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Interactive Comment

Interactive comment on "Out of Africa: High aerosol concentrations in the upper troposphere over Africa" *by* J. Heintzenberg et al.

J. Heintzenberg et al.

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Response to reviewer #1

Title: We regret to have angered rev 1 with our choice of title. Neither any of the colleagues whom we asked for pre-reviews before submission nor rev. 2 nor anybody in the audience of an international conference on which we presented the results under the same heading reacted in any negative way. On the contrary, several times we were commended for it. Thus we would like to keep it.

Page 2664, line 9: We fully agree with the referee that taking aerosol measurements onboard of an aircraft is a major undertaking. We dedicated one full length of a Ph.D. thesis to the development and characterization of inlet, sampling system and sensors of the CARIBIC aerosol payload. We regret that one of the technical papers about the aerosol inlet system was not cited in the original manuscript (Hermann et al., 2001).

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Discussion Paper

This was changed. Besides the thesis, which is available in English, the cited technical papers resulted from the five-year developmental phase of the payload, cover all questions raised by the reviewer.

Page 2664, line 20: See respective response to rev. #2.

Fig. 1. See respective response to rev. #2.

Fig. 2. See respective response to rev. #2.

Fig. 3. True local time was used. The respective text passage was changed.

Page 2667, line 9: Altitude information for all flights has been added to the text and Table 1.

Page 2670, line 8: We agree with the reviewer and did not mean to make any statements about measurements in-flight in general. This issue we dealt with in Hermann et al. 2001.

Response to reviewer #2

GENERAL COMMENTS

Altitude range: Table 1 now shows the altitude range $\pm$ 1standard deviation for each flight. Furthermore, the altitude data range of 10 km$\pm$1.5 km is indicated in the text.

Comparisons: As the reviewer stated her/himself the CARIBIC flights produced “the first observations of aerosol concentrations in the upper troposphere over Africa". For this reason the original manuscript did not dwell on comparisons with data from other parts of the globe. In the revised paper we added a discussion of our data vis-à-vis other upper tropospheric particle numbers.

CO data: The discussion of CO is now expanded in section 3 and the technical reasons are given for why direct correlations with the particle data (as was done for ozone) were not possible in the case of CO. Also several references were added connecting aerosol

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and CO data from biomass burning.

SPECIFIC COMMENTS

Abstract: The incriminated text was eliminated.

Section 2: “cut-off constant". At cruise altitude the D50 varies only by $\sim$1 nm or less. To indicate this to the reader we include the following sentence in the paper: “This threshold diameter varied by $\sim$1 nm or less during cruising flight conditions due to the associated pressure changes."

Section 2: “type of inlet". We added the explanations “forward-facing and blunt-lipped" to the description of the inlet system.

Section 2: “assumptions". Because we do not know the size distribution, the mean values of the inlet sampling efficiency in the respective size windows (4–12; 12–18; $>$18) were used.

Section 2 and 3: altitude and “upper troposphere". Table 1 now shows the altitude range $\pm$ 1standard deviation for each flight. In the introduction an explanatory sentence was added: “In terms of altitude these flights covered a range from tropopause levels near the mid-latitude end point to the middle troposphere in the tropics."

Section 2: “correction factor" and “total error estimate". As stated in the paragraph the factors contributing to the range of corrections are: inlet sampling efficiency, CPC flow rate, CPC coincidence, CPC counting efficiency. After reconsidering the range of these factors for the Africa flights we increased the overall range and expanded the corresponding text to “Together, these corrections account for a particle-size and number-concentration-dependent increase in raw particle concentrations by a factor of 1.2 to 3, the highest contribution to which the variable coincidence correction is. Over the range of cruise altitudes the individual corrections had general uncertainties of $\pm$4% (CPC coincidence), $\pm$3% (CPC flow rate), as well as $\pm$22% for N_4, $\pm$7% for N_{12}, and $\pm$5% for N_{18} (CPC counting efficiency). The uncertainties of the inlet sampling

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efficiency were $\pm$24% for N_{4-12} and $\pm$4% for N_{12} and N_{18} . Combining all errors results in overall average uncertainties of approximately $\pm$35% for N_{4-12} and $\pm$10% for N_{12} and N_{18} (cf. Hermann 2000)."

Section 2. “cloud artifacts" We added the sentences “Beyond all inlet and sensing uncertainties the question of artifact formation during cloud passages needs to be addressed. It will be discussed separately in section 3.4." Section 3.1 “Short periods". Sorry, but we did not make the incriminated claim. We did mention convective activity over summer Europe as alternative explanation for high upper troposphere number concentrations. The shortest spikes were less than 10 sec, which was added to the revised text. N.B.: Recent summer lidar observations over Leipzig indicated high aerosol concentrations over weeks up to the tropopause. Trajectories indicated very long-range transport, possibly more than once around the globe.

Section 3.1 “comparison with INCA data". We agree with the reviewer that the INCA data are not claimed to represent the North Atlantic flight corridor (and we did not make that statement either). From the INCA flight routes as shown in the paper Minikin et al, 2002, however, it is hard to imagine how emissions from the north Atlantic flight routes could have been avoided.

Section 3.2 “Fig. 3". We feel that three flights over the same tropical African region with high-resolution aerosol measurements contain more information than can be indicated by labels in the meridional profiles of Fig. 2 and would like to keep Fig. 3.

Section 3.2 “Air mass origin". Section 3.3. Is dedicated to the discussion of the limited information of trajectories concerning convective processes.

Section 3.2 “Discussion of ozone". As suspected by the reviewer we could not find a robust anticorrelation between particle data and ozone. Consequently, we eliminated the ozone discussion from the revised paper, including former Figure 5.

Section 3.2 “CO on other flights" CO was measured on all 6 flights and included in Fig.

5. However, for technical limitations, which were added to the discussion of CO no direct correlation with particle data was possible.

Section 3.3 “GISS", sorry about the typo. GIS = Geographic Information System

Section 3 “FFT". To answer the questions, we performed an FFT test including white noise and changed the respective text to “We used FFT analysis in order to compare the frequency distributions of the two populations (three size channels each). Over the range of 0.001 to 0.2 Hz the data smoothly followed an -5/3 power law, indicating a well-mixed underlying atmosphere without indications of chaotic processes as would have been expected for chaotic cloud drop fragmentation. Furthermore, the frequency distributions are very similar in all three size-channels, corroborating the results of the first test. In a next step, we simulated droplet fragmentation by adding white noise to the original particle number concentration time series. For that purpose, we assumed a “cloud" of 25 km width in the region of the ITCZ, which should generate a uniform white noise particle signal due to droplet fragmentation. For the mean absolute amplitude we took a value of 20% of the measured concentration. If we add these absolute white noise values to the original time series and repeat the FFT analysis, a significant deviation from the -5/3 power law in form of an increased level around the Nyquist frequency is found. Hence, because we do not find a deviation from the -5/3 power law for our original time series, a potential error caused by breaking cloud droplets can be assumed to be small compared to other experimental errors."

Section 3.4 “quantitative cloud effects". We are afraid, no. Additional ref. to the Minikin et al. Paper is given in section 3.1.

Section 4 CTM’s: There are indeed first global CTM’s that can use CN data, e.g., Wilson, J., Cuvelier, C. and Raes, F., 2001. A modeling study of global mixed aerosol fields. Journal of Geophysical Research, 106: 34 081–34 108.

Section 4 “CO discussion". The discussion of CO is now expanded in section 3 and the technical reasons are given for why direct correlations with the particle data were

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not meaningful. Also several references were added connecting aerosol and CO data from biomass burning.

Figure 1. We deliberately used projections, only to indicate the geographical “fetch" of the flights and added to the introduction of this figure “Also included in Fig. 1 are the horizontal projections of the 3-dimensional 5-day back trajectories calculated every 30 minutes along the flight tracks. With these projections the geographical “fetch" of the CARIBIC flights is indicated." Height information did not improve the readability of the plots

Section 4 “Conclusions". This section has been expanded along the suggested lines.

Table 2: “average". Medians and arithmetic averages of our data differ by less than 1% because of the large samples.

TECHNICAL CORRECTIONS

Section 3.2 “120,000". Sorry about the inconsistency in the original text. The set threshold was 100,000.

Fig. 2. Right-hand scale added as suggested. Incriminated plot symbols were eliminated. Rev. #1 suggested removing the shift of southbound and northbound. We tried that to begin with but the curves were too hard to differentiate despite their different colors.

Fig. 4. I wonder, which lines the reviewer is referring to. There are no lines in the plot except for diagonal help line. I am unable to eliminate the lines in the legend.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2661, 2003.