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Interactive comment on "A 3-D simulation of drifting snow in the turbulent boundary layer" *by* N. Huang and Z. Wang

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Authors' Responses to the Comments on the Manuscript "A 3-D simulation of drifting snow in the turbulent boundary layer" from reviewer 1

General Response: According to the suggestions of the referees' comments, we have made a substantial revision to the original manuscript so that a clear description on the research is displayed in the revised version. We deeply appreciate the time and effort you have spent in reviewing our manuscript. The detailed responses to the comments of the referees are as follows:

Major Comments:

(1) Comments from Referees:

Introduction: The literature review in the introduction is clearly inadequate. Drifting snow has been studied intensively for decades now and the work presented should be discussed in light of earlier model developments. Some of the relevant literature is cited but additional aspects need to be considered. Those aspects include conceptual differences to earlier models, which use RANS approaches such as Gauer (2001) and Schneiderbauer and Prokop (2011). It should be pointed out that your work is the first to use a meteorological model at such very small scales and compare to the use of fully-coupled meteorological models at larger scales (Vionnet et al., 2014). A very recent work that addresses similar questions (such as intermittency) as the work of the authors and which is also based on particle tracking in an LES generated wind field is Groot et al. (2014) but is not discussed. Further relevant work can be found in the references of the papers mentioned above. More specifically, the latter two works cited in l.9 p.303 do not have drifting snow as a (major) subject. You could cite other works from the same group of authors (Doorschot, Lehning) but those two are inadequately cited here. In addition, in the following lines, the representation of saltation in continuum models is critically discussed but it is neglected that Schneiderbauer and Prokop (2011) have shown, how characteristics of the saltation layer are nicely reproduced by such an approach. In the following, the inadequacy of context representation continues as the text suggest that only few works (Nemoto and Huang) form the basis for the current investigation.

Author's response:

Thanks the reviewer for his valuable feedback and attention to detail in reviewing this manuscript. Following the reviewer's suggestion, the introduction section has been rearranged.

Author's changes in manuscript:

The detail changes are as follows:

The works of Gauer (2001) and Schneiderbauer and Prokop (2011) are reviewed in line

57-61 in the revised manuscript as '3-D simulation of drifting snow gradually carried out in recent years. Gauer (2001) first simulated the blowing and drifting snow in Alpine terrain with Reynolds Averaged Navier-Stokes (RANS) approaches. Also, Schneiderbauer and Prokop (2011) developed the SnowDrift3D model based on RANS.'

The works of Vionnet et al. (2014) and Groot et al. (2014) are cited and introduced in the line 61-64 in the revised manuscript as 'Vionnet et al. (2014) went on a study of large-scale erosion and deposition using a fully coupled snowpack/atmosphere model. Groot et al. (2014) simulated the small-scale drifting snow with a Lagrangian stochastic model based on LES and the intermittency of drifting snow was analyzed.' The cited literatures of (Lehning et al., 2002; Bavay et al., 2009) in line 44 of the revised manuscript are replaced by that of Clifton and Lehning (2008).

The sentence 'Obviously, the above assumption is not in agreement with the real situation. In addition, these models could reveal neither the movement mechanisms of snow particles nor the factors affecting the behaviors of snow particles.' in p. 303 l.20ff has changed to 'These models have a significant role in promoting the drifting snow research although some information can not be acquired from these models, for example, the trajactory of particle and its movement mechanisms.' in line 47-49 the revised manuscript.

(2) Comments from Referees:

Model presentation and model choices: While it is in my opinion not necessary to reproduce the basic equations for LES as used in ARPS, the section should be used to present in a much clearer way, which parts of the models have been taken as used by others (e.g. Vinkovic et al., 2006) and which specific changes have been introduced by the authors for this study. I expect that some adaptations have been made specifically for snow but this is not clearly stated. At this stage, also a comparison with earlier formulations of snow grain-bed to flow interactions (e.g. Clifton and Lehning, 2008) can be made. This would be particularly useful to support the strong statement on p.306

l.16ff that the physical splash formulation by Kok and Renno (developed for sand) can be taken for snow. It would be good to show some validation for this. A major comment is also that the authors appear to have chosen a unconventional parameterization for the bed – flow interactions. It appears that aerodynamic entrainment is not modelled but that constantly particles are ejected into the air without respecting a threshold friction velocity or wind speed. The authors must justify this choice and (preferably) present some evidence/validation why their choice of entrainment / rebound / splash formulation is adequate.

Author's response:

Following the reviewer's suggestion, the 'Model and Method' section has been rewritten and modified and now it present in a clearer way.

Author's changes in manuscript:

1. The aerodynamic entrainment for snow (Clifton and Lehning, 2008; Groot et al., 2014) is included in the model as described in section 2.3.1 (line 131-150) in the revised manuscript. And the threshold friction velocity of this situation is obtained through a series of simulations and the sentence 'And we found the lower bound of friction velocity for a drifting snow is approximately 0.18 m/s for this situation.' was added in line 243-244 of revised manuscript.

2. The modified rebound / splash functions (Kok and Renno, 2009; Groot et al., 2014) that suit for snow saltation are introduced into this model. Their applicability and the choice of parameters are discussed in detail. More detailed description is offered in the 'Model and Method' section (line 151-201) in the revised manuscript.

(3) Comments from Referees:

Model Validation: The study uses experimental data from own measurements in a wind tunnel (as far as I have understood) plus experimental results from the literature. The comparison between model and experimental data occurs at diverse locations in the paper. This is a nice feature of the paper. I suggest nonetheless to improve the structure and introduce a separate model validation section, in which such comparisons are made and have a seperate results section, in which the main findings are shown and developed. Of course, the results section may also feature some experimental data comparison but a "model validation“ section would be very useful in my opinion.

Author’s response:

Thanks the reviewer for his attention to detail in reviewing this manuscript. Following the reviewer’s suggestion, a "Model Validation" section is added in the revised manuscript, in which the comparison with experiment results and effectiveness evaluation are mainly made in this section. The differences between model and observation results are also analyzed in this section.

Author’s changes in manuscript:

The "Model Validation" section is added in line 245-307 in the revised manuscript.

(4) Comments from Referees:

Computational domain: While the computational domain was obviously picked to generate a setting comparable to wind tunnel data, it may not have enough lateral (spanwise) or streamwise extension to have reliable results on turbulent fluctuations. Again much more details need to be given to fully judge the approach. From what can be understood, they appear to use two domains, in which the first domain serves to generate already turbulent input for the actual investigation section. This cycle region must be long and wide enough to generate reasonable turbulence statistics.

Author’s response:

Thanks for the reviewer’s careful reviewing of the manuscript. The simulation details, including computation domain, boundary conditions and instructions have completely changed.

Author’s changes in manuscript:

The modified description of the simulation details in line 203-227 the revised manuscript is as follows:

‘In this paper we have performed some wind tunnel experiments to obtain the initialization data for the simulation as well as to compare the simulated results with experiment results. The computational region is set as 16m*1.0m*1.5m and divided into two sections, as show in Figure 1. The first zone extending from x=0m to x=5m is used to develop a turbulent wind field and provide a steady turbulent boundary layer. In this simulation, the turbulent characteristics separating from our wind tunnel results are added on the initial logarithmic velocity profile at beginning and the inlet velocity of fluid will be equal to the wind velocity at the location of x=5m after 5 seconds, which realizes a long distance development of the turbulent boundary layer. The second zone is the blowing snow region from x=6m to x=16m, where a loose snow layer is set on the ground.

In this model, the grid has a uniform size of dx=dy=0.05m in the horizontal direction, and the average mesh size of dz=0.3m in the vertical direction. The grid is stretched by cubic function to acquire detailed information of the surface layer and the smallest grid is dzmin=0.002m.

The actual computation time is 30 seconds, in which the first 10s and the second 10s are respectively used for the development of turbulent boundary layer and the drifting snow, and the last 10s for data statistics. The dynamic Smagorinsky-Germano subgrid-scale (SGS) model is used in this simulation. For the flow field, we apply the rigid ground boundary condition at the bottom, the open radiation boundary in the top, the periodic boundary condition in the spanwise direction, the open radiation boundary condition at the end of the domain along the streamwise direction. The forced boundary is applied in the inflow as mentioned above. Additionally, the snow particles have circulatory motion in the lateral boundary and they will disappear when moving out of the outlet in the end of the domain.'

(5) Comments from Referees:

Conclusions: The main conclusions are not all very well connected to the results presented. The conclusions make a statement on the intermittency but this is not much discussed in the text and no quantitative indicators of intermittency are presented (see comment above). Also the strong statement on particle size changes with height needs further clarification. The authors cite the study of Gromke et al. and re-print some of the experimental data but do not mention that his data supports more a decrease of particle size with height than an increase – at least for the larger particles. As discussed above these findings are also in contradiction to field data from Antarctica and Canada.

Author's response:

Thanks for the reviewer's careful reviewing of the manuscript. The simulation processes are performed in a more reasonable way and the new results are analyzed in detail with respecting the comments above. The conclusions are modified base on the new results and reviewer's suggestions in the revised manuscript.

Author's changes in manuscript:

The revised conclusion section in line 395-406 of the revised manuscript is as follow: 'In this study, the3-D drifting snow process with mixed particle size in the turbulent boundary layer is performed and we conclude that:

(1) Turbulent fluctuation may significantly affect the trajectory of small snow particles with equivalent diameter dp=100 micrometer, while has little influence on that of particles with larger size. And the saltating particles can strengthen the turbulent fluctuation.

(2) Fully developed drifting snow swings forward toward the downwind in the form of snow streamers and the wind velocity is proportional to the concentration of snow particles at different locations of the turbulent boundary layer.

(3) The change of spanwise velocities of snow particles along height relies on the friction velocity and the spanwise velocity is one order of magnitude less than the streamwise direction in general.'

Detailed Comments:

Comments from Referees:

p. 302, l. 24: Why do you only mention Antarctic ice sheets here?

Author's response:

The reviewer is right. Drifting snow has significant impacts on global ice sheets. We have modified the expression in the revised manuscript.

Author's changes in manuscript:

The word 'Antarctic' has been deleted in line 32 of the revised manuscript.

Comments from Referees:

p. 306, l. 21: Bed – particle interactions are in principle fully deterministic, thus I would say that they "can be described" stochastically.

Author's response:

Thanks. The express here may ambiguous.

Author's changes in manuscript:

The sentence 'The grain-bed interaction is a stochastic process, in which the impact particles may rebound with a certain probability' has changed to 'When a moving particles impact on the bed, it may rebound into air again' in line 152 of the revised manuscript.

Comments from Referees:

p. 307, l. 6: Please justify choice of parameters and/or give a reference.

Author's response:

Following the reviewer's suggestion, the references are added for the choice of parameters.

Author's changes in manuscript:

The references (Anderson and Haff, 1991) and (Groot et al., 2014) are added in line 156-160 of the revised manuscript.

Comments from Referees:

p. 309, l. 24: Why do you use the density of ice here?

Author's response:

A snow particles is a small piece of ice and it's approximately regarded as a sphere (p. 306, l. 4), so the density of snow particle is equal to ice, which was also adopted by Nemoto and Nishimura (2004).

Comments from Referees:

p. 310, l. 1: Why do you choose not to use one of the commen parameterizations (e.g. Groot et al., 2014) for aerodynamic entrainment? See also major comment above.

Author's response:

Following the reviewer's suggestion, the aerodynamic entrainment parameterization of Groot et al. (2014) is used in the revised manuscript.

Author's changes in manuscript:

The aerodynamic entrainment for snow (Groot et al., 2014) is included in the model as described in section 2.3.1 (line 131-150) in the revised manuscript.

Comments from Referees:

p. 311, l. 11ff: This statement is misleading because many previous models actually

resolve the 3-D nature of the particle movement (e.g. Groot et al., 2014 and many earlier ones).

Author's response:

Thanks for the reviewer's careful reviewing of the manuscript. The misleading sentence has changed in the revised manuscript.

Author's changes in manuscript:

The sentence 'Different from previous models which are unable to clearly describe the drifting snow structure' has changed to 'Most previous models are unable to clearly describe the drifting snow structure' in the revised manuscript.

Comments from Referees:

p. 311, l. 16: Explain how you suppressed the influence of turbulence for this trajectory.

Author's response:

The particle trajectory without turbulence is calculated by another modified code of drifting snow (Zhang and Huang, 2008), in which the wind profiles are consistent for the two simulations.

Author's changes in manuscript:

The sentence 'in which the blue line denotes the motion trajectory that is not affected by the turbulence' has changed to 'The blue dotted line denotes the motion trajectory that is not affected by the turbulence and it is calculated by another drifting snow model (Zhang and Huang, 2008) with the same take-off velocity and wind profile.' in line 321-323 of the revised manuscript.

Comments from Referees:

p. 312, l. 5ff: Nice qualitative observation.

Author's response:

Thanks.

Comments from Referees:

p. 312, l. 10ff: I suggest to use quantitative measures of intermittency such as "deviation from randomness" or "coefficient of variation" and compare with literature values both from models and wind tunnel experiments as reported in the previous works discussed above.

Author's response:

Thanks. Here we focus on explain the formation mechanism of snow streamers and its moving process. We may quantitatively analyze the characteristics of intermittency in the future works.

Comments from Referees:

p. 313, l. 3ff: I suggest to report quantitatively how the transport rate increases with friction velocity and compare to published results.

Author's response:

Thanks for the reviewer's useful suggestion. A relation between transport rate and friction velocity is present and compared to published results.

Author's changes in manuscript:

The relation between transport rate and friction velocity is added in the line 258-262 of the revised manuscript as:

'The relationship of STR per width Q and friction velocity u* can be expressed as

Q=1.94*u*4.51

which is consistent with the experiment results of Sugiura et al. (1998) and the simulation results of Nemoto and Nishimura (2004).'

Comments from Referees:

p. 313, l. 12ff: I suggest to condense these dependencies in a figure and compare against available wind tunnel data.

Author's response:

Thanks for the reviewer's useful suggestion. Following the reviewer's suggestion, this part has been reanalyzed and rearranged base on the new simulation results.

Author's changes in manuscript:

This part of analysis has been replaced by a new figure and compared with experiment results (figure 5 and the introductions in line 263-268).

Comments from Referees:

p. 314, l. 5ff: Since these optimizations are closely linked to the entrainment / rebound/ splash formulation, a sensitivity analysis should be made to show dependence on splash function parameters. See also corresponding major comment above.

Author's response:

Thanks. Following the reviewer's suggestion, the suitability of entrainment / rebound/ splash formulation as well as the choice of parameters are discussed in the 'Model and Method' section and compared with relative works in the revised manuscript.

Author's changes in manuscript:

The modified introductions of entrainment / rebound/ splash formulations are present in line 131-201 of the revised manuscript.

Comments from Referees: p. 314, l. 26ff: I would think that the high-speed particles can also be a model error and the explanation offered (you can't capture them) should be supported by more evidence.

Author's response:

Thanks the reviewer. The explanation may unclear and changed in the revised manuscript.

Author's changes in manuscript:

The sentence 'This is mainly because the concentration of snow particles decreases with height increasing, making it increasingly difficult to be captured the high-speed snow particles.' has changed to 'It should be noted that the high-speed particles in this simulation are significantly more than those captured in the experiments (figure 6(b)). This is mainly because the mean velocity of snow particles increases with height increasing, our measurement is mainly set at lower positions due to the limitation of instrument and thus part of high-speed particles are not being captured.' in the line 282-286 of revised manuscript.

Comments from Referees:

p. 315, l.14ff: These observations depend again highly on the entrainment / rebound /splash function formulation and should be discussed in this context. For the dependency of saltation height on the friction velocity, a more physical discussion should be made.

Author's response:

Thanks for the reviewer's careful reviewing of the manuscript. This result has been recalculated based on the modified entrainment / rebound /splash formulation and a new simulation set. The adaptability and choice of parameters are discussed in the 'Model and Method' section.

Author's changes in manuscript:

A more physical and detail discussion about particle size changes with height is added in the line 302-307 of the revised manuscript as: 'However, it appears that the mean diameter increases with increasing height above the saltation layer. The main reason may be that the small particle trends to carry smaller inject velocity, while the larger

particle is just the opposite due to the stronger inertia. The rebound velocity is proportional to the incident velocity and thus larger snow particle will rebound with a bigger initial velocity.'

Reference: Nemoto, M., and Nishimura, K.: Numerical simulation of snow saltation and suspension in a turbulent boundary layer, J. Geophys. Res., 109, D18206, 2004. Zhang, J., and Huang, N.: Simulation of Snow Drift and the Effects of Snow Particles on Wind, Modeling and Simulation in Engineering, 408075-408076, 2008. Sugiura, K., Nishimura, K., Maeno, N., Kimura, T., Measurements of snow mass flux and transport rate at different particle diameters in drifting snow, Cold Regions Science and Technology, 27(2), 83-89, 1998.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/9/C368/2015/tcd-9-C368-2015-supplement.pdf

Interactive comment on The Cryosphere Discuss., 9, 301, 2015.