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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Effect of surface reaction on the cloud nucleating properties of mineral dust: AMMA aircraft campaign in summer 2006" *by* A. Matsuki et al.

**Anonymous Referee #1**

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General Comments:

This study focuses on cloud processing of dust particles during a special observation period of the African Monsoon Multidisciplinary Analysis campaign. The elemental composition and morphology of dried cloud droplets and clear-sky particles is measured and used in conjunction with back-trajectory analysis to improve understanding of atmospheric processing of dust. Measurements of the composition of individual dust particles and cloud-droplet residues as a function of altitude are rare, and so data from this study are valuable. The article falls within the scope of ACPD/ACP and would be a decent contribution with some modifications.



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A few general concerns:

1. The measurements in this study do not support the detailed discussion of the influence of composition on CCN activity presented in section 4.4. This topic could potentially be addressed by comparing simultaneous measurements of the composition of individual interstitial particles and cloud-droplet residues as a function of particle size and cloud supersaturation. In the current study, however, properties of cloud-droplet residues are compared with average properties of clear-sky particles with little size resolution or information on cloud saturation. Since the composition of clear-sky particles differs in layers above and below the clouds (Fig. 3), the clear-sky particles cannot be considered a reliable reference for the particles in the cloud layer. However, the authors suggest that the influence of composition on CCN activity can be determined from compositional differences of the clear-sky particles and cloud-droplet residues. The weakness of this argument is borne out by the inconclusive nature of their findings (e.g., see paragraphs 1 and 2, p. 1812). Section 4.4 should be condensed to reflect the limited conclusions that can be drawn from the comparison of clear-sky particles with cloud residues. Also, the title of the article should not focus on how surface reactions influence the nucleating properties of dust, since this question cannot be addressed in detail. It would be better for the title to refer more generally to cloud processing of dust particles.

2. The authors arguments for the source region of dust being close to the sampling location are unclear and/or unconvincing. For example, I do not understand how vegetation to the south and along the coast of Guinea excludes the possibility of long-range transport of dust to the sampling location (para. 2, p. 1806). Does not the prevalence of dust particles at high altitude suggest that they could have been transported from a long distance? If the dust was not transported a long distance, its prevalence at high altitude might indicate that it was lofted during a convection event close to the source region. However, the authors suggest on p. 1806 (para. 3) that convective events were important only for a limited number of samples. Also, the observations of processed



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dust particles on p. 1809 (para. 2) in the absence of local industries could indicate that the dust was transport from a long distance. In any case, the authors should present clearer and stronger reasoning for their claim of a local source region.

3. A few citations to previous work should be clarified: First, an erratum exists for the Krueger et al. (2004) paper cited on p. 1800. In the revised Table 1 of the erratum, there is a relatively small percentage of calcium in Saharan dust. A related article by Laskin et al. (JGR, 2005, doi:10.1029/2004JD005206) also reports minimal calcium in Saharan dust. The authors should cite the erratum article for the Krueger et al. (2004) paper. Also, citation of an article that supports the claim of high carbonate content for soil in the proposed source region of this study would be helpful. Second, I think that the particle size range predicted by Kelly et al. (2007) and cited on p. 1815 corresponded to a specific supersaturation. That supersaturation should be given along with the particle size range. Third, the Dusek et al. (2006) is not fairly cited on p. 1815, because Dusek et al. (2006) explicitly state (p. 1377 of that article) that composition can dominate size for dust particles.

4. In many places, the authors refer to carbonate particles. Particles that had high calcium content and rounded shapes were assumed to be carbonates in this study. While this assumption has some merit, high calcium content would also be associated with particles from soils containing gypsum. The authors should minimize the use of the word carbonates in this article and instead refer to calcium-rich particles or calcium-rich particles assumed to be carbonates. This distinction is particularly important in the abstract, because readers may get the incorrect impression that low-Z elements (C,O,N) were measured here.

Specific Comments:

p. 1798, line 15 (Abstract): Is sulfate necessarily secondary? Could not sulfate be a primary dust component for soils containing gypsum?

p. 1799, line 23: controversy is probably not the right word here. It is possible that dust



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could enhance precipitation under some conditions and suppress precipitation under other conditions, and so the situation does not seem controversial.

p. 1800, line 4: Is there an article that shows high carbonate content for soil in the source region for this study? If so, it would be helpful to specifically identify it here or elsewhere.

p. 1800: It would be useful to say a few words about the purpose of the AMMA campaign in the introduction.

p. 1801: Please list the specific size ranges of particles and cloud elements that are captured by each inlet and any known uncertainty for these ranges.

p. 1802, lines 4-5: The explanation for particle collection on the impactor stages should be made in terms of Stokes number rather than particle density.

p. 1802, line 17: Why focus on supermicron particles? Large particles would probably activate regardless of composition. To understand the importance of composition on activation, why not focus on somewhat smaller particles?

p. 1802, line 15: What is meant by dry condition? Can you report the RH?

p. 1803, line 16: Please state the relative humidity associated with the image in Figure 2.

p. 1804, line 4: Can you quantify the fraction of cloud residual samples that contained internally mixed sea-salt and dust particles?

p. 1805, lines 11-13: This sentence is a bit confusing. Is the idea that dust aloft was carried from a distance over the Sahel belt, or that dust from the Sahel belt was injected into the air aloft and carried to the sampling locations?

p. 1805, line 29: The word interstitial appears to be used incorrectly here, since regions outside of the cloud are being referred to.



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p. 1806, para. 1: If 10% of the total counts were collected outside of the cloud, and interstitial particles are 10% of the in-cloud counts, I am not sure why the condition on cloud operation time is necessary to make this point.

p. 1812, lines 2-6: This argument is not convincing. It appears that the influence of composition on CCN efficiency is being determined by comparing differences in cloud residue samples for individual days with the average clear-sky samples over the whole sampling period. I do not think the average clear-sky composition is a reliable reference for the in-cloud particle composition.

p. 1812/1813, 1.-3.: It seems to me that there are additional explanations to the ones listed. For example, maybe silicate particles were larger than the carbonates and activated more readily. Or maybe there were more silicate particles in the air mass where the cloud formed.

p. 1823, Table1: It would be useful to list the number of clear-sky and cloud residue counts observed for the categories in the table. Also, group 4 is listed as Calcite and referred to as carbonate throughout the article, but the mineralogy is listed as Calcite or Gypsum in the table. Calcite and gypsum are both present in soils and have very different reactivity. This issue not adequately discussed in the text.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1797, 2009.



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Interactive Comment