

Interactive comment on “Temporal and spatial variations of aerosol physical and chemical properties over West Africa: AMMA aircraft campaign in summer 2006” by A. Matsuki et al.

Anonymous Referee #1

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Questionnaire

> Does the paper address relevant scientific questions within the scope of ACP? > Does the paper present novel concepts, ideas, tools, or data? > Are substantial conclusions reached? > Are the results sufficient to support the interpretations and conclusions?

Yes, the paper addresses differences in aerosol composition, volatility and CCN activity over Western Africa during off-monsoon, transitional and monsoon conditions. The results of "in-situ" and offline measurements are shown and cross-compared. Interesting conclusions are drawn from the data, though – given the data scattering especially of

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the volatility/CCN measurements – some conclusions seem a little overstressed.

> Are the scientific methods and assumptions valid and clearly outlined? > Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Methods are described including their inherent assumptions. However, some vital assumptions (see detailed comments) are not explained, also only few info on device calibration and inlet system conditions are given, the latter ones being possibly relevant for volatility representativity. Maybe a little more raw data material (e. g., average "real" and not fitted size distributions would enhance the paper.

> Do the authors give proper credit to related work and clearly indicate their own new/original contribution? > Are the number and quality of references appropriate?

Sufficient references are used.

> Does the title clearly reflect the contents of the paper?

The title speaks of "temporal and spatial variations". If the term spatial refers to altitude only, it is fine, otherwise it should be rephrased.

> Does the abstract provide a concise and complete summary?

Abstract is fine, see specific comments.

> Is the overall presentation well structured and clear? > Is the language fluent and precise?

Structure and language are fine.

Specific comments

4464/1-9: The reader gets the impression, that the described project is able to make statements valid for all of West Africa. This is certainly not the case, as West Africa is significantly larger than the investigated region. Please clarify appropriately

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4464/24-4465/1: Too detailed for abstract. Please shorten.

4465/1: TEM-EDX – acronym without explanation. Though well-known to a certain community, it should be avoided.

4465/12-14: If aerosol basics should be addressed here, it should be exact: most sulfate is probably not directly emitted, neither by "anthropogenic" or "natural" sources (wherever the division may be), but formed later from the gas phase.

4468/chapter 2.1: How were the instruments measuring volatile/non-volatile species calibrated? Experience teaches that usually aerosol instruments even of the same type show deviations, unfortunately. How were the OPC and the SMPS joined together to a single size distribution (distributions in Fig. 4 are smooth)? What assumptions have been made to do so? At least, both are measuring different aerosol properties and should not give the same results.

4469/2-8: While the conditions for the volatility measurements are defined, the conditions for the general size distribution measurements remain undefined, except for the notion "in-situ". However, there are severe doubts that a measurement inside an aircraft can be addressed as in-situ for the outside air. What where the temperature and humidity conditions for the size distributions measurements as function of flight altitude and outside temperature and humidity? How much highly volatile material was lost during the transfer into the OPC/SMPS?

4469/13: "one spectrum"

4470/23-24: Please explain shortly why C, N, O is excluded. Not everyone may know the problems.

4471/1-3: Sentence can be omitted.

4474/2-9 and Fig. 5: 1. When the particles up to 1 μm show the same volatility behavior as the shown restricted size range, why the size range was restricted? 2. It is mentioned, that for larger particles, the volatile fraction has other values – but which?

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3. Data are extremely scattered. How significant is the trend for SOP2a2, which is pronounced by manual drawing? 4. In the paragraph about the size distributions it was mentioned, that averages showed less variance when it was done over particular trajectories rather the flight altitude. So, why for volatility flight altitude was preferred as discriminator?

4474/10-26: Given the highly scattered data points in Fig. 5 even after averaging (!), this interpretation without the performance of a statistical test on which confidence level the mean values are different is rather pointless.

4475/2: "... hygroscopicity ... controlled by particle size" is a little mistakable

4475/28-4476/13: Those results are interesting. However, what are the values of regression and correlation coefficients for the four populations showed in Figure 7? A value of approximately 1 as stated in 4476/1 might not fit to Figure 7. And was the range of 121-288 nm found to fit best to the CCN/CN ratios? Were tests made for other activation diameters? 120 nm might be a threshold set too high.

4476/14-16: Hydrophilic, -phobic, hygroscopic seem to be mixed-up in this paragraph (according to the first sentence, Monsoon is more hydrophobic, but according to the second one it is more hydrophilic, the latter fitting the figure).

4476/24-27: Is dust per se cloud-inactive (see also comment on 4481/26)? If so, what these particles could be composed of? SEM/TEM should tell.

4477/3: replace SEM and TEM by scanning and transmission or explain acronyms. Explain acronym EDX.

4477/8-11: Reword

4478/7: Is it possible to quantify (roughly) or "qualify", how often a residuum was found after evaporating the biomass burning particle? I. e., how often they were internally mixed?

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4478/13: Electron-transparent?

4478/13-21 and Fig. 8: - Cosmetic: Same font size and length for all axes. - Mention, that the C (and perhaps O?) signal also has contribution from the substrate. - Where does the conclusion of acidity for the particles with satellite droplets derive from? Probably they were droplets during impact, but of which chemistry? Bigg 1980 used CaF₂-coated substrates for an acid reaction of the particles.

4479/16: It is also interesting to see, that there seems to exist a persisting background mixture of these three components for all samples. Does this indicate locally well mixed air? Or rather aged air passing different source regions beyond the 5-day trajectory limit? Or is it just an effect of averaging and there exist samples without particular particle species?

4480/25: Though the term "internal mixture with sulfate" is technically speaking correct for K₂SO₄, to me that notion implies that K and S are present possibly as different compounds – was that the intention?

4481/10: "which are assumed"?

4481/26: It is reported that mineral dust is found together with soluble components in different places (e. g. Falkovich et al. 2001, J. Geophys. Res. 106D, 18029-18036; Mori et al. 2003, Atmos. Environ. 37, 4253-4263; Kandler et al. 2007, Atmos. Environ. 41, 8058-8074 and others), so a (large) difference is not necessarily to be expected.

4482/15-20: This paragraph may be suitable for an abstract, but not here.

4483/1: cloud active. . . particles being able to act as CCN?

Table 1/ Fig. 4: How well are the raw data described by the fitted distributions? Interpreting fitted spectra only by a forced number of modes may hide features of the "real" distribution.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 4463, 2010.

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