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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Surface Renewal as a Significant Mechanism for Dust Emission" *by* Jie Zhang et al.

Anonymous Referee #1

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General comments:

Zhang et al. 2016 present an analysis of wind tunnel experiments to examine the role of three dust emission mechanisms. While the authors try to address the role of surface renewal in dust emission, they also place much emphasis on the importance of aerodynamic entrainment compared to other emission mechanisms. I think this is the first study to address the surface renewal process based on wind tunnel experiments. I find that the paper is generally well written although some sections need to be restructured. The methods need improvements in several places. Often, I feel that the statements drawn by the authors lack sufficient evidence. More proper interpretations and in-depth discussions on the experimental data are needed in many places to support their conclusions. One big issue is that, although the wind tunnel experiments are well designed, the collected data sample is too small to tell real differences between

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the regression fitting of various dust flux formulations. Plus, no statistics are given by the authors to judge the performance of regression analysis. I recommend publishing this paper after the following issues are addressed.

Detailed comments:

(P-page, L-line; note that the ACPD public version is used in this review)

My very first comment is that, please use continuous line numbering (instead of restarting numbering on every page) in your future manuscripts. This really helps the review process. Regardless whether the journal has such a requirement or not, using continuous line numbering is always a good practice.

Section 2 reads more like literature review, rather than a well-organized methods section. I encourage the authors to add a few sentences right after the section heading to explain how section 2 is organized before diving into the subsections. Another serious issue of section 2 is the use of symbols and abbreviations that are difficult to follow, because the authors give a review of so many dust schemes. Are they all necessary to be included in the paper? The authors need to make it clear why having an entire section for literature review of these specific dust flux methods is needed, and how they are going to connect with the wind tunnel experiments.

Equation 2: Why do the authors refer to the Gillette & Passi vertical flux parameterization, and then relate it to the Marticorena &Bergametti method of the F-Q relationship? Marticorena&Bergametti had their own parameterization for Q and F. That being said, equation 3 only applies for the Q parameterization in Marticorena&Bergametti, not necessarily the schemes from other studies.

Equation 4: Is this Fb or Fc? Later in eq. 8, you used Fc, but never defined Fc.

Equation 5-7: Should all the F in these equations be Fc? Also explain what F(di) and F(di, ds) are.

Equation 8 is questionable. My expression is that there are no distinct differentiations

between the three emission mechanisms in the model parameterizations. After all, they are mostly derived from wind tunnel experiment data, which most likely represent all three dust emission schemes. It is difficult to separate the different processes in field measurements or wind tunnel experiments. Even if Fa, Fb and Fc are specifically defined for the three processes, they formulations share the same parameters. However in fact, the validity of these formulations is only limited to certain conditions (e.g., wind speed, soil sizes), which are not discussed in the paper at all.

Equation 9: I think it is necessary to show a plot on regression analysis on calculating u* and z0 for all three experiments. Show the statistics from the regressions as well.

Equation 10: You never explained what the Pm(d) and Pf(d) are, and where they come from. I suppose that they come into play in Eq. 6. If so, define them after Eq. 6.

P6L3: How long does it take the fan to reach the target wind speed?

Section 3.1: Explicitly describe the purpose of the three experiments, for example, what dust emission mechanism(s) are each experiment corresponding to? What real-world conditions (e.g., supply limited in S1, supply limited but with renewal in S2, unlimited supply in S3?) do the experiments represent? I think having one or two statements like that can help readers easily understand the purpose of the experiment setup.

Equation 15 and Figure 3: Equations (15-19) should not be in the Results section. Move them to Section 2. I encourage the authors to rewrite Section 2 and logically introduce the dust schemes/equations (remove those not needed).

In Fig. 3: Why are there only 4 u*/Q values (same for other figures)? I donot think 4 runs for each surface type is sufficient to provide a meaningful data sample for regression analysis. Also, no statistics of regression (e.g., RMSE) are given. You can show them in the Fig.3. In P7L25, it is hard to “see” performance difference of two regression methods because of lack of statistical metrics. And the statistics should make more sense if a larger data sample is collected. If the wind tunnel can be configured to

reach any target wind speeds, it should not be difficult to make more measurements at variable wind speed conditions in order to collect a sufficiently large data sample. I think this is a big weakness of the paper.

P8L15: The way the aerodynamic entrainment is calculated (F0-3min minus F3-10min) is not convincing. Please explain why there is no significant difference between the saltation flux between 0-3 min (unlimited supply condition) and 3-10 min (supply limited condition)?

P9L1-7: This part of discussions is questionable. The authors state that “with intensified surface renewal from S1 to S3, the relationship between dust flux and friction velocity increasingly resembled the aerodynamic entrainment under unlimited supply.” The authors show that the vertical dust flux is proportional to u*ˆ10 in S1 strong saltation condition, u*ˆ4 in S1 week saltation condition, u*ˆ6 in S2, and u*ˆ7 in S3. These n values still substantially deviates from the n=3 in Eq. 1. That means the F-u* relationship does not fall in the aerodynamic entrainment regime. By the rule, S1 supply limit state (n=4) is most close to the aerodynamic entrainment regime.

The authors also states that ”From this point of view, dust emission can be considered to be mainly driven by aerodynamic entrainment, whereas saltation and creep are responsible for surface renewal which restores the availability of dust for emission. In general, dust emission can be seen as the result of restricted aerodynamic entrainment.” I agree that saltation and creep is responsible for surface renewal; but that does not lead to the conclusion that during that process, aerodynamic entrainment is the main mechanism for dust emission. Saltation and aggregates disintegration are contributing to emission while they replenish the surface at the same time. The conclusion by the authors is not supported by any quantitative analysis that can prove the dominant role of aerodynamic entrainment in dust emission. Also, explain what ‘restricted aerodynamic entrainment’ means.

P9L32, the authors’ claim that “the last stage of S2 must be due to the contribution of

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aerodynamic entrainment" is not convincing. I understand that at high u* (last state of S2), surface renewal provides more erodible materials which increases the dust vertical flux. However, it is not necessarily due to the mechanism of aerodynamic entrainment. I think the authors are trying to emphasize the role of aerodynamic entrainment, but their analysis is groundless.

Considering the above comments, the abstract and conclusion sections of this paper must be rewritten. The summary #2 in the Section 5 is groundless and misleading. The authors state that n = 10 in the case of aerodynamic entrainment, but Eq. 1 shows n = 3 in the aerodynamic regime (Eq.1 was used throughout the paper to separate the aerodynamic entrainment regime). The authors state that aerodynamic entrainment is even a dominant process under certain circumstances. Please elaborate on that. What specific circumstances are they? I think the authors made lots of efforts to relate their experiments to aerodynamic entrainment, but the focus of this paper is on surface renewal. Many issues around that are not addressed, such as the renewal rate, dependence on wind speed/soil texture/soil size distribution/vegetation, biases in current dust schemes due to lack of surface renewal, and possible ways to introduce to dust schemes.

Minor comments:

Section 2.1, explicitly state that F is the vertical dust flux.

P1L24: there is -> there are.

P2L14: uplifted->uplift.

P2L15: inconsequential->insignificant.

P3L3: in equation 3, ηc is the soil clay content in percentage101.

Equation 4: η is already used in eq.3, use a different symbol.

P3L20: you already defined u*t above.

P5L21: if -> of.

P5L25: use dust vertical flux (not dust emission rate) to be consistent throughout the manuscript.

P9L9: η is already used in other places. use a different symbol.

P9L20: limit->limited.

Comments on Figures and figure/table captions:

Add S1, S2, S3 labels on the Fig. 2.

Add regression equations in Fig. 3.

Describe the horizontal dash lines in Fig. 4 caption.

Change 'dust emission' to 'dust vertical flux' in the Fig. 6 caption.

Table 2: Explain the meaning of the symbols in the caption (i.e. the parameters in the log normal size distribution).

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