This paper aims to investigate the effects of atmospheric turbulence on sediment emission rates. This topic is a central issue in recent investigations by the community with various papers suggesting important effects of turbulence on dust fluxes and their size distribution.

This paper contains two major elements: it implements a simple technique to generate quasi-convective turbulence in a wind tunnel. This is an important advance because the lack of wind tunnel experiments on sediment movement has so far been carried out under neutral or near-neutral conditions which do not allow the study of the role of large eddies on wind erosion. The second major point, well illustrated by Figure 4, is that the results show that the sediment emission rates are higher during quasi-convective turbulence situations. These are significant contributions that deserve a publication of this paper.

Nevertheless, in its form, some parts need to be better detailed to improve understanding or to avoid unanswered questions. Most figures need to be enlarged.

Comments:

Lines 17-18: I agree that “it has been shown in numerous studies that equation 1 is valid in general” but some papers have also challenged its validity (e.g. Martin & Kok, Science Advances, 2017; Andreotti, J. Fluid Mech., 2004). It is not the purpose of this paper to discuss this but the wording used could suggest that this debate does not exist.

Lines 74-75: I understand the first part of the sentence, which seems sufficient in itself. What more do you mean by "by comparing the shear stresses measured by the two devices"?

Lines 92-94: The description of the soils is really limited. On reading, one gets the impression that only the average particle size differs and this is not sufficient to then understand why $\bar{\gamma}$ are so different in table 3.

Line 141: the end of the sentence is not clear for me. When looking at figure2, the wind profile is not significantly modified for z<0.2m. The authors should be more explicit.

Table 2: This table needs to be strongly completed and better discussed. I do not retrieve the 25% and 15% difference in $\bar{\tau}$ for fan speeds 7000 rpm and 12000 rpm, respectively, as mentioned line 157, when comparing $u_*$from the NP and WP profiles. Are the $u_*$from Irwin sensors those obtained for the WP conditions. This should be clearer in the table (move WP to be centered). Moreover, there are 4 Irwin sensors in the wind tunnel: how do the authors use them? Is the reported data an average of the four sensors? If so, give the standard deviation. More generally, give the standard deviations since there are several repetitions of each experiment. Add the emission rate for each experimental condition and the associated standard deviation.

Why does $z_0$ change significantly for NP experiments: it should be constant unless there is an additive saltation roughness but the WP experiments have an almost constant $z_0$ which rules out this assumption. How accurate is the recovery of $u_*$ and $z_0$ from the wind profile? Does this accuracy depend on the regime (i.e. wind speed)?

Figure 4 is the key figure of the paper. It shows that the slight increase of $\bar{\tau}$ in quasi-convective conditions is not sufficient alone to explain the measured differences in the emission rates of the four soils. It implies that the perturbations of the shear stress, $\tau'$, are also responsible for a part of the differences in the emission rates. This is perfectly clear.

However, the lack of information on the different soils (S1 to S4) complicates the understanding of why the emission rates of the different soils as reported in figure 4 do not follow the order of $\tau_t$ (as shown in table 3). According to equation 12, the only explanation for that is that the different soils have different values of $\bar{\gamma}$ (as suggested by table 3 for NP experiments) but reasons or at least hypotheses allowing to understand this should be given. Furthermore, what could be the possible explanation for such large differences in $\bar{\gamma}$ observed for S1 and S4 but not for S2 and S3 between the two sets of experiments? I can understand that $\bar{\gamma}$ is affected by quasi-convective turbulence but it is difficult to understand why it would affect the four soils so differently.

The number given in Table 3 are very precise with two significant digits for all parameters but we have no idea of the uncertainties and how are significant the differences in $\bar{\gamma}$.

Moreover, the quality of the fits to equation 12 is not given while looking at figure 6 (and despite its small size), these fits seem worse for S1 and S4 than for S2 and S3.

Lines 200 -201: Does this suggest that the impact of convective turbulence should be rather limited on the total dust emission budget?