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Interactive comment on "The impacts of moisture transport on drifting snow sublimation in the saltation layer" *by* N. Huang and X. Dai

N. Huang and X. Dai

daixq08@lzu.edu.cn

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Thanks for the insightful comments and positive evaluation of our work. We have studied comments carefully and will do our best to revise and improve our manuscript. The comments by the reviewer are repeated and the responds are as follows.

In this study, a 2-D snow drift model is introduced. A saltation model is coupled with a treatment of moisture and temperature changes associated with snow drift. Much emphasis is placed on the description of the model, and it is relatively short on discussion of the model results. The paper is reasonably well written. A somewhat disappoiting aspect of the paper is that it does not have measurements for comparison or even published data for comparison. May be this can be improved.

Reply: Thanks for the insightful comments and positive evaluation of our work. As

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Discussion paper

mentioned in our manuscript, previous studies on drifting snow sublimation mainly concentrated on the suspended snow. Whereas the sublimation of salating particles was generally ignored due to the consideration that sublimation will soon vanish in the saltation layer for the feedback of drifting snow sublimation (DSS) may lead to a saturated layer near the surface. Therefore, there are very few existing studies and published data on snowdrift sublimation in the saltation layer. In this manuscript, we only give a comparison of snowdrift sublimation between saltating and suspended snow to clarify the importance of drifting snow sublimation in the saltation layer. Just like Prof. Yaping Shao, we also think that some measurements are necessary for validation of our simulation results. Unfortunately, the measurement of snowdrift sublimation in the saltation layer is very difficult to conduct at the present stage. Thanks again for the comments, we will try to conduct such measurements in our future studies.

The self-limiting nature of snow drift process is clearly revealed. This self-limiting process is similar to saltation of sand with no sublimation, but appears to be more complex, as it involves the moisture process. It is not clear however how the modified stability of the flow influences the self-limiting process.

Reply: Thanks for the insightful comments. In this study, a wind-blown snow model, balance equations for heat and moisture of an atmospheric boundary layer, and an equation for the rate of mass loss of a single ice sphere due to sublimation were combined to study the sublimation rate of drifting snow by tracking each saltating particle in drifting snow. Therefore, the influence of flow on the snow sublimation is mainly evaluated by influencing the processes of saltation movement (equation 5) and the horizontal advection of moisture (equation 12). Because the maximum mass loss of the sublimation for a single snow particle is less than one thousandth of the particle' mass during a process of saltation movement under the conditions of this study, we didn't evaluate mass change of the particle on the air flow.

The authors did not consider the effect of turbulence. While including turbulence may be more difficult, the authors may wish to discuss what might happen if turbulence is

included. This is also important, because the stability of the saltation layer also affects the profile of the mean wind. Indeed, I do not see where thermal stability is included in the model.

Reply: Thanks. We acknowledge the comment that some studies (Jasper F. Kok and Nilton O. Renno, 2009; Yaping Shao, 2010) did include the effects of turbulent in their saltation model and it was found that turbulent flow substantially affects the saltation movement of sand particles, mainly the movement of small particles. But the effect of turbulence on larger saltating particles is much less pronounced for their larger inertia and thus smaller susceptibility to fluid velocity perturbations. For example, Shao (2010) showed the effect of turbulence flow for 200 micrometres particles in a logarithmically-profiled airflow (u^*=0.5m/s) is small. In our simulations, the diameter of the snow particles is 200 micrometres. Furthermore, this study concentrates on the time-averaged contributions of saltating snow particles to snow sublimation. Therefore we didn't take into consideration of the effect of turbulence in this manuscript. Perhaps we need to include turbulent effects in our future work.

There are minor writing problems which the authors should carefully check again, for example, DSS is not defined.

Reply: Thanks for the comment. We will try our best to check our manuscript carefully again to modify the writing problems. Following the reviewer's comment, we have defined DSS as drifting snow sublimation in the first sentence of the abstract in the revised manuscript.

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