Reply to review comments by Evan Miles

September 20, 2021

The study by Dachauer et al utilizes UAV-derived digital elevation models to estimate the surface roughness (specifically, the aerodynamic roughness length) for the heavily-crevassed terminus area of four glaciers in Svalbard. This is an interesting and practical application of UAV data to a domain where field measurements are dangerous, if not impossible, so few estimates and no direct measurements of roughness are yet available for heavily crevassed ice. The authors utilize five contending approaches to estimating z_0 *from DEMS, and show that all approaches represent similar spatial variability across the study domains, and that some approaches show little scale dependence when an appropriate grid scale is used. For all methods,* z_0 *values in crevassed zones are considerably higher than values typically used for glacier ice, as expected. Overall this is a nice study, demonstrating the approximate range of roughness values to consider for these hard-to-reach parts of glaciers, but its impact may suffer from the difficulty in constraining or validating the models. Still, with some additional analyses to understand the importance of uncertainty in modelled* z_0*, the manuscript will be a nice contribution to the literature. I have a number of relatively minor suggestions for the authors to consider in preparing a revision.*

Reply: The authors are thankful to the referee for the precise and thoughtful comments. In the following text the comments of the referee, are written in *italic* and the line numbers refer to the original review version of the manuscript, if not specifically mentioned otherwise.

Main comments

1. *The fact that no ground control was used in the study should be clearly mentioned before the discussion. Certainly it would be difficult to constrain the glacierized portion of the DEMs, but some of the exposed bedrock around the glaciers' lateral margins could have reduced the positional errors. In addition, many drones now have RTK-GPS modules that are perfectly suited to the no-GCP application, and can achieve centimetre-accuracy (see the Chudley et al, paper; others are also available). I also think that the GPS accuracy values from the manufacturer are probably not reliable, at least in the Z direction (however the UAV also uses pressure sensors that may improve the relative altitude precision). At first glance this seemed to be a major limitation for the study, as poor georeferencing control can lead to DEM warping, as well as shifts. However, I actually think this should be a minor problem for your application, which focuses on the relative differences in surface topography, in part due to the detrending you have applied. In short, in the methods I would suggest that you acknowledge the challenges of establishing precise GCP controls for this type of situation, as well as that newer platforms mitigate these issues, but assert that this will not be a problem for your specific application*
   Reply: The authors agree with the comment of the referee. The lack of GCP control points as well as its minor impact on the results is now mentioned in the methods part (see L91 of updated manuscript). Additionally, alternative platforms such as RTK-corrections (Chudley et al., 2019) are also mentioned: "Due to an inaccessible glacier surface, no ground control points (GCPs) could be placed on the mapped area for georeferencing. No alternative georeferencing platforms such as real-time-kinematic correction (Chudley et al., 2019) were available. While this is a recommended procedure for future application of this technique, we point out that computation of z_0 requires quantification of relative topographic differences and so the impact of this shortcoming is minor".

2. *The authors have reoriented their DEMs to the dominant direction of glacier flow (at the terminus) to analyse roughness in the down-glacier and cross-glacier directions. I would encourage the authors to consider multi-directional wind patterns more carefully in the discussion. You might reframe some of the anisotropy discussion to consider that the down-and cross-glacier roughness estimates provide end-members for the temporally variable roughness experienced at a site on these glaciers.*
Reply: To address the mentioned concern, some parts of the anisotropy discussion were rephrased. In more detail, the strong focus on down-glacier wind direction was lifted by considering the study of Esau and Repina (2012). Additionally, the temporal variability of z_0 due to changing wind directions was addressed in the discussion: "However, Esau and Repina (2012) found the katabatically forced wind systems to be of less significance for tidewater glaciers, highlighting the influence of wind direction on effective z_0 values. Furthermore, the wind direction dependency indicates the temporal variability of the aerodynamic roughness length due to changing wind directions from daily up to seasonal time scales.".

3. *A key limitation for this study (and the possible advantage to using UAVs) is the inability to validate the estimates, which are well correlated to one another but differ in magnitude. This has been a problem for other similar efforts, even with local measurements (e.g. Miles et al, 2017), although there is promise to reduce the scale dependence (e.g. Chambers et al, 2021). For your application, the question is –how believable are the heavily-crevassed-area roughness values? This is very difficult to pin down, but I think the five methods tested provide constraint to within an order of magnitude. Maybe you can evaluate your estimates for the smoother area of some glaciers to refine this range further, but I think an important question is also –how precise do models need z0 to be prescribed? I think this is an important discussion topic for the manuscript, since you cannot constrain the values precisely; how different might turbulent fluxes be in crevassed areas considering the range of values? Is an unconstrained estimate already 'good enough' and not likely to change the results, or do we need to determine the accuracy precisely?*
Reply: The authors agree with the referee and decided to include the discussion of the required z_0 estimation accuracy for models into the manuscript. Therefore, the following text was added in the discussion: "The validation of the model estimates remains a big challenge due to the lack of reference values. Thus, it should be questioned whether the modelled z_0 range of about an order of magnitude is accurate enough for energy balance models. In general, an increase of z_0 by one order of magnitude will more than double the value of turbulent fluxes (Brock et al., 2000), justifying the need to consider the larger z_0 values on heavily crevassed glacier areas. Additionally, the relevance of further narrowing down the model estimates range in the future depends a lot on the field of application. For large-scale, satellite-based investigations an average value between all models (e.g. 0.1 m) for heavily crevassed glacier areas might be a sufficiently accurate approximation. However, small-scale investigations on individual glaciers likely benefit from more accurate z_0 estimates. It is here in particular, where our study shows that UAVs are the ideal platform for investigating aerodynamic roughness length". Additionally, the z_0 values for smooth ice was elaborated on the grid size justification section (see minor comments 26. L279-280).

4. *Related to the above, a very nice possible outcome would be to consider a reduced-complexity parameterization of roughness for crevassed areas. This could be related to crevasse spacing (or possibly depth) or a damage factor. I suggest this because 1) crevasses can be readily mapped using high-resolution satellite imagery (Pleiades or even PlanetScope), and 2) models of glacier dynamics are inceasingly able to resolve complex stress and strain patterns near glacier termini, and this could enable an improved link to surface energy balance. It is also worth noting that unlike most glacier surfaces, the roughness of a heavily crevassed glacier terminus is not likely to see much seasonal change, as the dominant roughness elements are unaffected by snow/ice melt processes.*
Reply: As stated on item 3. above and on item 18. (L202), the combination of satellite-based data and small-scale DEMs for reference reveals a great potential for the extrapolation

of z_0 estimations. However, this topic will not be discussed in more depth in the scope of this study. Therefore, the following sentence was added to the manuscript outlook: "In a next step, a combination of high-resolution DEMs from UAVs for reference z_0 values and satellite-based crevasse density estimates might approve valuable for future research". The authors further agree on the weaker seasonal change of heavily crevassed glaciers and addressed this issue in a way that they removed the attempt to explain some inter-annual variances by remaining snow-bridges (see minor comments item 20. L214).

Minor comments

1. *L7. 'best accounts' –as formulated, this sentence is a bit misleading, as you are not demonstrating that the moving-window approach is worse than the sub-grid approach (the strict interpretation of the sentence as written). Rather, your results indicate that 50m is a suitable distance for detrending, since the scale dependence for most approaches breaks down above that distance.*
   Reply: The authors agree with the referee. However, also for the moving-window approach, in the end only one z_0 value per 50 m x 50 m grid was provided (due to averaging the moving-window values within the grid). For clarification, the word 'best' was removed.

2. *L16. Suggest 'balance' to'lead to'*
   Reply: Suggestion implemented.

3. *L21. I'd recommend reformulating this sentence. z_0 is certainly not a constant surface characteristic. At the very least it changes considerably in time (e.g. Brock et al, 2006; Smeets studies), but of course turbulence is not only affected by the surface itself, but the wind speed and direction.*
   Reply: The word constant was removed and the following sentence was added for clarification: "It is a surface characteristics and therefore independent of meteorological quantities (Lettau, 1969)".

4. *L37. I don't recall Quincey et al (2017) looking at crevasses?*
   Reply: The authors agree since Quincey et al (2017) mentioned the same issue for debris on the glacier surface, which is comparable in this matter. Nevertheless, the sentence was rephrased for clarification: "A broader-scale, heterogeneous surface topography of obstacles (e.g. crevasses) makes the definition of z_0 values challenging (Quincey et al., 2017)".

5. *L88. Please add some details of the final configuration here.*
   Reply: The authors understand the urge for more details here. However, we deliberately decided against adding more details in order to keep the focus on the main outcome of the manuscript and especially since the settings were discussed intensively on the work of Dachauer (2020), which is cited in the manuscript.

6. *L89. The lack of ground control points should be addressed directly here.*
   Reply: The authors agree with the referee and added the following sentence: "Due to the inaccessible glacier surface, no ground control points (GCPs) could be placed on the mapped area for georeferencing."

7. *Figure 3. Could you add depiction of the moving-window formulation (e.g. Fitzpatrick et al, 2019)? The detrending approach is indeed quite crucial for obstacle definition.*
   Reply: As described on L144, the moving-window approach is based on the work of Fitzpatrick et al. (2019). The authors decided not to explain the models in full detail since they can be looked up in the original and referenced papers. Yet, for clarification the following sentence was added: "In the raster method all sub-grids were detrended row-wise and areas below the detrended plane were neglected, assuming that they would be effectively sheltered.".

8. *L113. 'all wind directions' –by this you mean the four directions of the coordinate system defined in Fig 2b, and not every 15 degree increment, for example*
   Reply: Exactly. For clarification, the word "four" was added.

9. *L120. 'a lot' appears in the text 'a lot'. Please consider a less colloquial formulation*
   Reply: 'a lot' was replaced by 'substantially'.

10. *L127. For consistency with prior descriptions, please indicate 'up-crossing' somehow here.*
    Reply: Since we did not use this term in our study, we simply added the notation "(often referred to as 'zero-up-crossing' in literature)".

11. *L132.This adaptation need to be established a bit more carefully. The use of cross-profile (instead of along-profile) obstacles dates back to Lettau (1969) if not before. The rationale is that if the bumps resolved in this matter equate to the silhouette area facing the wind. This is true when surfaces do not have a clear grain, in which case you get channelized flow rather than turbulence. Note that Munro also found a 4x difference based on transect direction for ablating ice that showed a clear grain (similar to your own magnitude of differences). I actually agree with this profile rotation considering the strong grain of the surface (since you have also considering skimming flow!), but some additional justification is needed in the text.*
    Reply: The authors agree with the referee and added the following sentences to further justify the adaptation: "This is because if a crevasse is aligned perpendicular to the prevailing wind direction, a wind-perpendicular transect is not able to detect the crevasse yielding to a relatively low z_0 value (for further explanation see Smith et al. (2016)). Such an adaptation is essential for heterogeneous and naturally streamlined roughness elements as those investigated in this study, since wind systems are influenced by the large-scale catchment topography and therefore often flow up or down the glacier (Quincey et al., 2017)". Furthermore, we referenced to Smith et al. (2016), where this adaptation is discussed intensively.

12. *L137. The precise implementation of the 'transect' approaches is not clear. Do you determine a z_0 value for each transect, then combine them (and how)? Or do you accumulate obstacles from all transects (as in Miles et al (2017))?*
    Reply: We followed an approach where for each row/transect a z_0 value was determined which then were averaged to one single z_0 values per sub-grid. For clarification, the following sentence was added: "Thus, the final z_0 value for each sub-grid was then calculated by averaging the individual transect z_0 values within the sub-grid.".

13. *L180. Technically, these are not 'cardinal' wind directions.*
    Reply: We agree that the term 'cardinal' might be misleading since the DEMs were rotated. Therefore, we decided to delete the term since the four wind directions were properly introduced in section 2.3.

14. *L194-5. Which are the 'both parameters'? Not clear.*
    Reply: For clarification, the sentence was rephrased: "Locally however, both mean and median z_0 estimates can vary about one order of magnitude with changing wind direction."

15. *Figure 5. Please increase the font size for all axes and the colorbar.*
    Reply: Suggestion implemented.

16. *Figure 6. Please use a log scale for the y-axis.*
    Reply: Suggestion implemented.

17. *Figure 7. Please increase the font size for all axes and the colorbar.*
    Reply: Suggestion implemented.

18. *L202. The similarity of values between glaciers (for the crevassed areas) raises an interesting question –can you parameterize z_0 based on crevasse density directly? If so, this would be a promising avenue to estimate z_0 (at least for the crevassed areas) without needing high-resolution DEMs.*
    Reply: The authors appreciate the interesting input which could bring this field of research one step forward. However, we would argue that this approach would only be able to end up as a rough approximation since crevasse depth would have to be guessed from other/similar

glaciers. This is an important shortcoming because the obstacle height is the most important control parameter over the output of z_0 (Nield et al, 2013). Nevertheless, a combination of small-scale, high-resolution DEMs for reference values with large-scale, satellite-based crevasse density estimates might contain a valuable potential for future research and therefore was mentionend in the outlook part of the manuscript as follows: "In a next step, a combination of high-resolution DEMs from UAVs for reference z_0 values and satellite-based crevasse density estimates might contain a valuable potential for future research."

19. *L212. It's nice to be able to compare two different years. This similarity is also not so surprising since the roughness elements (crevasses) are probably not as transient as for other glacier surfaces.*
Reply: The UAV images and DEMs show that the roughness elements slightly change location and shape but after all we agree that the roughness elements are not as transient as for other glacier surfaces, which justifies the way this topic was mentioned in the study by not further elaborating the reasons for the inter-annual z_0 deviations.

20. *L214. This sentence about the decrease does not make sense –do you think the 10% reduction is meaningful? I would be very sceptical.*
Reply: The authors agree with the referee and deleted the according sentence because the deviation is so small.

21. *Figure 8. Please eliminate the duplicate colorbar.*
Reply: Suggestion implemented.

22. *Figure 9. I would again recommend plotting this with a logarithmic y-axis. Please also annotate the median obstacle sizes determined for each site (then the 50m grid size used).*
Reply: The authors acknowledge the point of the logarithmic y-axis. However, after re-evaluation we decided to stick to the non-logarithmic y-axis since both the model differences and the grid size dependence of z_0 values can be highlighted more effectively this way. The 50 m grid size line was included in the graph and the average obstacle sizes were annotated in the caption.

23. *L241. The nadir views at interval probably do not resolve topography very far into crevasses.*
Reply: The authors agree and that's exactly what is meant with the sentence "... the lack of reflected light from the deep crevasses."

24. *L252. I believe you are arguing that you need a DEM with high precision (rather than high accuracy). I would agree (for a microtopographic z_0 calculation) as measuring the local features is more crucial than the elevation of those features. The manufacturer hover accuracy estimates are not terribly relevant for your purpose, though–I would suggest that this is the best-case accuracy. Without GCPs, DEM warping can be particularly problematic.*
Reply: We agree with the referee and added the following phrase for clarification: "[...] rather a precise DEM combined with a detrending approach for the investigation of the effect of relative distances". As for the lack of GCPs, we refer to the discussion on item 1. of main comments.

25. *L256–L262. It is not clear how you measure % distortion in this context. % of what?*
Reply: For clarification, the following phrase was added: "1.7 % (horizontal length deviation in % of DEM compared to the Sentinel-2 satellite image)"

26. *L279-L280. This is a nice theoretical justification of the choice of grid size, but 50m is still quite arbitrary, looking at Figure 9, which does not show a kink but a smooth progression that might just be an artefact of the logarithmic scale of z_0 variability. I think you could provide better evaluation of this choice of grid size–for example, what range of z_0 values does this give for the smoother domains, and how does that correspond to expected values? If in fact the transition from grain to form roughness occurs at 30m (or 70m) how much are the distributed roughness estimates changed?*
Reply: The estimated z_0 values for each grid size has indeed been considered for the choice of the 50 m grid size. For clarification, the following text was added: "Typical z_0 estimates for smooth glacier ice, which for instance can be found on the upper part of Fridtjovbreen, have a

length of about 1 mm (Brock et al., 2006). The choice of a 50 m grid size can be further justified since Figure 8 shows that grid sizes below 30 m do not provide high enough values to agree with literature values.". As already discussed on Figure 9, the grid sizes above 30 m estimate similar z_0 values. Therefore, in this range the consideration of average obstacle sizes was important for the final choice of 50 m.

27. *L298-299. Without a doubt, katabatic winds are a predominant wind direction for mountain glaciers, and are also important for tidewater glaciers, but external forcing also plays a role, especially for the latter group. The key drivers leading to their formation (altitudinal/temperature gradients, combined with topographic chanelling) are weaker at the polar tidewater sites studied here. There are some interesting results related to this from e.g. Esau and Repina (2012). Multidirectional wind speed can play an important role in temporal variations in z_0, and although you don't need to go so far as to consider turbulence footprints here (Steiner et al, 2018; Nicholson and Stiperski, 2020), I think it is useful to note that the cross-and down-glacier roughness elements both influence the effective z_0 at a site.*
Reply: The authors appreciate the input and slightly adjusted the original text by adding the reference of Esau and Repina (2012) as follows: "However, Esau and Repina (2012) found the katabatically forced wind systems to be of less significance for tidewater glaciers, highlighting the influence of wind direction on effective z_0 values.". Furthermore, the last sentence of the original paragraph was removed.

28. *L305. The validation is a challenge for this study. I wonder if you can consider real-world analogues to crevassed areas (outside of glaciology) that might have been investigated previously. Lettau (1969) included a variety of other surfaces (even including urban areas) that could be relevant reference points to check the order of magnitude.*
Reply: The authors agree with the referee and compared the z_0 estimates with values measured above villages. Thus, the following sentence was added to the manuscript: "Outside the field of glaciology, the heavily crevassed glaciers might most effectively be compared with villages, since buildings have similar obstacle density and height. According z_0 values are about 0.2-0.4 m which lies within the range of estimated roughness values in this study."

29. *L397. I'd recommend making your DEMs and code publicly archived.*
Reply: We are planning to publish the DEMs in a separate publication later this year. The code will be available on the following link: `https://github.com/ArminDach/z0_UAVs`

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