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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 #3

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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.

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.

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, but not really justified very clearly.

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 uâĹŮ 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.

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.

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 uâĹŮ 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 uâĹŮ, the values of the binding energies must be artificially reduced (i.e., divided by a $\beta > 1$) 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?

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.

“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 uâĹů 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

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