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Atmospheric Chemistry and Physics Discussions

Interactive comment on "African biomass burning plumes over the Atlantic: aircraft based measurements and implications for H_2SO_4 and HNO_3 mediated smoke particle activation" *by* V. Fiedler et al.

Anonymous Referee #2

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This paper describes the observation of biomass burning layers above west Africa during the AMMA project in 2006. The data describe the gas and particle phase characteristics of the plume and are used together with a model to predict CCN properties along a forward trajectory as the air advected across the Atlantic Ocean. The results show that H2SO4 in particular, but also HNO3 can deliver sufficient hygroscopic inorganic material to the biomass burning particles to ensure that they are effective CCN, even in BL cloud, after transport over the Atlantic Ocean.

The study is carefully put together and is thorough and informative. I can recommend publication in ACP. However, I have several points on both style and content that I would like to see addressed.

Whilst the study is largely written well, I feel that a number of plots are superfluous and can be combined.

Detailed comments

Page 3 (experiment): The first paragraph only refers to the existence of one plume and the heights referred to in this section do not match those in the abstract or later in the discussion.

Page 3 (experiment): There is no reference in the literature for the HNO3 by this CIMS instrument and no detection limits or accuracies are provided. This is a lynchpin measurement for this paper and as there is no literature to support the measurement elsewhere it is important that it is discussed in this paper.

Page 4: I fail to see what figures 4a and b add to the paper and recommend that the figures and associated text be removed.

Page 4: In the discussion of the influence of smelters: It would also be good to use forward trajectories covering the area of the smelting operations to ascertain whether their emissions could be advected close to the plume locations.

Page 4: Figures 5 and 6 need to be combined.

Page 5: It seems a little odd to introduce the plume identification with SO2 when CO2 data is available and this is commonly used to identify plumes in BB studies.

Page 5: The ascent and descent profiles for SO2 in fig 7 show a vertical offset. Is this a result of spatial and/or temporal changes in the air mass and reflects a slantwise shape to the layer or is it due to instrument response?

Page 5 para 2: Figure 7 is superfluous as 7a is shown in fig 9 and 7b in figure 8.

ACPD

10, C4201–C4205, 2010

Interactive Comment

Page 5: The authors state that particles in the size range 300-110 m were most abundant. This seems quite unusual and suggests that most particles are around the size of maximum scattering cross section. How does the size compare to other studies? This is worth discussing and referencing. Only the Reid et al paper is cited but the size is somewhat smaller than that given here.

Page 6: The vertical profiles show that there is a factor of 5 difference in HNO3 between the ascent and descent legs. The authors point this out but their explanations focus on differences occurring in the plumes. However, the differences pervade at all altitudes over a wide range of concentrations and temperatures. This is in contrast to CO which shows consistency above the layer. The reasoning provided doesn't appear to explain these features. The authors should discuss the agreement over the entire profile and not just the lower layer.

Page 6:

The accumulation mode number concentration is higher in the descent profile by an order of magnitude or more compared to the ascent, how can this be explained?

Page 6 (last para of first column) Changes to the aerosol will not be observed by the phase partitioning of the calculated amount of material in the plume as the existing surface area is large. However, at higher altitudes there remains an order of magnitude difference between the ascent and descent profiles yet the particle numbers are very much lower. Is there sufficient aerosol number to explain the authors hypothesis at these altitudes as well? Would the authors expect to see significant changes in the size distribution here too?

Page 8: The authors conclude that the BB particles are coated with H2SO4 and NH4NO3. There is no way that NH4NO3 can exist as an equilibrium droplet in the presence of H2SO4. If it is present, it must exist as a solid and hence the time history of condensation is important. The authors have made some statements to this effect but they are buried in other discussion. A discussion of the processes necessary to

ACPD

10, C4201–C4205, 2010

Interactive Comment

obtain the hypothesised aerosol for the reader to determine its feasibility and to link this to the back trajectory analysis.

Minor comments

Page 3: "Principally, HNO3 can be detected using. . . .." I would remove principally

Page 3: The first sentence of section 3 doesn't describe the figure correctly and should be revised.

Page 3: "..the southern hemisphere African continent" should be hemispheric

Page 4: "Plotted is the aerosol index AI (measures how much the backscattered UV wavelength of a polluted atmosphere differs from that of a pure atmosphere (a positive AI values means absorbing aerosols)." This needs to be rewritten, I suggest: "Plotted is the aerosol index AI, a product determined from the difference between a backscattered UV wavelength in a polluted atmosphere and a pure atmosphere (a positive AI values means absorbing aerosols)."

Page 4: "The plume of light absorbing particles is present preferably over.." use mainly rather than preferably

Page 4 Provide the latitude and longitude of Ougadougou

Page 4: last point in section 3: The figure shows that the lowest altitude of the advected air mass was over the south American coast and not over the mid Atlantic.

Page 4 section 4: first sentence doesn't scan and needs to be rewritten.

Page 4: ". . . to about 400 nmol/mol" should be pmol/mol

Page 5: How was the top of the MT layer discussion in the first paragraph determined?

Page 5 (bottom). "The local minimum. . . ". This should be the local minimum in the size distribution to be clear. It also implies that new particle formation is suppressed.

Page 6 "downlegs and uplegs" should be descents and ascents

ACPD

10, C4201–C4205, 2010

Interactive Comment

Page 6: “preferably during the climb. . . ” I am unclear what preferably means in this context.

Page 6: There is an order of magnitude difference between the OH estimates calculated using SO2 and CO. This implies an incorrect estimation of emission rate as the authors point out. However, such a large difference is well outside the ranges given for these gases in Andreae and Merlet 2001.

Page 7: The UT plume is introduced for the first time in para 2. This needs to be introduced much earlier.

Page 7: Figure 12 is poorly described in the caption and text. It can, in my opinion be removed, but if kept it needs to be described in full with more clarity than at present.

Page 8: Figure 15a. It would be useful to show the contributions of condensation and coagulation to this plot separately.

Page 9: References are needed to support the supersaturations for different clouds types provided.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7699, 2010.

ACPD

10, C4201–C4205, 2010

Interactive Comment