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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Increase of the aerosol hygroscopicity by aqueous mixing in a mesoscale convective system: a case study from the AMMA campaign" *by* S. Crumeyrolle et al.

S. Crumeyrolle et al.

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We thank both reviewers for their detailed and constructive comments on our manuscript. We have revised the manuscript attempting to take into account all the comments raised by both reviewers. We apologize for the delay due to the time required to perform the requested additional analysis.

General Comments of Reviewer 1 :

1/ In the experimental strategy, the authors claim that the air masses were the same before and after the MCS passage. This assumption allows us to attributed aerosol property modifications to the MCS passage.

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Interactive Discussion
Discussion Paper

We agree that the air masses before and after the MCS passage are not the same. Indeed, the trajectories plotted in Fig.12 (Before the MCS) and in Fig.13 (After the MCS) show two different origins. However, for both cases the origins of air masses found above the monsoon layer are Sahelian regions and these air masses follow the Harmattan flux that brings dry and hot air and dust particles. Before and after the MCS, the group 2 trajectory altitudes (Fig.12 and Fig.13) remain constant, and there is no air mass mixing between the monsoon layer enriched in polluted air and the SAL enriched in dust particles. Thus, it is reasonable to assume that the composition of dust particles brought by group 2 air masses is too similar (at least from an hygroscopic point of view) to be responsible for the change observed in aerosol properties before and after the MCS.

Furthermore, this study focuses on the aerosol vertical profile. Air mass trajectories clearly show the evolution of this vertical profile that was constituted of two layers (Boundary layer and Saharan air Layer) before the MCS and made of three layers (Boundary layer Intermediate Layer and Saharan air Layer) after the MCS passage. Additional air masses in the intermediate layer come from a zone where the MCS was formed, and the evolution of their altitude shows that they were down-transported by the density currents of the MCS (Fig.13). Because of this, dust particles originally transported at 5500m, whatever its origin, could be mixed with trace gases of the monsoon layer and be modified. Thus, we consider that only the density currents of the MCS can be responsible for the change in aerosols properties by allowing different layers to mix.

Finally, observations drawn from single particle analysis have shown that dust aerosols sampled in the intermediate layer after the MCS were chemically modified. Then, the modeling work has confirmed that these modifications resulted from a cloud processing within the MCS.

We have added some comments in the text to clarify these points.

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Interactive Comment

As suggested, we have plotted the vertical profile of CN concentrations at standard conditions and the tendencies observed on fig.5 for both flights are exactly the same.

2/ The authors quantify the modification of CCN properties by analysing the ratio of CCN number concentration to CN (D $>$ 3 nm) number density.

We agree that, in presenting the CCN/CN ratios, we should account for the bias due to nucleation mode particles. Indeed, the concentration given by the CPC 3025 is not a good reference because its cut-size is too low. If we integrate the SMPS spectra for particles with diameter larger than 50 nm, the ratio CCN/CN is bigger than unity for the majority of the samples. This result shows that a fraction of particles is then missing in the estimation of CN concentration. This is obviously worst at 80nm.

To better know the threshold diameter for estimating the CN concentration, we calculated the minimum CCN critical diameter for the most soluble particles, i.e. NaCl, and for the highest supersaturation that can be encountered in our case. Considering that the uncertainty of the supersaturation is about 25% for the Wyoming CCNC used, we did the calculation for a supersaturation of 0.75% (0.6*1.25). We found a critical diameter of 30nm. Given this result, we have now considered that the data given by the CPC 3010, which are more confident than those obtained by integrating the SMPS spectra, would be the best compromise to be used as a reference for CN concentration. We have added a part of this discussion to the text to clarify this point.

3/ The time sequences for the modelling study are different from the time when the measurements were performed.

The reviewer is true to say that the time shift between observation and model implies some speculative elements of interpretation. It is clear that if the model is able to reproduce at the right time the observed MCS it would help to objectively compare measurement with observations. Unfortunately, modelling MCS over west Africa with a forecasting mesoscale model is complex for two reasons: 1) Over west africa, there is few observations that are introduced in the analysis file (analysis of the ECMWF) used

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as the initial file and at the boundaries in the MesoNH model. It implies that the forecast in these areas is really complex and the forecasting scores are bad. 2) The initiation of MCS are also difficult to model due to stochastic processes as mixing turbulence, CAPE, radiation, and wind convergence. Given these two points, the model gives a good representation of the event, with a right localization of the three MCS developed over Niamey and Banizoumbou.

The time delay is less important than the location and the structure of MCS due to the stability of the flow in the ITD region. In the manuscript, when authors compare observations and model they keep the same time delay of 5 hours to respect the fact that the transport of each air layer need to be close and comparable. On the caption of Fig 11, we forgot to explain that the observed values (points) are 5 hours after the simulation respecting the same time shift. This point has been clarified in the new version of the manuscript.

Specific Comments of Reviewer 1 :

Table 1: please add the total number concentration measured by the CPC TSI 3025. This additional information permits the assessment of the log-normal fit and/or an estimate of the nucleation mode.

The data obtained by CPCs 3010 and 3025 have been added in table 1.

How do you identify biomass burning particles from EDX spectra which do not give a C signal?

The table for single particle analysis results have been integrated in the latest version of this manuscript. Presence of a dust particle can be identified by the typical irregular shape and the dominant X-ray peaks corresponding to Al and Si. Other minor types included particles rich in Cl and K that are linked to biomass burning. Few particles were found enriched in P and K, which may be attributed to biogenic origin. For more details on the single particle analysis and the impactor experimental setup, see Matsuki

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et al. (2008). This reference has been added in the manuscript.

Page 10068: You state that dust is not a good CCN because it is mostly insoluble. However, it is known that dust can act as good CCN because of its size which exhibits a weak curvature of the particle-water interface in contrast to smaller particles. Please comment.

It is usually known that the particle hygroscopicity depends on the size and the chemical composition of particles. Thus, coarser particles may act as CCN. In our case, the size spectra show three different modes (Aitken accumulation and coarse modes) centered on 80nm, 170nm and 550nm. Single particle analysis shows the presence of dust mainly in the accumulation mode. Thus, the Kelvin effect of dust particles is not significant in contrast with other particles of the accumulation mode. The chemical composition of these particles, so the Raoult’s effect, has a more important role.

Typographic errors of Reviewer 1:

Page 10058, Abstract, line 17: modify to "and washout of particles". This has been modified in the manuscript

Page 10058, Abstract, line 18, Page 10059, line 4: We agree, and it has been revised as suggested.

Page 10073, line 14, 15: The suggested phrasing is better, and we have changed the sentence in the new version of the manuscript.

Page 10062, line 20/21, Page 10067, line 21, Page 10068, line 13, Page 10075, line 14, Page 10077, line 13, Page 10083, line 19: These seven words were corrected.

Figure 5: We changed the X axis title {CN (cm-3)}.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10057, 2008.