Review on the manuscript entitled "Stochastic parameterization of dust emission and application to convective atmospheric conditions" by Klose and Shao

The two-fold goal of the manuscript is to 1) develop a statistical approach to modeling dust emission driven by convection, and 2) assess the convective-driven dust emission in a case study. Convective dust emission is thought as a potentially important mechanism contributing to total dust aerosol load in the atmosphere that is primarily controlled by the saltation bombardment and grains disintegration processes. The work has the potential to provide a new parameterization for convective dust emission; however, in my opinion, major revisions of the manuscript are needed to provide critical missing information on the derivation of the new convective dust emission parameterization and its testing and validation in WRF-Chem in the presented case study. My major comments are the following.

Specific Comments:

1) The authors need to provide a clear definition of strong and weak winds, strong and weak dust events, weak convective turbulence, etc. that are being used repeatedly in the text. This is important to delineate the range of applicability of the convection-driven dust emission scheme vs. traditional saltation-driven emission schemes. Further, the authors need to demonstrate that the weak convective turbulence condition actually occurred in the considered case study, i.e. at the lidar ground-based site and over the Taklimakan on May 23-25.

2) The new convective emission parameterization involves three major statistical components: the parent soil-grain size distribution, denoted psd, inter-particle cohesion, and surface shear stress. I will comment on each of these components one-by-one.

Regarding the psd of soil grains, authors need to clearly state the particle size range of soil grains for which the convective turbulent lifting will be possible, as well as show the range of sizes for the new scheme. Do author expect that the aerosol size distribution will be similar to that of the soil grains given that the aerodynamic lifting driven by convection will not disaggregate the lifted grains? If not, then psd of aerosol size distribution will need to be introduced or at least the discussion of flux F (e.g., Eqs. 5 and 13) as a function of size must be provided. In addition, better justification of the use of the soil texture is needed, keeping in mind that the measurement of soil texture involves complete disaggregation of soil grains. Better explanation of the minimally-disturbed psd and how realistic they are for the case study of the Taklimakan will be helpful.

3) The particle vertical velocity w_p depends on size and shape of particles, as well as their density. The authors consider size and density but assume spherical particles. I would suggest to evaluate the effect of non-sphericity on the aerodynamic drag coefficient to justify the assumption of spherical particles.
Of more concern is Eq.(5). Since w_p depends on size, it is unclear what F represents here. Moreover, the particle number concentration and mass concentration both depend on the size so Eq.(5) does not make much sense in its present form. It must be re-written to explicitly show the dependence on size.

4) The parameterization for the cohesive forces (Eqs. 15 and 16) requires better justification. How the coefficients in Eqs.15 and 16 were derived? An assessment of errors associated with this parameterization need to be performed, as well as resulting uncertainty in the dust flux (Eq. 13).

5) Regarding the parameterization of the shear stress, my main concern is how the joint pdfs of the velocity fluctuations were constructed from WRF wind fields. There is a complete disconnect on how WRF winds were used to implement this parameterization in the presented case study. Moreover, winds modeled with WRF will depend on the selection of model physics, e.g., the PBL parameterization, radiation scheme, etc. The model physics selection has not been discussed at all. Of particular importance is the choice of the PBL scheme and how turbulence is parameterized. Realism of modeled wind fields need to be examined, especially vertical wind component. How the probability density function of instantaneous shear stress is computed from modeled fields?

6) In my opinion, the presented case study is the weakest part of the manuscript. Regarding the comparison with lidar data to constrain the parameter α_N, it is important to explain what particle concentration is used in Eq.(23), i.e., the range of aerosol particle sizes for which this concentration was measured and at what vertical level. Then the authors need to demonstrate that their WRF model with the convective dust emission can actually reproduce the size distribution and concentration observed during the lidar measurements. I also can argue that Eq.(24) is an extremely simplified relationship between the lidar backscattering and particle concentration as to question the robustness of the assessment of α_N, as least some evaluation of errors is warranted.

7) Presentation of the case study of dust emission in the Taklimakan on 23-25 March requires major revisions. In addition to information on the WRF setup, this section needs to discuss the surface heat balance and its convective term to demonstrate the actual occurrence of convective turbulence and convective-driven dust emission during this time period (and at the lidar side). I doubt that the convective turbulence was affecting the entire Taklimakan Desert to the extent as Fig.7 might suggest. Wind fields and threshold friction velocity need to be examined to figure out where or not sandblasting processes were taking place, and validation of modeled dust against observations is needed. The authors might want to use observations from meteorological stations to support their modeling results as to the presence of dust in the atmosphere during the considered time period. Satellite imagery might be also helpful.

8) Errors associated with dust fluxes shown in Fig.7 and 8 need to be addressed.
How will the state of parent land surface affect the convective emission, e,g, soil moisture, crusting, the presence of vegetation, etc.? None of these factors were discussed in the manuscript in the context of the efficiency of convective dust emission but they are likely to be important.