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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.

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General comments: The size distribution (emission PSD) of the dust particles emitted from arid and semi-arid surfaces is crucial for the assessment of their numerous environmental impacts. However, this question has been much debated in the last two decades. Some experimental and theoretical studies emphasized the importance of the kinetic energy (increasing with the speed of the wind) of the saltating sand grains for this PSD, whereas others maintained that the PSD was constant. In addition, a quite recent study also showed that the stability of the atmosphere played a role: the more unstable the lower atmosphere, the richer the PSD in fine particles. The aim

of the present study is to re-analyze the data of the JADE experiment carried out in Australia for answering the following questions: 1) does the emission PSD depend on wind speed or not? 2) is it confirmed that the atmospheric stability plays a part?, and 3) why? For answering these questions, the authors select two contrasted events of the JADE field campaign: one that occurred during a hot day (Event 10) with unstable conditions, and another (Event 11) that happened immediately after but in stable nocturnal conditions. Then, they analyze separately the influence of 1) u* (and its probability distribution), and w* (a proxy of atmospheric stability) on the emission PSD. My opinion is that the demonstration of the authors is quite convincing, well organized, and their methods are scientifically sound. Finally, the study brings final answers to ancient questions: yes, the emission PSD does depend on wind speed (through the kinetic energy of the saltators), and yes atmospheric instability promotes the ejection of finer particles (because of the widening og the u* distribution as turbulence increases). In summary, this paper is a very important one and undoubtedly deserve publication in ACP after the few following comments have been addressed:

Response: We are most grateful to Referee 1 for the encouraging comments and constructive suggestions. Referee pointed out the usefulness of the study in clarifying the issue of particle size dependency on wind shear stress and possibly atmospheric boundary layer stability. There are several suggestions we will consider and modify the text accordingly.

Comments and minor concerns:

1) P. 1, line 26: F increases with Q, but it is not simply proportional to it. For a given surface, the so-called 'sandblasting-efficiency' (ratio of F to Q) is usually found to vary with u*.

Response: We will check this. Existing data do suggest that Q∼u* cubed, but F∼u* to power n and n is not necessary 3. This issue has been clarified in several earlier studies by the first author, together with Dr. Hua Lu, but we will modify the text to be

more precise.

2) P. 2, lines 45 and 48; correct the years for Kok's publications.

Response: Sorry about this. We will check and correct.

3) P. 2, line 62: in Ishizuka et al. (2008) and in this paper, u* is calculated over periods of 1'. This is too short to integrate the time-scales of turbulence in the surface layer (Dupont el al., 2019), but presents the advantage of following more closely the variations of the instantaneous wind speed to which saltation responds in quasi real-time. Conversely, Khalfallah et al. (2020) calculated u* over longer periods of 16'. The smoothening of the u* statistical distribution resulting from this averaging probably explains that they could not detect any notable influence of u* on the emission PSD.

Response: We would agree with the referee. For boundary-layer studies, to establish a flux-gradient relationship (here shear stress and mean wind), averaging over 1 min would not be enough. But for saltation, 1 min average gives the advantage to examine the fluctuations of saltation and dust emission. It is indeed important to have another look at this problem and see whether the comparison of this study and that of Khalfallah et al (2020) is a fair comparison.

4) P.3 line 64: JADE (not JADA)

Response: Thanks, we will correct.

5) P.4, line 88: In sedimentology, the texture of a soil is defined from the size-distribution of its particles after full dispersion. So, the soil is sandy loam.

Response: This is a good point. It seems that we need to have a look in the study of sedimentology. We have misunderstood this (may be for years).

6) P.6: Figure 4a. In the insert, the same color code as in the rest of the figure could be used to differentiate Event 10 from Event 11.

Response: Thanks. We will check this.

7) P.6; line144: 'do not substantially differ'. What does this mean?

Response: Indeed, this is a bit sloppy. We will try to be more precise.

8) P.8; line 177: what are the implications of the fact that the distribution of τ is skewed to smaller values?

Response: We will add a line here to be more specific. It seems to suggest that the LES results of Klose et al. (2014) seem to be qualitative reasonable.

9) P. 10, line 208: experiments 'were'

Response: Thanks. We will correct it.

10) P. 113, lines 229-235: Please rewrite this part to avoid repetitions and clumsy formulations.

Response: Thanks. We will try to improve.

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