Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-425-RC2, 2020

Atmospheric Chemistry and Physics
Discussions
Open Access
EGU

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

Anonymous Referee #2

Received and published: 15 June 2020

This paper addresses two key-questions that are debated by the scientific community working on wind erosion and atmospheric dust. The first question, debated from several years, concerns the dependence or not of the emission-dust PSD with the wind friction velocity. The second one, more recently laid on the table by Khalfallah et al. (2020), deals with the dependence of the same emission-dust PSD with the atmospheric boundary-layer stability. These questions are under debate mainly because obtaining relevant observations in natural conditions to investigate such dependencies is difficult. Long and complex campaigns are required for this, a drastic selection among the observations is necessary to isolate the best situations and, therefore, relevant data are scarce. Thus, one of the first interest of this paper is to propose a

re-analyze of the data from the JADE campaign (Ishizuka et al., 2008). Beyond that, this article offers an original and in-depth analysis of these data. Finally, it proposes numerical simulations to support the conclusions. More precisely, the paper provide convincing observations on the dependency of emission-dust PSD with the wind friction velocity and atmospheric boundary-layer stability. The experimental results reported in Figures 4, 5 and 6 are the key figures of this paper. The discussion on the PDF of u* and on its role on the intensity of the saltation is really convincing. The authors also propose interesting and elegant explanations for these dependencies by examining the role played by the wind friction velocity PDFs in cases of high and low u* and of different stability conditions. I am not sure that this paper will definitively close the debate (more experimental data will probably be necessary for this) but I am sure that this document is an important contribution in order to better understand what controls PSD dust emissions and to elaborate future experiences. The paper is well written and structured even if additional information is needed in some places. I recommend to publish this paper with minor corrections. 1. Introduction: The introduction is very well written, concise and gives a clear idea of the scientific context. lines 41-42 (and also mentioned on line 144-145): I agree with the authors that dust-airborne PSD measured very close to the surface can probably be assimilated to dust-emission PSD by assuming that the difference of the particle diffusivity compared at that of other scalar (and thus, its dependency on particle diameter) can be neglected. However, because this assumption is an important point of the paper, it deserves to be better discussed, especially because there are few experimental data on particle diffusivity, especially for small particles, and that most of the information we have come from models. In the same way, since the authors indicate that size-resolved dust fluxes were measured during JADE (line 67) a comparison between dust–airborne PSD and dust-flux PSD should be added to support this assumption, at least in the supplement. line 49: the only reference to the Pisso et al., 2019 's paper is not sufficient to support the statement that "The proposed emission-dust PSD is frequently used in dust models". line 52: replace airborne-dust PSD by "dust flux PSD" or "emission dust PSD"

2. JADE data line 64: replace JADA by JADE I know that the JADE experiment has been described in details in various papers, especially in Ishizuka et al., 2008, and does not need to be described again. However, the presentation of the data in this paper is too short: for example, it is never indicated that OPCs measured number concentrations that are further converted into mass concentrations assuming spherical particles of density= 2380 kg m-3. In the same way, Ishizuka et al. (2014) mentioned that the measurements for the bin 0.3-0.6 μm were not considered because of a significant difference between the two OPCs for this bin. In the present paper, this bin is used and the reason for that should be explained, at least to be consistent with the previous paper. However, it must be noted that this point is not critical for this paper since the contribution of this bin is negligible in mass as shown on figures 3 and 4. In the same way, the data from OPC measuring at 2 m high were not considered as relevant in Ishizuka et al. (2014) (because they does not correlate with the OPC measurements performed at the other heights) but are used in this paper. This should also be explained. Finally, line 96, it is indicated that no rainfall occurred as a consequence of the cold front crossing but in Ishizuka et al; (2008), it is mentioned that the rain sensor detected several very small precipitation events that have been also observed by the authors. Even if, as mentioned in Ishizuka et al. (2008) the drying of the soil was very rapid, this should mentioned. Figure 1: The measured dust concentrations are very high (several mg m-3!) even for event 11. This should be underlined in the text. Indeed, since the authors used airborne-dust PSD and not dust flux PSD, it is necessary to provide arguments showing that the measured dust PSD can be directly linked to dust emission. And such high concentrations of dust strongly suggests that the contribution of advection to the measured dust PSD is probably very limited. line 88: reformulate. A soil has only one texture. A formulation such as “The results of the analyze by method A correspond to (or suggest) a loamy sand texture while the results of the analyze by method B. . .” should be better. line 91: I appreciate the method consisting to select among various dust events those the best adapted to the objective of the study. However, the justification for selecting these two events is very short (“Event-10

occurred under daytime unstable, while Event-11 under night-time stable, conditions”). I imagine that they were other events among the 12 aeolian events recorded during JADE that occurred also in stable or unstable conditions. Are these events selected because the stability conditions were particularly constant for these two events? 3. Results

Figure 3: we have no idea of the number of points contributing to each u* category. This should be added in the legend of the figure; in the same way, no information is provided on the spread of the different points that allow to construct the PSD (standard deviation bars should be added). The authors write “that dust PSDs for Event-10 and -11 considerably differ”: maybe an additional panel reporting the difference between PSD10 and PSD11 for similar u* categories could better illustrate these differences in PSD.

Figure 4: This figure in which are averaged all heights and all u* is important since it clearly shows that the dust and saltation PSD shift between the two events. However, this averaging approach is not well introduced and it should not be obvious for the reader to understand why it is useful and relevant to make such averaging. The paper should explain that. The insert is too small and should be a figure by itself. Same comment concerning standard deviations as for figure 3.

On figure 6, there is a shouldering in the high values for the observed u* distribution corresponding to event 11. This suggests that the u* PDF could be bimodal for this event. Moreover, the adjusted Gaussian PDF for u* does not include this shouldering reducing the variance of the u* Gaussian PDF for event 11. This should be discussed.

The numerical simulations are interesting and illustrate the sensitivity of the impact kinetic energy to different parameters on which the stability conditions could act. They clearly suggest that larger variances in u* PDF, as generally observed in unstable conditions, generate stronger saltation and thus should be responsible for higher production of fine particles.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-425, 2020.