Atmos. Chem. Phys. Discuss., 11, C5030–C5048, 2011
www.atmos-chem-phys-discuss.net/11/C5030/2011/

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.

M. Sow et al.

sowmomo2003@yahoo.fr

Received and published: 16 June 2011

Dear Editor of ACPD,

First of all we would like to thank the three referees who have not spared their efforts to help us improve our paper. You will find below our point by point answer to their questions and concerns (the original questions appear in brackets). We have also prepared a revised copy of our paper in which the remarks of the reviewers have been taken into account. Best Regards, M. Sow, S.C. Alfaro, and J.L. Rajot

Anonymous Referee #1

[General comments: This study addresses the important dependency of the initial size distribution of emitted dust particles on surface properties and surface wind speed. Simulations of size-resolved dust emission fluxes are performed for three dust events using the Dust Production Model of Alfaro and Gomes (2001). The model is initialized and evaluated with the comprehensive set of field measurements of meteorological parameters, size-resolved dust emission fluxes and the soil size distribution presented in a previous publication by Sow et al. (2009). The model shows promising results in terms of total dust emissions, when a tuning factor is applied. The computation of size-resolved dust emission fluxes requires further model improvement. Alfaro et al. (2004) explained that a tuning factor is needed as "a certain dependence of [: : :] [the kinetic energy of saltating aggregates] to soil characteristics may exist in natural conditions". Here, the authors relate the underestimation of emission fluxes to lower friction velocities resulting from averaging of meteorological parameters. Is it possible that both explanations are true and that the binding energies have to be adapted to each different dust source? This might be also true for the mean diameters of the three populations of released dust particles.]

It is true that many factors could explain the need to change the values of the binding energies from one site to another; in particular the possibility suggested in 2004 by Alfaro et al. that the texture and the mineralogical composition of the soil could influence the value of the binding energies of the small (PM20) particles within the soil-aggregates. In order to check this important point, Alfaro (2008) tested a variety of different natural soils in wind-tunnel experiments and showed that the size-distribution of the fine particles released by the sandblasting process depended on wind speed but not significantly on the compositional and textural characteristics of the soil. This suggested that the mean diameter of the three populations of released dust particles as well as their binding energies might be in large part independent of the soil characteristics.

[If the friction velocities are systematically underestimated, a tuning factor must be also

ACPD
11, C5030–C5048, 2011

Interactive Comment

applied to the saltation flux (lowering the threshold friction velocity for initial particle mobilization).]

In fact, it is not the friction velocity itself which is underestimated. The problem lies in the fact that u* is calculated using long (several minutes) time averages and THIS leads to an underestimation of the effect of the short (but very efficient for erosion) wind peaks. This problem certainly affects saltation and fast response field measurements would be really useful for checking the predictions (intensity and size distribution of the moving soil-aggregates) of the saltation models too. Coming back to the tuning factors, the values determined empirically in this work compensate two effects at the same time: the effect of the underestimation of the instantaneous wind-peaks on saltation and (possibly) on sandblasting.

[Specific comments: Page 11078, Line 12: From a meteorological point of view, I would suggest "less windy" instead of "less energetic" (also later in the text, e.g., page 11093). Line 20: Add "in the DPM" after "than previously assumed". Page 11079, Lines 14–16: You might simplify or split this sentence into smaller parts to increase legibility. Line 24: In order to avoid confusion with model simulations in this study, wind tunnel tests should be referred to as experiments (also later in the text). Line 26: The phrase "finest PM20" is imprecise and inconsistent, give a size range here. Page 11080, Line 3: The comprehensive data set of meteorological and dust flux measurements is actually unique so far. However, the finding that the size of emitted dust particles depends on prevailing atmospheric dynamic conditions is not novel. Page 11083, Line 18: Again, replace "finest PM20" by a size range. Page 11084, Line 7: Are there also inorganic non-erodible elements? Use "etc." instead of ": : :".]

All the editorial changes suggested above have been taken into account in the revised text.

[Lines 14–19: Could the convective conditions interfere with the determination Page 11085, You could try to estimate a range of "gusty" u* on the basis of the wind speeds at 1-min resolution. At least, the range of surface wind speeds would give an idea of possible values of u*. Would that range of u* explain the order of magnitude of the tuning parameter?]

Regarding the possibility of using “gusty” u* values calculated at the resolution of 1’, please read the detailed answer given to one of the questions of Referee#2 (to put it in a nutshell, using the logarithmic wind profile for deriving u* is not possible over so short time periods because this would not fulfill the conditions of applicability of the Prandtl theory. This one requires averaging of the wind measurements other time periods of at least 6 or 7 minutes).

[Page 11087, the terms “horizontal mass flux” and “vertical mass flux” were never introduced before. Readers who are not familiar with this terminology might be confused, in particular as “vertical mass flux” only appears in the header of this section and in line 6 on page 11088. The definitions should be consequently used throughout the text.]

The terms “horizontal mass flux” and “vertical mass flux’ are now defined in the introduction

[Line 17: The explanation for “gsd” is not given until page 11091. The standard deviation was introduced as sigma on page 11083, please unify. Line 27: Correct “: : : have the values proposed by/assumed in Alfaro and Gomes : : : ” Page 11088, Lines 5–9: You should split this sentence into two to increase legibility. Page 11092, Line 17: Does “energetic conditions” mean “high wind conditions”? Line 25: What is the average of gmd defined? Page 11094, Lines 11-14: Restate the sentence. Field measurements do not provide the output of the model, but the basis for model evaluation. Lines 24–28: Split this sentence into at least two separate sentences. Page 11095, Lines 21–25: Split this sentence into at least two separate sentences. Page 11096, The second explanation of the saturation effect was not discussed before in the text. Table 1: Please, change the order of geometric mean diameter and standard deviation. The table caption needs revision: “Number, geometric diameter and standard devia-

ACPD
11, C5030–C5048, 2011

tion of 3 log-normally distributed populations, which represent the dry size distribution of loose soil aggregates at the Banizoumbou (Niger) super site."]

All these remarks have been taken into account.

[Table 3: For a comparison, you could add a column with the values assumed in the DPM by Alfaro and Gomes (2001). "particles/m2/s" use the same units as in the figuresOf z0? (#/cm2/s).]

This has been done.

[Figure 2: In the figure caption, there is no information on the fitted data. There is a significant difference between measured and fitted u* values for [0.7-0.75] and [0.75-0.8]. Which u* was used for dust flux calculation? If the fitted u* values were used, could the underestimation of the last u* bin explain the underestimation of dust emission fluxes?]

For each of the 3 events, the cumulated saltation flux was calculated with the measured values.

[Figure 3: It is difficult to identify, which diagram relates to which period. You should label each diagram and/or add an explanation to the figure caption. Figure labels should be readable from the right.]

This has been fixed

[Figure 4: A figure caption should provide information on what is shown in the figure rather than a repetition of the text. Figure 5: The left and right panels should be labeled, in order to clearly indicate whether a number or mass size distribution of dust flux is shown. What is the unit of u* in the legends.]

The captions of figures 4 and 5 have been modified

[Technical comments: Page 11078, Line 15: Add a comma after "In all the studied cases". Page 11080, Line 13: Correct "mass flux". Line 26: Spaces are missing between figures and units. Page 11081, Line 22: Correct "meet/satisfy these conditions".

ACPD

11, C5030–C5048, 2011

Page 11082, Line 23: A space is missing between figure and unit. Page 11083, Line 14: A space is missing between figure and unit. Line 17/18: Correct "appears" and "surface on which". Line 21: Omit "those". Page 11087, Line 11: Correct "0.5". Page 11088, Lines 5–9: Insert "is" between "beta" and "not fixed" Page 11089, Line 23: Delete "of" between "or" and "in terms of". Restate: ": : : a unit of mass released per second and square meter, : : :". Page 11091, Line 23: Omit " is tantamount to saying that it". Page 11094, Line 6: Delete "-" after "(CE4)". Page 11095, Line 27: Insert "the" before "observations". Page 11096, Line 13: Correct "for its support".]

All these technical changes have been made

Anonymous Referee #2

[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.]

One of the main reasons why we designed and performed the Banizoumbou field experiment was to test for the first time the ability of the DPM to reproduce some measurements made in natural conditions. Indeed, we believed that this kind of comparison could be of interest for the numerous modelers worldwide who had implemented the saltation (Marticorena and Bergametti, 1995) and the sandblasting (Alfaro and Gomes, 2001) schemes in their models of the dust cycle. Naturally, this original objective conditioned the measurements made on the field, which means that we do not necessarily have the data necessary for running other emission models. Note that the comparisons of size-resolved emission fluxes measured directly on the field with model outputs are also rather scarce in the literature and generally dedicated to the validation of a single model For instance, the very interesting work of Shao et al (2011) comparing the original measurements performed in the frame of JADE with the predictions of the model proposed by Shao et al. (2001) is quite recent. We cite this work in the revised version of our paper (it was not possible to do so in the earlier version because Shao et al. was accepted in JGR only after we had sent our own contribution to ACPD). More generally, we are convinced that initiating collaborations with the developers of the other geophysically-realistic dust emission models would be important for performing intercomparison exercises in which the pros and cons of each production scheme would be

ACPD
11, C5030–C5048, 2011

analyzed.

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

See our detailed answers below.

[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).]

The fact that particles with diameters in the same order of magnitude as the wavelengths of solar radiation are the most efficient scatterers is a direct consequence of Mie theory. We now quote this old work (Mie, 1908). We also quote the article of Sokolik and Toon (1999) in the revised manuscript.

[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.]

Comparing the strengths and weaknesses of each existing model is not within the scope of this work focused on the DPM. However, we repeat here our interest in a dust production model inter-comparison exercise which could be carried out in the frame of a future collaboration.

[“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.]

This recent work is cited in the revised version of our paper

[- “: : : 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

ACPD

11, C5030–C5048, 2011

Interactive Comment

of the measurements over periods of 15 mins". Please explain these statements and include relevant citations.]

The detailed answer to this remark is given a few paragraphs below (in the question about the relevance of u* as a quantifier of the momentum flux)

[- "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 particles present in soil aggregates do not only differ in size, they also differ in mineralogical composition. Moreover, size and composition are not independent (Schroeder and Blum, 1992). Indeed, the coarsest particles are dominated by quartz-like minerals whose crystalline structure is electrically neutral whereas the smallest particles (those with sizes <2μm) are essentially clay minerals. Most of these minerals have a sheet structure in which the original cations are replaced by other cations of smaller electrical charge (typically, Si4+ can be replaced by Al3+, or Al3+ by Mg 2+). These generalized substitutions lead to negatively charged minerals. In particular, this is the case of smectite, which tends to form very small, poorly-crystallized particles, and to a lesser extent of illite. In the case of Kaolinite, the sheets themselves are generally neutral but negative electrical charges appear at the limits of the crystal. The charge/volume ratio for Kaolinite is less than for the other clay minerals but increases as size decreases because of the growing relative importance of the crystal limits. Globally, depending on their composition the electrical charge of clay minerals

ACPD

11, C5030–C5048, 2011

Interactive Comment

varies between 0.10 and 1.50 mole of negative charges per kg of clay. In summary, because of differences in mineralogical composition, relatively coarse soil particles have smaller charges than those with diameters<2μm. Among these clay particles, the smallest ones (mostly smectite with diameters <0.5μm,) have greater charge/mass ratios than the others. This leads to a general strengthening of the inter-particle bonds with decreasing particle size which is faster than predicted by 'basic physics' on the basis of the surface to volume ratio alone.

Schroeder D. und W.E.H. Blum, Bodenkunde in Stichworten. 5., rev. und erw. Aufl., Hirt, Berlin (u.a.) 1992, ISBN 3-443-03103-X.

[- "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.]

Right, this work is now cited.

["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".]

ACPD
11, C5030–C5048, 2011

Interactive Comment

The size-resolved emission fluxes measured by Sow et al. were obtained using two cross-calibrated optical counters located at 2 different heights in the surface boundary layer. Great care was also taken to design and use particle samplers whose cut-off sizes were much above 20μm, thus ensuring that the size-distributions were not artificially biased by collection problems. Among the numerous erosion events sampled on the field, only 3 were retained after a quite rigorous data quality checking. When comparing the number size-distributions of the three emission fluxes the enrichment in submicron particles is clearly observed during the convective event. There are at least two possible reasons explaining why Shao et al. (2011) did not observe the same enrichment:

- The first reason is that during their erosion events observed on the field, the value of u* did not exceed 0.55 m/s which is the same order of magnitude of for our 2 moderates monsoon events ME1 and ME4 (with maximum u* values of 0.50 and 0.60 m/s respectively). Even if Sow al. (2009) found that ME4 was somewhat (2 times) richer in particles with diameter <2 μm than ME1, the most spectacular enrichment (10 times) in very fine particles was observed when u* reached 0.80 m/s during the convective event CE4. - - The second reason is that the enrichment in very fine particles could not have been detected as easily if only the MASS size-distributions had been considered because the fraction represented by the submicron particles in these distributions is very small. In other words, we believe that if the data of Shao et al. (2011) do not seem to indicate any shift towards smaller sizes during the most energetic events this might be due simply to the fact that these authors use mass size-distributions instead of number size-distributions. Regarding this point, it is interesting to note that all the recent studies who have highlighted the presence of a submicronic (0.1-0.5 μm) mode of alumino-silicate particles in the dust emission flux (Chou et al., 2008; Osborne et al., 2008; Kandler et al., 2009, Weinzierl et al., 2009, Formenti al. 2011) were using number rather than mass size-distributions. All these arguments are now given in the revised manuscript.

[- “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?]

The effect of humidity is indeed a very complex one. It is well known that in the case of fine textured soils such as those studied by Gomes et al. (2003) in northern Spain or by Ishizuka et al. (2008) in Australia humidity tends to favor the formation of more or less resistant crusts whose direct effect is to hinder the development of saltation. The type of physical crust that develops on sandy soils such as those of the Banizoumbou area is completely different (Valentin and Bresson, 1992). In the latter case Rajot et al., (2003) showed that the formation of the crust does not inhibit saltation because the cohesive part of the crust forms underneath a relatively thick layer of loose quartz grains always available for saltation. Noteworthy is that the same authors also showed that the ratio of the vertical flux of PM20 to the saltation flux (also called the ‘sand-blasting efficiency’) measured on a Banizoumbou agricultural field remained the same before and after a rain event (see fig. 8 in the cited reference). The fact that the development of the physical crust typical of sandy soils does not limit the sandblasting efficiency suggests that the binding energy of the PM20 within the soil-aggregates is not larger after a rain event than before. Note that this result is also consistent with the work of Gomes et al. (2003) who found that although the crusting of the surface of a fine-textured Spanish soil after a rain episode strongly limited the intensity of saltation, the sandblasting efficiency was in the same order of magnitude as the one measured over a sandy un-crusted Niger soil. In summary, there is no doubt that humidity favors the formation of bonds between sand-sized grains in soils with large clay or silt contents, which leads to a limitation of saltation and, consequently of the emission of PM20 particles. Conversely, in the case of sandy soils such as those of the Banizoumbou site, crusting does not inhibit saltation. Apart from this potential effect on saltation, observations suggest that the cohesion of the smaller grains to the large quartz grains does not change due to changes in soil moisture and other conditions.

[- “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.]

We fully agree that the momentum flux drives saltation and dust emission. However, as demonstrated by the intermittency of saltation observed on the field (see for instance the excellent work of J. Stout, 1998, on the subject) the instantaneous value of this flux, or equivalently of the stress exerted on the surface by the moving air, fluctuates rapidly in natural conditions. In fact the stress fluctuates at the same high frequencies as does the turbulence produced locally in the surface boundary layer. However, the determination of u* relies most of the time on more or less sophisticated versions of Prandtl’s theory -this is what we do when we derive u* and Z0 from the wind and temperature profiles measured on the field in Banizoumbou. This is also what Gillette did in his pioneering work, or what Shao et al. still do in the frame of the JADE experiment. The problem with this method is that it requires that the conditions of applicability of the Prandtl theory are met, which is not always simple. In particular, ‘the most essential part of Prantl-layer theory is the hypothesis of stationarity. . .’ (Zdunkowski and Bott, 2003). This means that von Karman’s equation can only be applied at time scales large enough for the fluctuations of local turbulence to be smoothed out by the averaging. The question of how long the averaging time should be is a difficult one but empirically, it has long been considered that an averaging time of 15 to 20’ was necessary. This

ACPD

11, C5030–C5048, 2011

rule of thumb has been confirmed recently by the work of Voronovich and Kiely (2007) who showed in their analysis of field measurements that the 'spectral gap' separating turbulence from larger-scale (e.g., synoptic) fluctuations depended only weakly on air stability and was located between 250 and 1100s (6 to 20'). Moreover, these authors demonstrated that the results of the momentum flux calculation were insensitive to the averaging time provided it was chosen between 7 and 109 min. This not only validates a posteriori our choice of 15', but also demonstrates that deriving u* values from 1 minute wind profiles would have been physically unsound. We have added a paragraph in our paper reminding this point.

[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).] The most popular equations of the saltation flux have been derived from wind-tunnel measurements (for a review, see Greeley and Iversen, 1995). This is in particular the case of White's (1981) equation on which the saltation model of Marticorena and Bergametti (1995) is based. In these experiments, wind speed is tunable but remains fixed for the duration of the measurements. In these conditions, the turbulence is necessarily less complex than the one existing on the field. Therefore, there is a possibility that the non-linear dependency to u* of the saltation flux derived from the laboratory experiments might be different from the one observed in natural conditions. Regarding this point also, we agree with the reviewer that field work is still necessary.

[- 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?]

See our answer to the same question above

ACPD
11, C5030–C5048, 2011

[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 _0.32 m/s from the measurements, whereas the fit uses u*t _ 0.40 m/s. Also, why are the lines squiggly and not smooth? This seems unphysical. Is this a numerical problem?]

As a matter of fact the threshold velocities are not assumed but calculated with the parameterization of Alfaro and Gomes (1995) using the soil dry size distribution and the average value of Z0 as input parameters. What happens in the case of ME1 is that the wind direction changed at the beginning of the event, inducing in turn an increase in soil aerodynamic roughness leading itself to an increase in threshold velocity.

[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.]

These changes have been made

[- 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.]

In fact, the left panel corresponds to the number size distribution and the right one to the volume ones. So the conversion is correct.

New references cited: Chou, C., Formenti, P., Maille, M., Ausset, P., Helas, G., Harrison, M., and Osborne, S.: Size distribution, shape, and composition of mineral dust aerosols collected during the African Monsoon multidisciplinary analysis special observation period 0: dust and biomass15 burning experiment field campaign in Niger,

ACPD
11, C5030–C5048, 2011

Interactive Comment

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

January 2006, J. Geophys. Res., 113, D00C10,doi:10.1029/2008jd009897, 2008.

Formenti, P., Schuetz, L., Balkanski, Y., Desboeufs, K., Ebert, M., Kandler, K. Petzold, A., Scheuvens, D., Weinbruch, S., and Zhang, D.: Recent progress in understanding physical and chemical properties of mineral dust, Atmos. Chem. Phys. Discuss., 10, 31187–31251, doi:10.5194/acpd-10-31187-2010, 2010.

Greeley, R., Iversen, J., 1985, Wind as a Geological Process on Earth, Mars, Venus and Titan, 333 pp, Cambridge Planetary Sciences Series, Cambridge University Press.

Kandler, K., Benker, N., Bundke, U., Cuevas, E., Ebert, M., Knippertz, P., Rodriguez, S., Schutz, L., and Weinbruch, S.:, Chemical composition and complex refractive index of Saharan mineral dust at Izana, Tenerife (Spain) derived by electron microscopy, Atmos. Environ., 20 41, 8058–8074, 2007.

Osborne, S. R., Johnson, B. T., Haywood, J. M., Baran, A. J., Harrison, M. A. J., and Mc- Connell, C. L.: Physical and optical properties of mineral dust aerosol during the dust and biomass-burning experiment, J. Geophys. Res., 113, D00C03, doi:10.1029/2007jd009551, 2008.

Mie, G.,Beiträge zur Optik trüber Medien, speziell kolloidaler Metallösungen, Annalen der Physik, Vol. 330, Issue 3, pp377–445, 1908.

Rajot J.L., S.C. Alfaro, L. Gomes, and A. Gaudichet, Influence of sandy soil crusting on horizontal and vertical wind erosion fluxes, Catena, vol 53/1, 1 – 16, 2003.
Shao, Y., Ishizuka, M. Mikami, M., and Leys, J.F., Parameterization of size-resolved dust emission and validation with measurements, J. Geophys. Res., 116, D08203, doi:10.1029/2010JD014527, 2011.

Stout, J. E.: Effect of averaging time on the apparent threshold for aeolian transport, J. Arid Environ., 39, 395–401, 1998.

Voronovich V. and G. Kiely, On the gap in the spectra of surface-layer atmospheric turbulence, Boundary-Layer Meteorology, Vol. 122, 1, 67-83, doi: 10.1007/s10546-

ACPD

11, C5030–C5048, 2011

Interactive Comment

006-9108-y, 2006.

Weinzierl, B., Petzold, A., Esselborn, M., Wirth, M., Rasp, K., Kandler, K., SchÂĺ utz, L., Koepke, P., and Fiebig, M.: Airborne measurements of dust layer properties, particle size distribution and mixing state of Saharan dust during SAMUM 2006, Tellus B, 61, 96–117, doi:10.1111/j.1600-0889.2008.00392, 2009.

Zdunkowski W. and A. Bott, Dynamics of the Atmosphere: A Course in Theoretical Meteorology, 719pp, Cambridge University Press, New York, 2003.

___________________________________ Anonymous Referee #3 [The article is an interesting assessment of a dust particle emission model compared with detailed observations and should be published. However several changes should be made to the article before publishing. The paper should provide more linkages to the available literature, not just focusing on work done by the group authoring this article. A review of alternative views of the size distribution, as well as the relevance of the question should be considered in the introduction.]

This remark also made by reviewer#2 is addressed in the revised version of the paper.

[In addition the paper identifies some problems with an existing model in the larger size distribution, but do not suggest solutions: it would seem that some sensitivity tests should be done, or use of other models of the emissions to check and see if other theories might represent what is going on here better.]

As indicated previously, we believe that comparing the predictions of all the existing models with the (few) available field measurements would be an interesting idea for setting up a collaboration project between the different groups developing dust emission schemes worldwide.

[A main conclusions is: From abstract ”We explain this need to reduce the binding energies by an underestimation of the wind velocity due to the averaging over periods of 15’ required by the calculation of the wind friction velocity.” This is really important,

ACPD
11, C5030–C5048, 2011

but not really justified very clearly.]

We have modified the sentence in order to make this point clearer.

[1. Are you sure you can't get more information about the U* from the data, or about the distribution of the u* from your data? Convince us of this: "As described in more detail in Sow et al. (2009), a value of uâ′L◦U and z0 is available for each minute of the events but the calculation of these two parameters involves an averaging of the measurements over periods of 15min." This averaging time turns out to be argued very strong to be a problem with the method, so please discuss again in more detail, perhaps in the methods section WHY you MUST average over 15 minutes, and no smaller time period can be used, and NO information about the distribution of winds over the shorter time periods can also be extracted. Pg. 11085; distribution of u*: can you use these distributions to correct your method and extrapolate to smaller time scales? Or are these assumptions wrong, and that's why beta has to be changed? "Conversely, wind speed fluctuates rapidly on the field and, due to the smoothing effect of the averaging over durations of 15 min, the experimental values of uâ′L◦U underestimate the effect of the largest wind values achieved during the averaging period. In order to counterbalance this misrepresentation of the most efficient wind speeds by uâ′L◦U, the values of the binding energies must be artificially reduced (i.e., divided by a _ > 1) for the model to remain able to reproduce the observed emission intensities at their real level." Can't you incorporate a better distribution of u* to see if it corrects this problem?]

For the need to average over periods of several minutes when using u*, please see the detailed answer given to one of the questions of Referee#2 (conditions of applicability of the Prandtl theory).

[2. If you think the problem is the wind distribution, don't change the binding energies, but rather your assumptions about the distribution of the winds.]

Fundamentally, the problem is due to the simplification of the physics required by most operational models who use u* as an input parameter. In fact, because of their fast

ACPD
11, C5030–C5048, 2011

response times (of the order of the second) erosion processes are rather driven by the rapid fluctuations of the wind stress than by longer time averages. Therefore, instantaneous wind speed would probably be a more appropriate parameter for quantifying erosion fluxes than u*. Note that some models working with wind statistical distributions of the Weibull type use pseudo-instantaneous quantities as input parameters but they are still rare.

[3. Are you sure it is not the binding energies or the model configuration that is the problem? Maybe a little more consideration of other sources of error.]

Physically, the binding energies are soil aggregate characteristics and cannot depend on the wind intensity. Regarding the model configuration, we agree that the scheme describing the pace at which the modes of PM20 are released must be revised. This is even one of our main conclusions. Unfortunately, accumulating more experimental data will be necessary before a new reliable scheme can be proposed.

["The rhythm at which the three modes of particles are released also needs to be revised in the model." I'm sure rhythm is not the right word, but I'm not sure of the meaning so I can't correct. There are many English errors: I list a few here: The 3 erosion events reported previously by Sow et al. (2009) respect these conditions." Replace 'respect' with 'fulfil'. "As reminded above, the wind friction velocity uâ′L°U and the aerodynamic roughness length z0 are derived simultaneously from the analysis of the wind and temperature profiles 5 monitored during the 3 erosion events." Replace 'reminded' with 'discussed'. "Nonetheless, when using the results of masse fluxes measurements performed over a variety of bare agricul- tural surfaces located either in the south-western part of the USA (Nickling and Gillies, 15 1989), in northern Spain (Gomes et al., 2003), or in Niger (Rajot et al., 2003), Alfaro et al. (2004)": masse should be mass]

These corrections have been made

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