Response to reviewers:
On the suitability of the Thorpe-Mason model for calculating sublimation of saltating snow

Varun Sharma, Francesco Comola and Michael Lehning

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A note to all reviewers

Please see the additional document found in the "comments_to_all.pdf" file

Response to Reviewer # 1

Opening Remarks:
We would like to thank Reviewer #1 for his/her detailed critique of the submitted manuscript and for asking different clarifications and questions. Broadly stating, the following additions and/or corrections were made to the article in response to Reviewer #1's comments:

- We has enlarged Section 2 with a more detailed description of the dynamics of the heat and mass transfer from a solitary ice-grain and made clear the approximations entailed.
- An new figure is added that shows the evolution of particle diameter and temperature is Experiment I.
- A visual representation of one of the LES performed in Experiment III is provided to make clear, the sort of LES that have been performed.
- The supplementary material has been updated with 5 additional figures detailing various results from the LES.
- The caveats and limitations of the current LES model setup have been more explicitly mentioned in the updated manuscript and a few future directions of research have been listed in the expanded concluding section of the manuscript.

A: Scientific Concerns

- ***A.1 : 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 1 K can mean strictly a temperature difference of 1 K (which I believe you mean) or adding 1 K 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.

Response A.1: We thank the reviewer for this comment and agree that perhaps using the word “perturbation” is misleading. We modify the text as follows:

With a small temperature difference of 1 K between the air and the snow surface, the errors due to the TM model are already as high as 100% with errors increasing for larger temperature differences.

- ***A.2 : P2 L1: There are actually three modes of transport, including creep.***

  Response A.2: We agree with the comment and the text has been modified as follows:

  Aeolian transport of snow can be classified into three modes, namely, creeping, saltation and suspension. Creeping consists of heavy particles rolling and sliding along the surface of the snowpack either due to form drag or bombardment due to impacting particles.

- ***A.3 : P3 L20: Is this truly a representative illustration? What is the ventilation rate of that bottle?***

  Response A.3: This analogy was used only to highlight the fact that there is a possibility of deposition of vapor on saltating ice grains. This possibility has never been explored and/or accounted for in the existing models that only assume a uni-directional exchange of water mass from the ice grain to the atmosphere (unless there is super-saturation of the atmosphere, which is usually not allowed in atmospheric models). On a beer bottle or anything from a refrigerator, we see a reverse process of extraction of vapor from the atmosphere onto the material and the atmosphere does not need to be super-saturated for this !

  In terms of ventilation rate, if we consider the Reynolds’ number of a beer bottle of diameter 5 centimeters with a ice grain of diameter 200 microns, there is approximately two order of magnitude of difference. To have the same Reynolds number, $|\vec{u}_{rel}|^{icegrain} \approx 250|\vec{u}_{rel}|^{beer\ bottle}$. Thus, for a typical relative velocity between a saltating ice grain and air of 5 m/s, the beer bottle’s relative velocity needs to be only 0.02 m/s for the same Reynolds number and exchange coefficients. This is entirely plausible. Thus, even in terms of Reynolds numbers, the beer bottle analogy works !

- ***A.4 : P4 L1: Thats very true!***

  Response A.4: *Skipped*

- ***A.5 : 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?***

Response A.5: We thank the reviewer for posing several critical points in this question. These questions strike at the heart of the message of the paper and thus it is extremely important for us to make sure that we are able to get our message across to the readers.

In the first set of experiments in Experiment I, the particle as well as the air have the same temperature of 263.15 K. However, the air is not saturated and thus there is a diffusion of mass from the ice grain to the air as described by Equation 2. However, since the temperature of the ice grain is the same as the air, there is no heat transfer. The initial energy for the sublimation must then come from the internal energy of the ice grain. The internal energy is nothing but the heat energy stored in the ice grain as represented by the grain temperature. As the internal energy of the ice grain is consumed, it's temperature decreases and as soon as this happens, heat transfer between the ice grain and the air commences. After a transient period, an equilibrium condition is achieved where the particle temperature becomes constant and all the energy necessary for sublimation comes directly from the atmosphere.

The Thorpe-Mason model neglects the initial consumption of internal energy for sublimation and instead assumes that all the energy for sublimation comes from the atmosphere. In fact, the Thorpe-Mason model, by means of further approximations, does not consider particle temperature at all ! In this manuscript we show that for ice-grains in saltation, it is important to take into account, the ice-grain temperature and its evolution.

Returning to Experiment I, in the second part, we vary the initial temperature of the ice-grain with the ice grain being warmer or colder than the surrounding air. Here, the interpretation become slightly more difficult. In the case where the particle is colder than the air, there is both the warming of the particle as well as deposition. The particle gains energy both from convective heat transfer (second term in the RHS of Eq 1) as well as gains mass (Eq 2). At a certain point in time however, the particle becomes warm enough (though still colder than air), that it begins to sublimate.

Note that the temperature (T_{Air}) and saturation (represented by σ_*) of the air surrounding the ice-grain does not change and all mass or energy gain/loss of quantities in the air as assumed to be advected way. We justify this because we considering the dynamics of a solitary ice grain, subjected to relatively strong air motions. A helpful image is to imagine a special hair-dryer blowing air onto a 200 micron ice grain. However, in the LES experiments in Section 3, all the feedbacks are taken into account.

Thus our motivation for Section 2 was to simply highlight the fact that particle temperature, and the coupled heat and mass exchange dynamics are important to account for, instead of the approximate solution presented by the Thorpe-Mason approach, particularly for the

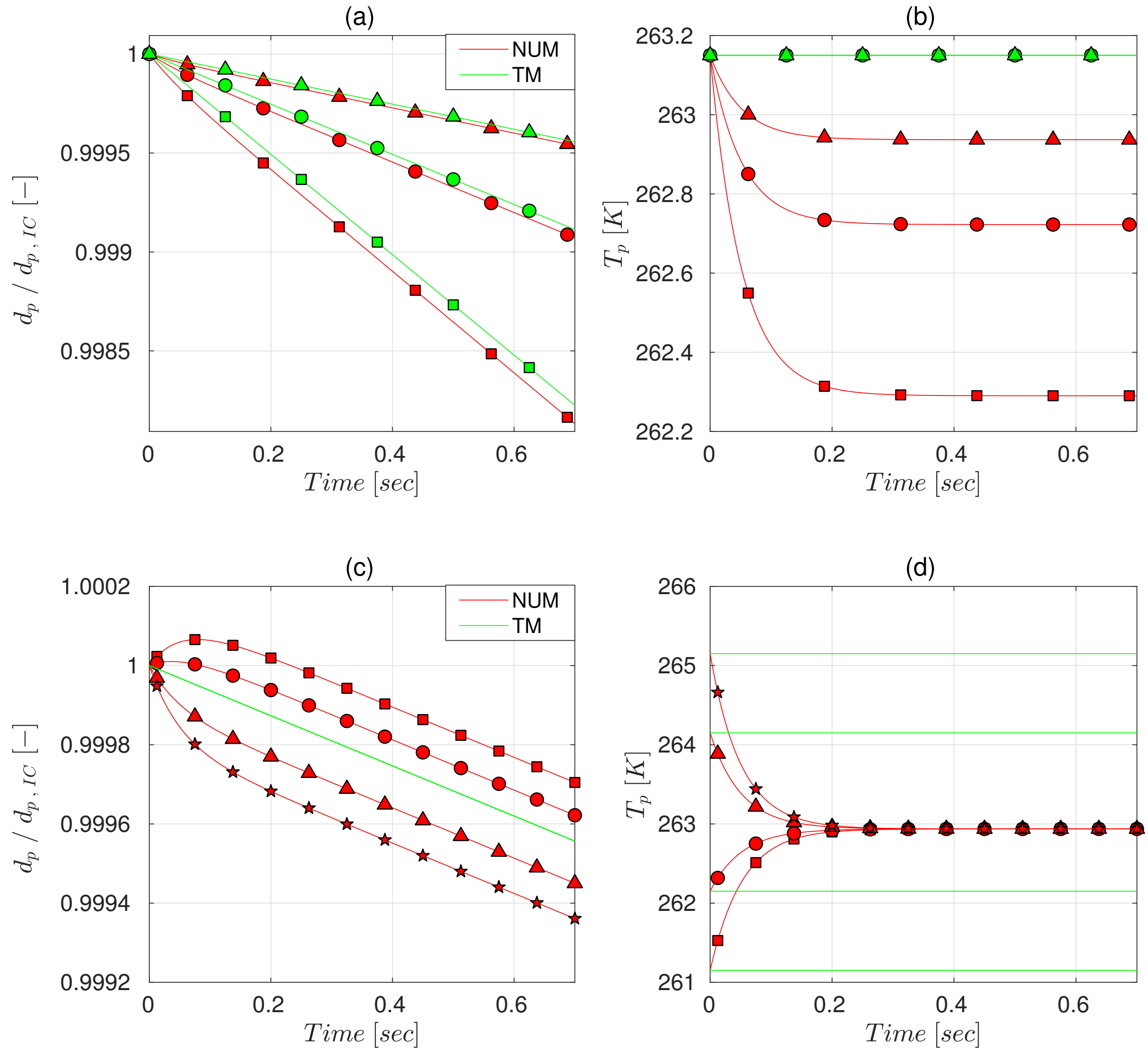

Figure 1: NUM and TM solutions for a particle of 200 μm diameter in different environmental conditions. **Experiment I-A**: Evolution of particle (a) diameter and (b) temperature; $T_{p,IC}$–T_{Air}= 0, σ_*= 0.8 (squares), 0.9 (circles), 0.95 (triangles). **Experiment I-B**: (c-d) same as (a-b) with σ_*=0.95; $T_{p,IC}$–T_{Air}= -2 K (squares), -1 K (circles), 1 K (triangles), 2 K (stars). Note that the particle diameters are normalized by the initial diameter of the particle ($d_{p,IC}$).

short time-scales that we are interested in.

In response to the points raised in A.5, we have decided to update Section 2 to be more explicit about the nature of the simulations performed and the simplifications of the experiments. We have split Figure 1 of the original manuscript into two independent figures (a figure each for Experiment I and II) so that the plots are more clear and add an additional figure (Figure 1 in this document) to describe the evolution of particle diameter and temperatures in the different experiments. The new figure is added below for reference. The change in the text can be seen in the updated manuscript with the differences highlighted.

- ***A.6 : 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?

Response A.6: We thank the reviewer for raising a pertinent point here and giving us a chance to clarify. Firstly, we do not make a conclusive statement as evidenced by the use of the word "likely". Since there is no actual data on particle temperatures measured in wind tunnels or in the field at present, we cannot make a conclusive statement and more research is needed. Thus it is only a conjecture at present. The results in Experiments I and II however are well-correlated to those from LES data in Experiment III and IV and thus there is credible support for this idea.

It is true that we do not take into account, the particle temperature and sublimation while it is at rest at the surface. The heat and mass transfer from the particle to the air begins only once it is lifted from the surface (either aerodynamically or due to splash entrainment). Secondly, it is indeed true that the wind speed is not constant across all heights of the trajectory of the saltating particle. This is the reason why we compute the relaxation time from relative velocities ranging from 0 to 10 m/s. These would correspond to the upper and lower bounds of the relaxation time for particle heat and mass transfer dynamics. We compare the mean and median residence times of the saltating particles to this "range" of relaxation times (the shaded region in Figure 2a in the original manuscript) rather than a single value of relaxation time.

- ***A.7 : 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.***

  Response A.7: This is great observation by the reviewer. However, we would like to point out that the errors in the cumulative heat and mass output in Figure 1c and 1f go to zero "very slowly" and in fact does not go to zero within typical saltation residence times. The quantity of relaxation time as we have defined is a far more robust measure to identify from simulations. It is also a more conservative measure as any particle with residence time lower than the relaxation time will, by definition, be lower than the measure proposed by the reviewer.

  As far as the exploration of the relaxation time over the parameters goes, we did in fact do this exploration. However ,it was found that the relaxation time depends only on the particle diameter and the relative velocity between the particle and the air. This is shown in Figure 2a (in the original manuscript) in the shaded region.

- ***A.8 : 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?***

Response A.8: Following the work described in this manuscript, we explored this interesting relationship a bit further and we have reasons to believe that this quantity can actually be derived directly from equations (1) and (2) of the manuscript. This derivation is not yet complete and we leave it for future publications.

- ***A.9 : P4 L31 Fig 1g-l: Please expand the negative range of T_P - T_{Air}. There are environments where fohn events can bring dramatic changes of temperature up to 10C over only a few hours!***

Response A.9: A similar comment was raised Reviewer #2 and so we have increased the range in the updated figure to -5 K to 5 K. Figure 1(g-l) in the previous manuscript are Figure 3(a-f) in the revised manuscript.

- ***A.10 : P5 L20: Look at parameterization***

Response A.10: This has been updated in the revised manuscript.

- ***A.11 : 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 PDEs.***

Response A.11: There are two principal reasons for using the LES. Firstly, we wanted to find out about the residence time of typical saltating ice grains. This information is not available in literature and so we decided to perform LES of a turbulent channel flow with a erodible snow surface as the lower "wall" of the flow. The surface acts as a source or sink of particles with simple stochastic models to account for different entrainment and deposition processes. The transport of particles is modeled by solving the equations of motion for each particle individually once the particle is eroded and is air-borne. The LES methodology for aeolian transport is well established and has been validated in the past. We realize that we have not cited past works in this section and have rectified this oversight.

The second motivation is in fact directly related to a previous comment by the reviewer (A.5). Unlike Experiment I and II, the air surrounding the particle (thinking from the frame of reference of the particle) is continuously evolving with different wind speed,temperature and humidity values. How do the two different approaches for computing sublimation (TM and NUM) compare in this scenario with complete feedback between air and ice grains ? This is question we answer in Section 3 using LES.

Within the LES context, by stationary turbulent flow, we intended to say that the logarithmic profile of the velocity is achieved and the time-averaged turbulent statistics (or Reynolds averaged statistics) are horizontally homogeneous and steady and vary only in the vertical direction. The wall-bounded channel flow that we simulate still has a significant shear in the vertical direction (as expected in the wall-bounded shear flows).

The LES also allows for simulating vertical gradients of temperature and humidity as would exist in nature. The vertical mixing of these scalars allows the sublimation of saltating ice grains to continue as dry air from aloft is continuously mixing downwards into the saltation layer. A detailed analysis of the heat and moisture budgets in the presence of the saltating ice grains will be presented in a future publication.

However, this and other comments have led us to believe that we have perhaps not motivated the use of LES sufficiently in the submitted manuscript, or described the LES in sufficient detail. Even though we go into great detail about the LES and the setup in the supplementary material, we expand the section 3.1 in the revised manuscript.

Additionally we submit a movie (Supplementary Video M1) illustration of the simulation we perform to make it clear the kind of LES we have performed.

- ***A.12 : P5 L32: Why was this not discussed in the previous experiments?***

Response A.12: The initial condition for particle was indeed discussed in the previous experiments but this was perhaps not clear due to lack of proper notation (no mention of $T_{p,IC}$ in Section 2 for example). In the revised Section 2, we explicitly state that we are imposing $T_{p,IC}$ in the experiments in Section 2 as well.

- ***A.13 : 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.***

Response A.13: We have removed the word "realistic" from the sentence. Adding "LES simulation of saltation" does not seem to be appropriate as Experiemt III is purely about using LES. The entire sentence now reads as follows:
The principle aims of Experiment III are to firstly quantify particle residence times (PRT) and their dependence on wind speeds and relative humidities and secondly, compute the differences in the heat and mass output between the NUM and the TM approaches during saltation of snow with complete feedback between the air and the particles.

- ***A.14 : P6 L5: These varying friction velocities are referred to as "low medium and high wind speeds" 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.***

Response A.14: We thank the reviewer to bringing to our attention this fact. The TO BE DONE !

- ***A.15 : 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?***

  Response A.15: In terms of the forces that the surface imparts to the overlying fluid, the surface is treated as a rough wall. The roughness is parameterized using a roughness length $(z_0) = 10^{-5}m$. This approach does not account for the windpumping into the snowpack. We mention this in the revised text.

- ***A.16 : P6 L15: Why a different range of temperatures than Section 2?***

  Response A.16: The range of temperatures in Experiment II has now been increased to -5K to +5K.

- ***A.17 : Fig 2a: Redo the plots so it is clear what is happening. I cannot understand anything from 200 μm to 1000 μm. 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.***

  Response A.17: We thank the reviewer for pointing out lack of clarity in Figure 2. This figure is the most essential part of the paper and thus, it is extremely important for us to make sure that it is well understood by our readers.

  - We have now restricted the figure to 600 μm. There are not enough particles larger than 600 μm and thus the statistics are noisy.
  - The x-axis of the figure is indeed logarithmic. We have added this information in the figure's caption.
  - The markers were added only for differentiating and labeling the different trend-lines. Not all data-points have been marked.
  - As mentioned in the submitted manuscript at P5 L21-22: *The snow surface consists of particles with a log-normal size distribution with a mean particle diameter of 200 μm and standard-deviation of 100 μm..* The particle size distribution (PSD) imposed on the surface comes from previous studies of modeling of saltation of snow. The PSD constrains the particle diameters that are air-borne and undergo transport. Also, we use a continuous spectrum and thus, when calculating statistics of mean and median residence times, we use a fixed bin size of 25 microns. As Figure 2a has a logarithmic x-axis, the measurements appear to be sparse at the lower range of the diameters.

- ***A.18 : P6 L23: There is no dependence on σ?***

  Response A.18: No, the relaxation time $\tau_{relaxation}$ does not depend on σ_*. This is one of the remarkable results of Section 2 and we now make this point more explicitly in the revised manuscript.

- ***A.19 : 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?***

  Response A.19: Once again, we feel that we could have perhaps done a better job in explaining the relationship between Experiments I/II and Experiments III/IV.

  The cumulative errors in Figure 1c,f *tend* towards zero but for a solitary ice grain. In the LES, a particle, original resting at the surface, is made air-borne (either due to aerodynamic entrainment or splashing), makes multiple hops across the snow surface, where is rebounds from the surface, and ultimately comes to rest, i.e, it impacts the surface and does not rebound. In the LES, there are many thousands of particles that go through this cycle during the course of the simulation. Since models parameterizing the erosion and deposition of the particles are stochastic, particles in saltating have a range of hops, distance traveled and residence times. Additionally are a range of particle diameters present in the flow. We track the residence time of each particle, and calculate statistics of mean and median residence time as a function of diameter.

  It is found that the smaller grains (with diameters less than 150 microns) have "on average" residence times that are longer than the relaxation time. Thus for these particles only, the cumulative errors averaged over multiple particles, will indeed *tend* to zero. The LES also have particles (with diameters greater than 225 microns) that have residence times "on average" larger than the plausible values of the relaxation time. Thus, for only these particles, the cumulative errors of mass and heat output will **not** go to zero. Summing all these errors for all the particles in the flow, the total error is non-zero. In fact Figure 3 shows precisely this error and it is found to range from 28% to as high as 40 % in Experiment III.

  Thus, the LES are not performed for a single ice grain, with different simulations for different particle diameters. The LES is performed of a turbulent channel flow with an erodible snow surface consisting of a distribution of particle diameters at the lower wall. The ice grains enter and exit the flow at the surface according to models governing the erosion and deposition mechanisms. The supplementary movie M1 will aid in making this point clear.

- ***A.20 : P7 L1: Please rephrase "larger-scale turbulence statistics." It unclear to me how any "larger-scale turbulence" can be represented in a 6×6×6 meter box. Is this not an increase in mean windspeed?***

  Response A.20: By "larger-scale turbulence statistics", we meant to say that the dynamics of the heavy particles to be invariant to different flow speeds. We simplify the statement as follows:
  This means that the dynamics of the heavier particles are unaffected by different wind speeds simulated in Experiment III.

- ***A.21 : 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.

Response A.21: We agree with the reviewer that this is indeed an interesting finding. We have added an entire section in the supplementary material providing a preliminary analysis of this phenomenon by showing the vertical profiles of the vertical buoyancy flux. However, as we explain in the "comments_to_all.pdf" document, this is an ancillary result that is not directly related to the core message of the paper. The role of buoyancy in mediating aeloian transport is a very interesting and as-of-yet unexplored topic. We are in fact working on this topic currently and hope to present results focusing on this topic in the coming months.

Coming to the additional questions posed by the reviewer, we answer them as follows:

– Accounting for surface temperature is not likely to have a major impact on the stability of the atmosphere in strong snow drift events that we are considering. Whether the snow is sublimating on the surface, or during transport, both processes are going to result in stable stratification of the atmosphere. However, the amount of sublimation and the resulting cooling is much more from the particles in air, in comparison to those lying on the surface. In our simulation, where we have fully developed saltation/snow transport, the effect of the sublimation, and stability due to surface sublimation is likely to be negligible in comparison to the corresponding effect emerging from particles in the air. Note that we have stably stratified air in our simulations as well. Just that the stability emerges due to sublimation of particles in the air and not on the surface. We agree that in intermittent snow transport conditions, the surface boundary condition will become important. This is a matter for further exploration.

– This effect would indeed be found if there is a temperature gradient present. Note that only the initial condition for temperature is fixed at 263.15 K. The temperature in the LES evolves with time and the atmosphere does become stably stratified.

– We stress again the fact that this, although an interesting result, is only ancillary to the core message of the paper and we stress upon this point more in the concluding section of the paper.

- ***A.22 : 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 researches been separating saltation from suspension on Fig 2a? This would be very informative as the paper by nature is a saltation study.***

Response A.22: We present results only for particles that saltate. There are indeed a few particles in "suspension", i.e, particles that once leaving the surface, never deposit during the course of the simulation. But the number of such particles is an order of magnitude lower than those that saltate. Residence times are thus computed only for particles that leave and return to the surface.

- ***A.23 : P7 L12: Very exciting finding!***

  Response A.23: We agree !

- ***A.24 : P7 L18: What is field scale?***

  Response A.24: We have removed this phrase in the revised manuscript.

- ***A.25 : 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?***

  Response A.25: No, intermittent transport is a very interesting phenomenon where a lot more research is required to simulate it properly. The initial friction velocities are chosen such that we have "fully-developed" saltation. Having said that, the TM would still underestimate the mass lost by the solid ice phase due to sublimation but the underestimation will be lower than those found in Experiment III.

- ***A.26 : Fig 3: Where have the particle diameters gone? What distribution of sizes are you using?***

  Response A.26: The particle size distribution (PSD) is imposed as described on P5 L21-22: *The snow surface consists of particles with a log-normal size distribution with a mean particle diameter of 200 μm and standard-deviation of 100 μm..* As mentioned earlier, we have now added a figure with the PSD in the revised manuscript.

  Fig 3 shows the "total" mass lost due to sublimation - from all the particles that have undergone sublimation during the simulation.

- ***A.27 : P8 L22: Not perturbations.***

  Response A.27: We have replaced "temperature perturbations" with "temperature differences" in the revised manuscript.

B: Technical Concerns

- ***B.1 : P1 L7 Please specify: snowpack surface temperature, snow particle surface temperature?***

  Response B.1: In the revised manuscript, the temperature of the snowpack surface temperature and the air flow is specified (as 263.15 K).

- ***B.2 : EQ3 What is "d"? d_p?***

  Response B.2: Yes, it is indeed d_p. This error has been corrected in the revised manuscript.

- ***B.3 : P2 L24 "Saturation σ_* ..." ? Do you mean sigma is saturation?***

  Response B.3: In fact, σ_* is the rate of saturation (or saturation-rate). The corresponding line is corrected in the revised manuscript as:
  saturation-rate $(\sigma_*) = \rho_{w,\infty}/\rho_s\,(T_{a,\infty})$.

- ***B.4 : P2 L27: Add space after sentence end.***

  Response B.4: The corresponding line has been corrected in the revised manuscript.

- ***B.5 : P5 L12: "an erodible"***

  Response B.5: Appropriate corrections have been made in the revised manuscript.
