Atmos. Chem. Phys. Discuss., 7, S342–S345, 2007
www.atmos-chem-phys-discuss.net/7/S342/2007/

Atmospheric Chemistry and Physics Discussions

ACPD

7, S342–S345, 2007

Interactive Comment

Interactive comment on "Size-segregated fluxes of mineral dust from a desert area of northern China by eddy covariance" *by* G. Fratini et al.

Anonymous Referee #1

Received and published: 28 February 2007

Fratini et al. describe the development and application of an experimental method to measure size-resolved mineral dust fluxes from a desert area in China based on eddy covariance. Such measurements are extremely useful to validate and improve current parameterizations of size-dependent particle transport and investigate particle emission processes due to wind erosion. The authors give a nice overview of recent work on mineral dust emissions and an excellent background on theoretical constraints of the eddy covariance approach. The introduction of the measurement system and of the investigated sites is followed by an interesting selection of measurement results focusing on a dust storm event. To date, the development of eddy covariance systems for the direct measurement of size-resolved aerosol fluxes is very challenging and this work is a valuable contribution to the efforts in this field of research. The paper is clearly structured and well suited for publication in ACP after addressing the following issues:

The authors introduce a new instrument to be used in eddy covariance measurements of aerosol fluxes. This should include an evaluation of the basic system performance (e.g., time response) and the measurement uncertainties (e.g., counting statistics):

In my opinion, it would strengthen the manuscript if the authors give some more information about the system performance. Specifically, the following issues should be addressed: What was the time response of the system? Was it sufficient for independent data acquisition at 5 Hz? From the information given in the manuscript, the flow in the sampling lines was laminar. How did this contribute to signal damping? Have particle losses in the sampling lines been taken into account?

The authors make several qualitative remarks about the accuracy of the measurements. My suggestion is to include a somewhat more quantitative evaluation of the measurement uncertainties providing estimates of how reliable the size-resolved measurements really are:

At a flow rate of 1.42 l min^{-1} (or 23.7 cm^3 s^{-1}), an acquisition frequency of 5 Hz, and a 1:20 dilution, roughly 0.25 cm^3 are sampled in each acquisition step. At typical concentrations of 100 particles cm^{-3} (as given in Fig. 3) this corresponds to 25 particles counted during each acquisition step. Even during the storm event, the total particle numbers peak at 1000 cm^{-3} corresponding to 250 particles per 5 Hz interval. If distributed in 18 size bins (e.g., Fig. 9a), most size bins will contain only very few particles and the concentration acquired at 5 Hz will be very sensitive to the presence or absence of individual particles. The uncertainty in measuring concentration can be estimated as $N_i{}^{-1/2}$ (where N_i is the particle number during one acquisition step). It could be reduced by aggregating size bins and - as suggested by the authors - avoiding the dilution step. The uncertainty in the flux measurement due to the uncertainty in measuring particle concentration can be estimated by

ACPD

7, S342–S345, 2007

EGU

$$\frac{\sigma_w \cdot c}{\sqrt{N}}$$

(σ_w, standard deviation of the vertical wind component; c, average particle concentration; N, total number of particles counted during averaging interval; cf. Buzorius et al., 2003, J. Aerosol Sci. 34, 747-764). With this in mind, are the counting statistics good enough to yield reasonable estimates of size-resolved fluxes as presented for example in Fig. 9a? Please include error bars for your particle flux results or give at least typical values of the measurement uncertainty that may be expected.

Some additional comments:

In Figs. 5, 9, and 10 you present particle volume, whereas in Figs. 4 and 6 you present particle mass. In my opinion, it would be useful to consistently present particle mass in all of these figures. Especially, the relations between particle fluxes in different size ranges (Fig. 9), as well as particle fluxes and the friction velocity (Fig. 10) may be even more interesting if your particle density measurements were included to yield particle mass emissions.

On p. 2139, Eq. 9 is obtained from Eq. 8 "if atmospheric stationarity and horizontal homogeneity are achieved". How was atmospheric stationarity evaluated during the storm event?

I could not entirely follow the paragraph that evaluates the effect of gravitational settling (pp. 2140-2141). How do you arrive at the settling velocities for optical particle diameters of 1 and 7 μm presented in the text?

In section 3.1, no reference is made to Fig. 1a and the abbreviation EOLO used in the figure caption (and elsewhere) is not explained in the text. Are both Fig. 1a and b necessary?

In the discussion of Fig. 8 on p. 2148, flux "values as high as 3x10^4 particles cm^{-2}

ACPD
7, S342–S345, 2007

Interactive Comment

EGU

s^{-1}" are mentioned. However, the highest flux value reported in Fig. 8 is approximately $2x10^3$ particles cm^{-2} s^{-1}. Please clarify!

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 2133, 2007.

ACPD

7, S342–S345, 2007

Interactive Comment