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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 10 July 2003

MS-NR: 2003-020 Title: Out of Africa: High aerosol concentrations in the upper troposphere over Africa Authors: J. Heintzenberg, M. Hermann, and D. Theiss

GENERAL COMMENTS

The manuscript by Heintzenberg et al. presents aircraft observations of condensation nuclei concentrations, mainly meridional transects, over Africa obtained from a commercial airliner (CARIBIC project). The data represent to my knowledge the first observations of aerosol concentrations in the upper troposphere over Africa. The authors report on very high CN concentrations in the ITCZ region.

The data presented are unique. The discussion of methods and data analysis is mostly clear and concise. The analysis of data is partially limited due the fact that the CARIBIC payload did not include other valuable information (like information on cloud presence).

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Discussion Paper

The manuscript is acceptable for publication in ACP, but I would recommend to ask for a couple of revisions according to the specific comments following below.

I feel it is of particular importance that the authors include more or clearer information/discussion on:

- the altitude range(s) their analysis is referring to

- comparison with other aerosol observations from tropical regions

- use of CO data as a potential tracer for near-surface air

SPECIFIC COMMENTS

Abstract. Lines 16-21. Does this belong in the abstract? It is merely a restatement of what is later given as a future perspective in the conclusions section.

Section 2, page 2664, lines 3-7. Is it known if the cut-off remains constant during different operation conditions (in particular on cruise altitude)?

Section 2. One or two more sentences should be said about the type of inlet. Is it sampling in forward or backward direction?

Section 2, page 2664, Lines 23-25 and elsewhere. To obtain corrections on raw CN concentrations, is it not necessary to make assumptions on particle size distribution (which was not measured). How was this dealt with?

Furthermore, the correction factor is stated to be in a range of 1.2 to 2. Which factors are contributing to this range (apart from coincidence corrections) and on how does the correction factor typically vary over one flight (time/spatial scale)?

What is the total error estimate for number concentrations given in this manuscript (including an error estimate for the correction factor)?

Section 2. The possibility of cloud artefacts, though discussed later, should be briefly introduced already in this Section. Possibly in connection to the inlet discussion.

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Section 2 and 3. Important information is in general missing: It is not clear to which altitude levels data, discussion and figures refer to. Typical cruise altitudes should be given. The title of the manuscript refers to the upper troposphere. How is this defined in this context? Where lower altitude data excluded in the analysis?

Section 3.1, page 2665, lines 19-23. What is the definition of "short" periods in this context? I doubt that all cases of high aerosol number concentrations over Europe are solely due to air traffic emissions. Unless this is referring to short concentration spikes (a few seconds at maximum) caused by direct crossing of aircraft plumes.

Section 3.1, page 2666, lines 21-25. A comparison with data of the INCA project has been made in terms of ratios between northern and southern hemisphere. How do absolute concentration levels compare? How do the CARIBIC data compare to other measurements in tropical regions? (See further comment below.)

To my knowledge, the INCA data for the northern hemisphere are not being claimed to represent the North Atlantic flight corridor but rather some background situation for that region. The discrepancy in concentration ratios between INCA and CARIBIC therefore could very well be related to the fact that the CARIBIC data are not extending as far south, as the authors already mention.

In this manuscript I am generally missing any further comparisons to results of other studies in tropical regimes. The CARIBIC results presented here are not put into any perspective of aerosol measurements from other parts of the world. Upper tropsopheric number concentrations on the order of 100,000 particles/cc are indeed very high but this gets only evident from comparisons to other observations. The authors will be certainly aware of the aircraft measurements campaigns made in (or including) other tropical regions. Some key papers to refer to might be the fairly recent ones of Clarke et al., Singh et al. etc.

Section 3.2, page 2667, lines 17-25. I wonder if Figure 3 is containing substantial information. This is a compilation of only three flights and the aircraft is at different

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geographical locations at different times of the day. Following the author's discussion, the important thing to point out appears to be the contrasting concentration levels during day and night time. Therefore, I would suggest to skip Figure 3 and rather include some information (labelling) on time of day in the sub-panels of Figure 2.

It is stated that low concentrations occur in accordance with trajectories originating from the upper troposphere or lower stratosphere. But how does the air mass origin look like in cases of high concentrations? Is there any substantial difference to the low concentration cases? I would expect that also in these cases air originates mostly from the upper troposphere. (As discussed later in the manuscript, uplift of air by deep convection is not well represented in the trajectory model.) I do not quite get the message from the discussion of the back trajectories.

Section 3.2, page 2668, lines 12-20. I am not convinced by the discussion of ozone in terms of an indicator of surface near air masses being lifted up to cruise altitudes. Can observed ozone values below 80 ppbv be considered as "low"? If the authors arguments are correct, should there not be a robust anti-correlation between ozone and ultrafine CN in the upper troposphere? Is this the case?

In comparison to ozone, should not CO be a more appropriate tracer to learn about the uplift of surface-near air masses? Why is this not discussed in Section 3.2? CO latitudinal profiles are only later shown (and very briefly discussed) in Section 4 (Conclusions) and Figure 6.

Section 3.2, page 2668, lines 21-24. Was CO not measured on flights other than 000728?

"... corroborating the results of the present study." The conclusion of the authors with respect to the origin of ultrafine particles over tropical Africa should be made clearer.

Section 3.3, page 2669, line 8. Define GISS.

Section 3.4, page 2671, lines 11-24. Can it be expressed somewhat more quanti-

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tatively what fraction of the CARIBIC flights would have been affected by clouds in general, and what fraction would have been affected by ice clouds according to the temperature criterion? Was this situation with respect to clouds/temperature similiar from flight to flight?

In the referenced paper of Minikin et al. it is mentioned that the statistics on aerosol number concentrations during INCA were essentially independent from including or excluding in-cloud data. This might be worthwhile to mention in support of the CARIBIC data, though aerosol inlets are not the same.

How would chaotic data show up in the FFT analysis? Have the authors any further support that the FFT approach would indeed qualify any cloud artefacts as such?

Section 4, page 2672, lines 1-3. Are not global CTMs still far from being able to use CN data for validation? (But this is rather problem of the models, not the observations.)

Section 4, page 2672, lines 5-7. The discussion of CO should be more broadened. See earlier remark.

Section 4. This section should also summarize briefly what the authors believe to be the most relevant causes for the very high CN concentrations observed in the ITCZ over Africa, based on the discussion earlier in the manuscript.

Table 2. The authors report only on arithmetic averages. Arithmetic averages of aerosol number concentrations are often biased by the presence of relatively few very high values. Would medians or geometric averages give substantially different numbers in this context? If yes, this should be considered for the manuscript.

Figure 1. Trajectories are projections only. Is it possible to include information on height?

TECHNICAL CORRECTIONS

Section 3.2, page 2667, lines 5-8. Was the threshold 120,000 or 100,000 (confer

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Section 2)?

Figure 2. Color coding red/black should be explicitly explained. I suggest to have a scale on the right hand vertical axis according to the shift down explained in the figure caption.

In the black curves I find there are "x" symbols included. Are these correct? If yes, what is there meaning?

Figure 4. Lines are too thin in my print-out.

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