The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-46-AC2, 2020

The Cryosphere
Discussions

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

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Received and published: 20 June 2020

Thank you for this thoughtful and detailed review. We have worked to address the reviewer's comments in the updated manuscript which has benefited significantly from the edits. General comment:

1) While this paper is generally well written, it is sometimes missing adequate detail and definition needed for the reader to adequately understand what was done. I would like to encourage the authors to go through the manuscript and provide more relevant background material and methodological details/definitions where needed. This is especially the case in the abstract section. I've outlined some areas that need more detail in my specific comments below. Although the authors have published many papers uti-

lizing this dataset, this paper needs to be stand alone and the reader should not need to have read these previous publications in order to understand the details relevant to the current study. The length of this manuscript is rather short so expanding sections where additional detail is needed should not cause any issue.

Thank you for this suggestion. The manuscript has now been expanded in the results, discussions, and conclusions sections as detailed below. Specific comments:

2) Lines 1 - 2: Does it make more sense for the title of the paper to be "Warm-Air Entrainment and Advection during Alpine Blowing Snow Events" based on the study design?

That is a very good point. We have changed that.

3) Lines 12 - 15: "Atmospheric sweep and ejection motions" should be further defined here.

In order to avoid technical language in the abstract, this sentence has now been changed to "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 turbulent time series from the Fortress Mountain Snow Laboratory alpine study site in the Canadian Rockies, Alberta, Canada during the winter of 2015-2016."

4) Lines 16 – 17: Define "event magnitude" on line 18.

This sentence has been changed to "A simple scaling relationship was derived that related the frequency of dominant downdraft and updraft events to their duration and local variance."

5) Lines 19 – 20: The "recurrence model" is not well defined. Also, the use of "model modeled described" should be revised.

This sentence has been changed as follows: "The downdraft and updraft scaling relationship described herein provides a significant step towards a more physically based blowing snow sublimation model with more realistic mixing of atmospheric heat."

6) Lines 20 – 22: Again, return frequencies and event durations is not well defined here.

This phrasing has been removed.

7) Abstract: More details about what the experiment was and where is was completed are generally needed in this section. The abstract needs to provide enough context for it to stand alone.

Thank you for this suggestion. We have now clarified the location of the study site, what kind of data we analyzed, and which methods were used to determine our conclusions.

8) Lines 36 – 37: This sentence needs further explained/rewritten. Are you suggesting that turbulent fluxes are calculated as a snow energy balance residual? This is not the case in most physically based snow models.

Thank you, this has been clarified. While physically-based blowing snow models often include terms for turbulent flux contributions, the energy balance is never closed, and these residuals are typically attributed to different processes that were imperfectly calculated. The exact contribution of latent heat is poorly understood, especially in this environment, as the true blowing snow sublimation contribution is often only seen as the piece that is missing from the final balance. We have clarified our phrasing in the text as follows. "To accurately calculate all contributions to boundary layer energy balances, latent heat flux estimates rely on an accurate sublimation model, and a precise understanding of how much energy is available for snow particle phase change."

9) Lines 55 – 56: Further define VITA thresholds here?

We have changed the phrasing to "VITA parameters" and included a more thorough explanation of the VITA analysis in section 2.2

10) Lines 56 – 60: It would be helpful to more specifically call out the "Blowing snow study site" in the text here so the reader isn't confused by the other meteorological stations when first referencing Fig 1. Furthermore, I suggest saying "These data are supplemented by observations of nearby temperature, relative humidity, and wind speeds at three additional meteorological stations within FMSL. . ."

Thank you, these sentences have been changed as follows: "Data used to validate this model consist of field measurements of three-dimensional wind velocities and sonic temperatures during blowing snow events at the blowing snow study site in the Fortress Mountain Snow Laboratory (FMSL), Canadian Rockies (Figure 1). These data are supplemented by observations of nearby temperature, relative humidity, and wind speeds at three additional meteorological stations within FMSL. This provides a boarder environmental context in which to understand potential thermodynamic feedback mechanisms beyond the blowing snow study site scale."

11) Lines 60 – 62: "return frequency" of what and "event magnitude" of what? Need to define these here.

This sentence has been changed as follows: "The scaling relationship also gives a real-world context for recent numerical studies on the impacts of non-stationarity on blowing snow sublimation rates."

12) Lines 65 - 66: Two ultrasonic sensors at which sites? Clarifying the site descriptions in the introduction will help make this clearer.

We have clarified that we are referring to measurements at the blowing snow study site.

13) Lines 101 – 102: VITA and quadrant analysis thresholds are discussed here before they are introduced in the subsequent equations which is confusing upon first read.

This has been clarified in the text. Section 2.2 has been significantly rewritten.

14) Lines 114 – 115: How were the ranges in the user identified thresholds in equation

1 and 2 that were tested in this study identified and defined?

This has been clarified in the text. Section 2.2 has been significantly rewritten.

15) Lines 116 - 118: Can you comment on the turbulent conditions that are not considered as sweeps or ejections when u' and w' are of the same sign? Are those potentially important turbulent conditions that need to be evaluated and considered in subsequent studies?

This has been clarified in the text. Section 2.2 has been significantly rewritten.

16) Lines 135 – 137; Figure 2: The colors of the y-axis scales on these plots should be revised to match the line color reflected in the figure legend (i.e. temperature y-axis scale should be blue and RH y-axis scale should be red.

This colouring has been corrected. Thank you.

17) Lines 139 – 144: Consider moving this information to methods section.

This information is originally presented in the last paragraph of the methods section, but is reiterated here for clarity for the reader.

18) Lines 167 – 169; Figure 4: Can you comment further on how the influence of the stabile atmospheric conditions and colder temperature near the surface may have resulted in the greater warmer deviations at the lower anemometer? These near surface temperature gradients over a snowpack are especially pronounced at nighttime as compared to daytime conditions (see Figure 3 from Sexstone et al. 2016; https://onlinelibrary.wiley.com/doi/abs/10.1002/hyp.10864). Therefore, in the absence of this steep air temperature gradient (more characteristic of daytime conditions), would we expect to see such strong temperature deviations associated with sweep and ejection motions?

Thank you for this interesting question. The sign and magnitude of the temperature deviations is directly related to the instantaneous gradient of air temperatures found during any measurement. For example, research in daytime convective boundary layers has found relative cold air contributions down to warm surfaces during sweep events (Garai et al,). We have added the following comments in the discussion: "Over short timescales, there is a direct physical relationship between temperature profiles and temperature deviations during mixing events. This is physically intuitive if one considers the relative temperature change at a doorway when opening a door of a warm building to cold surroundings. Because of this dependence on instantaneous conditions during a mixing event, however, relationships between average temperature deviation magnitudes and long-term temperature gradients are much less certain. Over the nights of investigation, there is no monotonic relationship between increases in average 140 and 20 cm sonic temperature differences and average sweep event temperature deviations. For example, on March 3, 2016 there was an average temperature difference of 0.9ïĆřC between anemometers, but the average downdraft (sweep) deviation was only +0.24ïĆřC. This is a smaller contribution than on January 21, 2016 where the air temperature difference was 0.5ïĆřC and the average sweep deviation was +0.28ïĆřC. That is, one should exercise caution if attempting to downscale long-term statistics to represent these purely local surface processes."

19) Lines 162 – 163: Based on their frequency, is it likely that the high resolution temperature increases associated with sweep and ejection motions could be resolved in the 15-min time-averaged data?

This question is unclear to the authors. The temperature deviations are measured as deviations from the 15-minute mean. Therefore, there would be no deviation if we changed the resolution of the deviations to match the mean. One would absolutely find evidence of these temperature fluctuations if looking at the 15-minute standard deviations associated with 15-minute mean data.

20) ine 189: I didn't see further discussion of this mixing process in the discussion section according with this statement. It would be good to elaborate on this in the discussion.

An additional three paragraphs have been added to the discussion.

21) Lines 243 – 244; Figure 6: Consider swapping the Ejections and Sweeps columns on this figure to be consistent with the presentation in other figures throughout the paper.

Thank you, this has been corrected.

22) 260 – 262 – Can you elaborate here on how you expect including these scaling relationships would alter biases in existing blowing snow sublimation models? For example, if a simulation of blowing snow sublimation was completed with existing models as well as using this scaling relation for warm-air advection, how would the results change? 22b) 263 – Please elaborate on the important environmental conditions that should be/need to be represented in future studies to further develop understanding of warm and dry air advection during blowing snow events. Given the study was completed at one study site only, it cannot be generalized that the study results could be applied to all snow covered environments where blowing snow occurs. What are the limiting environ- mental conditions of the current study (e.g., blowing snow events only observed during nighttime conditions over a limited range of atmospheric stability. . .or only sweep and ejection motions where analyzed?) and how can these be overcome in future experiments.

Thank you for the interesting questions. The following paragraphs aimed at illuminating these topics have been added to the discussion: “Over short timescales, there is a direct physical relationship between temperature profiles and temperature deviations during mixing events. This is physically intuitive if one considers the relative temperature change at a doorway when opening a door of a warm building to cold surroundings. Because of this dependence on instantaneous conditions during a mixing event, however, relationships between average temperature deviation magnitudes and long-term temperature gradients are not guaranteed. Comparing the nights of investigation, there is no monotonic relationship between increases in average 140

and 20 cm sonic temperature differences and average sweep event temperature deviations. For example, on March 3, 2016 there was an average temperature difference of 0.9ïĆřC between anemometers, but the average downdraft (sweep) deviation was only +0.24ïĆřC. This is a smaller contribution than on January 21, 2016 where the air temperature difference was 0.5ïĆřC and the average sweep deviation was +0.28ïĆřC. This is likely because long-time averages oversimplify the turbulent bursting process, and why eddy-covariance methods are suggested over bulk profile approaches to turbulent fluxes [Foken, 2006]. However, the present research has suggested a simple similarity scaling of the return frequency of turbulent events of intensity k_V as identified by modified VITA analysis, through the exponential relationship of Kailas and Narasimha [1994]. Such an empirical correction is compatible with the attached-eddy hypothesis [Taylor] and other similarity-scaling models of the turbulent boundary layer if the magnitude and frequency of bursts were defined to scale up with an increase in the size of turbulent eddies away from the surface. This scaling is evident in a decrease of characteristic frequencies of turbulent events when moving from 20 cm to 140 cm measurements (Table 2, document supplement), and a natural increase in modified VITA thresholds as the magnitude of turbulence measurements increases in Eq (1) and (2) for fixed k_V and k_Q. This view of boundary layer mixing provides a simple platform with which to model and investigate a gust-driven regeneration function of warm-dry air in the near-surface for blowing snow sublimation calculations. The inclusion of such a statistical recurrence model could provide an empirically defined quasi-periodic source of warm and dry air to blowing snow simulations. For example, this could be included in conservation of heat equations as a natural evolution of the constant entrainment and advection functions introduced by Bintanja [2001]. In this way, it is possible to represent the mixing of distinct parcels of air of different temperatures through commonly studied turbulent structures. Such a recurrence model would be computationally efficient and a significant step towards a physically based blowing snow sublimation model. Future high temporal resolution studies of blowing snow particles, air temperature and water vapour during sustained periods of above-snow-transport-threshold wind speeds would greatly benefit the research community. Short timescale thermodynamic feedbacks to humidity from sublimation could come from similar high frequency coupling analysis with closed path hygrometers or gas analyzers at multiple heights during blowing snow events. Fast response particle detectors could give further insight into relationships between atmospheric and particle motions. This would allow a more complete understanding of the advection-thermodynamic feedback balance during blowing snowstorms and advance the seminal profile studies of Schmidt [1982]. As advection processes are local by nature [e.g. Harder et al., 2016], characteristic frequencies of turbulent events will vary with location and atmospheric conditions. The small range of values of N_0 measured at this site during five months of this campaign suggests common flow phenomena will possibly dominate and aid in more universal applications of entrainment modeling, at least within specific seasons." In the conclusions, we have also suggested a longitudinal study would be greatly beneficial for understanding the variance in parameters necessary for this simple bursting model.

23) Line 269: Conclusions section should be numbered section 5.

Corrected. Thank you.

24) Lines 270 – 272: Leading the conclusions section with a sentence about saturation of water vapor during blowing snow events doesn't really fit with the scope of this paper since it was not a measurement directly made at the blowing snow site and only observed at auxiliary meteorological stations.

Thank you. This has been moved to another section of the conclusions.

Garai, A., and J. Kleissl (2011), Air and Surface Temperature Coupling in the Convective Atmospheric Boundary Layer, J. Atmos. Sci., 68(12), 2945–2954, doi:10.1175/JAS-D-11-057.1.

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