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The Cryosphere Discussions

Interactive comment on "On the suitability of the Thorpe-Mason model for Calculating Sublimation of Saltating Snow" *by* Varun Sharma et al.

Anonymous Referee #2

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General comments:

The manuscript by Sharma et al. presents a novel method to account for the effects of sublimation on blowing snow in the atmospheric boundary layer, based on recent high-quality Large-Eddy Simulation (LES). Of particular focus is the sublimation processes in the saltation layer; although numerous attempts have been done to reveal the sublimation processes in the suspension layer of blowing snow, investigations focused on the saltation layer were scarce. They found that regarding the saltation layer, the Thorpe and Mason model (a general method used in blowing snow sublimation) could yields erroneous results. Their findings will be of great importance to clarify an appropriate application condition and limitation of the Thorpe and Mason model. Additionally, the newly proposed method in the manuscript may become important in the

developing a better understanding of blowing snow sublimation.

The manuscript overall is very well written. The research is of good quality, LES runs with a Lagrangian blowing snow model are impressing. The methodologies and discussions are organized properly in general. Supplementary material is quite informative. Thus in general I think this work is highly appropriate for publication in The Cryosphere (TC). Additionally, the subject fits very well the journal.

I think it would be useful if the authors gave more information about the vertical profiles, in the latter calculations (EX. III and IV). I am wondering how the profiles such as wind speed, mass flux, temperature, sublimation rate etc. will change during saltation (e.g. before saltation, after the onset (transient regime), during steady state). I understand that investigation of the vertical structures is not the primary purpose of this study, however some interpretation of the vertical profiles seems essential. This will increase the credibility of the model.

Specific comments:

P.2, L.5: Perhaps it will be a good idea to refer the previous sublimation simulations in suspension. e.g., Xiao et al., 2000, An intercomparison among four models of blowing snow, Boundary-Layer Meteorology, 97, 109-135.

P2., L.9: "recent studies using high-resolution large-eddy simulations" – is the reference really use LES? I could not confirm in that paper.

P.2, L.24: "saturation ${\sigma_*}$" – does it mean rate of saturation?

P.2, L.25: "Groot Zwaaftink et al. 2011, 2014" should be "Groot Zwaaftink et al. 2011". In Groot Zwaaftink et al. (2014), mass loss due to sublimation are neglected in the calculation.

P.2, L.27: "Vionnet et al., 2014).In" should be "Vionnet et al., 2014). In" (please add a space).

P.3, L.9: The units should be given in Roman type, I think.

P.4, L.3: Is the first-order scheme sufficient in the calculation?

P.5, L.12: " a erodible" should be "an erodible".

P.6, L.15: Do you have any specific reason for the different temperature conditions (-5<dT<5 in EX. IV, -2.5<dT<2.5 in EX. II).

P.7, L.7-8.: "low initial saturation results in more sublimation and cooling near the surface, resulting in suppression of vertical motions." – This is interesting indeed. Could you show the modification of the vertical profiles (temperature, sublimation rate, wind speed etc.) to illustrate these processes?

P.7, L.18: "at a field scale" specifically, during realistic saltation of snow?

P.8, L.2: "here)." should be "here." (an unnecessary parenthesis).

(Supplementary material)

P.2, L.26 (S9): "ln" should be given in Roman type.

P.3, L.27: I think parenthesis is missing around the reference.

P.3, L.29: {\sigma_d_p} is undefined, I think.

P.4, L.2-4: Could you include the relevant references (Clifton and Lehning (2008) ?).

P.4, L.16-17: Is the rebound angle the same as that for sand? I think Kok and Renno (2009) obtained the results for sand. Do you hypothesis the angle is similar to sand saltation?

P.4, L.19-20: "dislodge additional additional particles" should be "dislodge additional particles".

P.5, L.22: "represented by f." f should be given in italic.

P.7, L.23: Is the first-order scheme adequate for the computation in this study?

P.8. "Time step" – All the elements (fluid, particle, and scalars) have the same timestep?

P.9, L.5: "It shows that that once" should be "It shows that once"?

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