We would like to thank Joshua Chambers for his insightful comments and for opening the discussion. The corresponding changes and refinements have been made in the revised paper.

Reply to comments from referee Joshua Chambers

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 valu- able insights given through comparison between microtopographic and meteorological measurements. While there is no independent validation of z0 values with other meth- ods 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 com- pound figures).

Reply: In the revised version of the paper, we adjusted the terminology of 'surface roughness' as 'aerodynamic surface roughness'. In the methods part, we cite these latest studies. Figures in the revised version have higher quality.

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.

Reply: We delete accordingly.

Introduction

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

Reply: Done

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

Reply: Thanks for your suggestions, we cited related literatures as

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. doi:10.1002/2017JD026510

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), 2541-2553. DOI:10.1002/esp.4198.

Line 42 – "direct measurement of z0 has been shown to be more accurate than previ- ous methods" – it is unclear what methods are referred to by this statement. Wind pro- file 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.

Reply: Thanks for your suggestions. We delete the sentence in Line 42 and rewrite as "Glacier surface z_0 has been widely studied through methods such as eddy covariance (Munro, 1989;Smeets et al., 2000;Smeets and Van den Broeke, 2008; Fitzpatrick et al., 2019), or wind profile (Wendler and Streten, 1969; Greuell and Smeets, 2001; Denby and Snellen, 2002; Miles et al., 2017; Quincey et al., 2017). However, micro-topographic estimated z_0 shows some advantages, such as lower scatter, rather than profile measurements over slush and ice (Brock et al., 2006), and ease of application at different locations (Smith et al., 2016)."

The "direct measurement" changed to "microtopographic estimated z0". The rest of the paper also changed accordingly.

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

Reply: Done.

Line 47 – as above.

Reply: Done.

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.

Reply: We added these references in the first sentence.

The following part has changed as " Initially, the Micro-topographic method was developed as snow digital photos were taken against a dark background plate. The contrast between the surface photo and the plate could then be quantified as an estimation of surface roughness (Rees, 1998). This method is still widely applied to quantify glacier surface roughness (Rees and Arnold, 2006; Fassnacht et al., 2009a; Fassnacht et al., 2009b; Manninen et al., 2012)."

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.

Reply: we cited one published energy balance analysis results by Qing et al., (2018). The add part "Energy balance analysis indicated that net radiation contribute 86% and turbulent heat fluxes contribute about 14% to the energy budget in the melting season. A sustained period of positive turbulent latent flux exists on the August-one ice cap in August, causing faster melt rate in this period (Qing et al., 2018)."

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.

Reply: Done

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

Reply: we have revised accordingly.

In the revised manuscript, We split Figure 2 to Figure 2 and Figure3. Figure 2 showed the automatic photogrammetry. Figure 3 illustrate the automatic and manual photogrammetry control points and check points, the control frame, and the detrended DEM.

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

Reply：The camera was 1.7m above ice surface and move along the control frame.

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

Reply：Plots should large enough to include obstacles to represent the glacier surface. The August-one glacier ice cap is generally smooth and uniform surface. We expect the 1.1*1.1m plot is large enough to represent the dominant roughness elements influencing z0. Additionally, the 1.1m*1.1m aluminum square is quite portable and easily apply at different locations of glacier.

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).

Reply: we have used photo and corresponding DEM data to represent the manual and automatic photogrammetry acquired micro-scale surface roughness.

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.

Reply: Done

In this part, we cite James & Robson (2014) and James et al (2017) for some critiques of using Agisoft Photoscan. We also include two debris-covered glacier z0 estimation paper based on Agisoft.

The new paragraph rewrite as "Structure-from-motion photogrammetry is revolutionizing the collection of detailed topographic data (Westoby et al., 2012; James et al., 2017). High resolution DEMs produced from photographs acquired with consumer cameras need careful handling (James and Robson, 2014). In this study, both manual and automatically derived photographs were imported into a software program, Agisoft Photoscan Professional 1.4.0. This software allowed us to estimate camera intrinsic parameters, camera positions, and scene geometry. Agisoft Photoscan Professional is a commercial package which implements all stages of photogrammetric processing (James et al., 2017). It has previously been used to generate three-dimensional point clouds and digital elevation models of debris-covered glaciers (Miles et al., 2017; Quincey et al., 2017; Steiner et al., 2019), ice surfaces and braided meltwater rivers (Javernick et al., 2014; Smith et al., 2016). In our study, we found that after new snowfall, it was difficult to match feature points in the photo sets. Three days of automatic data could not be processed. We estimated z_0 data for the missing days based on data from snowfall days at the automatic site."

Line 149: repetition of reference.

Reply: Done

Line 156: Smith et al (2016) calculated h* from the mean vertical extent above a de- trended 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.

Reply: For manual observation, the aluminum frame laid horizontally over the glacier surface. For automatic observation, the control field was also laid horizontally over the ice surface that lowered as the ice melted, and maintained a horizontally position between control field and ice surface. We have add the detrend method in line 185 as 'For manual photogrammetry, we put the aluminum frame horizontally over the ice surface, the plot is detrended by setting the control points at z axis of the same values. For automatic photogrammetry, the control field of wooden frame was also laid horizontally over the ice surface that lowered as the ice melted and maintained a horizontal position between the control field and ice surface. A DEM based approach enables the roughness frontal area *s* to be calculated directly for each cardinal wind direction (Smith et al., 2016). The combined roughness frontal area was calculated across the plot, the ground area occupied by micro-topographic obstacles is 1m^2. We used a DEM-based average ($\bar{z}_{0\_DEM}$) of four cardinal wind directions to represent overall aerodynamic surface roughness. Based on the half-hour wind direction data at the August-one ice cap, the daily upward wind direction

DEM-based z_{0_DEM} was also estimated at the automatic photogrammetry site. Considering that wind direction changed during the day, in this case we selected the prevailing wind direction to calculate frontal area *s*. The prevailing upwind direction DEM-based z_{0_DEM} was applied to calculate turbulent heat flux. Using the Munro (1989) method, $z_{0\_Profile}$ was calculated for every profile (n=1000) in both orthogonal directions for each plot at the automatic photogrammetry site. '

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

Reply: Done,

In the revised manuscript, we not only apply Munro(1989) method but also calculate the z0 based upward wind direction DEM based z0 to represent the aerodynamic surface roughness, and applied to calculate turbulent heat flux.

We have revised in the manuscript as 'Based on the work of Lettau (1969), Munro (1989) simplified the equation (1) by assuming that h* can equal twice the standard deviation of elevations in the de-trended profile, with the profile's mean elevation set to 0 meter. The aerodynamic roughness length for a given profile then becomes '

"

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.

Reply: Thanks for your recommendation about Fitzpatrick et al (2019) study about microtopographic methods, which have provide EC comparsion with DEM based z0 in multi-season . This paper also give detailed introduction about z_0 estimation from DEM. We have referenced this paper in our study accordingly.

We add a sentence as line 152 as "Fitzpatrick et al. (2019) also developed two methods for the remote estimation of z_0 by utilizing lidar-derived DEM."

Consider the 'surface roughness' is not specific. We have revised the surface roughness as aerodynamic surface roughness in this paper.

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.

Reply: we revised and provide uncertainty in the revised manuscript

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.

Reply: We agree, now the sentence is revised as "The average geo-referencing errors were fluctuate around 1 millimeter"

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

Reply: We have changed accordingly.

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

Reply: Thanks for remind, we delete this sentence accordingly.

Line 237: change 'covered' to 'covering'

Reply: Done

We have revised as' Data for ice surface roughness was collected from the automatic photogrammetry camera site from July 12 to September 15, a period covering the whole melting season.'

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

Reply: We have revised accordingly.

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'.

Reply: We have changed as "Aerodynamic surface roughness"

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

Reply: We have changed accordingly.

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? Addition- ally, in the methods it is mentioned that z0 is an average of all four directional values – were the individual values analysed for directional influence?

Reply: We did want analysis the four cardinal direction z_0 for manual data. But we did not strictly control the aluminum frame at certain direction during our field work at that time. We find at automatic site, at south to north direction z_0 seem larger than north to south direction z_0. We expect it is related with direct short wave radiation. We are not so sure. We need accumulate more field work to prove this.

In the revised manuscript of Figure 5, we have include DEM based four directions Z0 and prevailing wind direction z0. Munro profile method calculated z0 at two directions are also included.

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.

Reply: We totally agree. We have revised as" Ice surface roughness proved to have an interesting relation with altitude"

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

Reply: We have revised 'terminal' as 'terminus'

Line 269: Change to 'At higher altitudes'

Reply: Done

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

Reply: We totally agree, the sentence have revised as" Photogrammetric data collected manually revealed that ice surface roughness increased with altitude (Figure. 6c). From terminal to top, z_0 varied from 0.06 mm to 2.2 mm."

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.

Reply: Agree. We have delete the first sentence and the final two sentences tacked onto the beginning of the next paragraph.

Line 335: changed “account” to “accounted”.

Reply: Done

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.

Reply: we showed r2 in In Figure 9, in line 360, we reported the correlation coefficient (r). In Figure 11a and Figure 11b, we also reported r2, and in line 370 we reported r instead of r2.

In the revised version, we reported r2 instead of r.

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.

Reply: We have revised the discussion part accordingly.

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

Reply: Done

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 mag- nitude. 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).

Reply: we have revised this part and give some explanation why ice surface kept at certain domain during melting season, which is related with net shortwave radiation and turbulent heat flux. The former energy item seem increased z0. But turbulent heat flux seems smooth z0.

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

Reply: we have revised accordingly

Lines 430-433: this is a significant finding; however, there is something about the word- ing 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.

Reply: we have add the profile method and bulk method. Both method shown a similar relationship. A lagged correlation was also applied in the revised manuscript to indicate the relationship between main energy items and z0.

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.

Reply: we have revised accordingly. “the August-one glacier” changed to “the August-one ice cap” across whole manuscript.

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

Reply: We have deleted the last sentence. The revised part has changed as” This study found an exponential relationship between z_0 and L_S. The delicate role of z_0 played in the ice surface balance is still not fully known. Further comparative studies are needed to investigate the z_0 variation through eddy covariance, profile method and DEM-based z_0 estimation.”

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.

Reply: thanks for your suggestions. We have revised accordingly

References cited in comments:

Fitzpatrick, N., Radic´, 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. doi.org/10.5194/tc-13-1051-2019

Irvine-Fynn, T., Sanz-Ablanedo, E., Rutter, N., Smith, M., & Chandler, J. (2014). Mea- suring 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 to- pographic change detection with structureâĂŘfromâĂŘmotion photogrammetry: preci- sion maps for ground control and directly georeferenced surveys. Earth Surface Pro- cesses 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. Per- mafrost 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. https://doi.org/10.3189/S0022143000002197

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.

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Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-186, 2019.