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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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We are most grateful to Dr. Sylvain Dupont for carefully reading our manuscript and providing insightful and detailed comments for discussion. These comments are very valuable for us to improve our paper and approaching the truth.

Dr. Dupont first commented on the results of the Khalfallah et al. (2020) paper and pointed out possible discrepancies between his own analysis and the results presented in Khalfallah et al. Indeed, this paper is triggered by the study of Khafallah et al. which helped us to decide to finally have a thorough look at the PSD issue. We thought this issue was resolved until the questions raised by the Kok's paper which has caused a

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Discussion paper

stir in the dust research community. Now, our results show the PSD at dust emission is u* dependent. This seems also to be the view of Dr. Dupont. With respect to PSD dependency on atmospheric boundary-layer stability, our results seem to support the finding of Khalfallah et al. qualitatively, but we have some considerations of their interpretation why this might be so. We are not convinced that "diffusion" caused this dependency.

As we do not know exactly, how colleague Khalfallah et al. processed their data, we cannot judge the reliability of their conclusions, but Dr. Dupont is in a much better position to make the judgement, as he works with the authors of the afore-mentioned paper. With the insight Dr. Dupont provided, we will modify in the revised paper and to be more cautious with the interpretation of the results of Khalfallah et al., although we have certainly tried not to "rely" on their work.

The second point of Dr. Dupont is important, namely, that he is convinced of PSD dependency on u*, not necessary on ABL stability. Our view is somewhat different. In our paper, we have tried to make it clear that there is a mean u* and a u* variance, the PSD of dust at emission is not only dependent on the u* mean but also on the u* variance. This dependency arises because the saltation bombardment is non-linearly dependent on u*. In essence, this is the problem of saltation/dust emission intermittency. In a series of related studies (e.g. Klose et al. 2014), we have been considering how turbulence causes dust emission. Suppose the mean u* equals to u*t, then there would be no saltation and no saltation bombardment, but if u* has a distribution, then intermittent saltation and saltation bombardment will occur, and the PSD of dust at emission will be dependent on the PDF of u*. This really is the main point of this study, and the idea is already in several Klose et al. papers.

Now, does turbulence intensity (actually the PDF of u*) depends on ABL stability? We think so, as the large-eddy simulation of Klose et al. (2014) shows and also the JADE observations. We have carried out recently a wind-tunnel experiment, again showing the dependency of u* PDF on ABL stability and the strong impact on dust emission. The results of the wind tunnel experiments will be summarized and send for consideration of publication.

Thanks for mentioning the Kaimal and Finnigan (1994) book (Yaping Shao and John Finnigan have worked in the same group for some years and is one of the first readers of the book). The r_uw curve in Fig 1.9 of KJ (1994) book does not seem to apply here, because (1) there is nothing said about the variance of the shear stress only the mean; (2) it only states that shear stress normalized with the wind variances is fairly constant (not exactly constant, actually why not exactly?); (3) earlier measurement of shear stress was mainly down somewhere in the ABL at some level, not really at the surface; and (4) their Fig 1.10 actually shows that the variance of wind is dependent on ABL stability (i.e. the variance normalized with w* is fairly universal).

We agree with Dr. Dupont, we probably need to do more cases, as Ref. 2 also mentioned, but the other JADE cases are less complete and much more work is needed to process the data.

Dr. Dupont made several very nice suggestions.

(1) More figures for characterizing the events: to understand what happened during the erosion events 10 and 11, show time variation of dust and saltation PSD during the events. This is a good idea. We will look into this. (2) PSD of emitted dust flux: This seems to be difficult. Such a PSD was not observed. (3) Condensation: as far as we know, there was no condensation, but there were a few drops of rainfall accompanied with the cool change, although no rainfall was recorded. We will have to look into this to be able to answer. (4) Enhanced cohesion in night: this is an interesting point, but it is difficult to validate. In a separate study by the first author (unpublished), he is working on the modelling of soil moisture under extremely dry conditions. (5) Surface modified by saltation: Yes. This is likely, but we cannot validate this. As Dr. Dupont suggests, we can have a look at the saltation PSD evolution. (6) First justifications: Dr. Dupont is right. It should be interpreted differently. (7) u* variance: This is an important

point. As Dupont et al. (2018) shows that u* needs to be averaged over 15 to 30 mins for the flux-gradient relationship. However, there is no doubt that shear stress fluctuates due to large eddies, and shear stress has a PDF. This PDF is important to dust emission which rapidly responses to surface shear stress. The selection of 1 min for shear stress averaging seems to be reasonable, this is to assume that saltation can reasonably respond to shear stress variations on this scale. This is the whole point of this paper. We are willing to debate with Dr. Dupont on this in greater detail. (8) Saltation bombardment intensity: We have discussed this above. KF (1994) book, Fig. 1.9 states r_uw is almost independent of z/L, but r_uw is shear stress normalized with flow velocity variance which do vary strongly with stability, as their Fig. 1.10 shows. But we would agree with Dr. Dupont, this is an unsolved issue, because we do not fully understand how the laminar layer close to the surface behaves. There are theories about the possibility that the laminar layer breaks up. Our unpublished wind-tunnel experiment (measuring shear stress using Irwen sensors showing the fluctuations of shear stress related to large eddies). Again, the whole point is that we have to move away from the tradition "mean" flow characteristic approach and consider more the PDF of the turbulence quantities, which are important to understanding the PSD of dust at emission.

There seems to be a misunderstanding somewhere. What we try to say in justification 3 is actually that the diffusion aspect due to enhanced or not enhanced turbulence with respect to instability does not affect the saltation trajectory too much. In this sense, Dr. Dupont is right. But the initial velocities of the saltation particles seem to be important.

It is great to discuss with colleague Dr. Dupont.

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