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The Cryosphere Discussions

Interactive comment on "Spatial and temporal variations in glacier surface roughness during melting season, as observed at August-one glacier, Qilian mountains, China" *by* Junfeng Liu et al.

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General comments:

In this study, which is well within the remit of the journal, the authors present some interesting, hard-won (by the sounds of it) microtopographic and meteorological data from the August-one ice cap, China. They implement novel methods to collect some of their photogrammetric data automatically, in a location that is underrepresented in the glaciological literature.

Methods and data are presented and explained reasonably clearly, with some valuable insights given through comparison between microtopographic and meteorological measurements. While there is no independent validation of z0 values with other methods of obtaining z0 (wind profiles, eddy covariance), this is one of few studies that shows how the microtopographic methods used here can produce sensible values for melt volumes in the wider context of glacier monitoring. The temporal aspect of the work is a worthwhile inclusion, not just for the interesting nature of the data, but for the implications if such patterns were observed/studied elsewhere.

Overall it is well written and structured logically, and does not need much revision to make it publishable. Suggestions are fairly minor, although I would suggest that: 1) some terminology should be adjusted (see specific comments regarding 'surface roughness', 'direct measurement' etc), 2) methods need further justification, in that some additional studies should be read/cited (again, see specific comments) and 3) figures could be of higher quality generally (i.e. do not just use screenshots for compound figures).

Specific comments:

Abstract Seeing as your work relates to z0 and not albedo, I would remove the mentions of albedo from the abstract to avoid confusion.

Introduction

Line 32: here, and throughout the manuscript, make sure to add a space between citations listed in parentheses and separated by semi-colons.

Line 41 – missed references to more recent studies using wind profiles:

Miles, E.S., Steiner, J.F. and Brun, F., (2017). Highly variable aerodynamic roughness length (z0) for a hummocky debris-covered 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). Evalu-

ating morphological estimates of the aerodynamic roughness of debris covered glacier ice. Earth Surface Processes and Landforms, 42(15), pp.2541-2553.

Line 42 – "direct measurement of z0 has been shown to be more accurate than previous methods" – it is unclear what methods are referred to by this statement. Wind profile and microtopographic values are both estimates based on models. Please clarify or correct, and make sure it is clear throughout the rest of the paper that microtopographic z0 is an estimate, not a measurement.

Line 44 – "Current research has increasingly used direct measurement." Terminology needs adjusting to reflect the previous comment.

Line 47 – as above.

Line 49 – 51: The first sentence could be backed-up by several examples including Irvine-Fynn et al (2014), Smith et al (2016), Quincey et al (2017), Miles et al (2017), and Fitzpatrick et al (2019). The second and third sentences are confusing; while Kääb and Vollmer (2000) utilised aerial photography for photogrammetry, this was not used for a purpose related to ice roughness. The next sentence "Digital photos were taken against a dark background plate" does not refer to a part of the cited study, but rather to Rees (1999), who published the method mentioned.

Data and methods – overall this is very clear, and the photogrammetry details are nice to see.

Line 72: it would be interesting and useful background to include some information on the normal influence of the turbulent fluxes at this location.

Figure 1: Some scale would be useful in both panels. Is the figure a screenshot? Some artefacts have made their way into the top of the figure. Also some place names for context in panel (a) would help.

Line 93-94: Figure 2b does not illustrate the frame very well, in fact it is quite unclear what the image shows.

Line 99: in which direction did the camera move? Along the frame, or into it?

Line 117: what was the rationale for the plot size?

Figure 2: do you have any other site photos? Panel (b) is not very useful as it is, and some detail is not shown by panel (3).

Line 131: it might be useful to refer to the work of James & Robson (2014) and James et al (2017) for some critiques of using Agisoft Photoscan.

Line 149: repetition of reference.

Line 156: Smith et al (2016) calculated h* from the mean vertical extent above a detrended plane. Hopefully this important step has just been omitted from the text (in which case it should be added, as detrending is a vital part of the method), and not from your calculations.

Line 162: please reference Munro (1989) for the profile-based simplification of the Lettau (1969) equation.

Line 174: Fitzpatrick et al (2019) also provide useful discussion of microtopographic methods. In addition, please clarify terminology – I would suggest reconsidering the use of the term 'surface roughness' as it can refer to one of a number of metrics (Smith, 2014), and could be more specific.

Results

Section 3.1 Photogrammetry precision: while this is important to report, much of the text is summarised in the two tables and two figures. If you were looking to cut down on text, perhaps this section could be more concise.

Line 213: change geo-reference to geo-referencing. Also, I'm not sure which value is being referred to by saying that "errors were less than 1 millimeter", as most of the averages in the tables are >1 mm.

Line 216: define RMSE before the first use of the acronym (line 213), not after the second time.

Line 227: Note that the accuracy requirements given by Rees and Arnold (2006) were for 2D topographic transects, not 3D plots.

Line 237: change 'covered' to 'covering'

Line 237: "z0 was highly variable" – it's worth keeping some perspective here. While z0 varied, it did so by less than 3 mm.

Figure 5: There is a typo on the y-axis label which should read 'surface roughness'. Also please see my previous note on using the term 'surface roughness'.

Line 258: Should be 'both of which occurred in periods of transition'.

Line 261: This is an interesting finding. Can you provide more detail? Can you include the actual values for the manually collected data that show the same pattern? Additionally, in the methods it is mentioned that z0 is an average of all four directional values – were the individual values analysed for directional influence?

Line 265: While z0 certainly changed over time, I do not think it is correct to say that it was related to the date. It was different when measured on different days, but this is because of factors other than what day of the month it is.

Line 268: is the 'terminal' the same as the terminus of the glacier? The latter expression is more commonly used.

Line 269: Change to 'At higher altitudes'

Line 275: Please be more specific than just saying "Manual investigation" – I take it here you are referring to photogrammetric data collected manually?

Lines 306-309: I am not sure that a separate introduction is required here. The final two sentences could be tacked onto the beginning of the next paragraph.

Line 335: changed "account" to "accounted".

Line 360: the r2 value reported here is different to the one shown in Figure 9. This is also the case for line 370 and fig. 11a, and line 372/fig. 11b.

Discussion

Line 412: I do not think there needs to be a summary here – all of the information should be apparent from the main text.

Line 414: Do not need to cite these again here.

Line 416: I notice that the difference between ice z0 and snow z0 is very small. Can you comment on this in the text? Some find that the difference can be an order of magnitude. Were both surfaces at your site particularly smooth? Or could it be something to do with the size of the patch (thinking about the scale/resolution dependency of the microtopographic method – see Fitzpatrick et al. 2019).

Lines 422-425: this paragraph needs rewording so that the first sentence does not seem disconnected from the rest.

Lines 430-433: this is a significant finding; however, there is something about the wording in this sentence that I think should be addressed – as z0 is in this instance (using the bulk method) required to calculate the turbulent fluxes, arguing that the turbulent heat index (calculated with turbulent fluxes) is a determining factor seems circular. I think the statement could be made more clearly, perhaps referring to the association between the two rather than a causal relationship.

Line 434: Make sure terminology is clear here – you refer to the August-one ice cap, and then call it a glacier. In my understanding, these are different.

Line 439: The second sentence can be deleted, it does not add anything to the findings or argument.

Conclusion

I think comparison to other ice masses, and links to other studies/locations should be made in the discussion, with some thought given to whether you might find the same results where ice z0 and snow z0 have greater contrast. And, while it is important to acknowledge the site specificity of a study, further studies are always required and saying so in the conclusions is superfluous. Instead, the main messages from the paper (3 or 4 of them, as far as I can see) should be summarised here.

References cited in comments:

Fitzpatrick, N., Radić, V., & Menounos, B. (2019). A multi-season investigation of glacier surface roughness lengths through in situ and remote observation. The Cryosphere, 13(3), 1051-1071.

Irvine-Fynn, T., Sanz-Ablanedo, E., Rutter, N., Smith, M., & Chandler, J. (2014). Measuring glacier surface roughness using plot-scale, close-range digital photogrammetry. Journal of Glaciology, 60(223), 957-969.

James, M. R., & Robson, S. (2014). Mitigating systematic error in topographic models derived from UAV and groundâĂŘbased image networks. Earth Surface Processes and Landforms, 39(10), 1413-1420.

James, M. R., Robson, S., & Smith, M. W. (2017). 3âĂŘD uncertaintyâĂŘbased topographic change detection with structureâĂŘfromâĂŘmotion photogrammetry: precision maps for ground control and directly georeferenced surveys. Earth Surface Processes and Landforms, 42(12), 1769-1788.

Kääb, A., & Vollmer, M. (2000). Surface geometry, thickness changes and flow fields on creeping mountain permafrost: automatic extraction by digital image analysis. Permafrost and Periglacial Processes, 11(4), 315-326.

Lettau, H. (1969). Note on aerodynamic roughness-parameter estimation on the basis of roughness-element description. Journal of applied meteorology, 8(5), 828-832.

Munro, D. S. (1989). Surface roughness and bulk heat transfer on a glacier: compari-

son with eddy correlation. Journal of Glaciology, 35(121), 343-348.

Rees, W. G. (1998). A rapid method of measuring snow-surface profiles. Journal of Glaciology, 44(148), 674-675.

Rees, W. G., & Arnold, N. S. (2006). Scale-dependent roughness of a glacier surface: implications for radar backscatter and aerodynamic roughness modelling. Journal of Glaciology, 52(177), 214-222.

Smith, M. W. (2014). Roughness in the earth sciences. Earth-Science Reviews, 136, 202-225.

Smith, M. W., Quincey, D. J., Dixon, T., Bingham, R. G., Carrivick, J. L., IrvineâĂŘFynn, T. D., & Rippin, D. M. (2016). Aerodynamic roughness of glacial ice surfaces derived from highâĂŘresolution topographic data. Journal of Geophysical Research: Earth Surface, 121(4), 748-766.

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