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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

M. Lehning (Referee)

lehning@slf.ch

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General:

The paper presents a numerical study of drifting snow at small scale. The study is based on numerical simulations with the meteorological model ARPS. It is not clearly stated whether the authors introduce the particle tracking feature of the model themselves or whether this is taken "as is“ from other groups that are cited (Vinkovic et al., 2006). While I do think that the paper contains novel results, the current form of the paper is not ready for publication. The abstract does not state novel results. E.g. the long and complicated sentence in the abstract between lines 9 and 13 ony reports qualitative findings, which are already known. Interesting aspects of the main text are the time-space dynamics of maximum mass fluxes as related to coherent flow structures and the investigation of particle size separation. The findings on the latter point are, however, in partial contradiction to experimental results e.g. of Gordon and Taylor (2009) and Nishimura and Nemoto (2005) but also to earlier results (e.g. Schmidt). These contradictions need plausible physical explanation and could be made the main subjects of the paper if much more quantitative analysis can be provided and it can be shown that it is not a model artefact. More specifically, since the model produces an increase in particle size at the higher end of the saltation layer, this model behaviour must be analyzed and explained in detail. In general, the paper needs a more specific focus on certain aspects of saltating snow and follow a nice and stringent line of argumentation from introduction to conclusions.

Major Comments:

1) Introduction: The literature review in the introduction is clearly indadequate. 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

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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.

2) 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 snowgrain-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 treshold 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.

3) 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.

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4) 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.

5) 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.

Detailed Comments:

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

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

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

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

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.

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).

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

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

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.

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

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

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.

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.

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.

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