Response to anonymous referee #2's interactive comment on the manuscript "Surface Renewal as a Significant Mechanism for Dust Emission"

This is an interesting paper that uses wind-tunnel experiments to put forth the hypothesis that the renewal of fine particles in a soil's top layer is critical to dust emissions. This is an appealing hypothesis and this process is currently missing from models. They test this hypothesis using a series of wind tunnel measurements, which seem well designed. This article thus has the potential to be an important contribution.

Response: we greatly appreciate the positive comments from referee 2#.

However, there are several major issues with the article. Paramount is a major deficiency in how the results and discussion are presented. Almost throughout the "Results and Analysis" and "Conclusions" sections, the authors present hypotheses, of which their (otherwise very interesting) data are merely suggestive, as facts. Words such as "show" and "demonstrate" are used abundantly. This is not appropriate considering the level of evidence the authors present, and the enormous complexity of dust emissions. I will give a few (of many) examples of this below. The authors need to completely rewrite these sections. In particular, they should split up the "Results" and "Discussion" sections, to make it clear what are indisputable facts from their experiments, and what is their interpretation of these facts.

Response: many thanks for the constructive comments which will be adopted in the revised version.

In addition, there are some major scientific issues:
- The saltation bombardment section has major issues, which I'll list below:
* "c0 reflects the fraction of effective saltators, namely, grains available for saltation at a given friction velocity". This is inconsistent with Owen (1964), and also with the paper's own Eq. 18, where c0 is linked to the terminal velocity.

Response: in Owen (1964) model, c_0 is well defined as a function of the ratio between the particle terminal velocity and the friction velocity for uniform particles, shown as Eq. 18. If this model is applied to mixed particles, we thought c_0 may be affected by other factors, such as particle size distribution. We try to give some explanations of c_0 for mixed-particle surface, and we agree with the referee that c_0 should follow its basic physical meaning to be consistent with Owen (1964). Some modification will be made for this part.

* P. 7, lines 10-11: I'm not aware of any measurements supporting the idea that the number of available saltators depends on the (theoretical) thresholds for individual particles. Rather, when saltation is initiated, the splashing process can mobilize particles of a wide range of sizes (e.g., Rice et al., 1995). The authors should either provide experimental evidence for their viewpoint, or note the opposing view (even if they do not adopt it).

Response: as stated above, we will rewrite this part to make the interpretation reasonable.

P. 7, lines 13-14: Did the authors directly measure what particles constituted the saltators? If not, this is interpretation, yet as presented as fact.

Response: we did not directly measure the component of saltators and the expression of this part will be changed.

P. 7, lines 14-15: This similarly is interpretation presented as fact. The saltation flux depends on many closely coupled and complex processes. Linking a change in the flux to any one parameter (the fraction of effective saltators in this case) without directly measuring it is speculative. That's fine to do in the discussion section, but should be presented as such.

Response: accepted, we will split up the part of "Results" and "Discussion" to make the expression clear and easy to follow.

* For the fitting with equation (15), how was u*t obtained? Was it fit as well? And how was v_t calculated?

Response: for the dashed line in Fig. 3, u*t and c0 were obtained from regression analysis with Eq. (15); for the solid line in Fig. 3, c0 was calculated by Eq. (18), and v_t is calculated by $v_t = 1.66(\sigma_\varphi g d_s)^{1/2}$ (Shao, 2008) which will be added in revised version.

The use of Eq. (16) – (19) is very interesting. However, the procedure here is very unclear to me, and might have some scientific flaws. My primary concern is that the parameters in the u*t relation seem to be fit to the measurements, such that Eqs. (16) –(19) have, as far as I can tell, three tunable parameters (proportionality constant (c0?), r, and An). Since the data they fit to are only four data points, these fits are statistically not that meaningful (only 1 degree of freedom). Thus the conclusion that "the above method gives a more accurate estimate of Q than Equation (15)" needs to be put on a more solid statistical basis.

Response: actually, here c0 is determined by Eq. (18) (v_t is calculated by $v_t = 1.66(\sigma_\varphi g d_s)^{1/2}$ as stated above) and there are only two tunable parameters (r and An) left. We will add the coefficient of determination R^2 to judge the performance of different regression methods.

* Related to the above comment, please provide the fitted u*t(d) relationships for the three soils so that the reader can judge whether they are reasonable. This is necessary to judge whether the visually good agreement is due to a good description of the physics, or because of a sufficient number of tuning parameters. You could provide these fits in a supplement to the paper.

Response: we can add the fitted u*t(d) relationships for the three soils. But actually, this work is not focus on testing existing scheme of saltation flux Q. We measured Q and searched a good formulation of Q to reduce the uncertainty in the validation of dust vertical flux F which is considered to be intimate with Q. that's why we didn't discuss too much about the reason of the visually good agreement in the paper.

- Sections 4.3 and 4.4: A central argument of the authors here is that the dust supply for aerodynamic entrainment is maintained by the intense sand flux for S2 and S3, but not for S1, which has lower sand flux at a given u*. However, the authors should compare apples to apples here and thus compare data with similar sand fluxes, for instance u* = 0.37 m/s for S1 and u* = 0.23 m/s for S3. The S1 data point shows a large dust flux decrease during the first minutes, whereas the S3 data point does not. This is not explained by their hypothesis, and should be clarified.

Response: generally speaking, dust supply for aerodynamic entrainment is maintained by the intense sand flux. But the criticality of sand flux which may cause surface renewal also depends on the property of surface. A certain sand flux may cause soft surface (e.g. S3) renewal, but may not efficient for hard surface (e.g. S1). That's the reason for the phenomenon that the referee mentioned above.

- I found section 4.4 very difficult to follow. Please use paragraphs in this section and make sure that the text flows smoothly. More importantly, this section again uses many interpretations of the data and would benefit enormously from separation into a results (facts) section and a discussion (interpretations and hypotheses) section. As it is written, I cannot sufficiently judge the scientific merit of this section.

Response: accepted, we will split up the part of "results" and "discussion" for this section.

- Section 4.5 suffers from similar issues as the other sections, with many hypotheses presented as though they were measured experimentally (line 9-11 "Due to the neglect of the supply-limiting effect and of the variation of bombardment efficiency, all three models underestimated the dust flux at low friction velocity, but slightly overestimated at high friction velocity"; line 14-15 "With the increase of u*, the bombardment efficiency decreases because of changed surface property due to intrusive sand particles." ; line 18-19 "S04 appears to perform somewhat better than the others due to improved treatment for saltation bombardment and aggregates disintegration."; line 21- 22 "This shows that threshold friction velocity u*t represents different properties of the soil surface in the Owen model and the GP88 model.")

Response: accepted, we will rewrite these sections carefully to make the expression reasonable and clear.

Other comments:
- Please make line numbers continuous in revised article to make the review easier.

Response: accepted.

- In the literature I'm familiar with, the term "supply limited" is generally used to refer to a lack of supply of saltators, not a lack of supply of fine soil particles. The authors should clarify this point.

Response: accepted.

- Line 31-32, p. 1: Why do differences in dust emission after disturbing a soil indicate the importance of aerodynamic entrainment? This should be clarified or removed.

Response: surface disturbance may change the content of exposed free dust which is intimate with aerodynamic entrainment. We will clarify this in revised version.

- Sections 2.2 and 2.3: While the authors cannot be expected to compare their data against every single dust emission model, they should at least mention the other ones (e.g., Marticorena and Bergametti (1995); Alfaro and Gomes (2001); Kok et al. (2014)).

Response: accepted.

- Eq. (9): What is the averaging time for u(z)?

Response: the wind speed is averaged over three minutes.

- Line 15, p.5: This statement on sonification requires justification. For instance, the impact of saltating particles can chip and break them, which does not occur during sonification. Therefore, whereas sonification disaggregates particles, won't grinding result in the wearing down of individual (disaggregated) particles, thereby changing the size distribution?

Response: accepted, we will add more appropriate explanation for the method selecting.

- Please add a brief discussion whether the use of the gradient method is reasonable for your experiment. Compared to field measurements, your fetch is very small (a few meters, compared to 100s or 1000s of meters in the field). You partially compensated for this by moving your dust sensors close to the ground, but can you expect dust to be well-mixed (and thus follow a logarithmic profile) at only a few meters of fetch? How will this affect your results?

Response: in fact, the gradient method has been employed in the other wind-tunnel studies on dust emission (Borrmann and Jaenicke, 1987; Fairchild and Tillery, 1982). Our environmental wind-tunnel laboratory is designed to simulate atmospheric boundary layer and has been validated. We did not test dust concentration profile in this experiment, because of the limitation of surface material. But the dust concentration profile has been tested in previous study of dust deposition (similar but with different transfer direction from emission), and the results shown the dust could be mixed well with the fetch of 8m in our wind-tunnel (Zhang, 2014). Anyway, it is accepted that we should add more discussion on rationality of gradient method.

- Section 3.3: was the wind flow seeded with particles in your experiments? If not, do you expect your sand flux to be saturated? The results of Shao and Raupach (1992) suggest that you need more length than the 8 m of your set-up.

Response: the wind flow was not seeded with particles. So we could not assure that the sand flux is saturated. That why we measured the saltation flux directly and strived to searched a good formulation of Q to reduce the uncertainty in the following validation of dust vertical flux F.

- P. 7: please define d1 and d2 in Eq. 16. Also, the last d should be d_s

Response: accepted.

- P. 7: Please provide the value of the particle-to-air density

Response: we will add it in the revised version.

- In general, how exactly is the fitting performed? What quantity is minimized? Given that the data spans several orders of magnitude, it makes most sense to me to minimize the squared distance in log space, not in linear space (as the authors seem to have done).

Response: we used 'Origin' (software) with the function of 'nonlinear curve fit' to implement data fitting. The iteration algorithm is set as 'Levenberg Marquardt'.

- P. 8, line 3: does this refer to radius or diameter? Does this mean that the reported dust fluxes are limited to D (or r) < 15 um? Please clarify.

Response: that refers to diameter and the reported dust fluxes are limited to D < 15 um. We will clarify them.

- P. 8, line 10-15: There are a lot of hypotheses used here to interpret the data in terms of arising from either aerodynamic entrainment or saltation bombardment, and whether or not the dust supply was limited. These factors were not measured directly, so these interpretations should be presented conservatively, rather than as statements of facts.

Response: accepted, we will check and rewrite this part carefully.

- P. 10: The scaling of aerodynamic entrainment with u* to the 10th power seems a bit extreme. Can you put uncertainty bounds on this result? How does this compare against other literature measurements such as Shao et al. (1993) and Loosmore and Hunt (2000)? What could explain the differences? Also, since you did not actually measure just aerodynamic entrainment (saltation was always present, as far as I understand), this conclusion should be more conservative.

Response: we will give more information of the regression analysis.
And actually we compared our results to Loosmore and Hunt (2000). The difference may be caused by different surface roughness.
Although saltation was always present, we subtract the contribution of saltation from total emission flux to obtain the quantity of aerodynamic entrainment.

- P. 11: "Supply limit is the major reason to restrict dust emission." This statement illustrates the main problem with the paper in its present form. Your measurements do not show this because you did not directly measure the supply limitations. You are merely hypothesizing this based on other measurements. I think it's a reasonable hypothesis, but needs to be presented as such, and not as a fact or hard conclusion. This problem is persistent throughout the entire paper.

Response: we will check the manuscript carefully and revise the relevant presentation.