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Atmospheric Chemistry and Physics Discussions

Interactive comment on " Effects of particle shape, hematite content and semi-external mixing with carbonaceous components on the optical properties of accumulation mode mineral dust" *by* S. K. Mishra et al.

Anonymous Referee #2

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This paper is concerned with the optical properties of mineral dust aerosol after atmospheric ageing in the presence of pollution. Account is also taken of the hematite content and shape of the dust particles. Based on the assumption that mineral dust and carbonaceous aerosols form aggregate "semi-externally mixed" systems, with the size of the two (or three) combining particles relatively similar, DDA calculations are performed and presented giving new values of single scatter albedo (SSA), asymmetry parameter (g) and extinction efficiency (Q_{ext}).

While the methods and calculations in the paper seem to be fine, I am concerned about the introductory section which provides the justification for the carbon-dust mixing assumptions used throughout. Specifically, equally sized two- and three-sphere (and spheroid) systems, and the formation of black carbon / dust clusters (as shown in Fig. 1). Based solely on the references provided, conclusions stated in the paper are not immediately apparent (see specific comments below) and I would like to see more references explaining the choices made. The semi-external mixing seems only to be justified in the case of organic carbon / dust compounds.

The large percentage differences in optical properties mentioned in the abstract are also slightly misleading, since they compare (or appear to compare) carbon-dust aggregates to pure carbon spherical particles. Given the title of the paper, a comparison with pure mineral dust spheres might be more appropriate, as would comparing a full size-integrated accumulation mode distribution as opposed to single particles.

Finally, there are a very large number of typos, and several sections repeat themselves unnecessarily.

If the assumptions about carbon-dust particles can be more convincingly shown with more appropriate references, then this paper should be accepted subject to major changes to the language and introduction.

Specific comments

1) In the abstract, comparing the three-sphere model shapes to a spherical mineral dust particle with the same hematite content and VER might be more appropriate? Since the paper is interested in the radiative forcing estimation, and the whole mineral dust accumulation mode, the differences in optical properties for a distribution of particle sizes also might be more appropriate?

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2) p31256(7–10): The aerosol direct effect is not *solely* determined "by the optical properties of individual aerosols" as could be inferred by this sentence; e.g. Haywood

& Shine (1995, Geophys. Res. Lett., **22** p603) - surface reflectance can change the sign of the effect for absorbing particles.

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3) The introduction has several statements, which are not supported by the references given. For example:

p31256(2–4) Maria *et al.* (2004) does not describe pollution of mineral dust by carbonaceous components but the oxidation of pre-existing OC ("the organic carbon compounds [. . .] were most likely associated with the dust source and subsequently oxidized on the dust particles during transport."). This reference supports the overall premise that OC is present on mineral dust particles, but in the context of its use here, it is not valid.

p31256(17–18) The first reference (Chandra *et al.*, 2004) deals mainly with the coating of sulfate and sea salt with carbonaceous components and vice versa. Mineral dust is included as an additional external mixture, but not dwelt upon at any point.

p31257(2–7) These are the principle references supporting the semi-external mixing and are used throughout the work to justify choices of particle shape chosen. I do not find them overly convincing.
Li *et al.* (2003) says that "only minor amounts of mineral dust particles occur [. . .] Some are aggregated with sea-salt, and some are coated with ammonium sulfate". There is no mention of semi-external mixtures between dust and carbon.
I could find no mention of mineral dust at all in Alexander *et al.* (2008).
Zongbo *et al.* (2002) (should be Shi *et al.*) says that mineral grains had "trace amounts of soot aggregates adhering onto the surfaces" in the description for Fig. 2a. In the figure, the soot aggregates were so small as to be almost imperceptible; certainly not large enough for the $r_{\text{dust}}/r_{\text{carbon}} \simeq 1$ used later in calculations. This was also true of Clarke *et al.* (2004) Fig. 2.
Takhahama *et al.* (2010) shows mineral dust aggregated to OC and is the most

convincing reference.

p31257(7–8) The authors state that "the semi-external mixing is rarely modeled (Mishchenko et al., 2004)". In this reference's abstract, Mishchenko states: "It is concluded that aggregation is likely to have a relatively weak effect on scattering and radiative properties of two-component tropospheric aerosols and can be replaced by the much simpler external mixture model in remote sensing studies and atmospheric radiation balance computations". As such there would almost certainly be a better references to use in a paragraph justifying calculations involving semi-external mixtures.

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4) I would assume that optical calculations are made at ∼550 nm since refractive indices are given at this wavelength in Table 2?

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5) There is as lot of redundancy between sections 2 & 5 and between sections 3 & 4. These could be presented in a more coherent manner.

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6) Section 5 has unnecessary details such as computer parameters e.g. NCOMP.

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7) What does Fig. 2 show? Surely if the percentage of hematite in the mineral dust is changing, this should have no effect on the overall volume equivalent radius? Isn't the key along the left hand side the same as the z-axis (Rbc/Rdust)? Why does changing the effective radius change Rbc/Rdust?

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8) Figures 3–5: How many calculation points for hematite % and effective radius go into these contour plots?

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9) Figures 6–11: It might be interesting to see differences between new dust models and D-4 as well as absolute values.

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Technical comments

There are a very large number of typos in the paper that will need addressing. Erroneous capital letters are very common and some restructuring of the early sections to improve the flow of the paper would be appreciated.

The reference for Zongbo *et al.* (2005) should be for Shi *et al.* (2005).

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 31253, 2010.