Dear Referee,

We appreciate your constructive comments and wish to thank you for your effort. Our statements to each of your comments are as follows.

*[Referee] 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.*

In the context of dust emission, "weak wind" and "strong wind" are best defined by using the ratio of $u_*/u_{*t}$ where $u_*$ is friction velocity and $u_{*t}$ is threshold friction velocity for saltation. Weak winds refer to the situations when $u_* < u_{*t}$ and aerodynamic entrainment dominates the process of dust emission, while strong winds refer to the situations when $u_* > u_{*t}$ and saltation bombardment and aggregates disintegration dominate the process of dust emission. As already stated in the first draft of the paper, our approach is different from "traditional" emission schemes. However, the applicability of our approach is not delimited by the occurrence of convective turbulence. We suppose turbulence to appear in any case, differing from one another only due to varying production mechanisms (either predominantly shear or buoyancy generated). Thus, the only uncertainty or restriction to applicability is whether the parameterization of the momentum transfer is accurate. In the present model, we parameterized $p(\tau)$ on the basis of the convective scaling velocity ($w_*$) and boundary layer height ($z_i$). Additional inclusion of $u_*$ might lead to further variation of mean $\tau$ and thus different emission values. Further investigations on $p(\tau)$ are currently carried out in another study, which is indicated in Section 4.1.

Some modification to the text has been carried out in the revised manuscript to further clarify the above mentioned points.

A discussion on the synoptic situation of the case studied is given to comment (7).

*[Referee] 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.*

In fact, there is no boundary of particle size for which the turbulent lifting is possible. That is the main new idea of the new scheme. In the present approach, probabilistic distributions are applied to determine particle lifting. That means, lifting is extremely unlikely for large particles, but not impossible. In our model, we use four particles size bins to represent the range of particles smaller than 20 µm, but in theory there is no difficulty to use more particle size bins.

We don't expect the aerosol size distribution to be similar to that of the parent soil as dust emission is similar to a convolution of the probability density functions of $\tau$ and $F_i$ (cohesive force) (see Eq. 13 and Figure 1b). Yet we agree with the referee that there exists a problem of nomenclature of $F$ in Eq. 5. We will try to find a clear formulation in the revised manuscript. Thank you.

The minimally-dispersed psds used in our model were derived by analyzing different samples of each soil type, here sand, loam, sandy clay loam, and clay. As we only have detailed psd data for these four classes, we group all remaining texture classes to the previously named four classes. Minimally-disturbed psds can be seen as in situ psd measurements as they are obtained with the least disturbance possible where no breakup of aggregates occurs. Therefore, they represent the psd for the lower edge of emission in contrast to fully-disturbed psds, which are maximally disaggregated by mechanical forces and apply for maximum dust emission (Shao (2008), Section 7.2). We will include a more detailed explanation in the revised version of the paper. Many thanks for your comment.

*[Referee] 3) The particle vertical velocity wp 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.*

Thank you for this suggestion. An inclusion of particle shape in the model would be desirable, but is far beyond the current development stage. We introduce a completely new emission parameterization and at this stage of the model development, we feel it would make the problem more complex and we have therefore not included the effect of particle shape.

*[Referee] Of more concern is Eq.(5). Since wp 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.*

We agree that the formulation is not consistent (see also reply to comment 2). We will improve the notation in the revised paper. Thank you very much.

*[Referee] 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).*

At present, there are few measurements allowing the estimates of the pdf of cohesive force. The coefficients are estimated from a limited data set shown in Zimon (1982). In his book, the data of cumulative distribution function of the cohesive force are shown, from which the pdf of the cohesive force can be derived. The coefficients are estimated by fitting the pdf to the so-derived data. We are fully aware of the uncertainties in the pdf of the cohesive force. However, it seems to be sufficient to use the data of Zimon (1982) to illustrate our idea of stochastic dust emission.

*[Referee] 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?*

The pdfs of u and v wind components as well as their joint pdf are not computed on the basis of WRF wind fields, but with the help of similarity theory based on $w_*$ and $z_i$ (Section 3.2). We will go through this Section and emphasize that more clearly. Additionally, we will include information on the PBL scheme used in our simulation, many thanks for this remark.

*[Referee] 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 $\alpha_N$, 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 $\alpha_N$, as least some evaluation of errors is warranted.*

The main purpose of this paper is to present our new ideas on dust emission parameterization and introduce the newly developed approach. The case study was carried out to provide first results produced by the new scheme. As discussed within the paper, the case shown here is by no means optimal (see Sections 4.2 and 5) and we are aware that the obtained $\alpha_N$ value can only be regarded as a preliminary estimate.

There is no particle size information provided by the lidar data. The data contain, e.g., backscattering ratio (which we used in this study) in 60 m height intervals. The method of determining dust concentration (Eq. 24) by fitting backscattering ratio to measured near-surface dust concentration with predefined vertical profile is certainly a simple approach, but as indicated in the previous paragraph, the uncertainty in model parameterization is of more relevance and the comparison between model and lidar is only provisional.

*[Referee] 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.*

Figure R1 is an illustration of surface heat flux at 14 LST on 23, 24, and 25 March 2009, which fell between 100 and 250 W $m^{-2}$ in much of the simulation domain. At the fringes of the Taklimakan desert, it exceeded 300 W $m^{-2}$ on occasions. This shows that the case studied can be regarded as weakly convective, as indicated in the manuscript. The convectiveness of the boundary layer is also confirmed computing the ratio of $z_i/L$ (where $z_i$ is the height of capping inversion and L the Obukhov length)

It is appropriate to emphasize that our scheme of stochastic dust emission is a general approach and is not restricted by the occurrence of convective turbulence (see statement to comment 1). To retain the brevity, we prefer not to include the attached figure in the revised manuscript. Instead, additional discussions will be added to Section 4.2. We fully agree with the referee that observations for comparison with our scheme are desirable, but observations on convective dust emission are scarce and no suitable data is currently available to the authors. The use of satellite data is not suitable for the purpose, as the convective dust emission considered here is not of comparable prominence as dust storm, and the comparison with satellite data would lead to more uncertainties than answers. We are now planning to apply the scheme to regions with better data for more comprehensive validation. To do this, dedicated observations will be necessary.

*[Referee] 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.*

We are aware that these factors play an important role concerning the amount of emission and we discussed the need for improvements at this point in Section 5. Thank you very much for your comment. We will give more weight on this issue in the revised manuscript and include an estimation of errors associated with changes in the cohesive force.

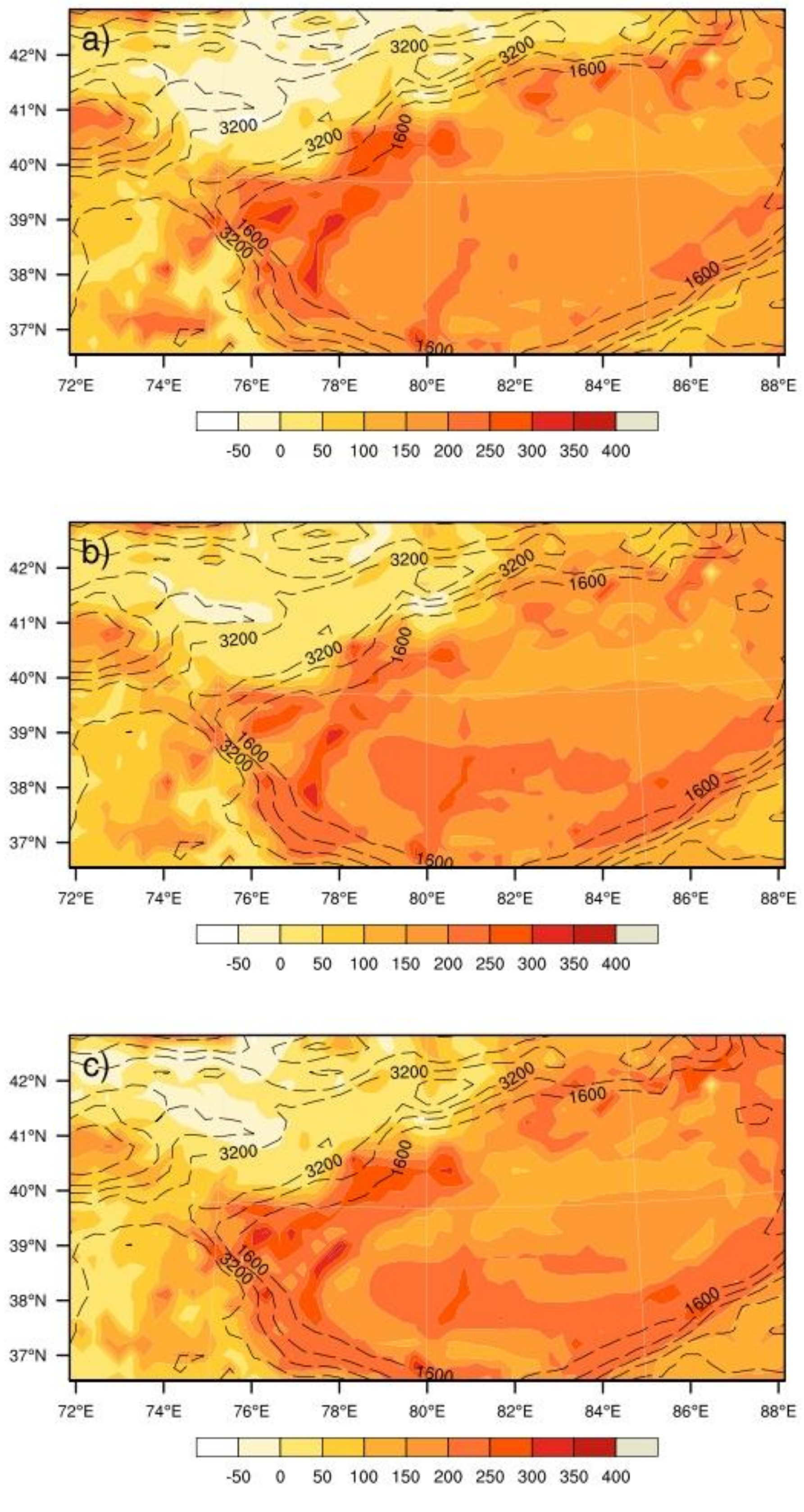


Figure R1: Simulated surface heat flux at 14 LST on a) 23 March, b) 24 March, and c) 25 March 2009.