Referee review for manuscript tc-2016-93, under review for The Cryosphere.

"Surface-layer turbulence, energy-balance and links to atmospheric circulations over a mountain glacier in the French Alps" by Maxime Litt, Jean-Emmanuel Sicart, Delphine Six, Patrick Wagnon, and Warren D. Helgason.

General Comments

The paper presents an analysis of micro-meteorological data from two periods on the mountain glacier St Sorlin. These data provide the platform to assess the contribution of turbulent fluxes, and their uncertainties, to modelled glacier surface melt in the context of different weather types. The measured turbulence spectrum is presented and compared to theoretical predictions that form the basis of the widely used Monin-Obukhov similarity theory. Large deviations from theoretical predictions are found in both weakly and strongly turbulent conditions. The ability to correctly model the turbulent heat fluxes using bulk aerodynamic methods is discussed in the context of weather types and the characteristic turbulence regimes they experience. The authors find that despite the divergence of measured turbulence from theoretical predictions, simple schemes are able to model the melt to within the uncertainty in observed melt.

The paper presents useful and insightful data that support emerging conceptual models of the influence of outer layer turbulence on surface layer turbulent fluxes in glacial environments. The analysis of weather types and the associated characteristic valley circulation, wind speed and temperature profiles and TKE relationships are compelling and link well together. The analysis of turbulence spectra and co-spectra is clear and fits well into the progression of analysis.

The attempt to link these fine scales of turbulence to surface melt, through the calculation of fluxes in using the bulk aerodynamic (BA) method, is ambitious but makes sense in the conceptual framework of the paper. However, the formal links between each scale of analysis are not always well made and this reduces the confidence in the interpretations made. The large scope also makes the paper somewhat disjointed. At times analyses are presented that are not entirely relevant to the key points e.g. a time series of net turbulent heat fluxes for each period. Other analysis necessary to support the points being made are missing e.g. a direct comparison of hourly turbulent heat fluxes from EC and BA methods along with their uncertainties. Despite these omissions, the methods are well described and logical with some gaps noted in the specific comments.

There are clearly some interesting interactions occurring between the magnitude of net turbulent fluxes and the quantity of melt, which deserve further elucidating. The EC method gives the lowest melt of all methods, yet the has the same or higher net turbulent heat fluxes as the other methods. Why do we see a breakdown of theoretical predictions, yet no impact on modelled melt? Further description of the processes occurring at the hourly and daily scale are needed to formally link uncertainties in the calculation method for turbulent heat fluxes to melt. Along with this, one of the key points introduced is the errors in the calculation of the turbulent heat flux, yet this is poorly addressed in later analysis. This would put the paper in a much stronger position to comment on the conditions in which the violation of current turbulent flux modelling methods will have an impact on melt.

The sub-setting of the analysis by weather types (i.e. Strong/ weak gradient wind forcing) is a particularly interesting approach. I find no issue in sub-setting the spectral analysis by TKE, rather than the weather types as for most of the other analysis. However, the discussion should focus around one or the other for clarity, rather than switching between the two. As it stands this switching is rather confusing.

On the whole, the paper is likely to make a useful contribution to glacier meteorology and surface energy balance with revision. Textual revisions and further analyses are needed for the manuscript to be acceptable, particularly analyses comparing hourly turbulent heat fluxes and their associated errors from each method. Some of the content is perhaps more suited to an applied meteorological journal, though I do not see that the manuscript is entirely out of the scope of *The Cryosphere* given the widespread use of energy balance modelling within the cryospheric community. Preferably, the emphasis on glacier melt and uncertainties/ validation of modelling approaches for turbulent heat fluxes can be further highlighted.

Specific Comments

P2 ln2. "englacial"- do you mean inside the glacier? Or rather in catchments with glaciers in them, which would be described "glacierised/ glacierized".

P2 ln11. Correct date - Anderson (2010). Also note that recent work has indicated the contribution of turbulent heat fluxes to melt was very likely overstated in this paper (see Conway and Cullen, 2016), so it is, perhaps, not the best reference to use.

P2 ln20. The work of Denby and Greuell (2000) and associated papers needs to be addressed in the introduction as this has been a key paper justifying the use of the BA method over glacier surfaces.

P3 ln18. Were any corrections for tilting of the radiometer measurements necessary given the high melt rates? If so, please detail the procedure used to correct for tilting of the mast, or if corrections were not performed please comment on the effect of tilt on the radiation values.

P4 ln5. Please explain why 1-hour runs were chosen over standard 30-minute runs?

P5 ln2. "apparition" -> appearance

P5 ln18. geopotential -> geopotential height

P5 ln19. Please explain the procedure used to 'analyse and compare' each day of the study period to the WP, particularly if this is an objective or expert judgement procedure.

P6 ln9. What do you mean by "bad" weather conditions? Please replace with a more descriptive comment.

P6 ln11. Table 3 -> Table 2

P6 ln15. More accepted acronyms are MOST or M-O theory, please use one of these throughout.

P7 ln20. What runs were chosen for the analysis of roughness lengths, and how was stability taken into account? This is especially important given the low wind speed maximum observed. Also, why was the EC data not used to analyse roughness lengths?

P7 ln25. The important small scale topography referred to by Smeets and van den Broeke (2008a) is on horizontal scales 5 to 10m, which suggests that the evolution of topography on Saint-Sorlin on the order of 20-30 cm over a few metres should have an impact on the aerodynamic roughness lengths. Also note that in another paper (Smeets and van den Broeke, 2008b) the authors find these same hummocks have a profound effect on scalar transfer. Please change this sentence to accurately reflect the papers conclusions.

P9 ln3. Please refrain from using parentheses to denote opposites and reword these sentences appropriately: i.e. "The symbols SW and LW stand for hourly mean shortwave and longwave radiation, respectively."

P9 ln10. Please justify the exclusion of heat from precipitation. This is only reasonable if the contribution of this flux can be shown to be negligible.

P10 Section 4.1.1. It is not clear how this text describing the temporal progression of the meteorology is central to the paper and should be shortened to a few sentences. Likewise, section 4.1.3 should be shortened and included in here.

P13 ln9. Please fix the use of the parentheses as per comment on p9 ln3.

P13 ln12. Please introduce the acronym GQR. Also do you mean $H_{EC} > 5$ W m^{-2} given the sign convention used in the paper?

P13 ln20. Is it reasonable to use z/L from the BA method when you show later that the BA method underestimates the sensible heat flux and therefore, is likely to incorrectly represent z/L? Also, please explain why z/L was calculated from the BA method and not directly from the EC measurements.

P14 ln12 and further references: "Kaimal curve" -> "Kansas curve". This is more descriptive and is consistent with p6 ln31.

P14 Section 4.2.3 The first two paragraphs can be shortened to a few sentences as it is not clear how the temporal progression of the SEB is central to the paper. Conversely, further discussion of the results in Table 2 are needed, as is a presentation of a direct comparison of turbulent fluxes from the EC and BA methods. This would ideally take the form of scatter plots of hourly fluxes that include error bars. At the very least some descriptive statistics of the correlation between and spread within each (measured and modelled) flux are needed.

P14 ln33 and further references. For clarity, acronyms for the two BA calculation methods need to be introduced earlier and used throughout, e.g. BA1, BA2 or BA_{pro} BA_{eff}. The descriptive names "the BA method based on the profile-derived roughness lengths" become confusing when comparing methods.

P15 ln2. The analysis of errors in the EC and BA methods needs to be presented as it is the key link between the representation of turbulence mechanisms and surface melt. This should include uncertainties on the figures given in Table 2 as well as error bars on any hourly fluxes presented.

P15 ln9. Previsions -> conclusions?

P16 ln8. The discussion here about the TKE budget is not well framed and more theoretical background is needed before the results are presented. As it is, this first sentence is unclear and needs to be broken up and reworded.

P16 ln16. "The EC method probably accounted for this" I don't understand what you mean here. The measured EC fluxes account for all of the extra turbulence observed as they are based on the same data. Perhaps you mean there is extra turbulence not captured by the EC method, or that the EC was not entirely in the surface layer. Please clarify this in the text.

P17 ln8. The use of the word "probably" here and elsewhere (p16 ln 8, section 6) suggests the interpretations and conclusions reached may not be well founded. It would be better to frame the results in the context of a certain conceptual framework, noting where the results agree or disagree with this framework.

P17 ln10. "were decorrelated from the surface fluxes". These analyses need to be shown in the paper.

P17 ln16. Where does the +/- 10% error on the surface height come from? Earlier an uncertainty of 0.1 m was stated. The uncertainty for each type is presumably some combination of the daily sum of melt for each type and the instrument uncertainty.

P17 ln33. Why were the BA fluxes not validated against the EC fluxes you already have?

P17 ln34. The effective roughness length is, in reality, smaller than the aerodynamic roughness length and larger than the scalar roughness lengths. Please change.

P18 Section 5.3. It is good reflect on the limitations of the particular weather typing method used, but the authors need to comment on if these limitations have a real bearing on the analyses made. If they do have a significant bearing on the results, then perhaps a different method needs to be chosen.

P19 ln20. 'Sublimation' -> perhaps 'evaporation' would be more appropriate given you state the surface was melting most of the period.

P19 ln28. "erratic discrepancies between EC fluxes and BA fluxes". These crucial elements of the analyses are not presented and need to be.

P20 ln2 to 8. These conclusions a very speculative considering no EC measurements are available and that both methods yield melt that is within the measurement uncertainty. Please revise.

P20 ln14. Please be more specific about which high latitude glaciers have large contributions of turbulent heat fluxes.

Table 1. It would be useful to know the maximum and minimum height for each instrument, or at least the standard deviation from the mean height.

Table 1. The accuracy of the CNR1 is listed as 0.4% - what is the justification for this figure, given the nominal accuracy should be on the order of 5 to 10%. Similarly, for the CSAT3, the accuracy is on the order of ± 2 % and ± 6% of the wind speed for attack angles of 5 and 20 degrees from horizontal. This equates to uncertainty on the order of 0.1 ms^{-1} or larger for typical wind speeds. A more careful justification of the accuracy values is needed in the text.

Table 2. Why were average wind speed, air temperature etc. from the glacier AWS not included here?

Table 2. The units for q would be more simply expressed as g/kg. Also the values seem an order of magnitude too high – typical values would be 5 g/kg and here they are 50 g/kg. Please check the units of these values.

Table 2. The table would be much better split into two or more tables that can be inserted at the appropriate sections (data, results). Also you state you do not analyse the "other forcing" category further, but present it here. It would be less confusing to exclude it entirely. Similarly, the inclusion of both the weather types and the TKE bands is confusing and should be clarified.

Figure 1. It would be good for this figure to be larger and for the photos to be in colour.

Figure 3. This figure is very hard to read due to the hourly data used, small size and lack of grid lines. Perhaps either daily means of each flux can be presented, or the figures made

substantially larger. Tick marks also need to correspond to some meaningful interval (rather than 1/6th of 20 days).

Figure 7. Caption: (b) spectra of w, (c) co-spectra of w and theta

Figure 9. These error bars seem unrealistically small – especially given the large spread in H from an uncertainty in surface temperature of +/- 1 K. Further justification of these errors is needed.

Figure 9. Please use a different shading for H+LE that is not the same as for melt.

Figure 9. Please explain where the 10% error bar for melt comes from.

Figure 9. Please explain why the relatively magnitudes of melt (Figure 9) for each method differ markedly from those for the net turbulent heat flux (Table 2). e.g. EC has the highest net flux in weak forcing, but the lowest melt?

References (not already in the manuscript)

Conway, J. P. and Cullen, N. J.: Cloud effects on surface energy and mass balance in the ablation area of Brewster Glacier, New Zealand, The Cryosphere, 10, 313-328, 2016.

Smeets, C. J. P. P. and van den Broeke, M. R.: The Parameterisation of Scalar Transfer over Rough Ice, Boundary-Layer Meteorology, 128, 339-355, 2008b.