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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Transport and vertical structure of aerosols and water vapor over West Africa during the African monsoon dry season" *by* S.-W. Kim et al.

S.-W. Kim et al.

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Reply to Anonymous Referee 2 Comments

[General comments] In the present paper, valuable observations are presented and a proper analysis using model-derived meteorological fields would be very profitable. However, major revisions are needed in many aspects in order to make the paper publishable. A thorough improvement of the paper is needed, particularly to clearly present the originality of the paper, to properly justify the occurrence of the cited dynamical mechanisms and to include proper background information and references.

We would like to thank the reviewer for his/her detailed and helpful comments. We are fully aware that taking time to provide in-depth reviews is a sacrifice and we greatly appreciate it. Responses to the comments of the referee are embedded below. We appreciate for good comments and providing several references.

[Major remarks] .

1) The main objective and new findings of the paper should be clarified. The two-layer structure (a dust layer below a biomass burning aerosol layer) was already the object of several papers (Chazette et al., 2007, Osborne et al., 2008, Johnson et al., 2008, Raut and Chazette, 2008, etc). The main transport pattern during the SOP-0 was explained by Haywood et al. (2008). Considering this, what is the new point regarded by the paper?

This reviewer obviously thinks that there is no new message in this article since the papers from DABEX AMMA-SOP0 already explain what leads to the vertical distribution of aerosol in this region. However, the results of this study help to verify this conclusion and present supporting evidence from a variety of measurement platforms, as well as increase confidence in the ability of both lidar and aircraft in situ instruments. This study also covers a greater range of time periods than those from DABEX (since this study looked at results from the year 2007 as well as year 2006). Several papers mentioned by the reviewer have different focuses, although those papers showed the vertical aerosol structures. Chazette et al. (2007) showed new methodology for the retrievals of aerosol extinction coefficient profiles from ULA(ultra-light aircraft)-based lidar measurements. They showed only one example in the paper; however, it is hard to generalize the vertical structure of aerosols. Osborne et al. (2008) and Johnson et al. (2008) also showed a short period of observation (14 flights of the BAe-146

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aircraft between 13 January to 3 February 2006), having focusing on physical and optical properties of mineral dust aerosol, and vertical distribution of dust and biomass burning aerosols and their radiative effects, respectively. Raut and Chazette (2008) focused on the radiative budget of aerosol and it was not the topic of this article. In addition, this paper investigates for the vertical distribution of dust and biomass burning aerosols in 2007 using spaceborne lidar observations. Moreover, an enhancement of water vapour within elevated biomass burning aerosol layer is firstly reported in this study. The results of this study support the conclusion of the DABEX / AMMA-SOP0 papers and give evidence from a wider range of measurement platforms (including satellite). We think that it is important to verify conclusions proposed in the past and test them with new data so this paper is clearly making a valuable contribution to the issue.

2) The description of the involved dynamical mechanisms is not clear. What is the actual origin of lifting in the 10 N fringe? Is it dry convection or deep convection? No justification is given for an increase in surface heating. In my opinion, if "transport" is included in the title, a thorough explanation of the circulation patterns and the involved dynamical mechanisms should be given. This aspect should be significantly expanded and properly justified.

In a revised manuscript, we remove ‘transport‘ in the title and rewrite to read ‘Evolution of the vertical structure of aerosols and water vapor over West Africa during the African monsoon dry season‘. Nevertheless, we add more detailed information on the dynamical mechanisms; especially updraft motions (also, see the reply of next comment).

3) The background information and references cited in the paper are fairly poor, particularly when it comes to West African meteorology. Many basic notions concerning the meteorology and atmospheric circulation in the region are omitted. Among other, the Inter-tropical discontinuity, the Saharan air layer and

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the African easterly jet are fundamental actors in the circulation patterns in the analyzed cases, but they are not mentioned. The difference between the ITCZ and the inter-tropical discontinuity should be explained.

We have added not only several papers that has published within the framework of DABEX AMMA-SOP0, after the submission of this paper, but also the bibliography related with regional atmospheric circulation over the study area. We also have clearly explained the concepts of ITCZ and the inter-tropical discontinuity. The inter-tropical discontinuity is apparent to the lower troposphere due to the convergence between the Harmattan and the monsoon fluxes. Usually, it is not co-located with the ITCZ, which is the rising branch of the Hadley cell (deep convection). Both move towards north in summer. We think that the lifting is due to the inter-tropical discontinuity, as it is already mentioned in the manuscript, without the term of 8220;inter-tropical discontinuity8221;. We have specified all these points in the revised manuscript.

Other important points:

4) The indication of many geographic locations is mainly vague and very difficult to follow when referring to the figures. The Sahara and the Sahel are very vast regions covering thousands of kilometres and more precise locations should be given.

We have precisely indicate the locations in the revised manuscript.

5) Back trajectories are an important support of the paper. The uncertainties involved should be clearly discussed and mentioned in the analysis. Some information is given by Knippertz et al., 2008.
Knippertz P, Ansmann A, Althausen D, Muller D, Tesche M, Bierwirth E, Dinter

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T, Muller T, von Hoyningen-Huene W, Schepanski K, Wendisch M, Heinold B, Kandler K, Petzold A, Schultz L, Tegen I. 2008. Dust mobilization and transport in the northern Sahara during SAMUM ; A meteorological overview. Tellus B. DOI:10.1111/j.1600- 0889.2008.00380.x.

We have added aforementioned paper, Knippertz et al. (2009), and discussed associated uncertainties in the backward trajectory.

6) It is not clear whether the paper analyses a complete period covering a season or a series of study cases. Only punctual observations are given and their significance is not analyzed.

Complementary data sets covering the African monsoon dry season from various platforms (ground, airborne and space-borne) over the study regions are limited. We have analyzed all available data from AMMA 8211;SOP0 campaign in 2006 and CALIPSO observations in 2008. From a series of case analysis, we have explained the vertical distribution of dust and biomass burning aerosols and their transports.

7) English language should be improved in general. More precision should be given for example for wind directions. Winds are either easterly or westward when they come from the east. Easterly, east and westward should be precisely used. Repeating of statements should be avoided when possible. To benefit from a clearer and more direct reading, the point of each section should be suggested at the beginning.

We have reworded all terms of wind directions and closely corrected English grammar throughout the manuscript.

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Particular remarks:

8) Please, rewrite the sentence in lines 21-23 of page 3

We have reworded the sentence.

9) Equation (1) is not correct. Please, rewrite, properly define the variables and indicate a reference.

We have corrected it.

10) What are the values of the lidar ratio used in the inversion of the ULA lidar signals? Please, give them and justify.

The lidar ratio (i.e., backscatter to extinction ratio) profile retrieved from the coupling between airborne lidar horizontal and nadir-shooting measurements was used in the inversion of lidar extinction profile. Details are given in the sections 3 and 5.2 of Chazette et al. (2007).

11) Why the ULA lidar signals are inversed and only attenuated backscatter CALIOP profiles are given? I recommend providing inversed signals in all cases, particularly to avoid ambiguities induced by atmospheric transmission. With the optical depth of the layers involved, it may be possible that the lidar return of a lower layer may be attenuated by a higher one.

As the reviewer pointed out, the lidar signal should be attenuated as it propagates. Although the attenuated backscatter CALIOP profiles are enough to explain the

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vertical structure of dust and biomass burning aerosols (e.g., Kim et al., ACP, 2008), we have retrieved aerosol extinction profiles to remove this ambiguity . We have also discussed the contribution, in term of AOD of the higher layer compared to the lower layer.

12) In line 5 of page 7, is it "derived" or simply supplied by a data provider service. Please, cite the website or the origin of CALIOP datasets.

All of CALIOP data (including 61538;8242;532 and 61538;8242;1064) were obtained from the NASA Langley Research Center Atmospheric Science Data Center (ASDC) via on-line web orders. We have added this information in the revised manuscript. Meanwhile, here the meaning of 8220;derived8221; is that the total attenuated backscattering intensity at 532 and 1064 nm (61538;8242;532 and 61538;8242;1064) were derived from the calibrated, range-corrected, laser energy normalized, background noise subtracted from the lidar return signal.

13) Page 9, lines 6-9 are not clear. Please, explain again.

We have reworded the sentence.

14) Page 9, lines 17-20. Please, be more specific with locations. What indicates the type of aerosols?

We have reworded the sentences to give more detailed locations. Regarding to the type of aerosols, however, it is hard to MISR-derived column-integrated AOD. Mostly, dust and biomass burning aerosols may contribute.

15) Page 10, lines 1-2. It is not true. The monsoon is a low level flow up to 2 km

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of altitude, well below the 700 hPa level. Please, clarify.

The reviewer8217;s comment is correct. We have reworded the sentence.

16) Page 10, line 7 and 19. Locations are vague and the point is not clear to understand. Where are the winds described? Which latitude and longitude?

We have reworded the sentences to give more detailed locations.

17) Page 10, line 18. North or south? Please replace convective by convection.

We have corrected it.

18) Page 11, line 3. Turbid imply that they are mainly opaque. Scattering layers is a better term.

We have corrected it.

19) Page 11, line 11. Indicate closed or open marks.

We have corrected it.

20) Page 11, line 15. Some fires are closed to the trajectory. Some mixing may occur.

We have mentioned the possibility of mixing of aerosols as seen from the trajectories.

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21) Page 11, lines 18-21. Please, rephrase.

We have reworded the sentence.

22) Page 12, line 6. It was not updraft motion prevailing in the region in Fig.2?

We have clarified the sentence.

23) Page 12, line 10. What is the importance of the clean air layer?

Layering is most common when the atmosphere is stable to vertical motions. This phenomenon has received considerable attention in connection with air pollution, since pollutants tend to accumulate beneath stable layers. Consequently, increases in visibility above stable layers are a common phenomenon. The clean air layer, another consequence of stable layers, that separated polluted air below and above (Hobbs, Nature 2002; Hobbs, JGR, 2003).

24) Page 12, lines 11-18. Please, explain the importance of the fact that one layer is more humid than the other (4 and 5.5 g/kg of water vapour mixing ratio).

As we mentioned in section 5.1 of the manuscript, enhanced water vapor within the biomass burning aerosol layer can effect on the stability.

25) Page 13, line 11. Please rephrase, it is not clear to which layer it refers.

We have reworded the sentence.

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26) Page 14, line 21. Why there is no mixing? In some of the cases of the figure, there is no clear air layer between the two aerosol layers. Some partial mixing could occur at the boundary between the two layers.

We have reworded the sentence to give the possibility of some partial mixing could be occurred at the boundary between the two layers.

27) Page 15, line 9. Please, avoid back and forth comments.

We separate the observation of enhanced water vapor (sections 4.1 and 4.2) and its explanation (here in section 4.3) to avoid complicated explanation.

28) Page 15, lines 15-21. Please rephrase. Justifications are not clear. What is the relationship between surface heating and deep convection? Why the large scale ascent is higher at 850 hPa than at 925 hPa (fig. 2)? An additional figure showing the location of mesoscale convective systems in the period would be very important in the discussion.

As we mentioned above, we add more detailed information on the dynamical mechanisms, especially updraft motions, with the concepts of ITCZ and the inter-tropical discontinuity. The maximum ascent speed is obtained in altitude since it results from convergence of air below and divergence above. Air parcels warmed by the surface reaches the free convection level (above 925 hPa) and the convection of saturated air develops until their equilibrium level is reached in the upper troposphere (possible ref. to a book of tropical meteorology).

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29) Page 16, lines 10-14. These references are not exhaustive. Please expand or omit the comment.

We have removed this sentence.

30) Page 17, lines 4-7 for example. Please include technical details in section 2 and mainly use the body of this section to make the scientific statements.

We have moved some technical information to section 2.

31) Page 18, lines 1-2 for example. Please compare the color ratio values to the ones shown in other publications (e.g. Cattral et al., 2005). More references on typical depolarization ratio values and variability is desired.

Cattrall, C., J. Reagan, K. Thome, and O. Dubovik (2005), Variability of aerosol and spectral lidar and backscatter and extinction ratios of key aerosol types derived from selected Aerosol Robotic Network locations, J. Geophys. Res., 110, D10S11, doi:10.1029/2004JD005124.

It is a good suggestion. We have added the comparison results of color ratio to the results from previous studies.

32) Page 18, lines 3-4. In order to make this statement, inversion of the CALIOP profiles would be needed.

Please see our previous reply (11)

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33) Page 18, line 11. Could it be the natural variability of biomass burning aerosols? Please refer to works as Cattrall et al. (2005).

We have added some discussion for the properties of these elevated layers, by comparing the works of Cattrall et al. (2005).

34) Page 19, lines 12-15. Please rephrase. Doesn8217;t it mean that it is simply stratified?

We have reworded the sentences.

35) Page 21, lines 23-24. What complex interactions?? The radiative budget is product of the various aerosols layers.
Here the interactions can be occurred due to the potential mixing between dust and biomass burning aerosols off the African continent (i.e., over the Atlantic Ocean).

36) Page 22, lines 1-3. What is the sense of this last sentence?

We have removed this sentence.

37) Figure 1: The location of the ITCZ and the inter-tropical discontinuity should be given in each case. The criterion of identification should be explained. Only important panels should be kept. Arrows should be bigger and I would recommend using streamlines.

We add the location of the ITCZ and the inter-tropical discontinuity, with the explanation of criterion. We leave only the plot at 850 hPa level, as we mentioned in the

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reply for reviewer 1.

38) Figure 2: The location of the ITCZ and the inter-tropical discontinuity should be given in each case. Vertical speeds for a pressure level which is below the ground level should not be displayed. Important locations as Niamey should be indicated.

We add the location of the ITCZ and the inter-tropical discontinuity as well as Niamey.

39) Figures 3. What is the abscissa variable in panel a? 32?

32, Julian day (i.e., January 1st is 1), indicates February 1st, as we noted it just below 32. However, we correct the label of x-axis to be read the time of each day for clarity.

40) Figure 6. Please, reduce the number of panels or show a synthesis figure. This extends to some other figures when applicable.

As the reviewer mentioned, six plots for coincident measurements from ULA-lidar, BAe-146 and radiosonde data are somewhat complicated. However, these figures help to verify the conclusion of this study and present supporting evidence from a variety of measurement platforms for different flights.

41) Figure 9. It is desirable that panels a and b show the same latitude sector.

If we match the same latitude sector in Figures 9a and 9b or Figures 13 a and

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b, the spatial distribution of whole aerosol plume cannot be shown. Therefore, we decide to keep the figures, but indicate the latitudinal range (equator to 2061616;N) showing in Figure 9b/13b.

42) Figure 9b. Latitude (N)?

Figure 9b is a night-time descending CALIOP flight. The label given in the horizontal axis is latitude (61616;N).

43) Please indicate the line numbers in the new version of the manuscript.

We add the line number in the revised manuscript.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1831, 2009.