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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.

Yaping Shao et al.

zhang-j@lzu.edu.cn

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Overall assessment

I read the manuscript from the perspective of a specialist in wind erosion and dust emission but a generalist to this specific focus of the manuscript. The authors provide a clear, logical development of the focuses of their manuscript on the different bases for explaining particle size distributions of emitted dust and the dependency of air borne dust PSD on atmospheric boundary layer (ABL) stability. The topic is valuable for the community and the work is well presented. However, I am not convinced by the approach used in the manuscript. I think the work in the manuscript omits uncertainty. If

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that uncertainty were included, I think it may lead to different / alternative conclusions. Therefore, to increase confidence in the results I think the omitted uncertainty must be tackled, in some form or other, before the work can be published. I provide below additional information on this point. I also think that some improvements in the structure of the manuscript will help the reader more easily follow the explanation of the work. In short, I am thoroughly supportive of the work. I think the manuscript needs to be revised to give confidence that the results are indeed detectable and therefore interpretations are robust to the uncertainty. The nature of the revisions I describe below I think, are consistent with a major revision, despite not being too difficult to achieve in a short period of time, if all other things were equal.

Response: Many thanks for Referee 3 for the constructive comments. She/he emphasized on the uncertainty of the analysis, and uncertainty related to roughness correction for saltation. Uncertainty analysis is always important, but the PSD difference we detected is mainly based on field observations and the differences are systematic. Investigation on the significance of the difference is possible, however, we pretty sure that the difference is statistically significant.

Main issues

Wind friction velocity uncertainty In the abstract, it is stated that friction velocity u* is a surrogate for surface shear stress and descriptor for saltation bombardment intensity. Line 32 states that “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”. The JADE field measured data are used to show that the (finely resolved) particle size distribution is dependent on measured wind friction velocity. In contrast to this approach, it is well known (cf. sediment transport models) that the wind energy available for saltation bombardment is not u*, it is the energy us*, which remains after wind momentum has been extracted by the roughness ‘canopy’. In other words, us*=u*.R where R is the partition of drag. Under controlled conditions with homogeneous material, smooth surface (bed) without ripples and the bed reset after each experiment, it may be reasonable to assume that R=1. However, the authors use field data which, even under the smoothest field conditions are very likely to cause R not equal to 1. For example, soil has different sized aggregates at the surface, stones occur in the field, plant residue may be fixed to, or lying on, the soil and ripples may occur intermittently during sediment transport (and that is to say nothing of intermittent crusts which may change roughness). The magnitude of R<1 (which may change over time, between events due to change in the roughness 'canopy') is the omitted uncertainty in the authors' methodology. For clarity, I think the authors should state clearly that they are assuming u*=us*. I think the authors must then ideally estimate, or at least approximate, the uncertainty of making that assumption (R=1) under field conditions when R<1. That 'model' uncertainty will then manifest as an error on u*. When the PSDs are aggregated under this explicit approach, that 'model' uncertainty (not to be confused with the standard deviation of u* already included by the authors) will demonstrate the extent to which there is a difference in the PSDs which is detectable. Any difference between the PSDs remaining after that 'model' uncertainty has been included will have accounted for the dependency of PSD on us* and R. I think the same issue of uncertainty occurs with the relation between the dependency of emission-dust PSD on u* and the boundary-layer stability. Where u* is based on field measurements, I think it requires the same (as above) expression of uncertainty. As above, this uncertainty is required due to the assumption that u*=us* when field conditions introduce uncertainty. Consequently, the results in the second half of the paper need to be similarly qualified with this uncertainty. The issue is brought to sharp focus by considering Eq. 8 of the manuscript. The sediment transport Q is related incorrectly to u*ËĘ3 (Webb et al., 2020). As above, the available energy for transport is us*ËĘ3. Whilst there are conditions when u*=us* and therefore R=1, in the field it is very unlikely that R=1. In this case, the uncertainty of the 'model' assumption u*=us*, needs to be considered (R<1). With this additional uncertainty the arising figures and interpretations may need to be re-evaluated.

Response: About the issue related to roughness correction, we are not sure whether

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it is relevant here, because using field measurements to detect changes in surface roughness (e.g. ripples) will be difficult, and will probably lead to more uncertainties in saltation estimates. But as saltation flux used in this study is measured, we do not see how the roughness correction comes into play. What can be done is probably some sensitivity tests on how erosion modified aerodynamic roughness length causes different saltation intensity, but then this is already done in the recent paper by Webb et al.

Manuscript structure

I think the manuscript mixes unnecessarily theory with results. I think the theory (Eqs. 4, 5, 6 etc. and related text) should be moved to the Methods section. In that Methods section I think it would be worthwhile explaining carefully how the parameter values of the modelling were chosen (rationale and assumptions) so that it is clear to the reader how the results have been produced. I find it strange not to have a Discussion section. I wonder if much of the detail in the Introduction would be better moved to the Discussion and then extended as necessary with additional context for the discussion. This would also help the Introduction quickly move the reader through the main issue.

Response: Thanks for the suggestions on structure. We will try to improve the manuscript accordingly.

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