Review of ‘Aerodynamic roughness length…’ submitted by Dachauer et al to *The Cryosphere*

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.

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
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.
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?

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.

Minor comments.

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.

L16. Suggest ‘balance’ to ‘lead to’

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.

L37. I don’t recall Quincey et al (2017) looking at crevasses?

L88. Please add some details of the final configuration here.

L89. The lack of ground control points should be addressed directly here.

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.

L113. ‘all wind directions’ – by this you mean the four directions of the coordinate system defined in Fig 2b, and not every 15degree increment, for example

L120. ‘a lot’ appears in the text ‘a lot’. Please consider a less colloquial formulation

L127. For consistency with prior descriptions, please indicate ‘up-crossing’ somehow here.

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.

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))?

L180. Technically, these are not ‘cardinal’ wind directions.

L194-5. Which are the ‘both parameters’? Not clear.

Figure 5. Please increase the font size for all axes and the colorbar.

Figure 6. Please use a log scale for the y-axis.

Figure 7. Please increase the font size for all axes and the colorbar.

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.

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.

L214. This sentence about the decrease does not make sense – do you think the 10% reduction is meaningful? I would be very sceptical.

Figure 8. Please eliminate the duplicate colorbar

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).

L241. The nadir views at interval probably do not not resolve topography very far into crevasses.

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.

L256 – L262. It is not clear how you measure % distortion in this context. % of what?

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?

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.

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.

L397. I'd recommend making your DEMs and code publicly archived.

Brock, B. W., Willis, I. C., & Sharp, M. J. (2006). Measurement and parameterization of aerodynamic roughness length variations at Haut Glacier d'Arolla, Switzerland. *Journal of Glaciology*, 52(177), 281–297. https://doi.org/10.3189/172756506781828746

Chambers, J. R., Smith, M. W., Smith, T., Sailer, R., Quincey, D. J., Carrivick, J. L., … James, M. R. (2021). Correcting for Systematic Underestimation of Topographic Glacier Aerodynamic Roughness Values From Hintereisferner, Austria. *Frontiers in Earth Science*, 9(May), 1–16. https://doi.org/10.3389/feart.2021.691195

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, 1051–1071. https://doi.org/10.5194/tc-13-1051-2019

Igor Esau, Irina Repina, "Wind Climate in Kongsfjorden, Svalbard, and Attribution of Leading Wind Driving Mechanisms through Turbulence-Resolving Simulations", *Advances in Meteorology*, vol. 2012, Article ID 568454, 16 pages, 2012. https://doi.org/10.1155/2012/568454

Lettau, H. H. (1969). Note on Aerodynamic Roughness-Parameter Estimation on the Basis of Roughness-Element Description.pdf. *Journal of Applied Meteorology*, 8, 828–832. https://doi.org/10.1175/1520-0450(1969)008%3C0828:NOARPE%3E2.0.CO;2

Miles, E. S., Steiner, J. F., & Brun, F. (2017). Highly variable aerodynamic roughness length (z 0) for a hummocky debris-covered glacier. *Journal of Geophysical Research: Atmospheres*. https://doi.org/10.1002/2017JD026510

Nicholson, L., & Stiperski, I. (2020). Comparison of turbulent structures and energy fluxes over exposed and debris-covered glacier ice. *Journal of Glaciology*, 66(258), 543–555. https://doi.org/https://doi.org/10.1017/jog.2020.23

Quincey, D. J., Smith, M. W., Rounce, D. R., Ross, A., King, O., & Watson, C. S. (2017). Evaluating morphological estimates of the aerodynamic roughness of debris covered glacier ice. *Earth Surface Processes and Landforms*, 42, 2541–2553. https://doi.org/10.1002/esp.4198

Smeets, C. J. P. P., Duynkerke, P. G., & Vugts, H. F. (1999). Observed wind profiles and turbulent fluxes over an ice surface with changing surface roughness. *Boundary-Layer Meteorology*, 92(1), 99–121. https://doi.org/10.1023/A:1001899015849

Smeets, C. J. P. P., & Broeke, M. R. (2008). Temporal and spatial variations of the aerodynamic roughness length in the ablation zone of the greenland ice sheet. *Boundary-Layer Meteorology*, 128(3), 315–338. https://doi.org/10.1007/s10546-008-9291-0

Steiner, J. F., Litt, M., Stigter, E. E., Shea, J., Bierkens, M. F. P., & Immerzeel, W. W. (2018). The Importance of Turbulent Fluxes in the Surface Energy Balance of a Debris-Covered Glacier in the Himalayas. *Frontiers in Earth Science*, 6(October), 1–18. https://doi.org/10.3389/feart.2018.00144