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The Cryosphere Discussions

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9, C2697–C2701, 2016

Interactive Comment

Interactive comment on "Wind tunnel experiments: cold-air pooling and atmospheric decoupling above a melting snow patch" *by* R. Mott et al.

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Response to Reviewer 2 (Richard Essery)

We thank Richard Essery for his valuable comments. We answered below to all his comments. Reviewer's comments are in bold while our answers appear in normal font. Changes in the manuscript are shown in cursive.

5414, 9 Momentum fluxes were also measured

Response: we changed the text accordingly to: Vertical profiles of sensible heat and momentum fluxes

5414, 15 Clarify in what sense “drainage flows were decoupled from the surface”

Response: Following the comments from Reviewer 1 we do not call the wind speed maximum a drainage flow anymore. We changed the corresponding paragraph to: For those conditions, the near-surface suppression of turbulent mixing was observed to be strongest and the ambient flow was decoupled from the surface enhancing near-surface atmospheric stability over the single snow patch.

5415, 9 A missing reference here: Liston, G. E. (1995). Local advection of momentum, heat and moisture during the melt of patchy snow covers. Journal of Applied Meteorology, 34, 1705-1715.

Response: we added the missing reference.

5415, 15 Burns and Chemel

Response: we changed Bruns to Burns.

5416, 15 Perhaps more importantly, high vertical resolutions are required

Response: Yes, the reviewer is right. We changed the sentence accordingly: Applying Large-Eddy Simulations, a high horizontal resolution of at least 5 m and near-surface vertical resolution of less than 1 m are necessary to adequately represent the formation of thermal internal boundary layers.

5416, 25 It is fairly obvious, but neither z nor delta have been defined.

Response: Yes, we changed the sentence to: …. (normalizing the vertical measurement resolution (dz) by boundary layer height (delta) in the wind tunnel (dz/delta) this means 0.016).

5417, 26 Make it clear that the depth of the concavity is being discussed, not the depth of the snow.

Response: To be more clear here we changed the sentence to: The cavity has a length

(l) of 1.6 m and a maximum depth (z_{max}) of 0.1 m (Figure 1).

5418 Z0 is a redundant quantity, being 0 by definition (and might be confused with z0 by boundary-layer meteorologists). I think it would be more clear to have z=0 redefined for each experiment to be the snow surface in E1 or the highest point of the concave surface in E2. How much did the surface change during the experiment, and is it reasonable to assume that most of the melt occurred between rather than during the measurement periods? Why were the measurement periods different for E1 and E2?

Response: Yes, we agree with the reviewer and changed the wording and definition throughout the paper and changed all figures accordingly: All heights are given relative to z=0, which is the height of wooden floor and the initial snow cover at fetch distance 0. For the concave setup, z=0 corresponds to highest point of the cavity.

5419, 3 Exceeding what threshold?

Response: we changed the text to: If a time-series was influenced by natural convection for more than 10% of the time, data were not considered for the following analysis.

5419, 9-13 This sentence and Table 2 contain exactly the same information; one of them is redundant.

Response: yes, we extended and revised the description in the text and skipped table 2. See details below.

5419, Equation (1) Either subscripts or conditions are required in the integral to pick out the quadrants. Response: We have added subscripts and changed the ext accordingly:

Quadrant analysis consists of conditionally averaging the shear stresses into four quadrants depending on the sign of the streamwise and vertical velocity fluctuations.

If u and w correspond to streamwise and vertical velocity and primes indicate the deviation from the average value, each quadrant event $<u\prime w\prime>_i$ can be defined as:

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$$<u\prime w\prime>_i = \lim_{T \to \infty} \frac{1}{T} \int_0^T u\prime(t) w\prime(t) I_i[u\prime(t) w\prime(t)] dt \quad (1)$$

where T is the length of the time-series, and I_i is a function that triggers of a specific quadrant Q_i:

$$I_i[u\prime(t)w\prime(t)] = \}^{1\forall(u\prime w\prime)\in Q_i}_{0\forall(u\prime w\prime)\notin Q_i} \quad (2)$$

The resulting types of motions are the following: outward motion of high-momentum fluid (Quadrant 1), ejections of low-momentum fluid (Quadrant 2), wallward motion of low momentum fluid (Quadrant 3) and sweeps of high-moment fluid towards the wall (Quadrant 4). The four quadrants are defined as follows:
Quadrant 1 (Q1): $u\prime > 0, v\prime > 0 \rightarrow (u\prime, v\prime) \in Q1$
Quadrant 2 (Q2): $u\prime < 0, v\prime > 0 \rightarrow (u\prime, v\prime) \in Q2$
Quadrant 3 (Q3): $u\prime < 0, v\prime < 0 \rightarrow (u\prime, v\prime) \in Q3$
Quadrant 3 (Q3): $u\prime > 0, v\prime < 0 \rightarrow (u\prime, v\prime) \in Q4$

While Q1 and Q3 motions are positive stress producing motions, ejections and sweeps contribute positively to the Reynolds stress. The negative contributions by Q1 and Q3 motions corresponds to the interaction between ejection and sweep motions. In neutrally stratified boundary layer flows, the main contributions to the Reynolds stress comes from sweep and ejection motions and both motions are nearly equal (**?**).

In our case all the events from each quadrant are considered and no event is discarded based on its magnitude. Therefore the analysis concentrates on the overall flow dynamics rather than focusing on the strength of the motions. The second (ejections) and fourth (sweeps) quadrants constitute a positive contribution to the production of turbulent kinetic energy and to the momentum flux towards the surface, while the other two constitute a negative contribution.

5420, 9 Table 1 shows ambient temperatures ranging between 8.5 and 14.0 C.

Response: we thank the reviewer for that hint. We changed the text accordingly.

5421, 19 It takes some faith to see “a distinct local wind maximum” in the profiles for E2V1, particularly without an estimate of the uncertainty in the measurements.

Response: as discussed above, we included an uncertainty estimation. Regarding the wording “a distinct local wind maximum” we skipped distinct.

Table 1 What heights were used in calculating the bulk Richardson numbers? I couldn’t make sense of the values. V in the caption is U elsewhere. Theta in the table is T elsewhere (and it isn’t potential temperature). The labels “c = concave, f = flat” are no used.

Response: we changed Table 1 to be consistent with labels used in the manuscript.

Figure 3 Is there any advantage to having X1 and X2 profiles on the same plots? Splitting them would remove the need for the complicated and confusing axes.

Response: we are aware that figure 3 is not easy to read but we have decided to keep the figure as it shows how the profiles are located within the cavity and over the flat surface. We think that the reader benefits from such an illustration. For a more concise description of the profiles we show the profiles in Figure 4 in high resolution. We now refer to Figure 3 more often.

Figure 4 Theta in the caption is T in the figure

Response: The reviewer is right that Theta should read T. We changed the caption for Figures 3 and 4.

Interactive comment on The Cryosphere Discuss., 9, 5413, 2015.