Review of manuscript “Insights into the effect of spatial and temporal flow variations on turbulent heat exchange at a mountain glacier” by Mott et al.

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General comments

This manuscript presents and discusses data from a novel field campaign conducted on the margin of the ablation area of a mountain glacier. Spatial patterns of near-surface turbulence along with wind speed and temperature are analysed during summer melt conditions for a series of case-study days with cross-glacier winds. The analyses aim to explore the interaction of advection, boundary layer structure and surface heat flux. The measurements are well designed, and the analyses presented draw a coherent picture of the interactions, despite the complexity of the many competing processing occurring. The limitations of the measurements (and indeed those in many previous studies) are recognised and discussed. While much of the analysis is exploratory and limited to scatter plots and linear correlations, plausible hypotheses are given for the patterns observed. These hypotheses should be addressed using large-eddy simulations in future studies to enable the mechanisms for the observed cross-glacier wind and its interaction with the down-glacier flow and turbulent heat fluxes to be analysed in greater depth. However, the quality and novelty of the data, the analysis presented and the hypotheses posed in the current paper is enough to warrant publication. While some of the conclusions are somewhat speculative, the paper makes an important contribution in that it highlights that the complexity of interactions between boundary layer and meso-scale dynamics in mountainous terrain limit the generalisation of results from specific locations to other glaciers, and that further efforts to measure and model boundary layers over mountain glaciers are needed if we are to properly understand the role of processes such as advection in models of glacier melt.

The manuscript would benefit from the addition of some context for the general meteorological conditions during campaign, especially timeseries of temperature and wind speed/ direction during the 5 selected days. This would provide the reader with a more intuitive introduction to the meteorology between relationships are discussed in later figures. These figures should also include an indication of time periods defined as ‘katabatic’ and ‘disturbed’ as this is unclear. In the discussion section, the authors should reflect further on the (potential) implications for measurements and modelling of turbulent heat fluxes, wind speed and air temperature distributions on other glaciers. Along with this the authors could provide more recommendations for future research.

Specific comments to improve the paper are provided below, but in general the paper is very well written, and figures well presented. My only concern with the analysis presented is the use of ratios to normalise temperature and wind speed in Figure 6, 7 and Table 1, and I would suggest the authors instead use anomalies (in K and ms^{-1}, respectively). This is especially important for temperature, where the fractional difference for the same change in temperature (in C) become smaller as daily mean temperature (in C) increases. If the authors wish to retain the current method, the theoretical basis for using ratios needs more explanation. The discussion of temperature differences between sites and situations is also very hard to compare with the current figures (see specific comments), but a change to anomalies and addition of timeseries of from each site should address this.

While the use of scatter plots makes it a little hard to interpret the density of data in certain figures, the ability to use colour warrants this approach. For some figures (Fig 9 and 10), histograms added along the x and y axes would enable the reader to see differences in the distribution that are discussed in the text (e.g. https://matplotlib.org/3.1.0/gallery/lines_bars_and_markers/scatter_hist.html).

In short, with some changes to clarify ambiguities of method and the presentation of additional results to support some statements, this manuscript will make a good addition to the literature.

Specific comments:

41 – the sensitivity of melt rate to air temperature is not only controlled by net longwave and turbulent heat flux, but also controlled by snowfall-albedo feedbacks – consider changing "controlled" to 'strongly affected' or similar.

48 – 'several studies' – worth adding additional references to this sentence or rewording.

49 – "near-surface warming" – it is unclear what is meant here – the katabatic models discussed in the previous sentence predict enhanced turbulent heat fluxes due to increased wind speed, not temperature. Please revise.

122 – please list the model numbers of the other instrumentation, including the young anemometers, the 2d sonic anemometer and the air temperature, rh and pressure sensors. Please also note if the t/rh sensors were passively or actively ventilated and if any corrections were made to raw data aside from the eddy-covariance data.

127 – it would be useful to expand further on the choice of 1-minute averaging period, as this departs significantly from often-used averaging periods of ~30 minutes. Perhaps present some of the analysis mentioned or comment on the effect of the short averaging period on, e.g. average heat fluxes.

147-155 – please clarify the criteria used to define katabatic vs disturbed conditions as there are several different versions given in this paragraph and the figure captions –

- i.e. did disturbed situation require wind shift from just W/NW or also E sector?
- please define whether 'time periods' on line 149 means 1-min or 30-min periods.
- Line 150 says that disturbed required WD shift of >50 degree over 30 mins, yet Figure 2 has many disturbed situations with average WD around 200 degrees?
- Figure 2 caption says katabatic required consistent WD during 30-min period – are there time periods that are excluded from the analysis as they do not fit either criteria?
- Are the data sub-set solely on one station (tt3), or classified individually based on WD at each station?
- Perhaps adding a timeseries of each case-study day, showing periods defined as katabatic and disturbed at TT3 would be useful.

223 – 'Flux footprints tend to be smaller during disturbed situations." Figure 3 shows a larger overall footprint area – perhaps worth clarifying that footprints for individual periods are smaller but the more varied orientation during disturbed conditions results in a larger overall footprint, if this is the case.

227 – Do you think the different instrumentation contributes significantly to the differences observed between level 3 and the lower two levels?

227 – Do you mean a secondary larger-scale wind system above level 2? If so, please clarify.

234 – "This extreme increase of wind speed with height is confirmed by preliminary numerical simulations (not shown)". As the reader cannot assess this without presenting the data, please remove or modify this sentence.

259 – 'higher streamwise momentum fluxes" please revise – I presume you mean "larger negative streamwise momentum fluxes"?

268 – 'on 2018-08-20' – I presume you mean on all case-study days? Please revise

277 – 'the temporal variability of flux profiles increased significantly for disturbed situations' – it is very hard to assess this statement from Figure 5 – please add further statistics to describe the mean and variability of the fluxes or reword.

Figure 6 – consider moving TT3 to the x axis on these plots as it is functioning here as a common variable (hence is more like the 'independent' variable).

Figure 6 – it is hard to assess the density of points in the scatter plot – consider using a transparency for the points so that more dense data shows as darker shades.

Figure 6 – the colour scale for disturbed conditions would be better to avoid white tones as the are hard to read. Scale used in Figure 9 would be better.

308-332 – there are many statements in this section at are not clearly supported by the data presented in Figure 6. The addition of timeseries of WD/WS and temperature from multiple sites would be of great benefit here.

310 – "significant increase in the near-surface air temperature of several degrees (Fig. 6d-f)" – this cannot be ascertained from the current figure 6 as the units are normalised. Please use anomalies as suggested in general comments section or provide additional results to support this statement.

314 – "Local air temperatures at the higher altitude station TT4 showed the lowest sensitivity to changes in wind direction at TT3." It is unclear how the data support this statement – please clarify and revise.

315 – "The katabatic flow seemed to persist at the higher altitude station TT4 when at the same time all transect stations already evidenced a westerly flow (Fig. 6b)." It is unclear how the data support this statement – please clarify and revise.

317 – "Air temperatures at the glacier tongue (WT1) appeared to be strongly affected by up-valley flows (Fig. 6f)." It is unclear how the data support this statement – please clarify and revise.

326 – "explain a larger spatial variability of the air temperature" – It is unclear how the data support this statement – please clarify and revise.

329 – Are the cooler temperatures during katabatic flows affected by diurnal changes in temperature? Ie. are katabatic conditions more common during cooler periods at night time?

Table 1 – what is U_T ?

342 – 'all four turbulence stations' do you mean 'all three turbulence stations' or 'all 6 turbulence sensors'. Also please list what height data is from

361 – ‘showed small spatial differences’ – this is very hard to interpret from Figure 7 – a histogram of differences between fluxes at different stations would support this.

362 – “despite significantly higher air temperatures observed at TT1” – this is not shown and needs to be supported by additional results – perhaps a histogram of temperature differences between each site in different conditions.

388 – what fraction of periods were excluded?

Figure 8 – does this figure include all periods from the 5 days, or only disturbed periods? Please clarify in the caption. Please also add units and level used for HA calculation.

423 – “Similar to heat advection, peak vertical turbulent heat fluxes coincided with peak V-component at the centerline.” - to what extent is this due to the correlation between mean wind speed and vertical fluxes? Please discuss.

424 – “Correlation coefficients R(w′T′,UT) were high between TT1-TT2 and TT2-TT3 station pairs with a slightly higher value for stations closer to the centerline.” It is unclear how this relates to the data presented in Table 1. Please revise.

Figure 9 - consider adding histograms to each axis. It is currently very difficult to compare the distribution of points between different conditions and sites.

Figure 9 - consider adding histograms to the y axes. It is currently very difficult to compare the distribution of points between different conditions and sites.

509 - The steep moraine sides are likely to play a role in the sheltering of the site closest to the glacier margin, especially considering the sharp slope transitions and short distances involved. Thus, the flow hitting the glacier edge may not be well developed and still be affected by lee-side flow separation etc, reducing its ability to influence the stable glacier boundary layer. This may be worth discussing further here.

528 – as the study only presents data from 5 days, it would be more meaningful to say “during five days that displayed a distinct disruption of down-glacier flow during a three week period in summer 2018.” Or similar.

541 – ‘induced by strong westerly winds’ – while this makes sense, the origin of the flow is still speculative so please revise.

552 – ‘At the peripheral areas stronger exposure’ – shouldn’t this be ‘weaker exposure’.

552 – As wind direction is not presented for TT1 it is impossible to assess if the ‘preservation of a very-shallow low-level katabatic jet’ is supported by the results. Figure 1 shows the WD is aligned at all levels at TT3 during disturbed situations – in order to support a katabatic jet at TT1 the wind direction would need to be maintained down-slope. The BL could still be decoupled at TT1 because of the strong thermal stratification, but this does not necessarily mean that a katabatic jet will exist at TT1. Please revise.

575 – “the frequency of such flows at other glaciers is not known” – this comment highlights that fact that the frequency of these flows has not been presented in the current study. This would be an easy and useful addition to the results.

Editorial comments:

16 – “the temporal change” -> “temporal changes”

121 – ‘while as the fifth tower (WT1), with at these’ –> ‘while at the fifth tower (WT!), these’

125 – suggest changing ‘methodology’ to ‘data processing’

141 – ‘our dataset is not allowing us a’ -> ‘our dataset does not allow a’

145-157 – suggest moving this section to the end of section 2.2 so that it proceed mention of katabatic conditions at line 137.

160 – please add units for HA and FD

163 – what height wind speed was used for HA calculations?

188 – add ‘for each case-study day’ to caption.

225 - ‘below 2.3 m above ground,” -> ‘below 2.3 m,’

275 – ‘measurement levels turbulence’ -> measurement levels, turbulence’

289 – ‘(TT1, TT2, TT3)’ -> (TT1, TT3)

Table 1 – please add the sensor height or level used to the Table caption. Please add units for w’T’

371 – please add height or level used for heat flux

377 – please add units for HA

497 – ‘supposed’ – ‘hypothesised’

560 – ‘surrounding terrain’ – ‘surrounding terrain in this study’