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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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I had great interest reading this paper. It is well written and well structured. The question of the sensitivity of the particle size distribution (PSD) of emitted dust on the friction velocity (u*) and thermal stability is indeed crucial and of great interest for the erosion community. In that sense, this study tries to answer to this question using the JADE field experiment.

This study follows the recent paper of Khalfallah et al. (2020) that showed a dependency of the emitted dust flux PSD to the atmosphere thermal stability using the WIND-O-V field experiment, for stability conditions ranging from near-neutral to slightly unsta-

ble (never stable as here). This was attributed by these authors to the dependency of the particle eddy diffusivity to the particle size. To be honest, my own analysis of the WIND-O-V data did not lead me to the same conclusions. I did not find any dependence of the emitted dust flux PSD to the atmosphere thermal stability. The enrichment in fine particles of the dust flux with increasing unstability claimed by Khalfallah et al. is in fact an impoverishment of their flux in coarse particles. In their statistics, this impoverishment and thus the stability dependency of the dust flux PSD, resulted only from few periods in two of their eight events, periods that should have been discarded due to a too low difference in dust concentration between their two dust concentration levels, not permitting the applicability of the flux-gradient approach. I am also skeptical about their justification of the PSD stability dependency based on the variability of the particle eddy diffusivity with particle size (0.3 to 9 μm) since particle trajectory-crossing effect should be quite negligible for such small particles. I would therefore not rely much on Khalfallah et al. (2020).

Interestingly, the present paper concludes as well on a larger fraction of fine particles of the emitted dust PSD in unstable conditions (event 10), by comparison with a stable condition event (event 11). As opposed to the particle eddy diffusivity argument of Khalfallah et al. (2020), this difference of dust PSD with stability is explained here by difference in saltation-bombardment intensity with stability. In particular, the saltation PSD was found different between the unstable and the stable events for similar u* values.

While I am convinced by the sensitivity of the dust PSD to u*, I am less convinced that there is also a direct sensitivity to the atmosphere thermal stability. In my opinion, there is not other thermal stability effect than the one already observed in u* (Eq. 5). You argued that the larger turbulence in unstable conditions enhances saltation-bombardment intensity, and thus emission of finer dust particles, as compared to stable conditions with identical u*. In my opinion, your argument that the flow turbulence intensity is larger in unstable than in stable conditions for identical u* is not demonstrated and disagrees with Fig. 1.9 of Kaimal and Finnigan (1994, p20). I therefore think that the differences observed on the saltation and dust PSD between unstable and stable cases (daytime and nighttime conditions, respectively) for similar u* are explained by something else than thermal stability. To the end, you just observed a difference of PSD of near-surface dust concentration and saltation between only one daytime event and one nighttime event for similar u*, and you simply attributed this difference to thermal stability but without any convincing demonstration. This could be explained by many other reasons. This demonstration of the thermal stability effect is important as I am afraid that another paper suggesting a thermal stability effect on dust PSD in continuation of the erroneous paper of Khalfallah et al. (2020) would leads the erosion community in a wrong direction. This explains this comment.

In my opinion, more figures should be presented in order to better characterize the events and to be more convincing:

- In order to understand what happened during the erosion events 10 and 11, I would expect additional figures showing in particular the time variation of the dust and saltation PSD during the events. For example, a 2D plot of the time variation of the dust and saltation flux PSD would be helpful. The figures presented in the manuscript only show averages over the whole events while both events are very different in time scale (10h for the event 10 and 6h for event 11) and more importantly in stationarity. The mean wind speed is relatively stationary during event 10 and very unstationary during event 11 (Figure 2b). The event 10 covers stability from near-neutral to unstable conditions; we would therefore expect to see as well a sensitivity with time of the dust PSD related to the stability variation for a similar u* if this dependency exists.

- The PSD of the emitted dust flux should be also presented to characterize the PSD of emitted dust and not only the PSD of the airborne dust measured here at 1, 2 and 3.5 m. A background concentration, in particular of fine particles, could be present during the events without any relation with dust emission, and the depth of the atmospheric layer where dust particles are dispersed should be quite different between the unstable

and stable cases as well as along the daytime event 10. Showing the difference of dust flux PSD between events 10 and 11 and their time variation would be much more convincing.

You should discuss other possible reasons (or discard them) for the difference of the dust and saltation PSD between the events 10 and 11:

- The event 11 occurred at night. Could it be possible that droplet condensation before or between the intermittent sub-events reinforced inter-particle cohesion and thus explains the lower fraction of fine dust particles of the emitted dust PSD at night?

- Event 11 also occurred after a strong sand drift. Could this strong sand drift have changed the PSD of mobile (weakly attached) particles at the surface, explaining the larger proportion of 70-80 μm particles in the saltation PSD for small u* (beginning of the event)? A time variation of saltation PSD would be helpful.

In the last part of the paper, the three justifications (called perspective in the text, lines 159-160) for explaining the dependency of the dust PSD on w* (stability) based on the saltation bombardment intensity, should be much more convincing:

- The first justification is related to the ABL similarity theory, showing that u* depends on the thermal stability (Eq. 5). In my opinion, this justification is not relevant here. It just shows that stability is already accounted for in u* and thus it does not demonstrate a direct relation between dust PSD and thermal stability for a constant u*.

- The second justification is on the stochasticity of u*, whose variance changes with stability. By definition, u* is a mean quantity characterizing indirectly the amount of momentum absorbed by the surface (square root of the absolute value of the momentum flux), by accounting for all eddy scales transporting momentum. To respect this last point, u* is usually estimated over 15 to 30 min time period for surface atmospheric boundary layer turbulence (Dupont et al. 2018). For an ideal event with stationary large-scale wind conditions and constant thermal stability (as in wind-tunnel), u* should be constant in time with no variance whatever the thermal stability. I therefore do not understand this argument or justification. The stochasticity of u* that you are observing in Figure 6 is due (1) to the non-stationarity of the mesoscale wind during the erosion events (see Figure 2b), and (2) to your choice of estimating u* at only 1 min time scale. Furthermore, eddies transporting momentum are smaller in stable condition than in unstable conditions. Your choice of using 1 min to estimate u* is questionable, especially for the event 10, as it certainly misses large-scale contribution to the momentum transport (see Dupont 2020), and the time period for estimating u* should be certainly smaller for stable (event 11) than unstable (event 10) conditions. The variance of u* in figure 6 depends mainly on the stationarity of the wind during the events and your choice of estimating u* at 1 min, but much less on the thermal stability. A more stationary large-scale wind condition during event 10 with the same thermal stratification would have induced a smaller variance of 15-min u* than during event 11. I am also not sure that the u* PDF of event 11 reflects the variability of u* during an erosive event in stable condition, as this PDF seems dominated by several periods without erosion following Figure 2a. For this reason, I am not convinced neither by this justification of a direct dependence of the dust PSD on the thermal stability. As I wrote above, this stochasticity of u* is only related to large-scale variations of the wind and to your choice of computing u* at 1min, which leads to not comparable u* between unstable and stable conditions.

- The third justification is on the enhancement of the saltation bombardment intensity with buoyancy production of turbulence. To demonstrate this point, a saltation model is used. The idea is to demonstrate that the flow turbulence intensity increases, and so the saltation bombardment intensity, with unstability while keeping u* constant. This means that additional turbulence not transporting momentum is produced with increasing unstability, for stability conditions that are still close to neutrality since the wind speed remains quite significant during erosion events. The model demonstrates that for a similar u*, the thermal stability represented here by L (Obukhov length) does not modify the impaction energy of saltating particles (figure 7a). This is, therefore, a strong

indication that stability does not directly impact saltation PSD and does not increase turbulence for identical u*. By only artificially increasing the turbulence of the flow while keeping u* constant (and with no variance, i.e. no stochasticity, in contradiction with the second justification), obviously the impaction energy of saltating particles is increased (figure 7b). Here, it was assumed that this additional turbulence is not efficient at transporting momentum (and so increasing u*). This artificial increase of turbulence has, however, no justifications. Consequently, this cannot constitute a demonstration. The last simulated case shows that for similar wind speeds, the impaction energy of saltating particles is increased with increasing unstability (sensible heat flux, figure 7c) but this is certainly not for a constant u*! Here, u* should be smaller for the negative heat flux case (low turbulence) than for the positive heat flux case. To the end, this third justification does not demonstrate as well a direct dependence between the saltation PSD (and this dust PSD) and the thermal stability.

In my opinion, near the surface where saltation occurs the turbulence is largely dominated by friction and much less by buoyancy. During erosion events, we are far from free convection conditions because of the strong wind, even during daytime. I doubt that an additional turbulence not transporting momentum would be significant in event 10 as compared to event 11. Following Kaimal and Finnigan (1994, Fig. 1.9, p20), the cross-correlation coefficient for the momentum flux (ruw), which gives an information on the ratio of the momentum flux compared to the level of the flow turbulence, is almost constant for $-1<z/L<1$ in the surface layer, i.e. for stability conditions where the Obukhov length is larger than 1 m, which should largely include events 10 and 11. This figure of Kaimal and Finnigan represents a strong evidence that the argument suggested in this paper for the dependence of the dust PSD on thermal stability based on the increased turbulence of the flow with unstability while keeping constant u*, is doubtful.

Hope that my comment is helpful and constructive.

Sincerely

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