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The Cryosphere
Discussions

Interactive comment on "A multi-season investigation of glacier surface roughness lengths through in situ and remote observation" *by* Noel Fitzpatrick et al.

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General comments

The work described in this paper addresses a key challenge: the characterization of the surface roughness lengths is highly relevant for glacier and snow energy balance modelling. It is totally in the scope of the Journal. The paper is well structured, nicely written, and proposes clear figures and tables which facilitate the overview of the instrumental setting and, to be more specific, of the different surface's temporal and spatial scales implied.

A new approach, the block-method, and the profile method, an enhanced/modified version of a previously published method, are proposed to characterize the surface roughness parameters, and these are clearly described. These are compared to the roughness lengths derived by inverting the equation describing the well-known bulk-aerodynamic approach, in which high frequency eddy covariance measurements are used to calculate the instantaneous fluxes. Footprint calculations and assessment are included which gives the results a strong basis, and all these methods have been implemented really carefully, using on-field data which are - from the paper perspective- of high-quality.

This paper should really be published, since the work is highly relevant for future investigation. Though, a few important key issues, pointing towards the basis of the above mentioned methods, are only briefly mentioned, thought they might play a fundamental role in the divergences observed in the results from the different methods. I don't think any key calculations would have to be remade, but at least a stronger discussion of these issues should be included, and an assessment of the impact that may have on the observed results, in order for readers and the community not to blindly follow these.

Please find these comments below.

The eddy covariance derived surface roughness and the assumptions of the similarity theory. All along the paper, it is assumed that the 30 min-averaged values of u*, T* and q*, are collected during flow conditions which are representative of the assumptions of the similarity theory, a necessary condition for the applicability of the bulk-formulation. This is guaranteed through the careful filtering and selection of the high frequency data, for various criteria (i.e, minimum wind speed, specific flow direction, near-neutral stability conditions, etc., section 2.4, lines 20 to 28). This ensures the scaling of the bulk formulation is valid only if the state of the turbulent flow is driven only by its interaction with the surface, in other words if turbulence is generated only by the interaction of the flow with the surface, and depends only on the stability of the surface layer. One key filter that ensures this conditions to be fulfilled is the one for stationarity of the flow and fully developed turbulence. Thought, even in these cases it is possible that additional turbulence is transported through by large eddies originating from outside layers – related processes, or being transported away, so that the actual measured u*, T*, q* do not scale with the mean flow surface properties (Hogstrom, 2002, Thomas and Foken 2005, Barthlott et al 2007, Litt et al., 2015), even under neutral stability conditions. In such cases it is most likely that the roughness provided by the High Frequency fluxes will not be representative of the surface characteristics, and so that the roughness inferred out from the actual surface state (DEM block, profile methods) do not reproduce the actual fluxes when using the bulk formulation. If the filtering retains only specific meteorological conditions for which the turbulent flow develops in a certain way, we could observe a persistent bias. I think this should be addressed in the discussion, and mentioned somewhere in the introduction. An assessment could be done. For example, is there an actual relationship between the value of the stationarity criteria, and the actual EC roughness values? Also, the presence of turbulent transport can be assessed through a spectral analysis or the analysis of the integral turbulence values.

Katabatic winds

A katabatic wind maximum is often present near the surface above glaciers, and this is mentioned in the text and discussion, but only briefly. Actually the formulation of the bulk method is not adapted to the presence of a katabatic maximum, since it induces turbulent transport (Smeets and al., 1998,2000). Though no real assessment is made upon that.

Assessment of errors and specifically surface temperature.

Errors on measurements on the EC derived roughness are not assessed. Though, these might be large. Also, the stability corrections, which are used to assess the neutrality of the turbulent flow, and to finally calculate the surface roughness, are dependent upon the surface temperature measurements which, I suppose (not stated

clearly in the manuscript) are derived from the Infrared radiometer readings (either an Apogee SI-111 or maybe the Kipp & Zonen CNR4 when the previous is not available). These measurements are directly linked to the value of the surface emissivity of the snow or ice. Which value is assumed for that is not clearly mentioned. How are you taking the uncertainty related to that into account?

A few specific comments.

The previous reviewer already provided the relevant comments upon the part describing the profile and block methods, here are some specific comments on the other parts:

1) Introduction. For roughness assessment you could also use detailed wind-temperature profiles (Sicart et al, 2014) 2.4) Data treatment, eddy covariance data: Could you provide the chosen threshold for stationarity and indicate the percentage of remaining data blocks after filtering? 2.5) mention somewhere how you derive surface temperature out of the SI-112 apogee instruments or the CNR4, more specifically what do you choose for the surface emissivity? 3.1) Line 23: provide the actual range explicitly. 4.1) Line 25: do you have any estimate of the actual height of the Katabatic wind maximum?

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