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Discussions

Interactive comment on "Turbulent Characteristics of Saltation and Uncertainty of Saltation Model Parameters" *by* Dongwei Liu et al.

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Review on the manuscript "Turbulent characteristics of saltation and uncertainty on saltation model parameters" By Dongwei Liu, Masahide Ishizuka and Yaping Shao

General comment : This paper aims at investigating the turbulent behaviour of the saltation flux based on experimental data from a previous field campaign (JADE Experiment, Ishizuka et al., 2008, 2014). From these measurements, the authors questioned the relevance of the models used to compute the saltation and dust fluxes in 3D models that described an average behaviour and ignore the small-scale dynamical features. The author propose to re-visit the determination of the coefficients used in such "average" saltation models to account for these effects. The subject is of great interest and the data set used by the authors offers a unique opportunity to investigate the impact of the turbulence on the total saltation fluxes and its variation with the par-

ticle size. However, the text is too succinct in many parts of the manuscript, some key information are missing and the conclusions drawn at each step of the work are not sufficiently stated and argued. The links between the different parts of the manuscript are not sufficiently explicit. I would thus suggest the authors to provide a deeper analysis of their results and to further improve the text and wording to make their work more convincing.

Major comments : Introduction : The introduction clearly state the position of the problem. Some suggestions to better organize the text are given in "Minor comments".

Part 2 : I would suggest to separate by subtitled the first part describing the computation method and the one describing the data and their pre-treatment. In fact the reading could be more easy if the data were described first and the computing method after.

A few lines to introduce the objective of the part concerning the computing algorithm and to make the link with the introduction are absolutely required. Several method are briefly described to end up with the one selected by the authors without arguing why this method is better adapted than the others to analyse saltation and wind friction velocities data sets. At the end of the chapter (page 4 line 141) the reader does not really know what is computed with this method regarding to the different results presented in the following parts.

On line 163-164, the author mention that the fitting of the vertical profile lead to inaccuracies in the estimation of Q, but that it would not affect the results of this study. A quantitative estimation of these accuracies is needed.

The authors used a data set of U* average over one minute. A discussion on the relevance of this time-scale would be welcome. U* is more commonly averaged over tens of minutes to represent the average effect of the main turbulent structures.

Part 3 : This part should be divided in subsections (time series and wind dependence of Q; Pdf; intermittency; power spectra). In general, the figures and interpretations given in part 3 are not sufficiently described and commented to be fully understood and appreciate.

a) Time series : Figures 1 and 2 shows times series of Q and U*, with a 12-days data set and a zoom on a two days data sets that is not included in the previous 12-days period. These figures (b) are not commented in the text and at this stage of the manuscript, the reader cannot understand why they are shown. Page 13, line 203-213 : the behaviour of Q is very different on day 71 and 72 and the authors argued that the hysteresis behaviour during these two days can be due to changes in surface properties and atmospheric turbulence. Is there any observational evidences for these differences or is it just speculative? If the atmospheric turbulence is different, one may expect different results for these two specifics days in the following parts of the paper. But they are no more evoked in the following.

b) Probability density functions : Figure 4 present the probability density function of the saltation fluxes for different particles sizes. How does the pdf of the total flux compare to the pdf of the size-segregated fluxes ?

The results concerning the pdf of the wind friction velocity and of Q is very questionable. The "modelled" Q is computed after fitting a Weibull function on the experimentally determined U*. Why isn't it computed directly from the experimental wind friction velocity? The authors argued that the Weibull function fits "well" the U* pdf, but the quality of the fitting does not appear to be so good on figure 5 : the number of wind speeds just above the threshold seems to be significantly underestimated while the highest winds seems to be over estimated by this function. Why not fitting only the values above the threshold or fitting U*3? This may improve the representation of the pdf and the quality of the modelled Q. The poor level of agreement between the computed and measured Q is also surprising since the correlation between the modelled and measured Q was of 0.7 for the same experiment and the same model (Shao et al., 2011).

The discussion on the impact of the soil size distribution (page 8 lines 263-269) is

not clear neither the conclusion that can be drawn. Could the impact of the soil size distribution on the modelled flux be estimated since it is an input data of the Shao's model?

c) Intermittency : The "Intermittency section" should include a more precise description on the way it is computed. Indeed, the fact that it is as low as 0.1 when the threshold is 0.2 m/s does not seem consistent with figure 1 : for the well identified saltation events (days 56, 57, 60, 61, 62, 63, 69) the saltation flux Q1m looks positive when u* is higher than 0.2. A lower value suggest that the intermittency is computed over the whole time series, i.e. including periods of high winds with no saltation. Integrating periods of high winds with and without saltation does not corresponds to the initial concept on intermittency which correspond to the fact that during a given event, the wind velocity can be successively below or above the saltation threshold. From one avent to the other many factors can act to prevent wind erosion on a given day compared to the others (precipitations, soil moisture). A table providing, event by event, the number of time steps with u*>u*c and the fraction of these time steps with Q>0 would make things more clear. The way the lower limit for Q is defined should also be described.

Figure 6 shows that the intermittency vary with the particle size and the authors conclude that the saltation of larger particle is more intermittent. An explanation could be the saltation threshold increases with the particle size (at least for particle diameter >80-100 μm)

d) Power spectra : The power spectra of the saltation flux and of the wind friction velocity is one of the most interesting result of the manuscript. The way it is computed should be described and the results further discussed and analysed.

It is quite common in the literature on turbulence to see normalized power spectrum of the wind velocity, including both the horizontal and the vertical components measured by sonic anemometers. The frequency is also often normalized to the height of measurements and the mean wind speed, which allows to compare the results from different sites. Here the authors show the power spectrum of the wind friction velocity as a function of the frequency of measurements. They should explain why and how they produce the results from figure 7. How should the power spectrum of the wind friction velocity compare to the “classical” power spectrum of wind velocity? The authors comment the behaviour of the spectrum for different frequencies and relate this to the typical time scale of dynamical processes. References to similar results in terms of wind spectrum would make the results more convincing. The figure also raises the question of the data set of Q used to compute the power spectrum. The scale of the frequencies extend down to 10-6, i.e. more than 270 days while the whole sampling period is less than one month. From figure 1, it seems that the saltation episodes do not last longer than a day. Are the data set for Q1m and Q1s limited to periods for which the measured Q(z) are non-null (and once again the way the minimum Q is defined should be described) or do they include periods with no saltation recorded ?

The similarity of the spectrum of Q and U* is a strinking results that should be further highlighted. The power spectra of Q1m and Q1s both exhibit a peak at 2.10-3 Hz (less than 10 min). What does this mean? That a 1min acquisition time step is sufficient to properly describe the way saltation is impacted by turbulence ? This is also an original results that should be further discussed.

Part 4.2 : The objective of this part is to test whether a probability distribution of u*t and c0 would improve the capability of the saltation model to reproduce the measured fluxes. This part also suffer for a lack of description on the method to estimate the pdf of ru*t and rc0 and on the way the modelled Q are finally computed for the final comparison with the measured Q. In this comparison, rather that the modelled and measured pdf of Q, one would expect a quantification of the benefit on the level of agreement between the measured and computed Q (correlation coefficients, RMSE, for example). It would be interesting also to test the change in the level of agreement with observations using the full distribution of the r parameters (figure 9) and the peak value only.

The author discuss the possible influence of the soil moisture, but the conclusion is not clear : the sentence “over the period ..” does not seems to be correct (a verb missing ?) and cannot be understood. It is not clear from the following sentence (“in this study .;”) whether the influence of the measured soil moisture is effectively accounted for in the modelled Q used to determine the distributions of r.

The discussion of the stochasticity of c0 in particular for weak saltation is not sufficiently linked to the discussion on the intermittency, which is mentioned only at the very end of the section.

From figure 6, it is expected that u*t may vary with the particle size. But only rc0 is found dependent on particle size. The authors should comment on this possible contradiction. They state that the most frequent values of ru*t do not differ substantially, but what about the parameters of the distribution ? And what range of variation is considered as substantial ? When the author described the way rc0 is determined, it is not clear how they combine the determination of ru*t with the determination of rc0. Once again a more precise description should be given before the presentation and discussion the results.

In the last section, it should be clearly specified how the computation is made : are the measured u*t used ? is the soil moisture effect included ? are the “optimally estimated” u*t and c0 corrected with the p(u*t) and p(rc0) ? If all these effects are included, what are the main sources of differences between the measured and modelled Q ? Does the level of agreement between the modelled and measured pds of Q depend on the erosion events ? What about the saltation flux cumulated for the different erosion events ? Depending on the application, the error could be acceptable, but in any case, it should be quantified.

Part 5 : Even if a few line of conclusion and perspective are given at the end of this section, I would suggest a to add more conclusive elements and some perspectives open by the presented work in terms of modelling but also in terms of improving the experimental set up for the coming field experiment.

Minor comment :

Page 1 line 38 : Replace Staut by Stout.

Page 2 line 68-69 : It should be stated that, beside the establishment of the flux equations, the value of the coefficient is generally derived from measurements.

Page 2 line 69-70 : I would suggest to skip a line before the sentence "the total (all particle size) saltation flux . . .". Since the size dependence of the flux equation was not proposed by Kawamura (1964) nor White (1979) but was mainly added for modelling applications.

Page 2 line 75 : I would suggest to skip a line before the sentence "Observations show, .." and to add references from the literature to give a range of c0 derived from observations.

Page 4 line 167-172. The temporal resolution of the atmospheric variable measurements should be given here.

Page 4 line 174-177 : I am not sure it is the right place to present the wins erosion model.

Page 7 figure 4, please specify Q1s and Q1m on the axes of the figure and use the scale scale for Q to make the two figures easily comparable.

Page 8, line 285-286. "This shows that saltation intermittency mainly occurs under weak wind conditions" : since intermittency is defined as the fraction of time the wind friction velocity exceed the threshold, isn't it obvious that it occurs mainly when the wind friction velocity os close to the threshold ?

Page 10 line 318, part "4.2" should be part "3.2".

Page 13, line 406-408 : Figure 9 reports the distribution of rc0 and ru*t, it does not

show that "for a fixed choice of u*t and co, even if they are optimally chose, a portion of the measurements cannot be represented by the model".

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