We thank Dr. Rosenberg for his recommendations and for his reflection on Rosenberg et al. (2012) (R2012) and its relation to our AMTD manuscript (C2013).

Before responding to Dr. Rosenberg's comments we feel that it is important to overview what C2013 accomplishes, and what it does not. Our size-calibration method derives an offset for each of the probe's three gain stages. This represents a relatively simple first-order correction for particle sizing performed by the PCASP. Motivating this is the requirement that we deploy aircraft instrumentation whose laboratory-response characteristics are documented, that we provide a correction to the manufacturer's calibration, when needed, and that we provide a history for each deployment. Our method is simpler than that described in R2012, but it represents what is feasible given constraints on the time that we can dedicate to this particular airborne measurement system. We acknowledge that our PCASP size-calibration is not comprehensive as Dr. Rosenberg's.

Further, we note a misunderstanding. On P4130-L25 we stated that the number of particles counted, during a 300 s testing interval, was between 10^4 and 10^5. Dr. Rosenberg interpreted this as a range of particle concentration, i.e., 10^4 to 10^5 cm^{-3}. In fact, in our testing, concentration values never exceeded 500 cm^{-3}. The concentration variation (test-to-test) is stated in C2013 (P4127-L22) and representative concentration values are provided in the Figures 2a-2b (P4129). We are certain that particle coincidence is not a source of ambiguity for our investigations.

> 1) The methods described here differ greatly from the methods discussed in R2012. In R2012 the instrument is calibrated in terms of the particle scattering cross section, because the instrument responds linearly to this property. Here the instrument is calibrated in terms of particle diameter. There are two potential issues here
>
> a. The instrument response to diameter is nonlinear and for particles larger that 1 µm is non-monotonic, see Fig C1.
>
> b. The authors use 1 calibration point to apply an offset to the manufactures specification.
>
> As can be seen in Fig. C1 if the particles used happen to fall on a spike or trough on the curve they cans significantly bias the results. In this case the calibration particles used are all of diameters less than 1 micron, where the response is not so spiky. From 0.3 – 1.0 µm one cold even consider it close to linear. If this is by design then the authors should report so and comment on the uncertainty based on extrapolation. It should be noted that PSLs are available over the whole range of the instrument even if the SMPS range is limited.
>
> Even if the response to diameter was linear it is not clear that an offset is the appropriate correction. There are potential sources of offset in the instrument, however there are sources of sensitivity variation as well. For example, the location of the sample in the laser beam and dirtying of the optics would change the instrument response by a common multiplicative factor. Given the points a and b above it would be good to see at least two data points per gain stage for calibration regions less than 1 micron – the minimum for a linear fit – and preferably more. This would at least allow an offset and a sensitivity correction to be derived. It would also be good to see a comparison with the R2012 methods to see if the results differ significantly.

As we show in Figure 3 (C2013) the diameter-threshold relationship (response) is non-linear for all three gain stages. In this comment, Dr. Rosenberg recommends at least two PSL sizes for each gain stage, and determination of the slope and intercept of the threshold-crossection response function. We note that this would be possible if we had adopted his approach of translating diameter to crossection – and thus linearizing the response. We did not do that.

However, using one additional PSL size, we did validate our method. We found that the PCASP-derived size differed by no more than 0.01 µm (PCASP-1), and negligibly (PCASP-2), from the

mobility-selected PSL test particle diameter (0.152 µm). These departures were evaluated as the difference between the mode of the size distribution (PCASP) and 0.152 µm. Results for PCASP-1 and PCASP-2 are provided in the two tables attached to this document. For two of the PCASP-1 tests, and for all of the PCASP-2 tests, the difference is less than 0.005 µm. Consistent with our paper, we round this difference to 0.00 µm. A non-zero difference is evident for the first nine PCASP-1 tests (Difference = -0.01 µm), but even this is equivalent to only a one channel offset. These results were not reported in C2013.

2) If the diameters of the bin boundaries increase we may expect to see a change in the bin widths also due to the nonlinear instrument response. The authors do not attempt to calibrate bin widths and should comment on the uncertainties expected.

As we stated in our introductory comments, our PCASP size calibration is a first-order correction of the manufacturer's threshold-diameter table. In general, our calibration will not alter the manufacturer's bin width. A ramification may be that a PCASP calibrated by our method will have bin widths which are not as accurately determined as those by Dr. Rosenberg's method. This is a good point, one that can be tested, but in our opinion, such testing is beyond the scope of C2013.

3) As mentioned in the manuscript, custom threshold tables can be provided to the PCASP. This was used in R2012 to zoom in on the region of interest during a calibration and could be utilised here where the accuracy is currently impacted by the standard resolution.

We endeavored to develop relatively simple calibration procedures, so we feel this recommendation would take us too far away from the objectives of C2013.

4) The concentrations reported for use during calibration seem rather high (10^4-10^5 cm^{-3}) At these concentrations we may expect coincidence to occur in the PCASP (i.e. two separate particles passing through the sample volume of the laser at the same time, distinct from an aggregate). Such coincidences may be responsible for some of the long tails observed which were not seen in data in R2012, see Fig C2. The PCASP reports the transit time of the particles through the laser beam meaning it should be relatively trivial for the authors to calculate the fraction of time during which particles are in the laser beam and hence the probability of coincidence.

We address coincidence in our introductory comments.

5a) The report of R2012 on p4131 line 25 onwards of the manuscript is not quite correct. We found a result that seemed consistent with particles not transferring from one gain stage to the next when it appeared that they should. We did not measure pulse heights so it is not clear if the pulse heights were lower than expected or if there existed a fault or design flaw in the PCASP hardware or firmware that caused the problem.

The term "pulse height" is used throughout R2012, but it was not used in the section of that paper relevant to this comment. Accordingly, we acknowledge that "pulse height" should not be used in our explanation of this aspect of R2012. Here is our revision:

"Rosenberg et al. (2012) also report on sizing calibrations of a SPP200-modified PCASP. When doing these calibrations at a particle diameter that was large enough to register in the lowest channel of an

adjacent smaller-gain stage, they noted that most of the counting events did not conform to their expectation. They commented that an "undocumented process" was preventing the expected counting into the smaller-gain portion of the histogram. The net result was that the width of the last channel of the larger-gain stage was broadened and that the width of the first channel of the smaller-gain stage was narrowed. We note that this narrowing is consistent with the diameter overlap we document for the high- to mid-gain transition. Rosenberg et al. proposed two workarounds for the ambiguity associated with narrowing (overlap): 1) merging the two channels (e.g., #4 and #5), to produce a size distribution with one less channel, or 2) setting the upper-limit diameter of the last channel of the larger-gain stage equal to the lower-limit of the first channel of the smaller-gain stage."

5b) Regarding the solution provided on p4132 line 9 onwards, we feel that this is a sensible approach and the authors should highlight that this creates a single bin which spans the gain boundary (it is not currently immediately obvious). It is somewhat analogous to the merging method of R2012, but with the advantage of maintaining the 30 bin resolution.

We agree that this is a reasonable approach; however, we do not agree with Dr. Rosenberg's conclusion that the NCAR's approach creates a channel that spans the gain boundary. Below is the NCAR table we obtained from Allen Schanot, June 12, 2009; it is evident that the upper-limit of channel #4 is at the top-end of the high-gain stage (threshold=4096) and that, by definition, the lower-limit of channel #5 is also at threshold=4096. There is not a channel that spans the gain boundary.

692, 1040, 1517, 2157, 4096, 4231, 4348, 4537, 4825, 5251, 5859, 6703, 8192, 8345, 8502, 8682, 8872, 9070, 9252, 9432, 9544, 9737, 9937, 10166, 10471, 10797, 11162, 11499, 11852, 12288

6) Regarding the baseline reference voltage, it is not clear to the reader whether this changes the minimum limit of the gain stage, the sensitivity of the gain stage or causes a constant offset to all bins of the gain stage. A figure showing a pulse or a series of pulses (either schematic or based on oscilloscope measurements from the instrument test points) would be useful to explain this with the baseline voltage and the equivalent thresholds marked on the y axis and would facilitate advances beyond the reporting by R2012. It is still not clear to me how exactly the baselines interact, nor does it seem that the manufacturer, DMT, is able to provide much insight. It seems that this is something that will only be established through some detailed experimental work.

Based on experimental work and conversations with DMT – both provided in Section 3.5 (C2013) - we have concluded that a decrease of the baseline voltage makes the apparent analog pulse amplitude smaller, and vice versa.

7) Again when discussing Fig. 5 of the manuscript on p 4134 it would be good to know the concentrations recorded from the two measurements to understand coincidence effects as such broad tails are not seen in Fig. C2 reproduced from R2012. These differences should be explained and if they do not arise from coincidence then they could be due to the different model and operation of nebulisers, i.e. if a nebuliser produces larger droplets then it will give a higher number of aggregates.

Please see our introductory comment about coincidence; we are confident the broad tails referred to here are not attributable to coincidence. Going further, we note that it is difficult to compare Fig. C2 and Fig. 5 (C2013). There are two reasons for this. First, in Fig. C2 the distribution is plotted versus pulse height (threshold), while the Fig. 5 distribution is plotted versus particle size. Second, Dr.

Rosenberg's reprogramming of the PCASP requires an unstated factor to transform threshold to particle diameter. It is our opinion that a much more relevant comparison is the one we evaluate between the EC-produced and PG-100 distributions (Fig. 5, C2013). On P4134 we elaborate on why the PG-100-produced distribution is broader.

8) What type of diameter equivalence is reported in Sect 3.7? It appears to be volume equivalence but it isn't stated explicitly. If there are coincidence effects in the data as well as aggregate effects do these impact the conclusions?

In the first two sentences of Section 3.7 we stated a common definition of the sphere-equivalent diameter, and also stated that we employ that definition in our analysis of aggregates. We encourage Dr. Rosenberg to elaborate why our definition is inadequate. Also, please see our prior comments about coincidence.

9) When investigating the scattering from aggregates the authors may again benefit from zooming in on the regions of interest to improve resolution. This may be particularly interesting for the three-particle aggregates where the authors discuss differences between linear and compact configurations.

This is a good suggestion. However, in our opinion, it is beyond the scope of C2013.

10) There is no mention of uncertainties in the manuscript. This is important, because unless uncertainties are provided there is no indication of the value of the calibration. Given the extrapolation based on one point per gain stage it is particularly important that the effect of this extrapolation upon uncertainty is assessed.

In our opinion, the first sentence of this comment is too categorical. We did evaluate concentration uncertainty and uncertainty due to PCASP sizing resolution. On the latter point, we do concede that R2012's reprogramming of the PCASPs diameter-threshold table is useful for resolving fine-scale features of the size distribution function. We also agree that future work should exploit that capability of the PCASP.

In our response to comment #1, we presented a validation of our PCASP sizing calibration. The result is encouraging. We also acknowledge that verification with additional PSL test particle sizes would be useful.

In his summary, Dr. Rosenberg goes back to his earlier point about the PCASP's non-monotonic response to supermicron particles. We concur with his cautionary statement about our use of one test particle size, at D=0.491 μm, to set the diameter-threshold relation of supermicron particles. This point should have been stressed in C2013. Still, it is important to keep a few things in perspective. The first is that Dr. Rosenberg's Fig. C2 extends well beyond the typical upper-limit of a PCASP. Second, the ambient concentration of supermicron particles is relatively small and this can add considerable error to the PCASP's estimate of the size distribution function independent of error that results due to non-monotonic response. Third, most of the ambient particles detected by a PCASP have diameter smaller than 0.3 μm.

PCASP -1

Date, mo/dy/yr	Dia. Shift High Gain, µm	Dia. Shift Mid Gain, µm	Dia. Shift Low Gain, µm	PSL, µm	PCASP, µm	Difference, µm	Channel with max spectral density
02/08/10	0.02	0.01	0.04	0.152	0.16	-0.01	5
02/08/10	0.02	0.01	0.04	0.152	0.16	-0.01	5
02/08/10	0.02	0.01	0.04	0.152	0.16	-0.01	5
04/08/10	0.02	0.01	0.04	0.152	0.16	-0.01	5
04/08/10	0.02	0.01	0.04	0.152	0.16	-0.01	5
04/20/10	0.02	0.01	0.04	0.152	0.16	-0.01	5
04/20/10	0.02	0.01	0.04	0.152	0.16	-0.01	5
07/23/10	0.02	0.01	0.04	0.152	0.16	-0.01	5
07/23/10	0.02	0.01	0.04	0.152	0.16	-0.01	5
02/02/11	0.00	-0.01	-0.06	0.152	0.15	0.00	6
02/02/11	0.00	-0.01	-0.06	0.152	0.15	0.00	6

PCASP-2

Date, mo/dy/yr	Dia. Shift High Gain, µm	Dia. Shift Mid Gain, µm	Dia. Shift Low Gain, µm	PSL, µm	PCASP, µm	Difference, µm	Channel with max spectral density
02/08/10	0.00	-0.01	0.04	0.152	0.15	0.00	6
02/08/10	0.00	-0.01	0.04	0.152	0.15	0.00	6
02/08/10	0.00	-0.01	0.04	0.152	0.15	0.00	6
07/23/10	0.00	-0.01	0.04	0.152	0.15	0.00	6
07/23/10	0.00	-0.01	0.04	0.152	0.15	0.00	6
02/02/11	0.00	-0.01	0.04	0.152	0.15	0.00	6
02/02/11	0.00	-0.01	0.04	0.152	0.15	0.00	6
12/06/11	0.00	-0.01	0.14	0.152	0.15	0.00	6
12/06/11	0.00	-0.01	0.14	0.152	0.15	0.00	6
11/16/12	0.00	-0.01	0.14	0.152	0.15	0.00	6
11/16/12	0.00	-0.01	0.14	0.152	0.15	0.00	6