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

**Anonymous Referee #1**

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Review of the manuscript tc-2015-138

Wind tunnel experiments: Cold-air pooling and atmospheric decoupling above a melting snow patch by R. Mott et al.

Summary

In this study a wind tunnel laboratory environment is used to study turbulent transport in stable condition over a snow surface, and in particular for a setup that represents a cold air pool. The topic is appropriate for The Cryosphere, the presented experiment is certainly innovative, the results seems reasonable, though not always very surprising. Moreover, the analysis can be extended in such a way that the results will more directly

link to the state-of-the-art issues in models development. The paper is somewhat difficult to follow, since the degree of referencing to figures and experiment abbreviations differs substantially from section to section.

Recommendation: Major revisions needed.

Major remarks:

1. Uncertainty estimates: The graphs present the results for the mean flow and the turbulent fluxes. However, these values will have a certain uncertainty that is not discussed in the paper. These uncertainties contain instrument uncertainties, uncertainties in the representation, as well as due to statistical uncertainties (since I assume that the presented profiles are the result of averaging many repeating results). My experience with analogous experiments is that these uncertainties can be substantial, and they need to be quantified.

2. A key advantage of a wind tunnel is the fact that experiments can be reproduced by repeating the experiment under similar conditions. How many times have the reported experiments been repeated? Repeating the experiments will strengthen the statistical robustness of the results.

3. Eddy correlation technique: I do agree with applying the "eddy correlation technique" in the study, and its application over a flat surface is correct. However, for the sloping terrain in the E2 experiments the so called planar fit corrections (as described in Wilczak et al 2001) should be applied. Has that been done?

4. Scaling: All results have been presented after scaling. The applied scaling (with the flow speed above the boundary layer and the temperature difference between ambient and surface temperature) closely follows the routine in the engineering community. I was wondering whether the authors tried to apply the traditional Monin-Obukhov similarity, in which local fluxes scale with the local gradients of temperature and wind speed. The experimental results to provide sufficient information to do so, isn't it? By

applying the results will connect more to the knowledge in the field of boundary-layer meteorology.

5. More scaling: For a the applied scaling (i.e. without time as a scaling variable), it is important that stationarity is reached during the experiment. This is unfortunately not discussed in the paper.

6. Definition of low-level jet and drainage flows. Multiple times the terms low-level jet, drainage flows, and wind maxima are used, but a formal definition is missing. I can imagine that the winds in experiment E2 will accelerate just after the pool has been reached, since the horizontal flow suddenly does not feel a underlying surface anymore. This has however not much to do with stratification or winds in the pool. However, can one call this a low-level jet then? Baas et al (2008) and Tuononen et al (2015) can provide some guidance on how to define low-level jets and other wind maxima in a more objective way.

7. In the introduction the authors mention that numerical models do often not account for cold air pooling. However, the paper does not pick up the opportunity to indicate the implications for modelling studies. I.e. how should model developers modify flux parameterizations in classic surface-layer parameterizations to account for the pooling effects?

8. Structure: In my opinion the conclusion section is too long and mixed with a discussion at the same time. Please consider to set up clearly delineated "discussion" and "conclusion" sections, each with their unique role.

Minor remarks:

P5414, ln 4: ...from the atmosphere to the snow surface. Just to make clear that you do not hint to the heat flux from the underlying snow/ice.

P5414, ln 16-18: Remove "Further work....temperatures"

P5415, ln 15: "Bruns" should be "Burns"

P5415, ln 20: “Bruns” should be “Burns”

P5415, ln 23: “A special case”: please be more precise what you mean with special case. Is it special because cold-air pooling is poorly understood, or because cold-air pooling does infrequently occur?

P5416, ln 9-13: I slightly disagree with this statement, since in very stable cases the boundary layer becomes rather shallow, and therefore measurements with a relatively short tower can characterize the complete stable boundary layer.

P5416, ln 9-13: Please also discuss the role of the footprint under the various ranges of wind speed.

P5416, ln 19: “optimal”. Please rephrase this statement. Wind tunnel studies indeed do have their advantages over field observations, but they also have several disadvantages that should be acknowledged here. For example it is relatively difficult to obtain Reynolds numbers that are representative for the atmosphere. Are the Reynolds numbers in the current study comparable to atmospheric surface layer values ($\sim$10ˆ6)? Not according to Figure 8 (please comment). At the same time the authors do not mention the possibility of to repeat experiments as an advantage of wind tunnels studies.

P5417, ln 3-5:The authors use melting snow here as medium to create stable conditions. However, I do not see the additional value of melting snow over melting ice, at least the impact of the snow roughness are not discussed in the paper. Could you please comment on this?

P5417: Section 2.1: the experimental setup is explained but the snow density, conductivity, etc is not discussed. Moreover I am curious whether temperature measurements were done in the snow pack? If the snowpack was vertically isotherm I do not expect heat conduction through the snow, but if this was not the case, heat conduction could play a role in the surface energy balance. The same holds for the melted snow that is penetrating the snow pack as liquid water. How has these processes been controlled?

P5418, ln 3: “decoupling”: multiple definitions for decoupling exist in the literature. It would strengthen the paper if a more formal definition of decoupling would be introduced, and be used to analyse the results.

P5420, ln 15: The Richardson number is mentioned here, but hardly used in the analysis later on, while it is an excellent quantity to characterize stratified flows as decoupling. For example, a vertical profile of Richardson number would provide more information than the vertical profile of the Reynolds number in Fig 8.

P5421, ln 1- 14: I am concerned that other processes than turbulence that govern the temperature are overlooked (at least it is not proven that they are negligible), i.e. advection and radiation divergence. For example, Savijarvi et al (2006) found rather strong impact of radiation divergence in stable boundary layers with low winds and with decoupling. Please comment.

P5422, ln 8-11: Figure 3 should be discussed in more detail.

P5422: from section 3.3 the paper become less easy to follow since the abbreviations for the experiments (as E1, E2, etc) are less frequently used.

Ln 5422, ln 25-29: The flux behavior close the ground: please provide more evidence that the reduced flux magnitude towards the surface is not an effect of lacking statistics (too few robust measurements to make up a flux estimate). In addition, if these observations have been made in the roughness sublayer, I do expect the flux magnitude to increase with height. Could the authors provide some guidance whether the measurement were taken in the roughness sublayer, the surface layer or the boundary layer.

P5423, ln 2: ....surface at X2 only ...

P5423, ln 22-25: I only see support for this statement in Fig 4d.

P5424, ln 7: explain in more detail what you expect here.



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P5424, ln 16: ...of sweeps (Q4)...

P5424, ln 21: ... drainage flows...: as mentioned above, I think the terms “low-level jet” and “drainage flow” are used quite loosely in the paper, so I have some reservations here that the suggested drainage flow is really a drainage flow. In this part of the analysis, the study could reveal a real drainage flow by repeating the experiment, but for a outer layer wind speed of 0 m/s. In that case the drainage flow would develop spontaneously.

P5424, ln 24-27: This result calls for an explanation, that is missing so far.

P5427, ln 8: remove statement that “stability had a minor effect”

P5427, ln 27: “intermittently”: intermittent turbulence was not observed during the experiment, so this statement is somewhat suggestive.

P5428, ln 11-17: It appeared to me somewhat surprising that at the end of the paper it appears that there are field data available to compare the tunnel experiments with. This would be very interesting to report.

Figure 2: Caption: ....temperature (top) and wind velocity (bottom)......

Figure 2: Please add error bars

Figure 3: This figure is not easy to read since the scale is at the bottom. In addition the figure is also very limitedly discussed in the text. Please rewrite and re-organize.

Figure 3: label the panels a-d, and label on the right whether the plots refer to “E1” or “E2”.

Figure 3: Please add error bars

Figure 4: Please add error bars, and explain the colour labelling of the lines.

Figure 5 and 6: Please reconsider how useful are these plots. I mean I would not have expected something different than these fluxes being dominated by Q2 and Q4. Also add error bars.

Figure 7: Labelling at top of panels like E2V1, E2V2 etc would be helpful.

Figure 7: I do not see low-level jets in the bottom three rows, while manuscript suggests they are there. Please use a more objective way to define a “low level jet” or “wind maximum”.

Figure 7: in the second row, the layer with the strongest wind shear coincides with a strong reduction in flux magnitude towards the surface. That is counterintuitive. Please add a vertical profile of Richardson number for a deeper insight.

Figure 7 and 8: Please add error bars.

References:

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