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The Cryosphere Discussions

Interactive comment on "Dry-Air Entrainment and Advection during Alpine Blowing Snow Events" *by* Nikolas Olson Aksamit and John Pomeroy

Nikolas Olson Aksamit and John Pomeroy

naksamit@ethz.ch

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Thank you for your thoughtful and insightful review comments on this paper. We have endeavoured to address them and think that by addressing them the resulting manuscript is greatly improved. Reviewer comments are preceded by number. General Comments:

1) The abstract lacks key information such as the period of study and the specific study site (Fortress Mountain in southwestern Alberta, Canada).

This information has now been included in L12-16 "To determine if specific turbulent motions are responsible for warm and dry air advection during blowing snow events, quadrant analysis and Variable Interval Time Averaging was used to investigate turbu-

lent time series from the Fortress Mountain Snow Laboratory alpine study site in the Canadian Rockies, Alberta, Canada during the winter of 2015-2016"

2) Section 2.1 should provide a short description of the study area and its climate. Provide the coordinates and elevation of the blowing snow study site, some information on the local topography and climate to provide the reader some geographical context.

The following paragraph has now been added at the beginning of Section 2.1 "Fortress Mountain Snow Laboratory (FMSL) is located in the Kananaskis Valley in the Canadian Rockies of southwestern Alberta, Canada. FMSL is surrounded by very complex terrain, with multiple nearby 2900m peaks having >100m vertical rock faces. The blowing snow study site is situated on a plateau at 2000m at the base of a closed ski resort, providing ample upwind fetch with minimal obstruction from trees or buildings (Figure 1 inset). Winter air temperatures at the FMSL blowing snow site typically range from -20ïĆř to +5ïĆřC, with frequent midwinter downslope chinook (föhn) wind events. Snow depth at the blowing snow site remains fairly constant through the midwinter at approximately 1 metre, with fresh snowfall frequently redistributed by wind events."

3) Are ultrasonic humidity measurements also available at the FMSL during this field campaign? If so, it would be quite interesting to see if blowing snow sublimation, and hence humidity, responds to the rapid air temperature and wind speed fluctuations during blowing snow events.

No, this data is not available, though, as mentioned in the conclusions, we would also be interested in analyzing these data in this context.

4) In any case, if both ultrasonic air temperature and wind speed data are available during the five blowing snow events, why not plot the corresponding sensible heat fluxes observed along with the meteorological data shown in Figure 2? At the very least, Figure 2 should include the corresponding wind speed data for all three sites.

Thank you for the suggestion. The corresponding wind speed data has been added to a supplemental figure, as well as highlighting periods conducive to blowing snow in Figure 2. We considered including the sensible heat flux estimates but to do so is full of uncertainty and likely errors and so is problematic for the following reason. Eddy-covariance calculations rely on assumptions of horizontally homogeneous terrain, time series stationarity, and identifying the single correct physical reference frame. Given the highly non-stationary processes we are discussing, it is not possible to select standard time frames for covariance calculations and would also be difficult to ascribe much meaning to these estimates, much as our earlier work identified problems in linking snow particle transport to EC estimates of shear stress. We think that including estimates of these fluxes would increase the uncertainty of the manuscript substantially and that the methods of estimating heat fluxes during such non-stationary flows needs to be reassessed in a very fundamental way. Essentially, we think that this analysis shows fundamental problems with using EC estimates of sensible fluxes over snow as earlier identified by Helgason and Pomeroy (2012) and suggested by Harding and Pomeroy (1996).

5) At no point does the text specify whether the relative humidity data recorded at the three other FMSL stations are with respect to water or to ice. Standard meteorological instruments usually provide the former, and so if this is the case, the relative humid- ity data must be converted to respect to ice to make any claims or conclusions about the absence of saturation during these five blowing snow events.

We agree and were showing relative humidity with respect to ice.

6) It should also be clear that the Powerline site is sheltered by trees and hence does not likely experiment blowing snow and may not reveal evidence of thermodynamic feedbacks from its sublimation.

Thank you. We mostly agree, though the patchy nature of the forest means that air masses measured in the clearing would be influenced by blowing snow. The other two sites are fully in the blowing snow flow zone. The introduction of the Powerline site

has been changed as follows: “The nearest complementary site is a sheltered forest (Powerline) station approximately 400 m away and 30 m higher in elevation [Smith et al., 2017]. Additionally, there are two exposed sites, including a ridgetop (Canadian Ridge) and lee side of ridge (Canadian Ridge North) that are both approximately 600 m downwind and 200 m higher in elevation. The Powerline station receives much less wind than the exposed sites or the blowing snow site and is much less susceptible to snow redistribution.”

7) Table 1 (mis-labelled as Table 2)

Corrected

8) provides data on Monin-Obhukhov lengths but it is not clear how these are derived. Similarly the definition for turbulence intensity reported in this table is not defined in the text.

These two variables have now been mathematically defined with citations in the text.

9) Blowing snow conditions are stratified into “sweep” events (u' > 0, w' < 0) and “ejection” events (u' < 0, w' > 0); yet there may also be blowing snow conditions when the horizontal and vertical wind speed anomalies are both of the same sign; therefore it is unclear why observations are provided only for the sweep and ejection events.

There are two reasons why we have focused on sweep and ejections motions. The primary reason is that they disproportionately contribute to Reynolds stress in turbulent boundary layers. When calculating friction velocity or other turbulence metrics that utilize velocity fluctuations, these motions have a significant influence. Second, this influence is connected to a history of coherent feature. identification in turbulence. Sweeps and ejections have been associated with specific bursting mechanisms, hairpin packet structures, and other theories of boundary layer flows. Outward and inward interactions (when u' and w' are of the same sign) do not make the same, nor have the same connections been made to vortical features in boundary layer flows. The following sentences have been added to the text “The subsequent analysis focuses on sweeps (u’>0,w’<0) and ejections (u’<0,w’>0) as they disproportionately contribute to the total surface Reynolds stress, are frequently used in models of turbulent boundary layer structures, have been identified to play crucial roles in boundary layer heat flux and aeolian transport [e.g. Bauer et al., 1998; Adrian et al., 2000; Garai and Kleissel, 2011; Aksamit and Pomeroy, 2017]. Please refer to Wallace [2016] for a recent review of the theory and experiments surrounding quadrant analysis and sweep-ejection cycling.”

10) Further to this, is an assumption made that blowing snow particles have no inertia and respond instantaneously to wind speed fluctuations?

No, this assumption is not made in the text, nor is it necessary for our results. It is unclear to the authors how this conclusion was made by the reviewer. Specific Comments:

11) P. 1, line 19: Fix the language in “model modeled described provides”.

This line has been changed to “The recurrence model described herein provides a significant step towards a more physically-based blowing snow sublimation model”

12) P. 1, line 27: Snow at the surface is often subjected to transport by wind only in relatively open and windy areas; areas such as the boreal forest and taiga are much less prone to wind transport of snow. The statement here should not be so general given blowing snow is not important component of the water budget in all areas experiencing snow.

Thank you – very true. We have clarified these statements as the following: “However, after snow has fallen, it is often subjected to sublimation while at rest or amplified in-transit sublimation during redistribution. Blowing snow redistribution can result in vast amounts of frozen water moving between basins or, in the case of sublimation, being removed entirely from the surface water budget in wind swept regions.”

13) P. 2, lines 48-50: Some prior studies (e.g. Grazioli et al. 2017; Déry and Yau 2001) have explored turbulent mixing and entrainment of dry air into the atmospheric boundary layer with impacts to the blowing snow sublimation and should be cited here.

Thank you. These citations have now been mentioned and included.

14) P. 2, line 54: Delete “in order”.

Corrected.

15) P. 2, line 59: Insert “air” before “temperature”.

Inserted.

16) Are the relative humidity data with respect to water or to ice?

This has been clarified and is presented with respect to ice.

17) P. 3, line 64: Perhaps this subsection could be titled “Field data”?

This title has been changed accordingly.

18) P. 3, line 75: How strong were the winds during the chinook event on January 21, 2016?

This line has been changed as follows: “This additional night, January 21, 2016 had much stronger winds, gusting up to 15 m s-1 because of the presence of a chinook event.”

19) P. 3, lines 76 and 77: The degree symbol is missing in the air temperature values reported here and elsewhere in the paper.

This has been corrected throughout the text

20) P. 3, line 83: Rather than “protected” use “sheltered”.

This has been changed.

21) P. 3, line 84: Replace “include” by “including”.

This has been changed.

22) P. 3, lines 87-89: At what temporal scales of the meteorological measurements are these coefficients of determination valid for? What are the associated probability values and sample numbers for each?

These values have been added to the text.

23) P. 4, Figure 1 and caption: Should the arrow on the map indicate the “Predominant” wind direction?

This has been changed. Thank you.

24) P. 4, Table 1: Note that this table is reported as “Table 2” but it should instead be “Table 1”.

This has been changed.

25) Under “Date”, the years for the events should also be reported.

This has now been changed. Thank you.

26) There is disparate information provided for the meteorological data, namely the range for wind speeds and Monin-Obhukov lengths and means for air temperatures. It would be more useful to have mean values and corresponding standard deviations for all events.

Thank you for the suggestion. Table 1 has been updated accordingly.

27) What do the “lower” and “upper” air temperature measurements mean?

We have clarified that we are referring to specific anemometers. Throughout the text, we have replaced the “upper” and “lower” designations with numeric 140 cm and 20 cm heights to differentiate between the two anemometers.

28) At what depth are the snow temperature measurements collected? Why not report one decimal value for the snow temperature measurements in a similar fashion as to the air temperatures?

These are snow surface measurements made in the first mm of the snow surface. These measurements were made manually and reported in a field notebook on the days of the experiment. Unfortunately, the temperatures were not recorded to the first decimal place so that precision did not exceed the accuracy of the thermometers used.

29) Apart from these meteorological variables, why not report the mean and standard deviation in relative humidity with respect to ice?

The formatting of the table has been changed in accordance with this suggestion.

30) P. 5, line 108: What does the subscript “v” denote in “kv”?

We have clarified that k_v refers to a user defined threshold in the VITA equation.

31) P. 5, line 110: Move “criterion” to just after “analysis”. Insert a comma after “1989]”.

This has been changed.

32) P. 5, line 113: In Equation (2), is a negative sign needed before “air” given the absolute value of this quantity is taken?

Equation 2 (and much of section 2.2) has been now been rewritten for clarity. This redundancy is no longer present in the text.

33) P. 5, lines 113 and 114: Equation (2) has a term v’ but the next line refers to w’.

This inconsistency has now been corrected.

34) What does the subscript “Q” refer to in “kQ”?

We have clarified that k_Q refers to a user defined Quadrant threshold event identification algorithm.

35) P. 6, line 128: Are the relative humidity data discussed here with respect to water or to ice? Standard meteorological instruments provide the former and so should be converted to respect an ice surface to establish whether saturation is indeed achieved, or not, during blowing snow events in subfreezing conditions.

These measurements are made with respect to ice and this has been clarified in the text.

36) P. 6, Figure 2: The color legend on the bottom right of the plot shows the air temperature in blue and the relative humidity in red; yet the tick labels on the y-axes show air temperature in red and the relative humidity in blue. As such it is not possible to interpret this plot. It would also be useful for interpretation of the meteorological time series to know when blowing snow was occurring during the 5 events shown here, perhaps as grey shading on the plots.

Thank you for noting this discrepancy. The coloring on these plots has been corrected. There were no blowing snow particle detectors at these stations so it is not possible to definitively say exactly when blowing snow was present. We highlighted times when the 15-minute average windspeed was greater than 3 m/s, a threshold for transport that has been noted at blowing snow study site previously. We have also included complementary time series of snow depth measurements and wind speeds in the document supplement, but a definitive highlighting of events is not possible.

37) P. 6, line 135: Again, specify if the relative humidity measurements are with respect to water or ice.

This has been clarified when the data is introduced on line 133.

38) At what temporal frequency are these data presented and at what measurement height?

It has been added to the caption that these are 15 minute average values at approximately 2 m heights.

39) Why not add the corresponding wind speed data here?

This has been included in the updated document supplement. Relevant blowing snow transport threshold information has now been included in the updated figure.

40) In the caption, change the text to “Flagged data have” and perhaps add a note that the y-axis scales vary between panels.

This has been changed.

41) The caption also states that there is limited correlation between sites for both variables yet on p. 3, line 88 it was reported there was high coefficients of determination for air temperature with lesser values for relative humidity.

This has been clarified. There is a high correlation.

42) P. 7, Figure 3: On the y-axis labels, spell out “Temperature”.

This has been changed.

43) P. 9, line 158: Add the corresponding years for the events.

This has been added. Thank you.

44) P. 9, Figure 4: On the y-axis label, spell out “Temperature”.

Changed.

45) P. 10, Figure 5: A color legend is missing from this plot and so the results cannot be interpreted.

This has been added.

46) P. 11, lines 190-191: Delete “It is interesting to note that” and start the sentence with “The probability”.

This has been changed.

47) P. 11, lines 201 and 203: Equation (3) includes a "KV" term but on line 203 the text refers to "KQ". Note also the text includes both upper case and lower case letters for these subscripts.

We have corrected the inconsistency in letter case. The use of k_V in equation 3 and k_Q in the following paragraph is, however, correct. As both k_V and k_Q are necessary for the modified VITA analysis, we restricted our model to events generated for one standard value of k_Q and analyzed those results.

48) P. 12, lines 211-212: What do all the subscripts used here mean?

These subscripts have been removed and the data has been moved to a new Table 2 that is much easier to interpret. Thank you for the motivation.

49) P. 12, lines 212-213: Fix the language in "common characteristic topographically induced flow."

This line has been changed to "This suggests persistent flow features at this site from one night to the next that may be due to a persistent topographically induced flow feature or turbulence generating mechanism at the study site."

50) 12, lines 228-229: It should be clear that these statements apply to the study site only and cannot necessarily be generalized.

Comments along these lines have been added to the last paragraph of the discussion and the last paragraph of the conclusions, as well as suggestions for how further investigation will reveal what of these bursting parameters can be regarded as universal.

51) P. 12, line 231: Replace the semi-colon by a comma after "[1993]".

This has been changed.

52) P. 12, line 234: Again, it might be useful to refer to prior studies such as Déry and Yau (2001) and Grazioli et al. (2017) that have considered turbulent mixing and dry/warm air entrainment effects on blowing snow sublimation.

Thank you. These additional studies has been referenced.

53) P. 12, line 235: Did all of these studies report humidity values with respect to ice saturation or with respect to water?

Thank you for bringing up this point. Unfortunately, this information is not included in all the mentioned studies.

54) P. 13, line 240: It is unclear what the statement "and thermodynamic feedback may require unphysical saturation bounds to be enforced" means. The Déry and Yau (1999) study imposed air at saturation with respect to ice at a lower boundary condition (at the surface) in their numerical model, a valid assumption over a snowpack. Please clarify this statement and how it relates to the present results.

Thank you for bringing this to our attention. This sentence has been removed as we were largely reiterating a point made at the beginning of that paragraph.

55) P. 13, line 250: Write as "1 s".

This has been changed.

56) P. 14, lines 270-272: Again, it is unclear if this statement is accurate given it is not known if the reported relative humidities are with respect to water or to ice. In any case, it is quite possible that the Fortress Mountain Snow Laboratory site is prone to downsloping winds aligned with the valley setting, thus leading to adiabatic warming and dry air intrusions near the surface. This may not be representative of other sites however, that experience blowing snow and so the results must be interpreted with caution as they may not be generalizable to other sites.

The relative humidity is with respect to ice for temperatures below zero. It has been clarified throughout the discussion and conclusions that these are not claims about behaviour in all boundary layers, and all turbulence phenomena are local in nature. The site receives predominately upslope flows from the valley bottom during most of the blowing snow events described here.

57) P. 15, line 300: This should read “Canada Foundation”.

This has been changed.

58) P. 15, line 311: Note the extra spaces in “effect”.

This has been changed.

59) P. 16, line 320: Insert the article # 4679 here.

This has been added.

60) P. 16, line 340: Add the volume and page numbers for this reference.

This has been changed.

61) P. 17, line 352: Is the number in parentheses “(12)” the volume number? If so, then remove the parentheses.

This has been changed.

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