

Interactive
Comment

***Interactive comment on “Stochastic
parameterization of dust emission and application
to convective atmospheric conditions” by
M. Klose and Y. Shao***

Anonymous Referee #1

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The authors present an original and very interesting contribution. The paper is well written and of high quality. It is appropriate to be published.

Some minor comments may be considered:

The abstract could/should better stress: What is new? What is the advantage of the new approach? ...compared to other, published (well established) schemes.

The authors may want to cite the recent overview paper of Knippertz and Todd, Rev. of Geophys.

It would be of advantage to distinguish more clearly between dry PBL convection and

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deep moist convection.

It is not clear to me whether the model approach is now applicable to dust devils or not? Can one also use such an approach to detect dust devils and convective plumes (e.g., by analyzing meteorological fields of temp, wind, pressure) and to quantify their impact on dust emission? Or, at least to see whether meteorological conditions are favorable or not?

At many places, more details and descriptions (in the derivation steps) would facilitate reading.

What is missing is a more extended comparison with literature values and results (obtained with other implemented, established schemes), and this not only for low winds.

Furthermore, a rough estimation (quantification) of the impact of this dry convection process (emission) should be given, i.e., what is the relative contribution to total dust emission. Renno stated a contribution of 35% by convective plumes and vorticities to the global budget. Does your study corroborate that?

It would be interesting to see (discuss) the size distribution of the emitted dust and sand. It is reported in the literature that not only dust but also sand particles (diameters > 60 microns) are lifted by dust devils. Is that true? What do your simulations show here?

Section 3.1: Can you provide more details regarding . . . cohesive forces can be treated as a stochastic variable. . . . Are the data of Zimon (1982) representative. I did not read that paper. What about chemical binding forces, they also show a stochastic behavior.

Section 4.2.1: When integrating the lidar data from ground to, e.g., 10 km height, what about the lowermost 1 or 2 km, where the lidar is not really trustworthy (overlap problems).

What about co-authorship of the Japanese lidar people. They contributed to an important figure (Figure 6). The problem is probably that this is a two-author paper.

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 3263, 2012.

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12, C751–C753, 2012

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