

## ***Interactive comment on “Experimental characterization of the COndensation PArticle counting System for high altitude aircraft-borne application” by R. Weigel et al.***

**Anonymous Referee #1**

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This paper describes the instrument package used on the M-55 Geophysica in the upper troposphere/lower stratosphere (UTLS) to provide size and volatility dependent number concentration for particles less than a few hundred nanometers in diameter. This requires the use of 4 nearly independent condensation particle counters (CPCs). The characteristics of each of these CPCs along with the sampling efficiency of the aerosol inlet and the volatility efficiency of the heated inlet are presented. While the paper has improved somewhat from the version I reviewed prior to it being posted as an AMTd submission, The paper is still too long, too tutorial, and too simple in its expectations the knowledge of a reader. This makes for a tiresome and confusing read.

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The paper is highly detailed with specific angles, lengths, residence times, . . . which is fine when they are important, but in many cases they are not important. The particular characteristics may only apply to particles larger than what are sampled, or the necessary particle size information is not available to use the detail presented. Such detail may be useful in a thesis for later specific reference at an institution, but is unnecessary for a scientific paper, where a great deal of prior knowledge is anticipated and a great deal less space and time for reading are available. Unnecessary detail dulls a readers interest and leaves one wondering if the authors understand what about their study is really useful.

I recommend the authors rework this paper again to further cull unnecessary material and that the paper is not accepted until this is complete. I detail below many areas where the paper would benefit from culling. Some of this is a repeat from my quick review, making me question the usefulness of such reviews.

I bracket wording suggestions with ellipses (. . .) to indicate text in the present manuscript. I use - - to indicate quotation marks since that symbol cannot be translated by the AMT web server.

Material which needs culling, consolidating, clarifying:

322.18-22 is quite awkward and provides no useful information. An abstract should summarize the results of a piece of work not that something was done or studied. What is the difference between inter-comparison and cross correlation and which is it and what is the result? What is the difference between measurement flights and mission flights and just flights, and what was found? The last paragraph of the abstract could be removed altogether.

Why throughout the paper is it the - - M-55 - -Geophysica- - - and not just the M-55, like the ER-2 and why the quotes? I think the aircraft needs to be fully specified just once and then the call number should be sufficient.

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324.25 - 327.5 This material needs to be moved and consolidated with the appropriate section, e.g. instrument description, laboratory tests, field measurements. It is too detailed and unnecessary for the introduction. In the introduction a short paragraph simply indicating that the COPAS involves 4 CPCs which can be operated between \_ and \_ km, that laboratory tests established the size thresholds for each CPC, that sampling efficiency is analyzed, and some limited field measurements are analyzed. Leave the details for the specific sections. A preview of the detail is not necessary here.

329.7-13 The COPAS CPCs - intended . . . 21 km- are mounted externally and thus subject to extreme ambient conditions such as pressures as low as 50 hPa and temperatures from +50°C (on the runway) to -90°C . . . 20 min. Important properties . . . are: . . .

330.4-6 Is the detail in the parenthetical expression important for this paper? If so how? Readers will believe that the aircraft experienced temperatures as low as -70°C

332.10-17: This paragraph can be summarized in a couple of sentences. The aerosol inlet was aligned to be isoaxial for the aircrafts mean angle of attack of 7°. Discrepancies of +- 1.5° around this are not significant for aerosol < 1000 nm. Then it might be mentioned that an angle of attach greater than 7° occurs as the aircraft climbs to near 7 km, thus measurements below this point suffer from anisoaxial sampling.

332.18-333.13 and Figure 4: This discussion and figure have no relevance to sampling of COPAS as soon as it is pointed out that both the probe head entrance and the probe inlet are sub isokinetic and therefore create a negligible sampling bias for particles < 500 nm, which are the particles which dominate the COPAS measurements. I also do not believe a run of Fluent is required to establish the sub isokinetic ratios. Are not flow rates and nozzle sizes sufficient for this?

Taken together the information in section 3.1 essential to this paper can by covered in a couple of short paragraphs.

Section 3.2 can also be shortened to just what is essential, which is basically that the results of Hermann et al., are used for the COPAS aerosol inlet since wind tunnel tests were not performed for the COPAS inlet. It is a little surprising that the 180° difference in orientation of the probe causes no impact on transmission for the Hermann et al. system.

Section 3.3, Tables 1 and 2: Here again essential information to inform the reader about what will be used is delayed until some rather useless (to this paper) calculations are completed (Table 1). Only after these are completed, covering the size range 6 - 100 nm for both the unheated and heated line, do the authors point out that the COPAS does not provide size information above 15 nm for the unheated inlets, and there is no size information for aerosol sampled from the heated line. Thus all calculations for sizes  $> 15$  nm cannot be used for the regular line, and all calculations for the heated line cannot be used. Why then must the reader suffer through a detailed description of the physical variations of the heated line and why is Table 1 included? This paper is not a proper venue for an academic exercise. The limitations on size information must be brought to the beginning of this section and only inlet details and calculations which are actually used presented, thus eliminating Table 1 and properly referring to Table 2. In the present manuscript Table 3 is not referred to.

337.9: - -The curves . . . - - This is a fancy, and totally unnecessary, way of saying the points are connected by lines, which is obvious from the figure and does not need to be stated.

Section 5: Here again a great deal of academic information is provided and an equation solved before it is pointed out that for the particle concentrations expected  $N < 1e4$  cm<sup>-3</sup>, that coincidence is negligible. In fact for a concentration of  $1e4$  cm<sup>-3</sup> with  $c=1e-5$  cm<sup>3</sup> the coincidence is 10% falling to 1% for  $N=1e3$  cm<sup>-3</sup> using Eq 1. Note  $c$  that must have units of cm<sup>3</sup> not cm<sup>-3</sup>. Thus all of the text 338.25-339.11 and the references to solving the equation iteratively is simply not necessary for the particle concentrations expected in the UTLs. See Figures 7-10 to make the point clearly.

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339.12-20: This tutorial on homogeneous nucleation of the working fluid is not necessary, and I have never heard this referred to as auto-nucleation. Readers of this paper will be aware that homogeneous nucleation can occur in any fluid if the supersaturation is high enough. I believe this is covered in first year of graduate study. Start this paragraph with line 21 stating that, - -To check for homogeneous nucleation of the working fluid the temperature difference . . . to end of paragraph.

340.2-3: This first sentence is redundant to 340.23-25, where it is more appropriate. Begin the paragraph with the second sentence which is a good introduction to this paragraph.

340.5-10: I believe it can be assumed that care is taken to avoid contamination in laboratory experiments and does not need to be stated with a for instance. Just describe the practices that were used, starting with, - -Prior to . . . - -

340.28: Phrases such as, - -It can be concluded that- - are unnecessary and distracting. The simple statement of what happens at an operational temperature of 250°C is clear enough.

342.3-4: The vertical coordinate is obvious and only needs to be mentioned in the figure caption.

342.19-343.16: The text could be well served by using two paragraphs one for the concentration measurements and one for the volatility measurements. The authors should remove text which just describes the plots, that is what the figure is for. What aspects of the profiles are important and why?

343.17-344.15: This material is out of place here. It needs to be included in the instrument description when each of these specific aspects of the COPAS are discussed earlier: the flow controller and angle of attack, the impact of high surface temperatures on the cooling oil and characteristics of the working fluid. As I recall profiles of the temperature of the cooling oil were given earlier. No need to repeat here, Here this

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information should be mentioned only briefly to remind the reader of the earlier discussion on these limitations. The last paragraph should be included in the paragraph on the volatility measurements.

Scientific faults, errors, questions:

323.23-24 This is a rather odd reference list for heterogeneous processes ignoring the early work and focusing almost exclusively on PSC formation. Borrmann et al. is good but I would expect a more diverse list to be included here.

323.29-324.1 None of the references here discuss the influence of Pinatubo on climate. The last three document the increase in stratospheric aerosol loading while the first two discuss the impact of that aerosol on stratospheric chemistry, but none mention climate. Either change the sentence or the reference list.

324.9-11 The Ansmann reference is confusing in this context. It was published prior to the generally accepted point of background after Pinatubo (after 1997) and the subject is stratospheric ozone loss by volcanic aerosol. In the next line e.g. seems out of place when 5 references are listed, and no reader is going to know what is meant by etc. You might also replace Deshler et al., 2003 with Deshler et al., JGR, 2006, which is more appropriate to this subject.

327.14-16 Here is another odd list of references (nine, but started with e.g.? Do the authors know what e.g. means?) all related to expansion type CPCs, whereas the subject of this paper is a continuous flow CPC. Why are there no references to previous work with continuous flow CN counters given, e.g. Wilson, Rosen, McMurray, . . .

Both Tables 1 and 2 present particle losses (in %) inside the aerosol tubes as a function of pressure for the regular channels, but they do not agree with each other for the same size particle and pressure. Why not?

Table 2. How is KL calculated?

335.27-336.3: This is awkward English. dp50 is not the - -smallest detectable particle,

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or cut-off, or threshold diameter.- - It is well defined as the point where  $N_{det}/N_{real} = 0.5$ . Make a clean definition for  $dp_{50}$ , leave it at that, and use it eliminating the use of cut-off, threshold here and elsewhere, which are misleading. Eliminate the text - - can be determined in dependency . . . end of paragraph.- - If necessary say how  $dp_{50}$  is determined, although I think it is pretty clear from the definition. Then mention with a new sentence that  $dp_{50}$  depends on the supersaturation which is determined by  $\Delta T$ .

338.11-14: I do not understand how the accuracy of  $dp_{50}$  which is generally  $\pm 10\%$  can be extrapolated to cover the size range 6 - 1000 nm. No measurements for  $dp_{50} > 20$  nm are presented, nor are such instruments used for particle sizes above 10 nm. So what does this collective accuracy refer to? Well above  $dp_{50}$  the instrument only provides the number concentration, not the size, and so all errors would be counting errors on concentration.

345.4-6: I do not understand why this statement and reference is included, when in the next paragraph it is stated that the HYSPLIT model was used for back trajectory calculations.

Figure 11: I am not sure how this figure helps, other than to suggest that there were many opportunities for the M55 to sample its own plume. I imagine winds are measured everywhere throughout the flight, why only one wind barb? Finally the flight path is not shown be a black line as stated in the figure caption.

Useful additions:

Table 3: It would be nice to include two more rows, one for  $\Delta T$ , and one for supersaturation. Then it would be obvious why there are the differences in  $dp_{50}$ .

Table 4: Add a column for the product of  $Q$  and  $t$ , which is approximately  $1e-5$  in all cases.

Figure 5 and page 337: It might be interesting to point out here that Figures 5A and B

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provide a direct measurement of the size dependent diffusional loss in the heated inlet tube compared to the regular tube when the heated inlet is not heated. How do these measurements compare to diffusional loss calculations for the heated inlet tube? The results of such calculation could be included as another curve on Figs. 5A and B.

Suggestions for clarification/readability

322.12 . . . yielding 50% detection diameters of 6, 11, and 15 nm at ambient pressure  
. . . 322.15 . . . number of non-volatile particles. . . Numbers are not volatile.

Figure 5: Move the pressure label, which is the only thing that changes in the lower right label of each plot, to the top of each plot and make it bigger so the reader sees immediately why the plots are different.

338.5: - -repeated- - is a little misleading. There were four measurements. 338.6: . . . deviation of dp50 for each . . .

346.1: Why therefore, because you trust the NO<sub>x</sub> measurements more than the air mass trajectories?

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Interactive comment on Atmos. Meas. Tech. Discuss., 1, 321, 2008.

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