The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-33-RC1, 2018

The Cryosphere
Discussions
Open Access

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

Anonymous Referee #1

Received and published: 10 April 2018

General Comments:

This is a very interesting and valuable paper that pursues an important topic in blowing snow studies. The manuscript addresses the suitability of the steady-state sublimation model of Thorpe and Mason using a high degree of modeling complexity. The manuscript is also unique in that it addresses the issue of snow sublimation at a much higher resolution than the typical hydrological modeling style. This work should be published.

However, the increased sophistication of the modelling techniques requires further clarification because of unanswered questions of how certain sub-processes of saltating particle sublimation are handled numerically. By nature of this paper's numerical approximation of reality, it is the onus of the authors to clearly explain how their models

Printer-friendly version

Discussion paper

(and their required assumptions) differ from the old model (and its assumptions). Given that none of this work is grounded in observation, a higher level of transparency is required to make this work truly beneficial.

The transient nature of these processes at this time scale are the most important result of this manuscript and require significant explanation of what is actually occurring in the model. One significant of LES is the ability to represent some sort of turbulent structure in an effort to more closely represent reality. As such a more thorough explanation of the turbulence driving the NUM model is needed to be of utility to researchers attempting to apply these findings in a real-world setting. This is especially true because of the parameterization of Nusselt and Sherwood numbers used to drive sublimation rates.

The experiments describe using a variety of particle sizes but does little to mention how this affects of modes of transport (and sublimation according to "mode"). Particle flux profiles and sublimation profiles with height would further inform the realism of this study. This could be put in supplementary material without serious restructuring of the manuscript.

Specific Comments:

P1 L8 (and throughout): This does not appear to be a perturbation in a functional sense as you are not perturbing a system. This is more like a sensitivity analysis, changing initial conditions. There is ambiguity in this phrasing as a perturbation of 1K can mean strictly a temperature difference of 1K (which I believe you mean) or adding 1K to the difference. I would suggest replacing perturbation so as to not mislead the reader into thinking they will be reading a manuscript using perturbation theory.

P2 L1: There are actually three modes of transport, including creep.

P3 L20: Is this truly a representative illustration? What is the ventilation rate of that bottle?

P4 L1: That’s very true!

** P4 L15-17: This is a very important change of sign. What is the explanation for the initial change from deposition to sublimation for the colder-than-air particles? Was this a period where the particle actually warmed up? Gained mass? Was the air surrounding the particle cooling through latent or sensible heat? It is very exciting that this information is finally available!

** This concept is overlooked throughout the paper. You have a wonderfully extensive data set. Explain whether or not sublimation is the only transfer of energy to your saltating particles. Please explain whether or not (and why) the particles actually warm in your simulations. Is there no thermodynamic feedback on the systems in section 2? Does Sigma star never change with time? Why or why not? Do values in Fig 1b,e actually affect the change in the ambient or near-particle air, or did the model assume these energy exchanges were "advected" away?

P4 L21-24: This is an interesting idea. However, there appears to be some serious assumptions used to reach this conclusion. Please clarify the following: Did you assume there is no ventilation or sublimation of particles when they are in contact with the surface, and is it assumed that the wind speed is constant across all heights of the trajectory of a saltating particle? Admittedly, there is certainly a connection between relaxation times and residence times, but it would increase the quality of the paper to convey either what assumptions are necessary to make the conclusions from Figure 2a to be truthful?

P4 L21: Relaxation time is great, but what about the time that $Err_{Q,M}$ goes to zero (Fig 1f)? This value seems equally as important, as it appears to be a lower bound on the timeframe in which stationary wind/transport conditions are required to allow all the numerical errors to cancel out. This paper would benefit from an exploration (surface plot) of relaxation time over the parameters (σ_*, $T_P - T_{Air}$), and supplement Fig 1 a-c very well.

P4 L27: Can you speculate as to what is the physical (or numerical) meaning of this scaling relationship? Or is this a purely empirical finding?

P4 L31 Fig 1g-l: Please expand the negative range of $T_P - T_{Air}$. There are environments where föhn events can bring dramatic changes of temperature up to 10C over only a few hours!

P5 L20: Look at parameterization

P5 L26: It is unclear to me how using this stationary flow is fundamentally different from your steady state model. Was the LES used because it is a more sophisticated framework in which to calculate these fluxes? Besides the evolution of friction velocity is Figure S2, I am afraid I have missed the point of using such a complex tool to solve some PDE's.

P5 L32: Why was this not discussed in the previous experiments?

P6 L3: Please stop calling this realistic saltation of snow. The parameterizations and assumptions necessary to run this numerical model make this statement misleading. Please rephrase as LES simulation of saltation or something similar.

P6 L5: These varying friction velocities are referred to as "low medium and high windspeeds" in line 33. What wind speeds were necessary for these values? Friction velocities do a poor job of representing turbulence in even subtly complex terrain, and as saltation is a drag-driven process, at the very least, mean wind speeds should be included in the manuscript, and extensive time series of turbulence statistics (Turbulence Intensity, TKE, shear stress, etc.) in the supplementary material. As this research is conducted to benefit those that models in natural settings, and those natural setting will be much more turbulent than the LES, and that turbulence is what is driving the ventilation rates, more information about the model is needed.

P6 L5: Where is "the surface" defined for this stress calculation? And how is that surface defined? How can that be reconciled with the fact there is windpumping into the snow pack? Or is this a Reynolds-stress-based value?

Printer-friendly version

Discussion paper

P6 L15: Why a different range of temperatures than Section 2?

Fig 2a: Redo the plots so it is clear what is happening. I cannot understand anything from 200um to 1000um. The diameter plot markers appear somewhat logarithmically. Try plotting with a logx scale? And why do the residence time measurements become more sparse at smaller particle sizes? Please redo the symbols as they are confusing and inconsistent, or eliminate them altogether.

P6 L23: There is no dependence on σ_*?

P6 L29: Please elaborate why the values of mass loss are wrong. It appears in Fig 1c,f that the cumulative errors go to zero over time. Why is this no longer the case with LES?

P7 L1: Please rephrase "larger-scale turbulence statistics." It unclear to me how any "larger-scale turbulence" can be represented in a 6x6x6 meter box. Is this not an increase in mean windspeed?

P7 L3-8: This is a very interesting finding! This suppression of vertical motions and how the model responds should be elaborated on! A comparison of the vertical turbulence statistics amongst the experiments is necessary as they all assume uniform initial air temperature (P6 L5 comment). How does vertical mixing in the LES deal with this over time? Logic would imply that this same suppression of vertical mixing could also be caused by a colder snow surface temperature and increased stability. Why have you disregarded particle surface temperature in your PRT experiments? Would this effect be found if a temperature gradient as found in nature were present, or would the numerical effect be overwhelmed by the near-surface temperature gradients? As it stands, this statement cannot stand alone and the conclusion needs more development and supporting data/plots would be very beneficial.

P7 L3-8 These are very small particles, can they be considered in "Suspension?" Obviously, there is a full spectrum of motions, but approximately where have other re-

Printer-friendly version

Discussion paper

searches been separating saltation from suspension on Fig 2a? This would be very informative as the paper by nature is a saltation study.

P7 L12: Very exciting finding!

P7 L18: What is field scale?

P7 L21: Can anything be said about the low end of the friction velocity domain where intermittent transport dominates? Would TM over or underestimate in that case?

Fig 3: Where have the particle diameters gone? What distribution of sizes are you using?

P8 L22: Not perturbations.

Overall this is will be a great contribution to the field, and will no doubt be referenced extensively. However, at its current state, the manuscript needs to be expanded. It is much too short for the gravity of the conclusions.

Technical Corrections:

P1 L7: Please specify: snowpack surface temperature, snow particle surface temperature?

EQ3 What is "d"? d_p?

P2 L24 "Saturation $\sigma_*\ldots$" ? Do you mean sigma is saturation?

P2 L27: Add space after sentence end.

P5 L12: "an erodible"

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-33, 2018.