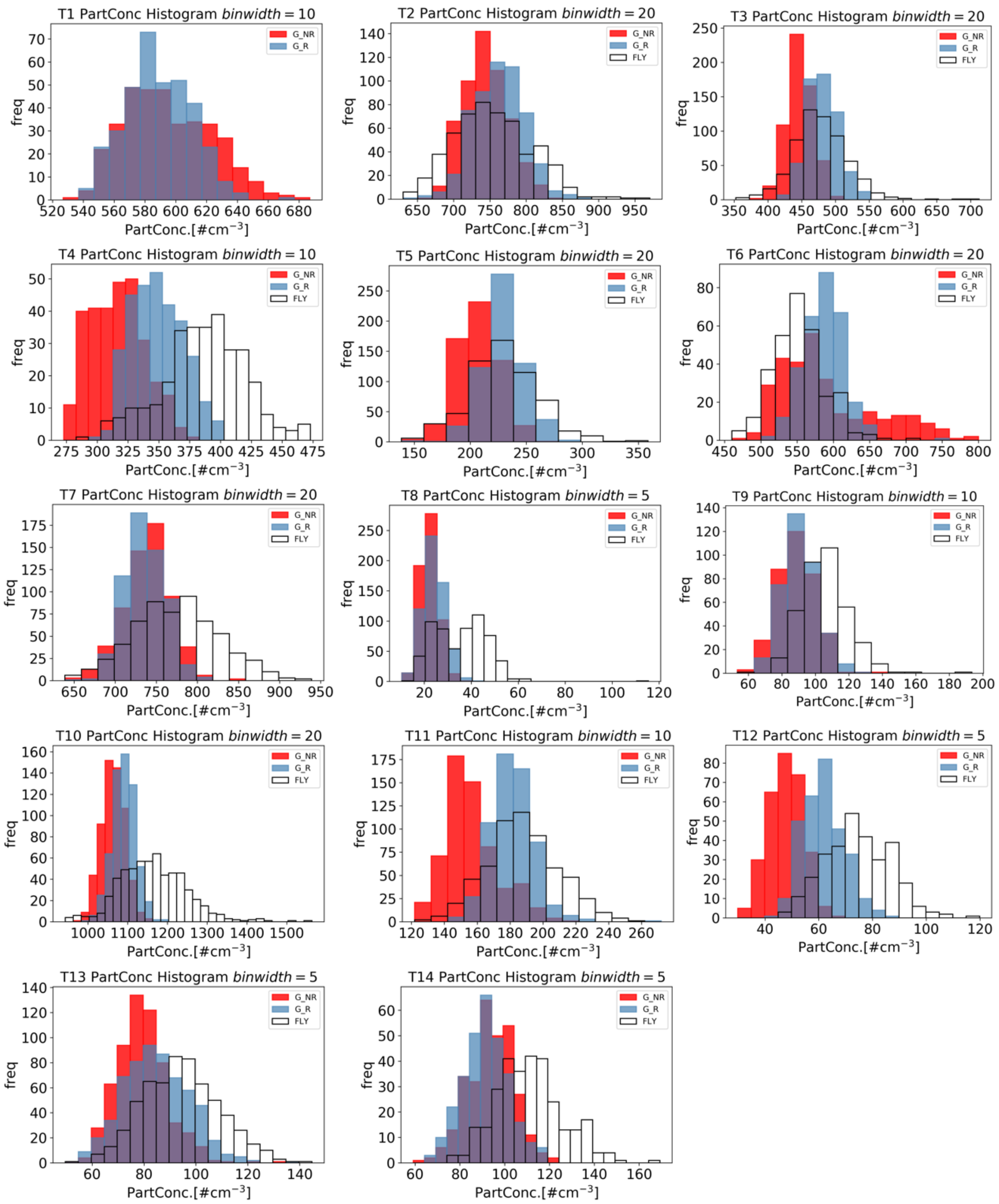

Figure 4. Probability density functions (PDFs) of PNCs in each case. A constant bin width is utilized

across the G_NR, G_R, and FLY PDFs of each flight.