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Atmospheric Measurement Techniques Discussions

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

R. Weigel et al.

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First of all, the authors would like to thank the referees for their fair and very detailed comments and for helpful suggestions for improvement.

Material which needs culling, consolidating, clarifying: RC: 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.

AC: Abstract is comprehensively modified.

RC: 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.

AC: 'Geophysica' is removed from the manuscript except in the abstract and the introduction.

RC: 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.

AC: The Introduction section is significantly shortened and too detailed parts are consolidated with latter sections which the text was related to.

RC: 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: . . .

AC: The passage is changed according to the RC to avoid misunderstanding.

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

AC: this section is shortened to the minimum information which is, in our opinion, important for the overall technical specification (accuracies, technical solutions, etc) of the system.

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

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7 km, thus measurements below this point suffer from anisoaxial sampling.

AC: The text in this section as well as before is modified according to the suggestion of the referee.

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.

AC: Figure 4 is eliminated and the text is significantly shortened.

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 preformed 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.

AC: The section is shortened to a minimum of essential information.

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 - 100nm 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

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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.

AC: Table 1 contained calculated values for particle diffusion losses for all aerosol lines (from the probe inlet until the detection chamber of COPAS). During the processing period of this paper according text describing table 1 has already been deleted. Thus the table 1 remained wrongly in the submitted manuscript, pulled out of any context. In the most recent manuscript version table 1 has been deleted. Former table 2 contains the important amount of particle losses within the inlet device which the calculation of the correction factor for the ultrafine particle mode is based on. Furthermore the section is shortened and arranged as suggested by the reviewer.

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.

AC: According text is eliminated from the manuscript.

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-3, that coincidence is negligible. In fact for a concentration of 1e4 cm-3 with c=1e-5cm3 the coincidence is 10% falling to 1% for N=1e3 cm-3 using Eq 1. Note c that must have units of cm3 not cm-3. 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.

AC: All unnecessary text within this section is reduced to the minimum information.

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

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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.

AC: Shortened according to RC suggestions although the authors think that the use of the term ’auto-nucleation’ is not misleading it is eliminated from the text to avoid introducing an unnecessary new term.

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.

AC: According sentence is eliminated within the manuscript.

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 . . .- -

AC: Text eliminated according to suggestions.

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.

AC: Text removed according to suggestions.

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

AC: Sentence removed according to RC suggestions.

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

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aspects of the profiles are important and why?

AC: The total aerosol and the non-volatile part are distinguished. Nevertheless, one major objective of this section is the illustration of the instrumental limits at the extreme cases of tropical and arctic measurement conditions. Therefore the section title is modified to clarify.

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 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.

AC: The authors do not fully agree with the RC as the short introduction part of section 8 clearly explains that the examples are used to illustrate the instrumental limits of operation. Thus it was less intended to provide a scientific discussion on the measurement data (it is shortened according to the RC because, we agree, it is too descriptive) but more to show the behavior of the instrument at extreme locations.

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.

AC: According reference list is enlarged by Wennberg, et al. 1994; Murphy and Ravishankara, 1994, Carslaw et al., 1994. to provide a reference list of more general character.

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.

AC: section is eliminated from the manuscript according to the suggestion of Referee.

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.

AC: section is eliminated from the manuscript

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, . . .

AC: References are replaced in the manuscript.

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?

AC: Table 1 has been eliminated from the manuscript. Further information is given above.

Table 2. How is KL calculated?

AC: The calculation is now explicitly mentioned within the text. Additionally, the according table is modified.

335.27-336.3: This is awkward English. dp50 is not the - -smallest detectable particle, or cut-off, or threshold diameter.- - It is well defined as the point where Ndet/Nreal =0.5. Make a clean definition for dp50, 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 dp50 is determined, although I think it is pretty clear from the definition. Then mention with a new sentence that dp50 depends on the supersaturation which is determined by delta T.

AC: According to the suggestions one specific definition is implied and consistently used throughout the manuscript.

338.11-14: I do not understand how the accuracy of dp50 which is generally +- 10% can be extrapolated to cover the size range 6 - 1000 nm. No measurements for dp50 $>$ 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 dp50 the instrument only provides the number concentration, not the size, and so all errors would be counting errors on concentration.

AC: The 10% accuracy mentioned in the text refers to the measurement of total number concentration (within the size range covered by COPAS for flight measurements from 6- 1000nm). The 10% are not related to the dp50. According text in the manuscript is modified to make this clear.

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.

AC: The trajectory modeling by Corti et al. was generally used for these studies to evaluate the probability of own contrail crossing when model results were available. For the case the RC is referring to, unfortunately, such results were not available. Thus the HYSPLIT data were used to estimate the plume age. The text was modified to make this clearer.

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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.

AC: The figure is supposed to demonstrate the high number of own contrail crossings and it works much better than any text can describe. Wind barbs are added also for the points denoted as feature 2.1 and 2.2 but to avoid overloading the figure, not at any other points. Additionally the wind parameters are included.

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 dp50.

AC: Additional rows are added in table 3 showing the delta T and the supersaturation.

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

AC: Additional column is added in Table 4.

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

AC: The authors fully agree as such a study has already been considered before but the initialized analyses only yield results with several uncertainties. One problem is given by the fact that for such studies and a reasonable argumentation the calibrations measurements should have been extended to larger particle sizes (to make sure that the maximum asymptotic counting efficiency is fully reached) and repeated for COPAS I, particularly at 70 hPa, because of enhanced uncertainty of the measurement points.

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For future calibrations of the COPAS instruments such studies are intended. However for this manuscript it was decided that a comparison of the calibration measurements with the calculation does not contribute with trustworthy information.

Suggestions for clarification/readability 322.12 . . . yielding 50% detection diameters of 6, 11, and 15 nm at ambient pressure

AC: Corrected

322.15 . . . number of non-volatile particles. . .. Numbers are not volatile.

AC: Corrected

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.

AC: Corrected

338.5: - -repeated- - is a little misleading. There were four measurements.

AC: Corrected

338.6: . . .deviation of dp50 for each . . .

AC: Corrected

346.1: Why therefore, because you trust the NOx measurements more than the air mass trajectories?

AC: The sentence is modified - Having proof of contrail crossing by the trajectories AND the NOx observation made us focusing on this feature.

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