Authors' Responses to the Comments on the Manuscript

"A simulation of the large-scale drifting snow storm in a turbulent boundary layer"

General Response to the Comments:

According to your comments, we have made a substantial revision to the original manuscript such that a clear description on the research is displayed in the revised manuscript (the directly changes can be seen in the revised manuscript with changes highlights). The detailed responses to comments of referees are as follows (see blue part in this reply):

Responses to Comments of Reviewer#2:

General Comments :

[Comment] The submitted manuscript described novel large-eddy simulations of large-scale blowing snow-storms. While the models utilized are well-established, such a phenomenon has not been previously explored using LES. The results of the simulations and their description and analysis are interesting and this reviewer feels that this study may be published in TC. However, there are some major concerns that should be addressed before hand. The comments are listed below ordered by section.

[Response] Thanks for your careful reviews and relevant comments. A substantial revision to the original manuscript has been made according to your kind advice as listed in specific comments, please see our point-to-point response below.

Specific comments:

[Comment 1] Section 2.1 : There seems to be misunderstanding about the use of the SGS velocity approach of Vinkovic et al. The SGS velocity is defined with respect to the frame of reference of the particle and not the flow. Thus, the splitting of local wind velocity as 'large-scale' and 'subgrid-scale' computed using Eq. 4 is incorrect.

[Response 1] Thanks for your relevant comment. In order to correct this mistake, the sentences 'Namely, the local wind velocity $\tilde{u}_i\left(\vec{x}(t)\right)$ is composed of a resolved Eulerian large-scale part $\tilde{u}_i\left(\vec{x}(t)\right)$ (obtained from the linear weighting of surrounding grid points) and a fluctuating SGS contribution $u_i'(t)$.' have been changed into 'Namely, the local relative is expressed as $V_{ri} = \tilde{u}_i\left(x_p\right) - u_{pi} + u_i'$, in which $\tilde{u}_i\left(\vec{x}_p\right)$ is the resolved large-scale wind speed at the particle's position and is determined by the resolved wind speeds of surrounding grid points through the linear interpolation algorithm.' in line 129-132 of the revised manuscript. Besides, the contents about SGS velocity have been moved to section 2.2 of the revised manuscript for a better understanding.

[Comment 2] Section 2.3 : Note that τ is not the total fluid shear stress but the total shear stress. When there are negligible particles, say at z > 1 m, τ and τ_f are equal. In lines 148-149, why is the ejection number set to 1 ? where does this value come from ? Sugiura and Maeno measured a much higher value.

[Response 2] Thanks for your careful reviewing. According to your comment, the expression 'total fluid shear stress' has been modified into 'total shear stress' throughout the revised manuscript.

On the other hand, the splash model of Sugiura and Maeno (2000) determines the relation between the ejection number and the speed and incident angle of the impactor, and the ejection number includes both rebound and ejected particles. They measured a much higher ejection number during the development of the drifting snow. However, we set a saturated saltation layer as the bottom boundary condition for particles, in which case the numbers of impact and lift-off particles should be equivalent (one impactor corresponds one ejected particle). Thus, the ejection number of 1 comes from the steady saltation condition.

In order to make it more clearly, the description 'and $\langle v_i \rangle$ is set to be the threshold of impact velocity, which is determined by setting ejection number

$n_e = 0.51 v_i^{0.6}\theta_i^{0.16}$ equal to 1.' has been modified into 'and $\langle v_i \rangle$ is set to be the threshold of impact velocity. Considering the steady-state saltation condition (one impact particle generates one ejecta on average), $\langle v_i \rangle$ is determined by setting ejection number $n_e = 0.51 v_i^{0.6}\theta_i^{0.16}$ equal to 1.' in the revised manuscript, as shown in line 170-172.

[Comment 3] Section 2.4 : Why is the initial potential temperature and relative humidity of the atmosphere described ? Is it relevant for the discussion ?

[Response 3] Thanks for your careful reviewing. As a matter of fact, the initial potential temperature and relative humidity of the atmosphere are used to determine the air density. In the revised manuscript, the content '$\rho = p(1-q_v/(\varepsilon+q_v))(1+q_v)/(R_d T)$ is the air density, in which p, q_v, R and T are the pressure, the specific humidity, the gas constant (287.0 $J\,kg^{-1}K^{-1}$) and temperature of the air, respectively, and ε=0.622 is a constant.' has been added in line 84-86 of the revised manuscript.

[Comment 4] Section 2.4: The imposition of constant heat flux at the surface is perhaps the most questionable point for this reviewer. The study of Pomeroy and Essery found the 50 W/m2 flux for a brief period of time (20 mins perhaps) during which, there was no blowing snow. In fact for most of the study, the sensible heat flux is either negligible or negative. The imposition of a constant heat flux at the surface is in effect creating a convective boundary layer that is providing a constant supply of energy in the form of vertical motions.

[Response 4] Thanks for this relevant comment. Typically, the atmospheric turbulence is generated and maintained by two forces: wind shear and buoyancy force. Most studies set the heat flux to zero, which corresponds to an ideal shear-driven planetary boundary layer (PBL). However, these two forces may act together to modify the flow field in actual situations (Moeng and Sullivan, 1994). In this study, a small heat flux is added in the shear-dominated PBL to produce a 'intermediate PBL' that is closer to the real situation (A buoyancy-dominated convective PBL generally requires a heat flux larger than 200 W/m^2). Although the surface heat flux may be changed during drifting snow, however, the smaller surface heat flux basically not affect the structures of drifting snow storms, also see the analysis in [Response 9] and [Response 11].

In order to make it more clearly, the description 'Actually, this condition corresponds to a 'intermediate' turbulent boundary layer that dominated by wind shear force. Thus, the structures of the drifting snow storm should not be affected by the changing surface heat flux significantly if the surface heat flux is small. Further simulations with different values of surface heat flux (<100 Wm^{-2}) also prove this point.' has been added in line 204-209 of the revised manuscript.

[Comment 5] Section 2.4: line 179: How many snow particles are present in one particle parcel ?

[Response 5] Thanks for your comment. In this simulation, one particle parcel represents 10^7 snow particles. The description 'In this simulation, each particle parcel contains 10^7 snow particles.' has been added in line 214-215 of the revised manuscript.

[Comment 6] Section 2.4: What is simulation time step for the flow as well as for the particle dynamics?

[Response 6] Thanks for your comment. In this simulation, the large and small time steps (acoustic wave integral) for the wind field calculation are 0.1 s and 0.02 s, respectively. Besides, the particle time step is determined by the minimum of particle relaxation time $T_p = \rho_p d_p^2 / 18\rho\nu$ to ensure a smooth particle trajectory (Dupont et al., 2013). The description 'The large time step and small time step (acoustic wave integral) for the wind field calculation are 0.1 s and 0.02 s, respectively, and the particle time step is determined by the minimum of particle relaxation time' has been added in line 215-217 of the revised manuscript.

[Comment 7] Section 3.1 : This reviewer (as well as the readers !) would highly appreciate vertical profiles of horizontal wind speeds simulated for different u?

[Response 7] Thanks for your comment. According to your suggestion, the simulated horizontal wind speed profiles for different friction velocities are added in the revised manuscript, as shown in Fig. R1.

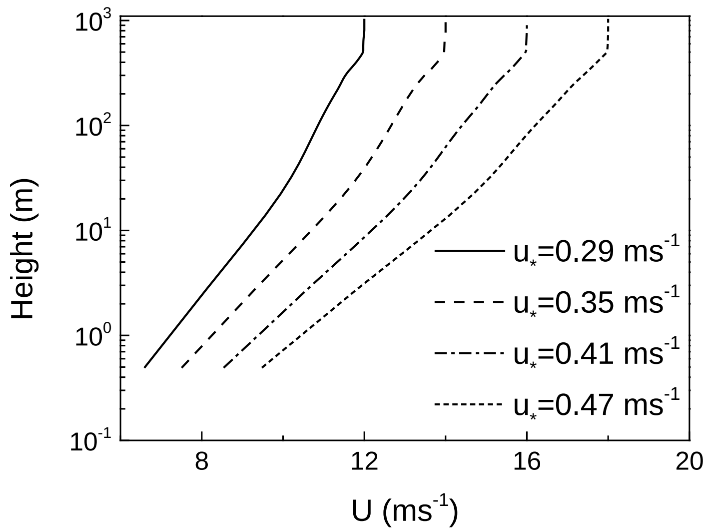

Figure R1. Horizontal wind speed profiles under various friction velocities.

In the revised manuscript, Fig. R1 and the description 'The mean horizontal wind speed profiles of the fully developed turbulent boundary layer under various friction velocities are shown in Fig. 7b. The horizontal wind speed increases with height and changes into a constant above the boundary layer. The rapid decrease of the snow transport flux occurs at about the top of the turbulent boundary layer, mainly because turbulences become weaker above this height and less particles can be transported to a higher altitude.' have been added, as shown in line 292-297 and Fig. 7.

[Comment 8] Section 3.2 : lines 250- 253 : The exponentially decaying transport flux profile is used to describe the saltation layer only and not the suspension layer.

[Response 8] Thanks for your careful reviewing. According to your comment, the sentence 'In previous studies, the transport flux profile is commonly described using an exponential decay form based on the extrapolation from measurements near the surface (Mann et al., 2000;Nishimura and Nemoto, 2005;Schmidt, 1982, 1984;Tabler, 1990), which may result in a considerable underestimate of the total transport flux.' has been modified into 'In previous studies, only the transport fluxes at the near surface are commonly measured (Mann et al., 2000;Nishimura and Nemoto, 2005;Schmidt, 1982, 1984;Tabler, 1990), thus, the features of the entire transport flux profile is largely unclear, which may result in considerable uncertainties about the total transport flux.' in the revised manuscript, as shown in line 307-312.

[Comment 9] Figure 7 and the corresponding text is a good result - but how are these numbers affected by the surface heat flux imposed ?

[Response 9] Thanks. According to your comment, the effect of surface heat flux q_s on the structures of drifting snow storm is examined. The results indicate that the structures of drifting snow storms are less affect by the surface heat flux when it is small (e.g., $q_s \leq$ 100 Wm^{-2}). As shown in Fig. R2, the proportion of the suspension flux above h_c to the total suspension flux is only slightly affected by the surface heat flux, and the influence of surface heat flux becomes weaker and weaker with the increasing friction velocity, mainly because larger friction velocity results in stronger turbulence under the actions of wind shear.

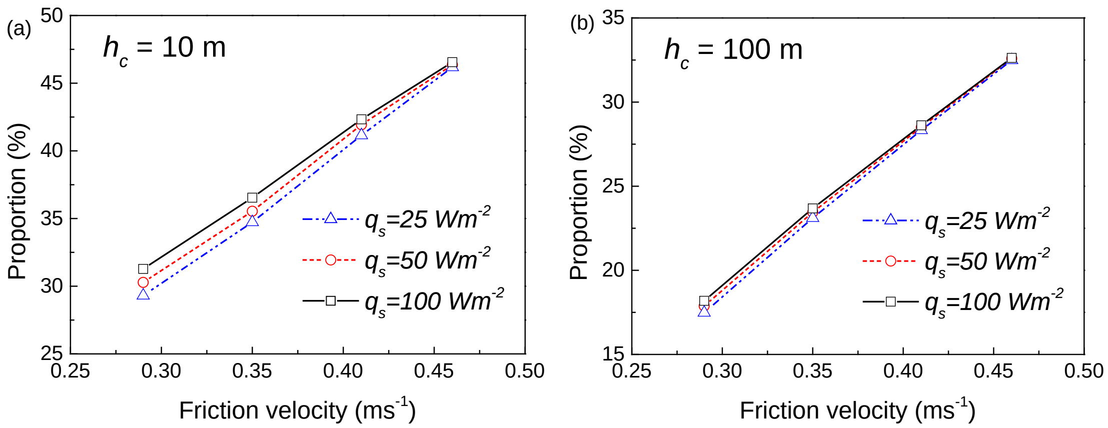

Figure R2. Proportion of suspension flux above h_c to the total suspension flux under various friction velocities and surface heat fluxes.

In the revised manuscript, the description 'From Fig. 8 (b), it can be seen that the proportion Q_c to the total suspension flux is only slightly affected by the surface heat flux, which indicates that the structures of drifting snow storm are not sensitive to the surface heat flux under this condition. The influence of surface heat flux is also weakened by the increasing friction velocity, mainly because larger friction velocity results in stronger turbulence under the actions of wind shear.' has been added in line 324-329. And Fig. R2 (a) has been added in the revised manuscript, as shown in Fig.

8 (b).

[Comment 10] Section 3.3 : Lines 273-274 and Figure 8 : what is meant by snow storms without atmospheric turbulence ? How was this simulation achieved ? This is extremely unclear.

[Response 10] Thanks for your comment. Actually, the model settings for Fig. 8 are the same as other simulations. The only difference for the example in Fig. 8 is that the resolved wind speed at particle's position is replaced by a given value obtained from the standard logarithmic profile during calculating the particle's trajectory. In this way, the effect of large-scale turbulent structures on the development of the drifting snow storm is removed.

In order to make it more clearly, the description 'This simulation is achieved by replacing the resolved wind speed at particle's position ($\tilde{u}_i\left(\vec{x}_p\right)$) with a given value obtained from the standard logarithmic profile, and the other model settings and simulation procedures stay the same with other simulations. In this way, the effect of large-scale turbulent structures on the development of the drifting snow storm vanishes.' has been added in line 339-344 of the revised manuscript.

[Comment 11] Section 3.3 : Figure 10 and the corresponding text : This reviewer feels that this result is extremely dependent on the imposed heat flux at the surface – How is this 'thickness' dependent of the surface heat flux? The snow particles in the present case seem to reach the top of the computational domain!

[Response 11] Thanks for your careful reviewing. Indeed, the thickness of the drifting snow storm is directly related to the boundary layer dynamics, and surface heat flux may change the structures of the drifting snow storm. However, as discussed in [Response 4] and [Response 9], a smaller surface heat flux may not changes the depth of the turbulent boundary layer significantly. This is because a smaller heat flux generally modifies the air temperature profile at the near surface, as shown in Fig. R3. In this figure, the surface heat flux is increased to 100 W/m^2 for the purpose of exploring the effect of surface heat flux on the flow structures, and the domain height is also increased to 1500 m. Compared with the initial air temperature profile, the air temperature at the near surface is increased, and the increment decreases with height to form a temperature gradient. The maximum air temperature increment is about 0.25 K, and the predicted air temperature is almost coincident with the initial profile above 600 m.

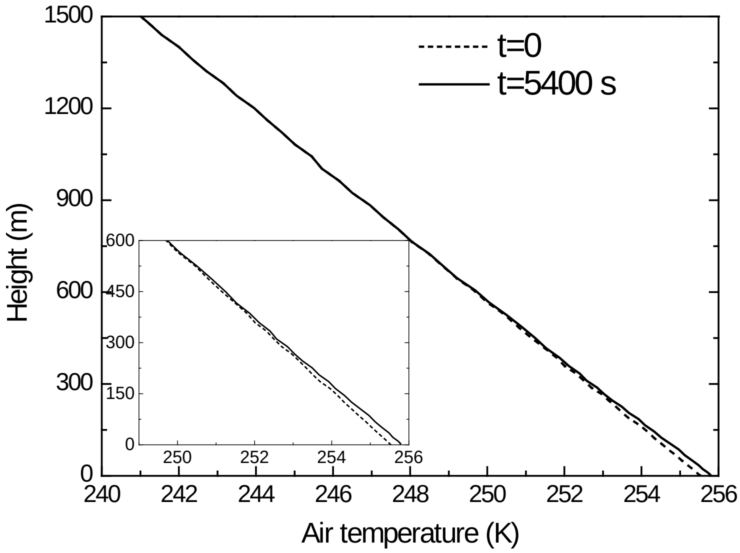

Figure R3. Air temperature profiles at different moments (The domain height is 1500 m, and the surface heat flux is 100 W/m^2. Other simulation settings are unchanged).

Thus, the current height of the domain is enough for the wind shear dominated turbulent boundary layer. A much larger surface heat flux may result in a buoyancy force dominated turbulent boundary layer due to stronger vertical convections. However, the turbulence structures as well as the depth of the buoyancy dominated turbulent boundary layer is quite different from those of the wind shear dominated turbulent boundary layer (Moeng and Sullivan, 1994). And we may further explore the structures of drifting snow storms in a buoyancy force dominated turbulent boundary layer.

In the revised manuscript, the description 'Higher domain heights are also tested with the same model settings, and the thickness of the drifting snow seems basically unchanged. Drifting snow storm with difference thicknesses may be achieved by changing the initial state of the air and surface heat flux.' has been added in line 388-391 of the revised manuscript.

Finally, once again we appreciate you for your good and comprehensive comments. Those revisions according to your comments really make this manuscript improve a lot.

Thank you!

Yours sincerely,

Zhengshi Wang, Shuming Jia

References:

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