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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.

R. Mott et al.

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Response to Reviewer 1:

We thank Reviewer 1 for her/his valuable comments on the manuscript. We answered below to all his/her points.

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.

Response: Following your advice we estimated the uncertainty of mean quantities and turbulent fluxes. We now show profiles of uncertainty for each measurement point calculated from the formula below. Due to readability of profiles, we decided to show the figures in the supplement of the manuscript. We present here one example figure (Fig. 1) showing the profiles of wind velocity and turbulent momentum and heat flux of experiment E2V1 with its uncertainty estimates as error bar for each point. We added the following paragraph to the method section: We estimated the uncertainty, considering both the systematic and random components. The random uncertainty is significantly larger than the systematic uncertainty. The random uncertainty was considered as follows: u_q=σ_q/(âŇľqâŇł $\sqrt{}$N) with N the number of independent samples separated by the integral-time scale of u. We did not appreciate significant differences applying the method of \citet{Mann}, therefore we adopted the simplest method. We spatially averaged the total uncertainty (systematic and random) within the first 0.1 m from the snow surface where turbulent mixing is mainly occurring. The uncertainties of the mean streamwise velocity and temperature fall below 4\%. The uncertainties of the fluxes are larger and between 11\% and 46\%. The large uncertainties of the fluxes reflect the high level of turbulence in the flow together with the mean values approaching to zero towards the surface.

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.

Response: In general, it is true that the wind tunnel has the advantage to repeat measurements several times under similar conditions. You should, however, keep in mind that we measured flow fields above a melting snow cover under typical spring conditions. This means, that we could only perform measurements in spring or autumn. We conducted our experiments on a manually designed concave depression made with compacted natural snow. The procedure we adopted allowed the construction with sufficient precision. Much care was taken to ensure smooth transition between the upstream fetch and the snow-fetch as well as to construct a smooth concave section. Even if we could have rebuilt the snow concave section for further tests we might not have been able to achieve the accuracy required to replicate exactly the same surface in the test section, thus we could not repeat our experiments without affecting the flow and significantly increasing the measurement uncertainty. In summary, repeating the measurements had the following limitations: Measurements could be only performed during typical spring conditions: when natural snow has been available and temperatures are clearly above the melting point but also not too high to create untypically stable conditions All measurements had to be finished within one day and within a few hours of that day, when air temperatures were high enough during daytime The snow cover was melting and we had to prepare the snow cover for each single experimental design. Although the snow cover was prepared carefully, small differences in the surface characteristics between single measurement days were present that added an additional degree of uncertainty to the measurements (see uncertainty)

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?

Response: In the original version we decided to keep the same coordinate system for both measurement locations. We now applied the planar fit approach for X1 to correct the data following Wilczak et al 2001 and changed the figures accordingly. The results show, however, only minor changes to the prior one and do not change the typical shape of the flux profiles.

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.

Response: We thank the reviewer for this suggestion. We feel, however, that considering the scope of the study, profiles of local fluxes scaled with the related local gradient will not add a substantial value to the conclusions of the study. While it is correct that local scaling is applied in particular for stable conditions (Nieuwstadt et al. 1984, 1985), we are interested in the bulk effect when stratification interacts with local topography. Since the main goal is the investigation of boundary layer decoupling above a melting snow cover under typical spring conditions (small temperature gradients) and over different surface shapes, we are interested in the direct measurement of turbulence profiles. Instead of scaling local fluxes with the related gradient we introduced profiles of the Richardson number to show the ratio between buoyancy produced turbulence to turbulence generated by shear (see figure 8a). This has the advantage for the community to interpret the results with respect to typical “law of the wall” parameterizations used in numerical models.

Nieuwstadt, F. T. M.: Some aspects of the turbulent stable boundary layer, Bound.-Lay. Meteorol., 30, 31–55, 1984.

Nieuwstadt, F. T. M.: A model for the stationary, stable boundary layer, in: Turbulence and Diffusion in Stable Environments, edited by: Hunt, J. C. R., Clarendon Press, 149-179, 1985.

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.

Response: We applied high-pass filtering to the acquired time-series to avoid potential contamination from low-frequency waves from the approaching flow. For the experimental design we had to ensure statistical representativeness of the results. Therefore we conducted experiments with a long acquisition (100s) for the flat case, where the experimental designed allowed that, to ensure high statistical stationarity of the fluxes. Unfortunately the need to complete our experiments in a given time before the melting process would cause appreciable changes in the snow surface shape, led us to reduce the duration of the acquisition in case of the concave section. We designed our experiments by sampling the flow at least hundred times the integral time scale (Tropea, C., Yarin, A.L. and Foss, J.F. eds., 2007. Springer handbook of experimental fluid mechanics. Springer Science & Business Media). As shown in the figure below (Fig. 1) the integral time scale ranges between 0.02 and 0.05 for all the experiments. The choice to sample for 20 s in case of the concave section represented a reasonable compromise between the need of the two requirements. Fig. 2 shows an example of the resulting integral time scale obtained by integrating the area below the autocorrelation function until this reaches zero. We will include a short note on stationarity of the flow to the methods section and als add the figures to the supplement.

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.

Response: we thank the reviewer for this comment. We agree that the wind velocity maxima are not strong enough (also compared to the well-defined criteria of Tuonen et

al., 2015) to call it a pronounced LLJ. We changed this throughout the paper and also do not link that peak to a drainage flow anymore. We agree with the reviewer that the flow acceleration is primarily due to the detachment of the flow from the surface. We changed the text accordingly: The low-level maxima in velocity might be caused by the acceleration of the flow behind the topographical step (the highest point of the cavity) due to the detachment from the surface.

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?

Response: In the introduction part we mention that numerical models fail to reproduce boundary layer decoupling at the lowest few centimetres above the ground mainly due to the vertical resolution. We do not have an explicit advice for modellers how to easily modify classic surface-layer parametrizations to account for boundary layer decoupling. This is beyond the scope of this study. Measurements show that low wind velocities and sheltering promotes boundary layer decoupling over metling snow surfaces. Applying LES models a sufficiently high vertical resolutions close to the ground will allow simulating this effect. We now explicitly refer to LES models in the intro-part: Applying Large-eddy simulations, high horizontal resolution of at least 5 m are necessary to adequately represent the formation of thermal internal boundary layers. What cannot be captured with that resolutions is the strong suppression of turbulence due to strong atmospheric stability at the lowest centimetres above the snow surface \citep{mott15}.

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

Response: The conclusion and discussion sections are revised accordingly. All parts discussing the results are moved to the newly introduced section: Discussion: Indications of boundary layer decoupling within the cavity.

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.

Response: changed

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

Response: we agree with the reviewer and skip the last sentence of the abstract.

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

P5415, ln 20: "Bruns" should be "Burns"

Response: changed

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?

Response: We agree with the reviewer that this statement is not very precise. We changed the sentence to: Cold-air pooling can also occur over snow fields that are located within topographical depressions where associated atmospheric decoupling is mainly driven by the cooling effect of the underlying snow on the air.

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.

Response: Yes, but only if a high number of sensors is used to measure the vertical profile and only if sensors with very small path lengths are used to measure small eddies close to the surface. We added these specifications to the manuscript: In the field, atmospheric profiles obtained from eddy-correlation measurements are typically

based on a few measurement points, which makes it difficult to capture boundary layer dynamics of shallow internal thermal boundary layers. Especially turbulent heat fluxes close to the snow surface are difficult to measure in the field because of the relatively large path lengths of sonic anemometers and consequentially the low vertical resolution of measurement points close to the surface. Although measurements conducted by Mott et al. (2013) indicated that boundary layer decoupling over a melting snow patch especially occurred during low ambient wind velocities, the effect of the topography via sheltering could not be analysed because the "varying one parameter at a time" approach is difficult if not impossible to achieve in the field.

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

Response: We now discuss the role of footprint in the discussion part: Comparing the flux profiles of the different experiments and at different locations, we have to note that the locations measured at the downwind locations X1 and X2 will have varying footprints for the fluxes depending on wind speeds. At the same time, the differences between profiles at X1 and X2 give a first indication of how fluxes change in the streamwise direction. It is clear that lateral transport of heat and momentum plays an important role for the given conditions and that the net effect of a topographic depression on the boundary layer above still needs to be systematically analyzed.

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 (10ËĘ6)? 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.

Response: Wind tunnel studies are not designed to match the full-scale Reynolds number. It is considered that as soon as the flow is turbulent (also in the near-surface layers) the flow is Reynolds number independent. In our case the presence of the upstream roughness elements increases the mechanical turbulence. We agree with the reviewer that field measurements also have clear advantages. We therefore changed the paragraph accordingly: Wind tunnels provide controlled conditions to measure the boundary layer dynamics above cooled surfaces \citep{Ohya2001,Ohya2008}. Furthermore, the available measurement techniques also allow us to measure vertical profiles of turbulent quantities with a high vertical resolution of approximately 0.005 m (normalizing the vertical measurement resolution (dz) by boundary layer height (δ) in the wind tunnel (dz/δ) this means 0.016). This comes at the expense of reduced eddy sizes and directional variation of winds in the tunnel.

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?

Response: Since we wanted to investigate the boundary layer above melting snow we did not see any advantage using ice instead, which of course has a different roughness length but is also more difficult to handle in the wind tunnel.

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?

Response: No temperature measurements were done in the snowpack, but the snowpack was already isotherm since the snowpack was already wet and melting. Thus, heat conduction did not play a role. We included the information of isothermal snow-

pack in the manuscript.

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.

Response: The reviewer is right that there are multiple definitions of decoupling in the literature. In this study we are especially interested in boundary layer decoupling that is characterized by a suppression of turbulence near the ground causing a significant reduction of vertical momentum transfer towards the ground (which would normally increase towards the surface where shear is strongest). We added this definition to the manuscript.

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.

Response: we agree with the reviewer that a profile of the gradient Richardson number provides important insight into the ratio between buoyancy produced turbulence to turbulence generated by shear. Assuming a critical Richardson number of 0.25 (following Stull, 1988), results nicely show that the Richardson number only exceeds the critical value for the low wind velocity case and only within the concave section, further indicating boundary layer decoupling for this case only. We added Figure 8a showing vertical profiles of the local Richardson number.

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.

Response: We agree that as turbulence is unable to transport large amounts of heat or momentum in decoupled boundary layers, other transport processes such as radiation flux divergence will become relatively more important. However, the main focus of the analysis is the description of the external factors “curvature” and “temperature difference” on the turbulent fluxes and not a full assessment on all possible effects. We now mention however in the text that other things such as radiation balances may also change (especially in the field) and expect that these effects are extremely small over the length scales of our wind tunnel: Comparing wind tunnel with field experiments, we have to consider that in the field other meteorological processes such as radiation may also become important drivers for cold-air pooling and boundary layer decoupling. We expect, however, that these effects are extremely small over the length scales of our wind tunnel.

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

Response: we are aware that figure 3 is not easy to read but decided to keep the figure as the figure 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.

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.

Response: we revised the manuscript accordingly and now use the abbreviations throughout the paper.

P 5422, ln 25-29: The flux behaviour 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.

Response: Please see our comments on the uncertainty above. Raupach et al. (1991) give roughness sublayer heights approximating 2h, where h is the mean height of the roughness elements. Considering the low roughness lengths of snow the roughness sublayer would be below 1 cm. Since all measurements are above 1 cm above the surface, this means that measurements were not within the roughness sublayer. If we assume the boundary layer height to be 0.25 m in our wind tunnel (Clifton et al., 2006), then the lowest 10% of the boundary layer approximately are within the surface layer. This would mean the lowest 2.5 cm. Part of the lowest few points are within the surface layer and the rest are within the boundary layer. We added a note on the roughness sublayer to the text: Assuming that all measurements are conducted above the roughness sublayer, then turbulent momentum and vertical turbulent heat fluxes within the stable internal boundary layer are expected to increase with decreasing distance to the snow surface \citep{essery06}.

Clifton A, Rüedi JD, Lehning M (2006) Snow saltation threshold measurements in a drifting snow wind tunnel. Journal of Glaciology 52(179):585{596

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

Response: the peak is evident for X1 and X2. We changed the text to: For low wind velocities, profiles of momentum fluxes feature a distinct peak approx. 0.03 - 0.04 m above the local surface for X1 and X2(Figure 3, Figure 4).

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

Response: we changed the text to be more clear here: For higher wind velocities, however, the suppression of the vertical heat flux is confined to the lowest 0.01 - 0.02 m of the ABL and is much stronger for the downwind distance X2, where the maximum depth of the cavity is reached (Figure 4d).

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

Response: we now clearly state what we want to discuss here: As stated in the methods section, 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 their contribution \citep{wallace}. In the following, we will discuss the near-surface profiles resulting from our experiments with flows characterized by a changing atmospheric stability towards the surface. We particularly want to distinguish between turbulence phenomena observed for the flat and the concave setup.

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

Response: changed accordingly.

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 an outer layer wind speed of 0 m/s. In that case the drainage flow would develop spontaneously. Response: please see comments on major point 6) above.

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

Response: This is discussed in subsection Discussion: Indications of boundary layer decoupling within the cavity. We now refer to the following subsection in the text. The strong suppression of turbulent motions close to the surface is a clear indication for boundary layer decoupling at low wind velocities and will be discussed in the following section.

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

Response: we changed the text, now reading: While the stability had only a small effect on flow dynamics over the flat snow field, it strongly influenced the near-surface flow behavior over the concavely shaped snow patch, especially if the free-stream wind

velocity was low.

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

Response: we changed the sentence now reading: Thus, for typical melt conditions of a seasonal snow cover, only the special experimental conditions with a concavely shaped snow patch and low free-stream wind velocities allowed the development of near-surface vertical profiles of turbulence that are typical for very stable regimes \citep{mahrt}, when the maxima of turbulence is reached in a layer decoupled from the surface.

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.

Response: there are different turbulence measurements available. Already published turbulence measurements at the Wannengrat test site indicated boundary layer decoupling for low wind velocities but measurements did not show if boundary layer decoupling is caused by wind velocity only or also due to the sheltering. Following that, these measurements were a motivation for wind tunnel experiments. We introduced a few sentences in the introduction part to clearly state this: Although measurements conducted by Mott et al. (2013) indicated that boundary layer decoupling over a melting snow patch especially occurred during low ambient wind velocities, the effect of the topography via sheltering could not be analysed. Furthermore, measurement results only gave a rough estimate on boundary layer dynamics close to the surface.

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

Response: we changed the figure accordingly.

Figure 2: Please add error bars

Response: Since the profiles are calculated from one experiment only we do not show an error bar. We show, however, profiles of mean and turbulent quantities including uncertainty estimates of each measurement point (see mjor comment above). Please see also the comment above regarding the repeatability of measurements and the error analysis for the turbulent quantities.

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.

Response: we are aware that figure 3 is not easy to read but decided to keep the figure as the figure 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 3: label the panels a-d, and label on the right whether the plots refer to "E1" or "E2".

Response: we labelled panels a-d and also included E1 or E2 to the panels.

Figure 3: Please add error bars:

Response: Please see comment above - Error bars will not be included for flux profiles but we will show profiles of uncertainty estimates. Please refer to the general discussion of flux errors in the text.

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

Response: We added the labelling of the lines to Figure 4. Please see comments above for error bars.

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.

Response: Yes, it is not surprising that fluxes are dominated by sweeps and ejections.

The results however underline the occurrence of boundary layer decoupling (nicely visible in the strong suppression of all local motions within the concave section at the lowest centimetres) for the E2V1 experiment despite rather low Richardson number. BUT what marks a clear difference to the distribution of quadrant motions for neutral flows is the clear dominance of sweeps at the height corresponding to the wind velocity height above the cold pool – that is shifted towards the surface for higher wind velocities.

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

Response: Following your suggestion, we included labelling at top of panels.

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".

Response: please see my comment above.

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.

Response: yes that is counterintuitive but is a direct result of boundary layer decoupling as it is now defined in the Introduction. We added Figure 8a to show the local Richardson number. We define boundary layer decoupling to be characterized by a suppression of turbulent mixing near the surface causing a significant reduction of vertical momentum transfer towards the ground (which would normally increase towards the surface where shear is strongest).

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

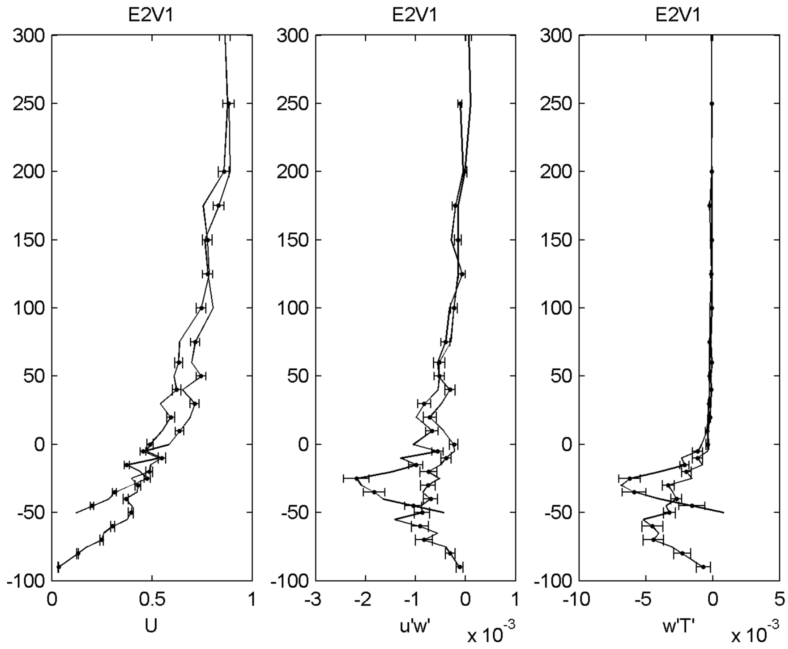
E2V1
E2V1
E2V1
U
u'w'
x 10^-3
w'T'
x 10^-3

Fig. 1.

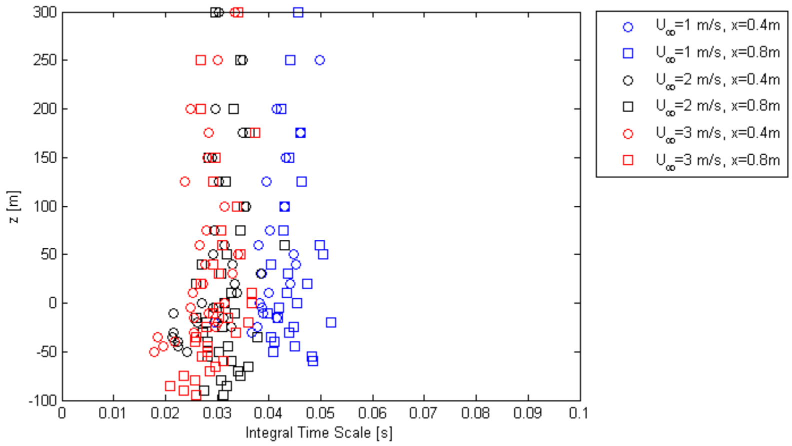
U∞=1 m/s, x=0.4m
U∞=1 m/s, x=0.8m
U∞=2 m/s, x=0.4m
U∞=2 m/s, x=0.8m
U∞=3 m/s, x=0.4m
U∞=3 m/s, x=0.8m
z [m]
Integral Time Scale [s]

Fig. 2.

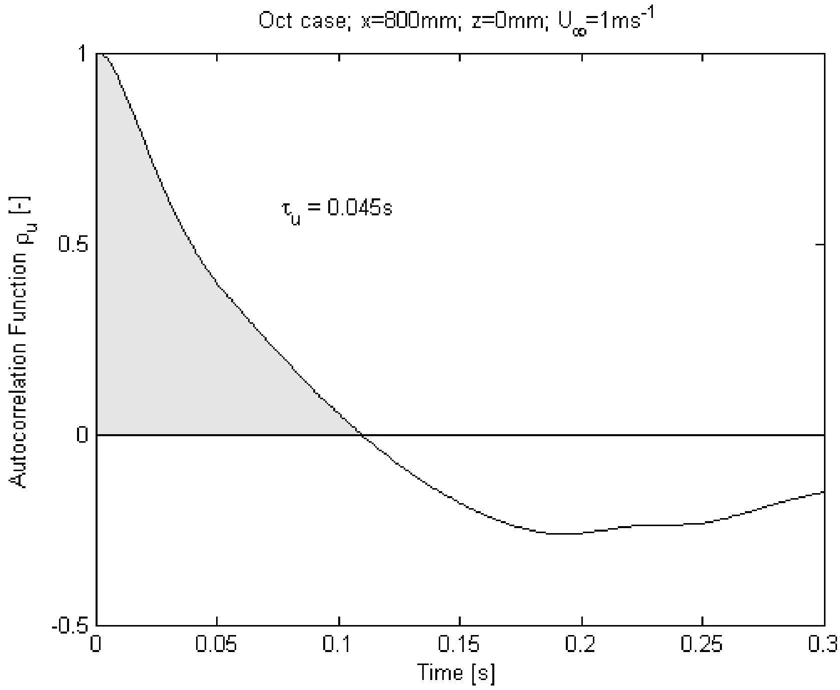
Oct case; x=800mm; z=0mm; U∞=1ms-1
Autocorrelation Function ρu [-]
τu = 0.045s
Time [s]
1
0.5
0
-0.5
0
0.05
0.1
0.15
0.2
0.25
0.3

Fig. 3.