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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 provement.

General comments:

RC: 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… Some of this is a repeat from my quick review, making me question the usefulness of such reviews.

AC: We accept the criticism. We have significantly shortened the paper to remove all unnecessary information, tutorial sections etc. We would also like to point out that the time period for technical corrections after receiving the quick review was not sufficient for us to thoroughly rework the paper according to the reviewers suggestions. We are confident that a much more concise manuscript is presented now. Overall, the text was shortened by ˜27%, one table and one figure were eliminated as well.

General comments: RC: First, there is no discussion of why FC43 is better working fluid than butanol for aircraft operation. What physical properties of butanol are unsuited for low-pressure operation? Or is there a different reason?

AC: A detailed discussion about different working fluids is not the objective of this manuscript but is given in Herrmann et al. 2005 which is clearly referenced within the manuscript. Note that Herrmann et al. only show empirically the superior low pressure detection efficiency, a reason for this behaviour remains speculative.

RC: Second, there are problems in scaling the NOx emissions of the Geophysica to the ER2. NOx emissions are very sensitive to the exact temperature history as combustion products cool after combustion. Even nominally similar engine designs can have very different NOx emissions. I do respect the attempt to derive some emission factors. Perhaps it would be better to just compare the CN/NOx ratios directly and not try to get an emission factor per unit of fuel burned. Or be very clear that you are only making an order of magnitude estimate of the emission factor. Are there water data with enough precision to see the water from combustion? That would lead to an emission factor.

AC: We agree that it is not straight forward to determine NOx emissions of the Geophysica from comparison with the ER2 (or other aircraft). We only want to derive an order of magnitude estimate here. This is now stated in the text.

Specific comments RC: The introduction is too long. The history of stratospheric aerosol measurements does not need to be reviewed for what is basically an instrument description paper. For example: shorten p. 323 lines 11-21, the entire discussion

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of Mt. Pinatubo (323-324) can probably be eliminated.

AC: Introduction is significantly shortened according to the RC.

RC: p. 327 line 21 discussion of 1.2 nm particles in the lab could be eliminated.

AC: done.

RC: Section 3.3: Transmission efficiency will depend on the inlet being accurately oriented into the flow, especially with a sharp-edged inlet that can stall when the angle of airflow is large. How was the inlet oriented with the local airflow at the two locations on the Geophysica? Was the angle of airflow relative to the aircraft determined in flight by, for example, flying a 4-hole probe in place of the inlet?

AC: The transmission efficiency is strongly dependent on the inlet orientation related to the free airflow in flight, but it is also dependent on the particle size. We assume that for our forward facing sharp-edged inlet the effect of misalignment is negligible for the COPAS-relevant (submicron) particle size range. The absolute inlet orientation is not measured during flight but the inlet orientation related to the aircrafts longitudinal axis is known (it is mounted with negative mean angle of attack) and the angle of attack of the M55 Geophysica during the flights is measured. Thus, it is known that for cruising flight period the inlet is oriented isoaxially during the main part of a flight.

RC: p. 335 line 20: mentions table 3 for calculated results but Table 3 caption says it is experimental data.

AC: Text referred to wrong table; now corrected in the manuscript.

RC: p. 340. If 250 C does not fully evaporate ammonium sulfate, then why not go a little warmer? 300 C does fully evaporate ammonium sulfate and it would make results near the tropopause that much easier to interpret.

AC: The main objective of using the aerosol vaporizer is for evaporating the H2SO4-H2O aerosol, particularly in the UT/LS region. This was the reason to choose the

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heating temperature related to the sulfuric acid aerosol vaporization only. For future investigations related to ammonium sulfate it would be interesting to have an adjustable and/or higher heater temperature such that other chemical aerosol species besides sulfuric acid can be studied. Anyway, 250 C is given as the aerosol line wall temperature to fully evaporate the H2SO4-H2O aerosol even at the lowest pressure conditions encountered in Geophysica measurements. Further technical improvements of the heater system are necessary to go to higher temperatures with our system in flight (thermal isolation, safety installations, aircraft certification, etc).

RC: p. 347 discussion of relative fuel usage of ER2 and Geophysica: As mentioned above, when comparing different engines fuel flow is not a valid basis for estimating NOx emissions.

AC: See discussion above, we emphasize that we only try an order of magnitude estimate.

RC: p. 348 discussion of not crossing the centerline of the plume. If the emission index is working properly, then it should account for differing dilution at the centerline and edges of the plume. However, this issue is small compared to the larger problem of assuming the Geophysica engines have similar NOx emissions.

AC: The dilution at the plume edges yields definitely just small error within the given derivation of particle EI. Anyway, we think it should be mentioned as a possible origin of errors.

RC: p. 349 line 10: Use of a shroud is not likely to matter except for the largest particles, and there are few enough of those not to significantly affect a CN counter. On the other hand, bounce of ice off the shroud might create new problems.

AC: The comment about the shroud and that it (citing Referee 2) "is not likely to matter except for the largest particles" which are too rare to effect the CPC measurements, we absolutely agree with. However, picking up the previous discussion initialized by referee

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2 concerning the transmission efficiency dependent on the inlet alignment: With the use of a shrouded inlet we consider the possibility to gain the transmission efficiencies for particles with diameters smaller 1 micron during ascent and descent (possible inlet misalignment) periods by use of a shrouded inlet. The idea is to use the shroud to keep the air flow around the inlet entrance laminar and isoaxial also in extreme angel-of-attack periods during ascent and descent.

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