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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of the size-resolved dust emission fluxes measured over a Sahelian source with the Dust Production Model (DPM) predictions" *by* M. Sow et al.

Anonymous Referee #2

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This article tests the Dust Production Model (DPM), which was previously developed by some of the authors, by comparing its predictions to extensive field measurements of dust emission by Sow et al. (2009). Although the article presents some interesting results, the article is insufficiently novel to justify publication in its present form. More detailed comments follow below.

Broad comments:

- This article appears insulated from recent literature and fails to put itself into the proper context by citing related previous work. In fact, the majority of the references are articles by the authors themselves. This problem will need to be corrected by

substantially expanding the cited literature in a possible resubmission. I point out some specific references that need to be cited in the more detailed comments below, and also point out specific relevant findings in the literature that the authors appear to be unaware of.

- The scope of the article is very narrow. Essentially, the central question of the paper is "Is a particular dust emission scheme (the DPM) consistent with a particular set of measurements?" Except for a better description of how best to tune the parameters in the DPM theory, the article thus presents little new knowledge that wasn't already included in the excellent previous article of Sow et al. (ACP, 2009). I therefore consider the present article to be insufficiently novel to warrant publication in a relatively broad journal like ACP. The authors either need to expand the scope of the article to make it appropriate for ACP or send the article to a more specialized journal.

- Related to this previous comment, the authors appear unaware of the existence of other size-resolved dust emission schemes in the literature (for instance by Shao (JGR, 2001 and other articles) and Kok (PNAS, 2011)). In order to both balance the article and put it in the proper context, the authors need to discuss whether these schemes can also describe the measurements satisfactorily. This seems particularly appropriate since there appear to be many discrepancies between the DPM theory and the measurements (as discussed in the text and is evident from figure 5), even after tuning both the lognormal modes and the binding energies. Do these other theories suffer from the same deficiencies? Expanding the article in this manner will also help broaden its scope and make it more appropriate for ACP.

- The article contains several errors (detailed below) that will need to be corrected for a possible resubmission.

Detailed comments:

- Places where references need to be included: - "Among the lifted particles, the smallest ones [. . .] are the most optically active." Please cite a relevant article for this statement, such as Sokolik et al. (1999). - As mentioned above, other (size-resolved) dust emission models need to be discussed in the introduction in order for the article to be placed in its proper context. The authors should clearly explain the strengths and weaknesses of each model, and discuss the differences with the DPM. - "Until quite recently, there was a complete lack of sufficiently detailed field observations. . .". This is no longer correct - please cite the recent size-resolved dust emission measurements of Shao et al. (JGR, 2011) here. - ". . . a sliding average over periods of 15' as required for the calculation of this parameter. . .' (p. 4) and "the calculation of these two parameters involves an averaging of the measurements over periods of 15 mins". Please explain these statements and include relevant citations.

- "The binding energy of the PM20 particles within the soil aggregates is a decreasing function of their size." This statement is inconsistent with basic physics and must be removed. Cohesive binding forces, for example the Vanderwaals force, usually scale with either the first or second power of the particle size (they are surface forces, after all), and thus decrease with particle size. See for example the comprehensive treatment of cohesive forces by Castellanos (Adv. Phys., 54, 263 – 376, 2005) and the classic work by Hamaker (Physica IV, 1937). There is also a brief review in Shao and Lu (2000). What the authors might mean instead is that the behavior of smaller particles is more dominated by the binding energy because the surface to volume ratio increases with decreasing particle size.

- "The only data available [. . .] were non size-resolved mass fluxes" (p. 3). This is incorrect, since Gillette et al. (JGR, 1974) reported size-resolved mass fluxes several decades ago.

- "we analyzed the data collected during 3 different, fully-documented erosion events and their results confirmed for the first time the laboratory finding stipulating that the emission flux was proportionally richer in very fine particles during the strong erosion event than during the moderate ones" (p. 4). I find this statement misleading for two reasons. First, Sow et al. (ACP, 2009) found that the size distribution did not change

substantially with wind speed during a given emission event. There were changes between emission events, but it's unclear what caused this because there might have been changes in soil conditions between the events (the "finer" dust event was a year after the other two events). Second, the recent article of Shao et al. (JGR, 2011) did not find any shift to finer dust with increasing wind speed (see their figure 12 in particular). For these two reasons, it's an overstatement to say that the Sow et al. (2009) measurements "confirmed" that higher wind speeds produce finer aerosols. The authors should explain the caveats I noted and maybe say something to the effect that the Sow et al. measurements "are partially consistent with".

- "the vast majority of the sand grains appear to be quartz grains at the surface of which the very fine PM20 particles are stuck. Because any inter-annual significant change in the size of the quartz grains is unlikely, the size distribution of the sand grains will be assumed to be identical in 2006 and 2007" (p.5). But the cohesion of the smaller grains to the large quartz grains can clearly change due to changes in soil moisture and other conditions. Did the authors account for this?

- "Usually, the models used to simulate saltation do not use instantaneous wind speeds as input parameters but rather the friction velocity whose value is the result of an averaging over periods of at least 15 minutes (see above). Because saltation is a non-linear process whose intensity increases much faster than wind speed, this averaging might lead to an underestimation of the quantitative role played by the very short, but intense, wind peaks" (p.6). The friction velocity quantifies the downward transport of momentum flux through the fluid. It is this momentum flux that drives saltation and dust emission, and u* is thus an accurate quantification of this momentum flux from boundary layer theory. Models of saltation flux and dust flux have been calibrated against measurements of u*, and so the non-linear dependence of the saltation and dust flux on the instantaneous wind speed is inherently included in most models. This statement by the authors should thus either be removed, supported with data or theory, or supported by citing appropriate reference that show this explicitly (which I'm not sure exist).

- I find the explanation for why the binding energies need to be divided by a factor of 2.5 – 5 relative to the wind tunnel experiments not very convincing. Have the authors considered the possibility that the binding energies were different in the field due to higher soil moisture content?

- Figure 4: What are the threshold friction velocities used to fit the curves? These should be listed on the figure. Do they correspond to the measured threshold friction velocities? This seems doubtful for the ME1 event, where the u*t seems to be $\sim$0.32 m/s from the measurements, whereas the fit uses u*t $\sim$ 0.40 m/s. Also, why are the lines squiggly and not smooth? This seems unphysical. Is this a numerical problem?

Technical corrections: - "Entrained" should be "transported" or "advected" in the 6th sentence of the introduction. - Please define the parameters u* and z0. - The comma in Eq. (1) should be a period, and u* should also be devided by sigma squared. - The authors sometimes use commas instead of periods to denote decimal places. - Figure 5: As the authors point out in the main text, the measured dust flux for the first size bin (0.3 – 0.4 um) is elevated above that of the neighboring bins. This is clear from the panels on the left (the volume size distribution), but not from the panels on the right (the number size distribution). It seems to me that this data point was not correctly converted from the volume to the number size distribution.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 11077, 2011.