Referee #1 Yaping Shao

We wish to thank Yaping Shao for his very valuable comments. Below we provide detailed responses to each comment separately. Our responses to reviewer comments are in blue normal text.

General comments

González-Flórez et al. use field measurements to investigate size-resolved dust emission. This is a comprehensive study, covering a number of issues important to dust research. The authors have made a major effort to push forward the dust research boundaries. This effort is significant as indeed we still do not know enough about dust emission processes, and our capacity of quantifying dust emission is still unsatisfactory, in particular size-resolved dust emission. In recent years, there has been a number of studies on this topic, with contradictory outcomes.

We really appreciate the positive perspective of Yaping Shao on our work.

It is probably useful beforehand to clarify that there is never any doubt that dust emission, including the particle size characteristics of emitted dust, depends on the balance between the forces lifting dust and the forces resisting the lifting. In some studies, the emphasis is placed on the former, while in others on the latter. For example, Khalfallah et al. (2020) and Shao et al. (2021) emphasized the importance of the forces to entrain the dust, implicitly assume the soil conditions are the same. These authors never said that the resistance forces, such as soil binding due to soil moisture, are not important. Of course, they are important, as Dupont (2022) suggests. In fact, the use of minimally and fully-dispersed PSD (Shao, 2001) is an attempt to represent the soil binding strength, while the BFT Kok (2011a, b) assumes the dependency of binding force on particle size is universal (which is, in my view, extremely unlikely). When we discuss these earlier papers, I believe, we should bare in mind the explicit and implicit assumptions made and present the discussions in a sound framework.

We thank Yaping Shao for this comment and overview, with which we agree. In fact, in the introduction we briefly describe some distinct aspects of current theories/approaches in relation to the variability of the PSD at emission that broadly reflects your comment : *"In the particle size range up to* $\sim 10\,\mu$m *in diameter, some theoretical frameworks predict a higher proportion of emitted fine particles with increasing wind speed during saltation along with dependencies of the PSD on soil properties (Shao et al., 1993; Alfaro et al., 1997; Shao, 2001). In contrast, the emitted PSD is posited to be relatively independent of wind speed and soil properties in another theoretical framework (Kok, 2011a), based on Brittle Fragmentation Theory (BFT)."*

The main conclusion of this paper seems to be that dry dust deposition is important to the size-resolved dust emission. This seems to be a sensible conclusion to make, but the line of argument seems to me both interesting and confusing. In large scale models (e.g. Klose et al., 2021), dust emission and deposition are treated separately. We must therefore clearly define which dust flux is being studied, the part of entrained by wind, or the net dust flux. Both dust emission and deposition are fluxes which serve as boundary conditions for the diffusion process in the atmosphere. At this stage of parameterization, dust emission is parameterized without considering ambient dust concentration, while deposition is parameterized by considering ambient dust concentration profile (following this study, deposition flux equals deposition velocity x dust concentration at a reference level). We know dust concentration in the atmospheric boundary-layer (ABL) is stability dependent, because stability affects both dust concentration profile and deposition velocity (Yin et al., 2022). It thus seems to be contradictory to state that “size-resolved dust emission” is not ABL stability dependent, while dust deposition is important. But deposition depends on ABL stability (or not?).

As discussed in the introduction of the paper, most studies relate the diffusive flux PSD obtained at a few meters above the surface to the emitted dust flux at the surface, assuming a constant dust flux layer and neglecting the gravitational settling and turbulent dry deposition. The gravitational settling term is assumed to be small for dust smaller than $\sim 10\,\mu$m. The diffusive flux PSD is afterward used directly to constrain or evaluate dust emission schemes, or even to assess to what extent the emitted dust PSD may be affected by atmospheric forcing and soil properties, neglecting the deposition component of the net dust flux at the surface. In previous studies, using modeling, Dupont et al. (2015) and Fernandes et al. (2019) have shown the potentially large effect of dry deposition (including losses by turbulent and Brownian motion, and inertial impaction) upon the diffusive flux PSD.

In our paper, we analyzed this effect based on field experimental data. Specifically, we discussed the potential role of dry deposition in shaping at least part of the variability observed in the diffusive flux. In the revised version of the paper, we also provide estimates of the emitted flux by accounting for dry deposition. For that purpose, we estimated the dry deposition velocity by tuning a dry deposition velocity parameterization to fit our measurement data (see Sects. 2.4, S10 and Appendix D).

Regarding the dependence of deposition fluxes on ABL stability, we revisited Yin et al. (2022) and we found that in their case a large range of stability conditions (from unstable to stable) were covered, while our dust events in Morocco (driven by saltation) comprised a narrower range of stability conditions (mostly near-neutral, forced convection regimes) where very likely the stochasticity of u_* remained stable and poorly impacted by convective motions as in Dupont (2022). This means that for our range of stability during dust events, the deposition velocities are unlikely stability dependent. Therefore, it seems not contradictory to observe no stability effect on the diffusive dust flux PSD and for the diffusive flux to include deposition. We have added a corresponding remark in the text. In addition, Yin et al. (2022) argued that u_* is stability dependent because of the increasing stochasticity of u_* in unstable conditions, but in Dupont (2022) (WIND-O-V data) that u_* stochasticity remained quite stable for dust events driven by saltation.

The method of Gillette et al. (1972) for computing diffusive dust flux is widely used. Normally, it is not a big problem if we only want to provide an estimate for the total dust emission and assume diffusive dust flux is the same as the emission flux. But Gillette et al. (1972) method is problematic to use for the purpose of this study, as it concerns size-resolved dust emission. There is a discussion in Section 7.1 of Shao (2008) on the different definitions of the fluxes, and why the Monin Obukhov similarity relationship may not apply here. I believe, line 285 of the paper, $\phi_d = \phi_m$, is problematic, even if the Csanady (1963) approximation holds, due to the gravitational settling. As this paper emphasizes on the impact of dry deposition on diffusive flux, a correction to Eq. (9) seems to be warranted. The correction (e.g. Shao et al., 2011) may result in stronger corrections (with respect to Gillette et al. (1972)) to larger dust particles and hence lead to somewhat different size-resolved dust emission.

We agree that for coarse particles, our assumption $\phi_d = \phi_m$ may not be well justified. However, the correction for heavy particles used in Shao et al. (2011), which represents the change in the turbulent diffusivity due to the trajectory crossing effect, appears small here (see Fig. 1). We therefore decided to not account for it.

The correction term for heavy particles C that would multiply our Eq. 9 is given by Eq. 1.

$$C = \left(1 + \frac{\beta^2 v_g^2}{\sigma_w^2}\right)^{-1/2} \tag{1}$$

where β is a dimensionless coefficient relating the fluid Lagrangian integral time scale, the integral length scale of the Eulerian fluid velocity field and the standard deviation of the turbulent velocity. In Csanady (1963), it is said that β is very close to 1, so we assume β=1. The settling velocity $v_g(D_i)$ is calculated for each size bin as $v_g(D_i) = C_c \sigma_{pa} g D_i^2/(18\nu)$ where C_c is the Cunningham slip correction factor, $\nu = 1.45 \cdot 10^{-5}\,\mathrm{m^2\,s^{-1}}$ is the air kinematic viscosity, $g = 9.81\,\mathrm{m\,s^{-2}}$ is the gravitational acceleration, D_i is the mean logarithmic diameter in bin number i, and $\sigma_{pa} = (\rho_d - \rho_{air})/\rho_{air}$ is the particle-to-air density ratio. The unbiased variance of turbulent velocity σ_w is calculated from the w component of the 3-D sonic anemometer placed at $1\,\mathrm{m}$. Figure 1 shows the mean correction term for heavy particles per size bin only considering the periods where diffusive flux was positive in all the diameter bins above the anthropogenic mode (as discussed in Sect. 3.3.1).

On several occasions, the impact of fetch (and haboob and wind direction) is mentioned. The effect of fetch is to generate a horizontal advection which influences the diffusive flux, while the Obukhov similarity (and hence the Gillette et al. (1972) method, i.e., Eq. (9)) assumes horizontal homogeneity. Again, the dust concentration equation is:

$$\frac{\partial c}{\partial t} + u\frac{\partial c}{\partial x} + \frac{(w - w_t)\partial c}{\partial z} = \kappa_p \frac{\partial^2 c}{\partial z^2} \tag{2}$$

Under the assumption of steady state and homogeneity, the above equation reads

$$w_t \frac{\partial c}{\partial z} + \kappa_p \frac{\partial^2 c}{\partial z^2} = 0 \tag{3}$$

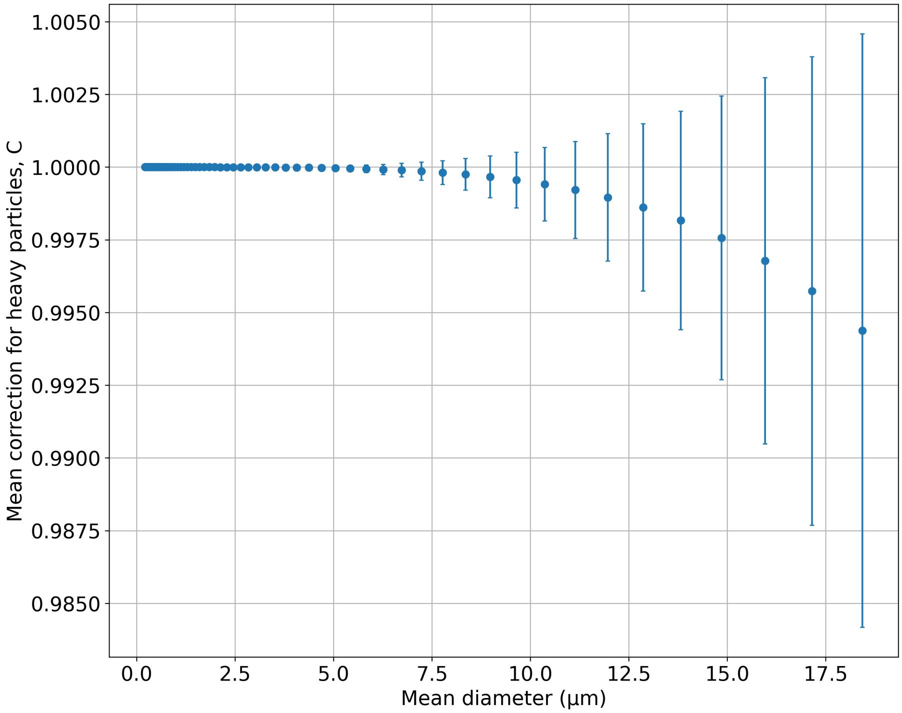

Figure 1. Mean correction factor for heavy particles per size bin. Only the samples where diffusive flux is positive in all the diameter bins above the anthropogenic mode (as discussed in Sect. 3.3.1) have been used. Error bars represent the standard deviation.

(by the way the Obukhov similarity assumes $\kappa_p \frac{\partial^2 c}{\partial z^2}$=0). If there is a fetch effect, then the horizontal homogeneity assumption of Obukhov (and hence Gillette et al. (1972)) is no-longer valid and the above equation reads for steady state

$$u\frac{\partial c}{\partial x} + \frac{(w-w_t)\partial c}{\partial z} = \kappa_p \frac{\partial^2 c}{\partial z^2} \tag{4}$$

It seems contradictory to me to apply the Obukhov similarity to analyse the data and then conclude that the fetch effect is important to the size-resolved dust emission, as some sort of physics-based interpretation, rather than to say it may be the uncertainty related to the use of the Obukhov theory. The statement related to wind direction and haboob is probably also attributed to advection.

The fetch effect mentioned in the paper corresponds to a long distance dust source fetch effect of the order of $10\,\mathrm{km}$ in the Eastern direction and $60\,\mathrm{km}$ in the Western direction. This fetch effect leads to a small-continuous increase of dust concentration along the fetch, and thus an increase of dust deposition while the dust emission remains unaffected. Importantly, this dust fetch effect does not impact the wind dynamics. Consequently, the conditions of application of the Monin-Obukhov similarity theory are valid for the wind velocity fields. In the formulation of the dust flux-gradient method from Gillette et al. (1972) (Eq. 9), we can therefore still relate the local momentum flux to the surface friction velocity ($\langle u'w' \rangle = -u_*^2$) and replace the mean wind velocity profile using the pseudo logarithmic form with the usual stability functions (Sect. 2.2 in Dupont et al. (2021)).

We agree that a long distance variation of the dust concentration implies horizontal dust advection. In fact, in presence of a non-zero diffusive flux (emission or deposition or both), there should always be advection as particles should accumulate or diminish within the boundary layer, assuming (1) that the boundary-layer depth does not change after (let's say) more than $1\,\mathrm{km}$ from the upwind border of the dust source and (2) that the dust flux at the top of the boundary layer is negligible. If the horizontal dust advection term is significant near the surface, then the estimated diffusive dust flux at several meters height cannot be considered equivalent to the surface flux, and the applicability of the flux-gradient method between a 2–3 m depth layer could be questionable. Hence, the question is: is this horizontal advection of dust large enough to induce a significant vertical gradient of the vertical diffusive flux near the surface, and thus to invalidate the constant dust flux layer hypothesis used to relate the diffusive flux to the surface flux?

We estimated the advection term as follows. Assuming that dust emission, surface roughness and thermal stability are similar between western and eastern erosion events for equivalent u_* (and therefore equivalent mean $<u>$), then udC/dx can be approximated as $<u>(C_w - C_e)/(F_w - F_e)$, where C_w and C_e are the mean dust concentrations at the intermediate height between the two Fidas of the western and eastern direction events of intensity u_*, respectively, and F_w and F_e are the dust-source fetch lengths in the western and eastern directions, respectively, being $F_w = 60\,\mathrm{km}$ and $F_e = 10\,\mathrm{km}$. As an estimation, we considered the mean wind speed $<u>$ measured by the 2-D sonic sensor at $2\,\mathrm{m}$ high during erosion events, i.e. $<u_{2m}> = 8.34\,\mathrm{m\,s^{-1}}$, corresponding to u_* between 0.35 and $0.43\,\mathrm{m\,s^{-1}}$. For this u_* interval and particles of $0.6\,\mu\mathrm{m}$ diameter, the advection term is $udC/dx = 5.1 \times 10^{-4}\,\mu\mathrm{g\,m^{-3}\,s^{-1}}$ and the diffusive flux is about $F = 2.5\,\mu\mathrm{g\,m^{-2}\,s^{-1}}$. If we neglect the gravitational settling term for these fine dust particles, then it means that the vertical gradient of the diffusive flux is $dF/dz = udC/dx = 5.1 \times 10^{-4}\,\mu\mathrm{g\,m^{-3}\,s^{-1}}$. Between the two concentration measurement heights ($1.7\,\mathrm{m}$ difference), this corresponds to a variation of the diffusive flux of about $8.7 \times 10^{-4}\,\mu\mathrm{g\,m^{-2}\,s^{-1}}$, i.e. 0.035% of the flux. Between the intermediate height between the fidas and the surface ($z_{int} = 2.65\,\mathrm{m}$), it corresponds to about 0.054% of the flux. Hence, for fine particles we find it reasonable to conclude that the dust flux layer is constant, and thus that the flux-gradient method is applicable between 1.8 and 3.5 m, and that the 2.65 m high diffusive flux can be related to the surface flux. We can conclude the same for larger particles regarding the small horizontal advection but we have to account for the gravitational settling term for the constant dust flux layer.

Specific comments

In specifying the similarity functions, z0 is sometimes considered and sometimes not, e.g., Eq. (2) and (5). How important is z0?

We thank Yaping Shao for making us aware of this slight inconsistency. We have revised our use of the flux-profile relationships and are now consistently including z_0 as the lower integration limit. Comparison with our earlier calculation confirmed that the impact of this change to our results is negligible. We have also revised our formulation of Eq. (1), which now reads:

$$\overline{U}(z) = \frac{u_*}{\kappa}\left[\ln\left(\frac{z}{z_0}\right) - \Psi_m\right] \tag{5}$$

as in Kaimal and Finnigan (1994). Also for consistency, we have removed "neglecting $\psi_m(\zeta_0)$" in line 222.

In general, I find the work very well done, and the authors have thoroughly studied the literature. But it is (for me at least) a very heavy paper with lengthy descriptions. I find the abstract very long. It may be trying to solve too many problems at once (size-resolved dust emission, deposition, fetch, haboob, BFT etc.). I believe the paper would have a larger impact, if it were more concentrated on the core issues.

We thank Yaping Shao for acknowledging the quality of our work. We agree with him that the paper is long. Indeed, Jasper Kok (the other reviewer) suggested to split the original paper into two separate papers (one focused on bulk dust emissions and one focused on size-resolved dust emissions). However, after careful consideration we have ultimately decided against splitting the paper. We believe that our comprehensive analysis of the dust PSD and its variability (the core and novel part of the paper), benefits from the exploratory analysis of the time series of bulk dust concentration, dust and saltation fluxes and meteorology at our site, along with a first view of the bulk saltation and sandblasting conditions. In order for this first part to become a separate paper, additional substantial developments and more exhaustive/detailed analyses would have been required, which are beyond our current scope. Our analysis of the PSD and its variability is comprehensive by design. It attempts to elucidate the potential causes of the PSD variability and therefore involves discussion on the deposition, fetch, haboob, and BFT. However this does not preclude future more specific studies and papers on the different aspects. We note that the revised version of the paper includes some structural changes and additions to accommodate suggestions by both reviewers and to improve the quality of certain parts of the text. We have also reduced some parts of the text, moved material from the main paper to the Appendix and from the Appendix to the new Supplement document.

Here is the list of all relevant changes made in the manuscript:

- Abstract is now more concise
- Most of the appendices and associated figures along with the updated Fig. 10 (ratio of dry deposition to the estimated emitted flux), now Fig. S32, have been moved to the new Supplement material document.
- Part of the specifications needed to calculate the scattered intensities of the PSLs and the aspherical dust, originally described in Sect. 2.2.2, have been moved to the Appendix A to reduce the length of the paper.
- Original lines 420-435 along with the Fig. 5, both associated to the relationship between z_0 and u_*, have been moved to Sect. S7 in the new Supplement material document to reduce the length of the paper. Additionally, as suggested by Jasper Kok, in this new revised version, our z_0 measurements have been fitted not only to the relationship derived by Charnock (1955) but also to the modified Charnock's model proposed by Sherman (1992), which uses a more physical relation and accounts for the presence of a threshold (Sect. S7 in the Supplement).
- Subsection 2.4 in the Data and methods section is now called "Estimation of the size-resolved dry deposition and emitted fluxes" to include the methodology associated to the dry deposition velocity, dry deposition fluxes and estimated emitted flux. This was motivated by comments and suggestions from Jasper Kok. Appendix D contains details about the parameterizations used for dry deposition velocity.
- Subsection 3.3 in the Results and discussion section has been renamed as "Variability of the dust PSD at emission" and now only includes the first three subsections.
- The content in original Subsect. 3.3.4 has been split into the new Subsects. 3.4 "What explains the observed PSD variations? Potential roles of dry deposition and fetch length, aggregate disintegration, and haboob gust front" and 3.5 "Evaluation of the estimated dry deposition and emitted fluxes". The latter containing mostly the new results related to the estimated emitted flux, including the new Figs. 10 (which is based on the original Fig. J1 placed in the Appendix) and 11. Additional figures are shown in Sects. S11 and S12.
- The original Subsect. 3.3.5 "Comparison with Brittle Fragmentation Theory including super-coarse dust" is now the Sect. 3.6 and its two associated figures have been updated and now include also the normalized estimated emitted flux PSDs.
- As suggested by Jasper Kok a small discussion of the changes in the diffusive PSD with u_* and wind direction in quantitative terms has been included in Sect. 3.3.2 along with some additional plots in Sect. S13 of the Supplement. Also a quantitative comparison between diffusive and estimated emitted fluxes considering dust particles as polystyrene latex spheres and assuming tri-axial ellipsoids has been added in Sects. 3.5 and 3.6, respectively.
- All the sections have been updated according to the comments of the reviewers

Additional changes

- As suggested by Jasper Kok, we determined u_{*th} using both a linear and a 3/2 fitting of the saltation flux as a function of wind shear stress (Martin and Kok, 2017). Results are depicted in Sect. S3.
- Figs. 2 and 3 have been updated. Their new grey areas highlight times with $u_* > u_{*th}$. Also, we realized that when plotting Figs. 2f and 2g we had forgotten to remove the bins with diameters below 0.25 μm, considered unrealistic due to border measurement limitations. This has been corrected in the revised version.

- As recommended by Jasper Kok, Figs. 5, S6 and S7 have been updated to show the fits for $u_* > u_{*th}$. Also, Tables S1, S2 and S3 containing the obtained parameters from each regression curve along with their 95% confidence intervals have been updated accordingly.
- Figs. 7, 9, S12, S14 and S16 have been updated to adjust the y-axis limits and facilitate the comparison between figures.

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