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Interactive comment on "Measurements of dust deposition velocity in a wind-tunnel experiment" *by* J. Zhang et al.

Anonymous Referee #1

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1. General comments

This is an interesting study and I recommend publication after the comments below have been addressed.

2. Specific comments

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

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

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.

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.

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.

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.

lines 129-130. Please provide a number.

line 135. 2200 kg/m3 looks low for pure SiO_2. Are you sure the value is correct? Did you verify it experimentally?

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

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an assumption; not as a fact.

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.

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.

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?

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.

line 182: the associated VERTICAL dust flux

line 183: I would write Fdi instead of Fi

line 195: It might be useful to provide a justification for this (neutral boundary layer, high wind speeds, . . .).

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.

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

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deposition velocities, very comparable to SS80.

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 ...".

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.

3. 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"

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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

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