Review report on

Insights into the size-resolved dust emission from field measurements in the Moroccan Sahara

By Cristina González-Flórez, Martina Klose, Andrés Alastuey, Sylvain Dupont, Jerónimo Escribano, Vicken Etyemezian, Adolfo Gonzalez-Romero, Yue Huang, Konrad Kandler, George Nikolich, Agnesh Panta, Xavier Querol, Cristina Reche, Jesús Yus-Díez, and Carlos Pérez García-Pando1

Gonzalez-Florez 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.

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.

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, MONARCH), 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, ACP, Large-eddy-simulation …). 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?).

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, Physics and Modelling of Wind Erosion) 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. 2011a) 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.

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

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

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

$$w_t \frac{\partial c}{\partial z} + K_p \frac{\partial^2 c}{\partial z^2} = 0$$

(by the way the Obukhov similarity assumes $K_p \frac{\partial^2 c}{\partial z^2} = 0$). 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

$$u\frac{\partial c}{\partial x} + \frac{(w - w_t)\partial c}{\partial z} = K_p \partial^2 c/\partial z^2$$

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.

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

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.