Response to anonymous referee #1's interactive comment on the manuscript "Measurements of dust deposition velocity in a wind-tunnel experiment"

We would like to thank the anonymous referee for his critical and insightful comments related to our manuscript. This critique has motivated us to examine and revise the manuscript. The details of responses are shown as following.

Specific comments

1. Please make sure the revised manuscript is written in correct English (check before submitting).

Response: we will check the manuscript carefully and improve the expression.

2. line 26. Note that Sow et al. measured dust deposition; not dust emission.

Response: the work of Sow et al. indeed relates to dust deposition but emission. This mistake will be corrected.

3. General remark regarding the introduction: Please note that, as stated by the authors, the efficiency of most dust deposition samplers that have been used in the past is low, but for several of these samplers the correction factors are known. Applying these corrections leads to a much better agreement between measurements and model results, up to discrepancies as small as 15 % or even less. So far, the agreement between dust emission measurements and dust emission models has not yet reached this level of similarity.

Response: we agree with the reviewer for that our knowledges of dust emission and deposition are both far from complete. Generally, the discrepancies between measurements and model results are caused by the low-quality of measurement data and the un-reasonability of models. The "correction factor" of deposition sampler is normally ascertained relative to a water surface, which is considered the best alternative for simulating a perfectly absorbent surface. This is a good way to compare the collection efficiencies between different samplers and partly improve the quality of data measured by sediment samplers. But the surfaces we normally interested in do not like water. And generally, the surface collection processes over different surface are also different, because of different laminar layer and different surface condition. This reason should causes uncertainty of the data measured by sampler. Additionally, the lack of detailed information of experimental condition is another reason for poor-quality of existing measurements. These are the motivations for our work. And also, the existing deposition schemes are not perfect, which is discussed in a companion paper "A new parameterization of dust dry deposition over rough surfaces".

4. Lines 26-28. It looks somewhat odd that papers that were published AFTER the conceptualization of dust emission schemes served as the basis for these schemes. I suggest

re-writing the sentence.

Response: we will change the sentence to "As far as dust emission is concerned, several wind-tunnel and field observations have been carried out (e.g. Gillette, 1976; Shao et al. 1993; Ishizuka et al. 2008) which became to be the basis for the conceptualization of dust emission schemes (Shao, 2001, 2004; Marticorina and Bergamatti, 1995; Alfaro and Gomes, 2001)."

5. Fig. 1: It is entirely normal that discrepancies occur between the tested surfaces. Dust deposition is determined by the properties of the particles, the properties of the fluid, and the properties of the deposition surface itself. Deposition velocity is defined as the ratio of deposition flux to (airborne) concentration, and it thus depends on ALL factors influencing deposition except dust concentration. Therefore, the authors should be careful when they state that the 'scatter seriously undermines the value of the measurements for validation of models" (lines 61-62). To allow for correct comparisons, models should be adapted to the conditions under which the experimental data were obtained.

Response: the sentence will be changed to "Although the scatter may caused by the uncertainty in measuring techniques and differences in the experimental conditions, the lack of knowledge in measurement precision and the detailed information of experimental conditions undermines the value of the measurements for the validation of models."

6. line 123: Confusing. Are there 2 rows of 6 outlets each, or 2 rows of 3 outlets? Fig. 3 suggests that there are 2 rows of 3 outlets each, 6 outlets in total.

Response: that is "2 rows of 3 outlets".

7. lines 129-130. Please provide a number.

Response: the exact height of measuring point is given in **Table B2**.

8. line 135. 2200 kg/m3 looks low for pure SiO2. Are you sure the value is correct? Did you verify it experimentally?

Response: the dust we used in our experiment is spherical fused silica powder produced by Bestry Performance Materials Co.,Ltd. The parameter ρ_p=2200 kg/m^{-3} is provided by the producer. This value has also been validated by Archimedes drainage method before the wind tunnel experiment.

9. line 140: Note that this has never been experimentally confirmed. In fact, even a water surface may cause some rebound, although it will remain very low. I agree that the acceptance of a no-rebound condition is necessary to test the "classic" dust deposition schemes, but the no-rebound condition must then be presented in this manuscript as an assumption; not as a fact.

Response: the sentence will be changed to "For both surfaces, the possibility of particle rebound should be low, especially for particles bigger than 1 μm and thus it is reasonable to assume that…"

10. line 145. It would be good to define what a Gobi surface is. Most readers of this journal will not be familiar with this term.

Response: line 144-146 will be changed to "The experiments are then operated over sand, sandy loam, Gobi (a surface consists of sands and gravels) and tree surfaces to produce a more complete dataset."

11. line 150. So you applied oil to the wooden surface to make it sticky. Then I suggest you include this information in the earlier descriptions.

Response: we will change line 71-73 to "…ranging from a sticky-smooth wood surface which oiled by lubricating oil (wood surface hereafter) to a rough vegetation surface…"

12. lines 153-159. Unclear. Were the data from these 10 heights measured simultaneously or in repeated runs? I suppose the latter because the PDA measures in only one point. If measurements were not performed simultaneously, how confident can one be of the reproducibility of the concentrations (you state in line 159 that you use the data for determining the vertical dust concentration profiles)? Did you perform tests to check this?

Response: it is indeed in repeated runs. Actually, a device was fixed at a certain height to measure dust concentration. We used this method to monitor the stability of dust feeding and also the reproducibility of the concentration profile.
Line 53 will be changed to "Profile Measurement: about 10 points of different heights are selected and be measured one by one."
In line 159, we will add "At the same time, a device is fixed at a certain height to measure dust concentration with the purposes of monitoring stability of dust feeding and verifying reproducibility of the concentrations."

13. lines 169-170. This way of presentation is very confusing. I suggest listing the classes:
0.5-1.5 um, 1.5-3.0 um, etc.

Response: we will change to "…0.5-1.5 μm, 1.5-3 μm, 3-5 μm, 5-10 μm, 10-15 μm, 15-20 μm, 20-25 μm, 25-30 μm, 30-50 μm, 50-80 μm, 80-100 μm, 100-150 μm, 150-200 μm and 200 μm."

14. line 182: the associated VERTICAL dust flux

Response: Accepted.

15. line 183: I would write Fdi instead of Fi

Response: Accepted.

$$F_{di} = \frac{m_i}{V} \cdot w_{pi}$$

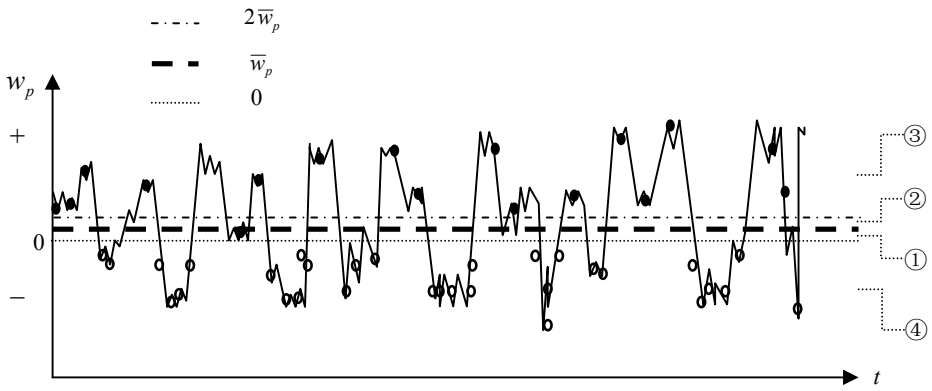
16. line 195: It might be useful to provide a justification for this (neutral boundary layer,

high wind speeds, …).

Response: we will change the sentence in line 195 to "While $\overline{w}_a$ is generally considered to be zero under the condition of neutral boundary layer…"

17. Fig. 7: Since the circled numbers 1 and 2 are larger than the thickness of the corresponding layers I would add a short line ("arm") to the circles, pointing to the corresponding layer.

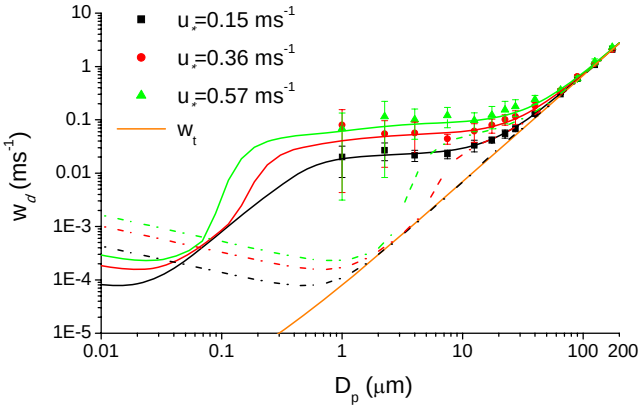
Response: The figure will be improved as shown below:



18. Lines 261-262: It is very unfortunate that the raw SS80 data are not shown. According to my calculations, the deviations with the authors' measured deposition velocities should be really large. For example, for 1-μm particles and u* = 0.57 m/s and z0 = 0.31 mm, SS80 predicts a deposition velocity two orders of magnitude lower than what the authors measured. The Sehmel and Hodgson (1978) model also predicts much lower deposition velocities, very comparable to SS80.

Response: we agree that the predictions of SS80 without considering particle growth effect (i.e. the case of RH=0%, as shown below) are seriously lower than our measurements. But if particle growth is considered (RH=100%), the predictions agree with our measurements very well. As we known, the dust we used is insoluble in water, and does not satisfy particle growth mechanism involved in SS80. So the good agreement should be a coincidence. This result indicates that the surface resistance is really low, which should be caused by the contribution of waves and spray droplets but enhanced terminal velocity of grown (larger) particles. Because this is not the topic of this paper, we directly set the surface resistance to zero (i.e. $1/w_D$=0) in this paper.

In other words, SS80 only suits for calm water surface. The distinct deviations between SS80 and our measurements should be caused by ignoring effect of waves and spray droplets in the model.



Deposition velocity against particle size under different wind conditions over water surface. The symbols are averaged results of w_d and the error bars represent the variability of the results. The curves are the results predicted with the SS80 scheme. The solid lines are obtained with RH = 100%, and the dashed lines with RH = 0%. The height of the measuring point is 25 mm above the water surface.

19. line 265. Confusing. Are the effects of waves and spray droplets included or not included in the SS80 scheme? To my knowledge they are not, so it looks like line 265 should read: ": : : are NOT included : : :".

Response: it is NOT included.

20. line 303: Some explanation of the correction formula might be useful. The ratio wt/ku* appears in the exponent, which suggests that corrections were (also) made for vertical differences in concentration.

Response: Line 300-303 will be changed to "To facilitate comparison, we have therefore corrected all data to the same reference height (1 m) using the following formula obtained by solving dust concentration equation under assumptions of horizontal homogenous and constant deposition flux. "

Technical corrections

line 10: delete "the".
line 14: same remark (2x).
line 17: delete last "the".
line18: velocities
line 80: capitalize "tunnel".
line 82: delete "the".
lines 83-85: Something is wrong with this sentence. Please correct.

line 88: delete “the”.
line 93: across
lines 95-98: please correct the sentence.
line 134” silicium dioxide (not silicone dioxide)
line 139: “a wood surface and a water surface”
line 167: Replace “For” with “Because” and “bigger” with “larger”.
Fig. 11, caption: delete “is”
Fig. 12, caption: delete “are” and add a full stop after “surface”.
line 317: replace “expected work” with “expected to work”.
line 356: dominates
line 357: dominates

Response: we will improve the manuscript according to the suggestions of “Technical corrections”