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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

This well-written paper provides an original and significant contribution to the study of roughness lengths on mountain glaciers. It was extremely interesting to read and it provides new insights into roughness length estimation through quantitative comparison of extensive and rigorously collected data sets. The analysis does much to close the gap between eddy covariance and topography derived aerodynamic roughness estimates (section 3.2.1 was very interesting in that regard with some highly citeable findings).

The paper itself is the correct length, contains clear figures and is very easy to read. I believe it to be publishable in almost its present form, though I have a few minor points listed below. These are generally points of clarification. However, I did find the synthetic DEM approach to investigate the effect of smaller scale roughness on roughness length to be rather unconvincing. I do not feel that the paper needs this in any case.

Specific comments

(1) Other studies estimating z0 on mountain glaciers

The introduction is clear and concise, presenting a clear rationale for the study while acknowledging previous work. In general, this paper adequately cites and gives ample credit to other studies. However, I can identify a few specific papers that contain findings of some relevance to the study here. In the majority, they add greater weight to the arguments and findings presented. While I agree with the comment (P3, L6) that "Similar studies on mountain glaciers are very rare", there are a few examples for Himalayan (debris-covered) glaciers that might be worth considering (Quincey et al., 2017; Miles et al., 2017).

Miles, E.S., Steiner, J.F. and Brun, F., 2017. Highly variable aerodynamic roughness length (z0) for a hummocky debrisâĂŘcovered glacier. Journal of Geophysical Research: Atmospheres, 122(16), pp.8447-8466.

Quincey, D., Smith, M., Rounce, D., Ross, A., King, O. and Watson, C., 2017. Evaluating morphological estimates of the aerodynamic roughness of debris covered glacier ice. Earth Surface Processes and Landforms, 42(15), pp.2541-2553.

(2) Block estimation method

The paper presents an interesting DEM-based method for estimating z0 from microtopography. Overall, this appears rigorous and correct, but I do have a few minor questions for clarification regarding the implementation of the method.

- Equation (7) would benefit from some clarification. It took me a while to understand that, but I was simply confused by the definition of ‘lines’ and ‘rows’. Aside from that, it makes sense – Figure 2 is extremely helpful. How does this work when the blocks have a larger cell dimension? Do you assume sheltering across the whole 11 m?

- How were edge effects of using a moving window dealt with? Was it extrapolated or padded with zeros?

- Is the result of equation 8 not really a localised z0? Why is it given different notation and called a drag value?

- What are these “assumed footprint areas” (P9L18)? It sounds interesting, but I’d like to see more details (mentioned again P18, L7). Actually, the finding on P12L30 that equally weighted cells gave similar results is very interesting and helpful for studies going forwards. Is the assumed footprint related to this?

(3) Profile estimation method

I found this method to be very interesting. In fact, it differs markedly from the conventional ‘profile’ approach (e.g. Munro, 1989) in that wind parallel profiles are taken, rather than perpendicular profiles. This works well when dealing with topographic data at the scale available here (1 x 1 m). I find myself agreeing with the authors’ approach here, but I think the difference between this and previous work could be flagged. Once again, I have a few points for clarification:

- The cut-off wavelength of 35 m seems to be important, but looking at Figure 3c, I cannot quite see why this was decided upon.

- Equation 10 shows that the absolute value of the elevation difference between cells is used in the estimate of s. Is this correct? Is it not more appropriate to consider the differences facing the wind (i.e. only considering positive differences, rather than turning negative differences into positive values)?

- As with the block method, how were edge effects dealt with?

Munro, D.S., 1989. Surface roughness and bulk heat transfer on a glacier: comparison with eddy correlation. Journal of Glaciology, 35(121), pp.343-348.

(4) DEM scale sensitivity

I find the creation of the synthetic DEM at a finer scale than the LiDAR data to be the least convincing aspect of the paper. In my opinion, it detracts from the key messages of the paper and is better off being removed entirely. It would make a useful discussion point for further work, but I am not convinced that the data support this analysis.

Technical corrections

P4L22: data was -> data were

Table 2 – are z and zu defined in the text anywhere?

P10L30 – should the second instance of ‘parallel’ read ‘perpendicular’?

P10L33 –re-word ‘demeaned’!

Section 3.2.3 – to confirm, is this the 1 x 1 m data?

P16L32 – note typo in citation

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