Response to reviewer #1

1. The instrument uses 658 nm laser light with an open-path configuration. Isn't there any influence of stray sunlight on the measurements? If not, is there any special measure for the daytime use?

*I have added a section on stray light which describes a two fold approach to minimising and correcting for stray light*

2. The authors wrote at page 9 that Time-of-Flight (ToF) data is used to "reject" signals that may come from "a large body or agglomeration of particles." Does this mean that such particle signals are not on the record? There is alternative approach in that such signals (including ToF data) are also recorded, processed, but may be removed in the data analysis phase. Is there any reason to reject those at the onboard circuit? I ask this question because this treatment may miss cloud signals if particles are greater than 40 µm. For example, in Figure 14, at 5 km, there is another ~100 % relative humidity layer. It is not clear whether there was no cloud or there were clouds with particles much greater than 40 µm.

*In section ELECTRONICS, I have clarified how particles above/below the measurable size cannot be counted, and therefore how the choice of ToF rejection criteria is justified. i.e. if a 50um particle causes the detector to saturate, and wont be counted, then realistically, we don't need to extend the time-of-flight criteria to include very large particles as they cannot be counted anyway.*

3. The description of the assembly, optical set-up, and sensing area is sometimes not easy to follow. I understand this because the instrument is three dimensional, but I have some ideas to improve this. I suggest the authors to (1) define the common x, y, and z axes for Figures 1 through 4, (2) show the axes explicitly in each of Figures 1 through 4, (3) avoid terminologies such as "width/depth", "left/right", "above/below" etc., but use the axis to specify the direction. Furthermore, please consider to use a common set of identifiers e.g., (a), (b), … in these four figures; for example, in Figure 2, (a)⍰(b1), (b)⍰(b2), (c)⍰(b3), (d)⍰(b4) (and keep Figure 1 as it is), and use e.g., (i), (ii), … instead of (a), (b), … in Figures 3 and 4. The mirror schematic in Figure 2 is very different from those in Figures 3 and 4, which made me confused at first. The

laser light schematic in red in Figures 2, 3, and 4 may be improved by making them more consistent across these figures, and add an explanation in caption Figure 5 about how the "major axis" and "minor axis" correspond to the laser light schematic in Figures 2 through 4. Finally, please add the dimension information to Figures 1 through 3 as much as possible, so that it becomes easier to read the text by referring to these figures.

*Figures have been updated with your suggestions*

Section 1 Introduction: - I think MODIS and MISR are not lidars. –

*I will amend this in the revised manuscript*

*Changed to 'instruments' rather than lidar*

There have been several particle instruments for balloon sounding. Some examples can be found in the Introduction of Fujiwara et al. (2016). Other instruments include LOAC (Renard et al., 2016) and POPS (Gao et al., 2016). - I think that an OPC for dropsonde may be new. I assume that the dimension, shape, and configuration of the UCASS was determined so that it can be mounted in the dropsonde launcher. If so, please explicitly write the conditions under which an instrument can be used as a dropsonde. Also, the strength of the dropsonde over the balloon sonde system may be discussed (e.g., the former can be more easily targeted to a specific airmass including a specific cloud system). - Because the examples of aerosol particle measurements shown in this paper are actually only for Saharan mineral dust, the role of mineral dust particles on the climate may be explicitly discussed. - The mass of the instrument 280 g should be explicitly written here (near the term "lightweight"; and also in Section 2).

*I have added a brief description of other balloon based instruments at the end of the introduction section*

Section 2 Instrument Design, subsections 2.1 through 2.2: - See the major comments. - Why is the sensing area ("0.5 mm2") an area, not a volume? - It is helpful for the readers to summarize (e.g., to prepare a summary table for) the particle signal data (i.e., I1, I2, ToF, pulse height, etc.) and their relation with the criteria for data quality assurance (i.e., particle path inside/outside the sensing area, agglomeration of particles, etc.) Please also add the information how the numbers 0.4, 17, 1, and 40 µm for the particle size limits were actually determined. - Figures 3 and 4 indicate that the detector only collect light reflected on the mirror. There is no contribution from the directly scattered light (i.e., around 120 deg.)?

*You are correct in that any particle passing within a particular volume will cause a pulse on the detector. However, we discuss a sensing area, as the airflow is perpendicular to the laser beam. Therefore, any particle passing through a particular AREA will travel through the depth of the beam. It is the area presented orthogonal to the airflow that dictates the sampling rate. The depth of the laser does not impact the sampling rate, it only affects the time-of-flight. I have tried to clarify this further in the manuscript, perhaps the updated figures will help to illustrate it*

Section 2.3 Electronics: Perhaps, the explanation of the "gain" i.e., "high gain version" and "low gain version" is first described here at page 8. In the current manuscript, the gain is mentioned first in the Calibration section at page 13, and it is not very clear what the "gain" actually is.

*A paragraph is added (2$^{nd}$ paragraph in electronics) to describe the gain and how.*

Section 2.4.2 Calibration Measurements - Have you used all the calibration particles for each of the two different gain versions? In Figure 8, the set of particles shown is different between the two. For example, what is the response of the high-gain version to soda-lime 37.36 µm particles? - What were the ToF values for these experiments? How about the frequency distribution? Were the ToF values simply used onboard for removing agglomeration cases?

*The high gain version has a smaller upper size limit. In each panel, the x-axis represents the full 4096 bins available. On the top panel, you can see that the measured size distribution for the 14.4micron calibration beads is cut off at the larger sizes, and the full distribution is not captured. Any pulses above 4095 cannot be counted. Therefore, the additional particles used for the low-gain calibration would not be seen at all by the high-gain version as they are off the scale. The ToF data during calibration is used in the same way that Tof data is used in experiments, where only overly low or high ToF values are rejected, corresponding to particle sizes below/above the measurable size range are rejected. I will clarify this in the revision*

*I have added some explanation about the upper limits of the measurement, i.e. why the distributions are cut off at the top end because the particles are beyond the measurable size range. This should explain why the very large particles cannot be used to calibrate the high-gain version as they would not be measured at all.*

Section 2.5 Air flow Management: - It looks strange that the air flow inside the instrument can be greater than the background air flow because the drag on the inside wall would reduce the air flow speed inside. Do you have some actual measurements showing this (see e.g., Appendix B of Fujiwara et al., 2016)? - It is not clear why a double pendulum system would "inhibit the movement of UCASS". Is that due to a rather heavy radiosonde located below that acts as a drag? A double pendulum system might even give chaotic motions (depending on the mass of the second object and the air drag). Furthermore, in general, the payload may also move along a circle or ellipse in the horizontal plane. - Related to the above two comments, in Section 1 or 2.1, an explanation is necessary why the instrument shape has been designed like this, i.e., not symmetric along the air flow; this would give additional complication here. - In the end, all these factors go to the uncertainty of the measurements. The term "Management" in the section title may not be appropriate; more appropriate would be something like, Evaluation of the measurement uncertainty due to air flow uncertainty?

*I have added some additional description explaining why the air flow is higher, why the UCASS is asymmetric and also how the double pendulum, although chaotic, reduces the amplitude of oscillation.*

Section 3.1.1 Dropsonde system - The date information for the sounding in Fig. 14 is necessary. (See also the major comment on this sounding.) - What about the results from the other 5 soundings?

*Added the date. Not all data is shown for brevity, the selection of in-situ results are intended to illustrate potential uses for the UCASS and so instances are selected where comparative data exists. Not all dropsondes were launched during dust events, or at times with AERONET scans and therefore the data is of little interest to the reader. Wording has changed to focus on the 2 examples shown.*

Section 3.1.2 Upsonde system - Description on the PCASP onboard the research aircraft is necessary.

*Added in section 3.1.2*

*RESPONSE TO REVIEWER #2*

Major comments: 1) Calibration process is incomplete. The described calibrations using particles of known diameters can only determine the sensitivities of the instrument, but not counting efficiencies (as a function of particle diameter). This is especially important for small particles (near the lower detection limit of 0.4 µm). Note that the inter-comparison results shown in Figs 16 and 17 are not sufficient to validate the UCASS. This is because the particle mass concentration or liquid water content is only sensitive to large particles. The authors need to demonstrate the counting efficiency by comparing the UCASS to a proven OPC or CN instrument.

*A section 'counting comparisons' has been added, comparing UCASS concentrations to a TSI 3330*

2) It is a bit disturbing to see the wide (up to 1 order of magnitude) and inconsistent (between the low- and high-gain channels and between various sizes – note especially peaks of 0.753 and 3 µm PSLs and 11.58 µm soda lime) spreads of the instrument responses to PSL and other particles (Fig. 8). The authors seem to attribute it to calibration particles. But it is hard to believe the PSLs have such large spreads. If it is due to the real PSL spread, the authors ought to be able to reduce the spreads by using a DMA (at least for particles < 1 µm) and redo the calibration. If not due to the calibration material problem, then the authors need to provide an explanation.

*I have clarified in the manuscript that these graphs show instrument response and that this is not linearly proporational to size. A section has been added 'sizing comparisons' to better illustrate the sizing capabilities*

3) If the large spreads shown in Fig. 8 are due to an instrument problem (such as the imperfection of sensing area definition/particle rejection as described in Fig. 4), then the size resolution of the instrument is not great. Detailed analysis is needed to show the true size resolution.

*This has been addressed in the above comment*

4) The description of the optical assembly is very difficult to understand. A better Fig. 2 should help.

*Description and diagrams have been updated*

Minor comments:

1) Fig. 1 is not well done. Appears to be hand drawn?

*Figure has been redone*

2) Fig. 16 needs to be improved. C2

*Figure has been redone*

3) It is hard to get a clear understanding of the electronics design. A circuit diagram should help (such as Fig 4. In Hill et al., J. of Atmos. And Ocean Tech., 2008).

A simplified box diagram has been added

*4)* A quick search for Alphasense mirror and First Sensor detector didn't yield any useful results. Please add web links or state that they special orders.
*Added 'available through special order'*

*5)* "f(x)" is not defined in Fig. 8.

*This is now defined*

RESPONSE TO REVIEWER #3

Major comments

Sections 3.1 and 3.2 show intercomparisons with a PCASP and a CDP respectively. The subject of this paper is an instrument which counts and sizes airborne particles, so it is confusing that the comparisons with other counting and sizing instruments is accomplished using data which has been converted to mass per unit volume. Especially given the motivations outlined in section 1 to improve understanding of aerosol radiative direct and indirect effects which are in large part controlled by particle number and surface area rather than mass. It can be difficult to compare three dimensional plots of size distribution as a function of time or height. However, it would be useful to see at least either a time series of particle number alongside the mass time series, or an averaged size distribution for UCASS and PCASP / UCASS and CDP in sections 3.1 and 3.2. This would improve confidence in the sample volume calculation outlined in section 2.5 as well as in the sizing accuracy of UCASS.

*More lab comparisons have been added to show the counting and sizing abilities of the UCASS, and therefore this section has been left as it is as it makes use of available data and illustrates the potential uses in the field.*

Minor comments

Section 2.1

Figure 2 uses a different but similar looking labeling system to figure 1. It might be worth numbering the parts in one of these diagrams, although this change is not essential. Figure 2 also appears to be less well drawn than the other figures in section 2. It might be worth tidying it up.

*Figures in this section have been redone to follow a universal coordinate system and set of identifiers*

Section 2.2

This section should possibly have some reference to dealing with coincidence errors or at least an estimate of the number concentration at which coincidence errors are likely to become significant.

*This is limited by the speed of the electronics, which become problematic before coincidence errors are expected, this is clarified in the 'electronics' section.*

Section 2.3

Line 3 on page 9 and subsequent parts of the paper contain references to 4095 bins of amplitude displacement. This is initially a bit confusing because in this context the output of a voltage converter as described is (very) often referred to as "Analogue to Digital Counts" or AD counts. P9 L8 - It would be interesting to know why such a large range of particle time of flight is accepted. P12 L10 Typo: none-turbulent should be non-turbulent. See also P14 L4 (none linearity), P19 L12 (none cloudy) and others.

*Nones changed to non, AD corrected*

Section 2.4.2

Presumably the sheath flow was added in order to accommodate the large volume of air flowing through the instrument. It would be useful to state this. It would also be interesting to know the length of the dryer column. A flow velocity of 5 m/s might not provide sufficient time to dry a flow containing PSLs using most conventional dryers. On page 13 line 6 the authors discuss the use of PbP data to eliminate bin width related artefacts. They appear to be writing about exactly the same measures they describe in section 2.3 (page 9 line 3), but using completely different terminology. This is confusing. PbP pulse height recording is a more widely understood terminology than that used in section 2.3 so it would be useful to standardise to this. Figure 8 has f(x) as the Y axis label. This is normalised counts, but is not defined in the text or the figure legend. Figure 9 on page 15 shows an additional step in the probe calibration relating scattering cross section to instrument response. It is more usual to see the calibration mode diameters plotted on top of the Mie curves as presented in figure 10. It would be useful to see the calibration added to figure 10 as well as (or even instead of) figure 9.

*I have stated the reason for the sheath flow*

*Wet samples are passed through a drying chamber first, have changed this in the text to state that this drying column AIDS the drying, therefore suitable for dry dispersion, or almost dry wet samples. Section 2.3 changed to used PbP terminology. F(x) defined in caption. Data points added to calibration curves as suggested*

Section 2.5

More description about how the angles of oscillation were calculated would be interesting. Also, on line 2 of page 18 the authors give an airspeed of 5.4 +- 0.3 m/s. Reading the values for +-5 degrees from figure 11 seems to show a range of around 4.5 to 5.6 m/s. The authors should show how the former figure was arrived at.

*Changed this to mean and SD.*

Section 3.1.2

The explanation of the differences between the UCASS and PCASP measurements sounds a little speculative. It raises a question about why these data are being used for an intercomparison if their imperfect colocation means they are not comparable. The agreement between the probes seems OK, so this could be left out major comment above being addressed). P22 - Figure 16 appears to be at insufficiently high resolution or has been comressed using an excessively "lossy" method. Can this be re-plotted?

*The use of field data is really intended to highlight the various uses of the UCASS and show (albeit speculatively) how it can be used to complement campaign datasets and potentially be used as an alternative where flights may not be possible. It is not intended as controlled proof of the instrument performance. Due to the additional laboratory tests included in this version of the manuscript, this section has been left as it is*

Section 3.2

Change "figure ??" to figure 17 on the first line of page 23. The discussion on page 23 of the time of flight rejection causing under counting contains a mistake. The short time of flight of fast moving particles is rejected on the basis that it looks like short duration electronic noise, not on the basis that it looks like a large aggregate particle. At least accordin to the reasoning in section 2.3 (page 9).

*Typo corrected*

*ToF corrected*

Section 4

Line 8 of page 25 mentions the use of an 8+ point sizing calibration. Was this type of calibration applied to all probes contributing data in section 3, or was this done once as an instrument characterisation exercise?

*I have added that legacy measurements were done using a rougher calibration prior to the availability of borosilicate standards.*