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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Dependency of Particle Size Distribution at Dust Emission on Friction Velocity and Atmospheric Boundary-Layer Stability" *by* Yaping Shao et al.

Yaping Shao et al.

zhang-j@lzu.edu.cn

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We greatly appreciate Colleague Dr. J. Kok for his comments. That Dr. Kok took time to provide such thoughtful comments shows the need to clarify the dust PSD issue. We have not discussed in great details yet, but will simply provide a quick reply.

First, "Airborne PSD as emitted dust PSD": to our best knowledge, dust emission PSD has been directly observed. All dust emission PSDs reported are airborne dust PSDs. We welcome our colleagues to correct us, if we are wrong. The JADE airborne dust PSDs are of good quality and are probably close(r) to dust emission PSD.

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Discussion paper

The argument that dust advection depends on u* is interesting, but does not seem to apply here. Advection is $\sim u\partial C/\partial x \sim u_* \partial C/\partial x$. In case of weak dust concentration gradient, advection does not play a major role. The JADE site is fairly homogeneous and the dust PSDs are measured close to the surface. Therefore, we can safely exclude the influence of advection on dust PSD.

Second, "Consistency of Evidence": Fig. 12 of Shao et al. (2011) was included at recommendation of a referee. It may be that, in that analysis, too much averaging and too small u* intervals blurred the dependency of dust PDS on u* and ABL stability. We did not carefully examine the individual runs as we are doing now. Colleague Dr. Kok and others have made an excellent suggestion, we do need to have a look at the statistical significance of the results.

Statistical significance test is generally lacking in dust related studies and this is partially why we have so much confusion in aeolian research.

Third, earlier results: I like this suggestion of Dr. Kok very much. But, to be honest, this is difficult, as it is hard to get to the bottom of the various data sets. I believe Dr. Kok and colleagues have properly estimated the error margins of the previously published data and the averages may pointing to "universal dust PSD". However, I still think, to better understand the physics, we need to do case studies.

Fourth, statistics: This is a very good suggestion.

5th Line 30-32: "Since inter-particle cohesion depends on particle size, d, the fraction of dust emitted must also depend on d. Thus, for a given soil, the particle size distribution of dust at emission (emission-dust PSD), ps(d), must depend on saltation bombardment or on friction velocity" and line 140-1 "u* is a descriptor of saltation bombardment intensity". This argument implicitly assumes that the impact speed of saltating particles increases with the friction velocity. It is highly intuitive that it would, but there is a very solid body of research that indicates that particle impact speed actually does not depend on friction velocity for transport-limited saltation. This lack of

dependence of particle speed on wind speed was first proposed by Ungar and Haff (1987) because particle-wind feedbacks force an approximately constant saltator impact speed. It has since been confirmed by a large body of experimental (e.g., Namikas (2003), Rasmussen and Sorensen (2008), Creysells et al. (2009), Ho et al., (2011), Martin and Kok (2017)) and numerical (e.g., Duran et al. (2011), Kok et al. (2012)) work. The authors can of course present evidence to support their viewpoint counter to this literature, but I recommend acknowledging this extensive literature.

This is interesting. Let us make two thinking experiments. Exp 1: u* = u*t, particle creeps and has impact velocity 0. Exp 2: u* > u*t, particle saltates and has impact velocity larger than 0. This shows particle impact depends on u*. But thanks for pointing out the study which conclude differently. We will have to learn how it is possible that impact is u* independent.

6th Line 48-9: "Kok (2011a, 2011b) then proposed an emission-dust PSD and estimated its parameters from airborne-dust PSDs." That's actually not quite correct: Kok (2011a) only used emitted dust size distribution because airborne-dust PSDs are a convoluted sum of emission and advection (see comment above and by Sylvain Dupont). Also, the years on the references are incorrect (I corrected them in the quote above).

Thanks. We will check this.

7th I'm a bit confused how to interpret the 0-0.25 m/s u* category in the present paper's Figure 3, as this would include events without saltation where dust is not actively emitted but only advected. I suspect the authors are only using data for which saltation was occurring. If so, I recommend that the authors note that. And if not, I recommend the authors subset the data to only include active saltation data.

Saltation is intermittent and occurs below 0.25 m/s u*. This is a point we try to make, namely, turbulence (and saltation intermittency) plays an important role in dust PSD. It seems that this point did not come cross clearly, as this also appears to be the

impression of colleague Dr. Dupont.

Many thanks to Colleague Dr. J. Kok.

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