

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

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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 # 2

### Opening Remarks:

We thank Reviewer #2 for his/her critique of the submitted manuscript. We have updated the manuscript based on the advice received and provide in the following section, a point-by-point response to the questions posed and the clarifications sought.

Based on Reviewer #2's comments, we have added an entire section in the supplementary material providing vertical profiles of some mean and turbulent quantities for some of the LESs performed.

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## A: Concerns in the main text

- **A1 : 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.*

**Response A.1:** We have now added a host of references in this section. The updated sentences read as follows:

*This is true for both field studies (Mann et al., 2000), where sublimation losses are calculated using measurements, usually at the height of  $O(1\text{ m})$ , and in mesoscale modeling studies (Xiao et al., 2000; Dry and Yau, 2002; Groot Zwaafink et al., 2011; Vionnet et al., 2014),*

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- **A.2 : P2., L.9:** *“recent studies using high-resolution large-eddy simulations” is the reference really use LES? I could not confirm in that paper.*

**Response A.2:** We re-checked the reference and it is indeed correct that Dai and Huang, 2014 did not use LES but rather a RANS type simulation. We have edited the text to read as follows:

*However, recent studies using high-resolution steady-flow, Reynolds-averaged Navier-Stokes (RANS) type simulations (Dai and Huang, 2014) claim that sublimation losses in the saltation layer are not negligible,*

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- **A.3 : P.2, L.24:** *“saturation  $\sigma_*$ ” does it mean rate of saturation?*

**Response A.3:** We agree with the view that confusion that using the word “saturation” may cause. We have edited all references to  $\sigma_*$  as *saturation-rate*.

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- **A.4 : 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.*

**Response A.4:** This is indeed true and we have corrected this mistake.

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- **A.5 : P.2, L.27:** *“Vionnet et al., 2014).In” should be “Vionnet et al., 2014). In” (please add a space).*

**Response A.5:** This edit has been made in the revised manuscript.

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- **A.6 : P.3, L.9:** *The units should be given in Roman type, I think.*

**Response A.6:** All units have to updated to Roman type in the revised manuscript as well as the supplementary material.

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- **A.7 : P.4, L.3:** *Is the first-order scheme sufficient in the calculation?*

**Response A.7:** Yes, given the extremely small time-step of 50 microseconds, the first-order scheme is indeed sufficient. We mention the time-step in the revised version of manuscript as follows:

*For the NUM approach, Eqs. (1) and (2) are solved in a coupled manner using a simple first-order finite-differencing scheme for time-stepping with a time-step of 50  $\mu$ s.*

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- **A.8 : P.5, L.12:** *“a erodible” should be “an erodible”.*

Response A.8: This has been corrected in the revised version of the manuscript.

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- **A.9 : 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).*

Response A.9: We have updated the temperature range in EX. II to be between -5 and 5K. The corresponding figures as well as the associated text have been updated as well.

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- **A.10 : 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?*

Response A.10: Please refer to the section “ A note to all reviewers” with regards to this question.

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- **A.11 : P.7, L.18:** *“at a field scale” specifically, during realistic saltation of snow?*

Response A.11: We have modified this line to read as follows in the revised manuscript: *We can directly assess the implications of differences in grain-scale sublimation between the two approaches on total mass loss rates during saltation at larger spatial scales as simulated using LES in Experiments III and IV.*

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- **A.12 : P.8, L.2:** *“here).” should be “here.” (an unnecessary parenthesis).*

Response A.12: We have corrected this in the revised manuscript.

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## **B: Concerns in the supplementary material**

- **B.1 : P.2, L.26 (S9):** *“ln” should be given in Roman type.*

Response B.1: We have corrected this in the revised supplement.

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- ***B.2 : P.3, L.27: I think parenthesis is missing around the reference.***

**Response B.2:** We have corrected this - by removing the reference to Groot Zwaafink et al., 2014.

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- ***B.3 : P.3, L.29:  $\sigma_{d_p}$  is undefined, I think.***

**Response B.3:** This line is now as follows:  
*characterized by the mean,  $\langle d_p \rangle$  and standard deviation,  $\sigma_{d_p}$*

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

**Response B.4:** We have added this reference in the revised supplement.

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- ***B.5 : 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?***

**Response B.5:** This is indeed true, but a previous work (Nalpanis et al. 1993) made wind tunnel experiments with different granular media, including sand and snow and found that the saltation geometries, ejection and rebound angles are invariant. We cite this work in the revised supplement.

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- ***B.6 : P.4, L.19-20: “dislodge additional additional particles” should be “dislodge additional particles”.***

**Response B.6:** This has been corrected in the updated supplement.

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

**Response B.7:** Yes - considering that we use an extremely small time-step of 50 microseconds, it is adequate. Additionally, a higher order method would require additional memory.

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- **B.8 : P.8.** *“Time step” All the elements (fluid, particle, and scalars) have the same timestep?*

**Response B.8:** Yes - all equations are progressed in time using the same time-step.

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- **B.9 : P.9, L.5:** *“It shows that that once” should be “It shows that once”?*

**Response B.9:** This has been corrected in the updated manuscript.

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