Reply to review comments by Maurice Van Tiggelen

September 20, 2021

General comments

This preprint addresses the important issue of the unknown aerodynamic roughness of inaccessible, heavily crevassed, tidewater glaciers. In this preprint, UAVs are used to map at high-resolution the elevation of the terminus of four tidewater glaciers in Svalbard. Five different methods, all based on the semi-empirical equation from Lettau (1969), are then used to map the aerodynamic roughness length for momentum z_0*, thereby quantifying the large spatial variability of* z_0 *over these glaciers. Different sensitivity experiments are done, which confirm how much the modelled* z_0 *depends on the chosen elevation grid, but also on the wind direction. This is well timed research, as atmospheric models have increasingly higher resolution and start resolving smaller parts of a glacier or ice sheet, including the very rough terminus of marine terminating glaciers. This research is also relevant, as the very rough nature of these surfaces is expected to increase turbulent heat fluxes and therefore runoff compared to smoother surfaces. The novelty in this preprint lies in the fact that multiple existing methods are compared over four new areas. Overall, this preprint is well written and follows a clear and logical structure. Furthermore, the UAV digital elevation models (DEMs) are of high quality. The results section is interesting and the discussion addresses many uncertainties in this field. Nevertheless, the preprint can be improved further by clarifying several statements. Moreover, an important shortcoming of this work resides in the choice of the drag model to estimate* z_0 *from the measured DEMs. The associated (potentially large) errors should be addressed in more detail, as there is no in-situ data to compare the model with. Finally, some parts of the discussion could be removed and/or shortened to make it more comprehensible. Therefore, I recommend publication after revision.*

Reply: The authors highly appreciate the constructive and thoughtful comments. In the text following below the referee's comments are written in *italic* and the line numbers refer to the original review version of the manuscript, if not specifically mentioned otherwise.

Specific comments

While the choice of using UAV DEMs with a grid size of 50m and an input resolution of 25cm/pixel is motivated, the choice of the semi-empirical equation by Lettau (1969) to estimate z_0 *is not clearly motivated. This equation relies on important assumptions, such as the absence of a displacement height and of a roughness sublayer, and the neglect of inter-obstacle sheltering. There is no reason to believe that these simplifications hold for such a complex, urban-like, surface. Besides, there is no mention of the typical turbulent flux fetch footprint, or about the value for the drag coefficient* $c_d = 0.5$*, which are both known to greatly influence the modelled* z_0*.*
It may be argued that it is outside of the scope of this research to improve this model, yet it is important to know why the Lettau (1969) model was used over more recent models. One of these models is also mentioned in the preprint (Macdonald et al, 1998). This is even more relevant due to the fact that the performance cannot be assessed with in situ data, such as wind profile of turbulent flux measurements.

Reply: The authors agree with the referee that the Lettau (1969) model is based on many assumptions which not necessarily hold for complex surfaces, such as highly crevassed tidewater glaciers. However, we would argue that the choice of the five models is exactly motivated to face this shortcoming. While the two transect models are based on many assumptions (namely roughness elements are of equal height, uniformly distributed, isotropic and not affected by any sheltering), the three raster method models do not rely on these assumptions since the frontal area s can be calculated directly (Smith et al., 2016). Therefore, the choice of the five models can be justified in a way that they highlight the influence of these assumptions and further show the impact of other parameters such as h^* on the z_0 estimates. For clarification, this justification was added to the methods part of the manuscript. Additionally, the errors/shortcomings associated to each model and variable definition was further elaborated in the discussion of the manuscript. The ground area S which corresponds to the chosen grid size and the according profile length is a simple approximation to the real fetch footprint. The justification of the grid size was discussed extensively from L97 onwards of the updated manuscript version. However, the simplification allows us to estimate z_0 values for the four wind directions and additionally provides uniform parameterization throughout all glaciers and models (this justification was added to the paper discussion). We find the proposed concerns coming along with the choice of the drag coefficient to be reasonable. However, for this study we decided to stick to the in glaciology commonly employed drag coefficient definition of c_d=0.5 and to discuss the shortcoming of this assumption in the discussion part of the study.

Other comments:

1. L16: *replace "this heat exchange" by "the radiative heat fluxes", at least if this is what is meant here.*
   Reply: The sentence was rephrased as follows: "Both sensible and latent heat fluxes balance lead to this heat exchange on the surface and therefore have a large impact on the meltwater production and the surface energy balance of glaciers."

2. L21: *the statement "it is a constant surface characteristic" is not clear, and seems to contradict the main conclusion of the preprint. Do the authors refer to the fact that the aerodynamic roughness length is often taken as a constant in atmospheric models? Or that it does not depend on meteorological quantities?*
   Reply: We refer to the fact that z_0 is independent of any meteorological quantities. An according sentence was added: "It is a surface characteristics and therefore independent of meteorological quantities".

3. L23: *Please rephrase "following the bulk approach". In its current form this statement may be confused with the bulk approach to estimate turbulent fluxes. I believe the authors refer to a different bulk approach.*
   Reply: The authors refer to the bulk approach to estimate turbulent fluxes. For clarification, L24 was adjusted to: "The bulk approach for the calculation of those turbulent fluxes is very popular...". Additionally, "following the bulk approach" was changed into "have determined z_0 values of glacier surfaces based on the bulk approach".

4. L27 (& L3): *Consider using "uncrewed" instead of "unmanned". Or remotely piloted aircraft system (RPAS).*
   Reply: The authors very much acknowledge the point of that comment and the importance behind it. However, since the term 'unmanned' is the official technical term used for such vehicles (e.g. in regulations), we decided to use it. Nevertheless, we added a sentence defining other used terms: "often also called uncrewed vehicle systems or drones" to acknowledge the issue.

5. L28: *Please clarify how UAV overcomes the spatial coverage limitation of LiDAR. Aren't UAVs also limited in the area they cover?*
   Reply: For clarification, the following sentence was added: "... since they are more flexible in their use and less limited by local topology as they provide a bird's-eye perspective."

6. L35: *Consider referring to the recent work by Van Tiggelen et al (CryosphDiscuss 2021, https://doi.org/10.5194/tc-2020-378). They give estimated z0 values for very rough ice & crevassed areas in west Greenland in their Figures 9 & 10.*

Reply: The 'typical values range for rough glacier ice' was adjusted and the suggested reference added. Furthermore, the study was mentioned in the opening sentence of that paragraph describing existing research.

7. L37: *Consider rephrasing "makes it hard".*
   Reply: "makes it hard to define" changed to "makes the definition of z0 values challenging"

8. L87: *The thesis of A. Dachauer could not be found online. Consider adding a public link with DOI to this reference.*
   Reply: A DOI of the thesis does not exist, but a link to the pdf file was added to the reference.

9. L90: *It is already assumed here that the mean wind direction coincides with the mean glacier slope, while this is only explicitly written at L95. Consider moving L95 before L90 for clarity.*
   Reply: The sentence was rephrased as follows to acknowledge the mentioned issue: "This allowed the estimation of z_0 values for the following four wind directions: down-glacier, up-glacier and cross-glacier from both sides"

10. L96: *Please explain why assuming that the mean airflow is parallel to the slope means that the aerodynamic roughness is less influenced by the small-scale roughness features.*
    Reply: The sentence was rephrased for clarification (see comment 11./L99). Additionally "means" was replaced by "justifies the assumption"

11. L99: *Please clarify what is meant by "since small-scale roughness elements do not represent the real topographic expression".*
    Reply: The following two sentences were added/rephrased to clarify the topic: "In other words, only looking at the small-scale surface roughness elements would lead to the wrong roughness parameterization, since they might be located on the inner side of a large-scale roughness obstacle not exposed to the whole mean airflow. Accordingly, the chosen grid size must be large enough to include the macro-structure of the surface, since small-scale roughness elements do not represent the real topographic expression."

12. L112: *"[...] all four wind directions [...]".*
    Reply: suggestion implemented.

13. L117: *Is the value for $c_d = 0.5$ from Lettau (1969) realistic for crevasses?*
    Reply: This is indeed a broadly discussed issue and potentially leading to an impact on the z_0 results. However, we decided to stick to that widely adopted definition and added the following sentence on L356 of the updated manuscript version in the discussion to draw attention to this assumption: "Furthermore, the widely adopted drag coefficient of Lettau (1969) $c_d = 0.5$ corresponds to an average form drag effect on roughness elements. Its rationale is widely discussed since it does not necessarily hold for heterogeneous locations (Quincey et al., 2017)."

14. L119: *Please rephrase "turns out". What do the authors exactly mean here?*
    Reply: "turns out" changed to "is". Additionally, the sentence was rephrased to make it clearer: "roughness elements are non-uniform" was added.

15. L127: *Are the statistics immediately calculated on the transects taken from the detrended sub-grid? Or are all the individual transects detrended once again?*
    Reply: The detrending happened row-wise (e.g. for each individual transect) at the first place. The sentence was rephrased for clarification: from "Each row of the detrended sub-grid was treated..." to "Each row of the sub-grid was detrended and treated... "

16. L134-136: *I would argue that these statements are true only if the Lettau(1969) formula is used. More sophisticated models can be applied to a detrended profile that do take into account sheltering and obstacles of different spacing or height.*
    Reply: All models are based on the Lettau (1969) formula, but the raster methods were modified in a way that they take into account sheltering and different obstacle sizes (see reply to comment 42.)

17. L144: *Which parameters are calculated row-wise in the first two raster models, besides? Possibly refer to Table 3 for clarity.*
Reply: As already stated on L148, s and h^* are calculated row-wise. A reference to Table 3 was added.

18. L157 (and L6) : *It is not clear here whether z0 varies by three or by four orders of magnitude. Please specify what is meant by "up to three (locally even four)".*
Reply: For one model the range of z_0 values is up to three orders of magnitude. Between different models this can of course add another order of magnitude. The authors agree that this creates confusion and therefore decided to remove the phrase "(locally even four)".

19. L158: *"The highest values": what are these values?*
Reply: "The highest values" corresponds to the larger values (dm-m scale) in the mentioned range of z_0 values. The following sentences were rephrased for clarification: "The largest z_0 values (decimeter to meter scale) were found close to the glacier front where crevasses are big and steep using the transect methods and for winds blowing parallel to the flow direction of the glacier. The lowest values (milimeter to centimeter scale) were estimated with the raster methods for smooth, crevasse-free ice and for cross-glacier wind directions."

20. L167: *do the authors mean averaged over all four cardinal wind directions? Or has the data been rotated over all 360 degrees?*
Reply: The authors mean averaged over all four cardinal wind directions. For clarification the sentence was rephrased to: "over all four wind directions".

21. L171-172: *refer to Table 4.*
Reply: Suggestion implemented.

22. L178: *At this point it might be useful (for future studies) to give a (short) interpretation on why the transect method yields a significantly larger z0 than the raster method. Also see comment below about L322.*
Reply: suggestion implemented.

23. L186: *This is hard to see on Fig5 with the given color scale. Consider changing the colormap or adding annotations in Fig5.*
Reply: For clarification, the text was rephrased as follows: "However, Fig. 5 illustrates that larger roughness elements, which can be found close to the glacier front, for instance, present a stronger wind dependency because they vary more strongly with changing wind directions (from dm to m scale) compared to areas that are less crevassed like on the upper part of Fridtjovbreen (similar z_0 values in mm scale for all wind directions)."

24. L190: *Are the results shown in Fig5 different for wind blowing from the right than for wind blowing from the left? If so, it would be useful to explain why. If not, consider removing panels c) and d).*
Reply: They are different but very similar. To clarify, the following sentence was added: "The two cross-glacier (up- and down-glacier, respectively) wind directions lead to very similar z_0 values since they are both calculated on the same transect but from opposing wind directions."

25. Fig6: *Please add to the caption what is denoted by the vertical extent of the boxplots (standard deviation, or quantiles?). Consider using algorithmic y axis, as all the means/medians seem clustered near y = 0m.*
Reply: The following sentence was added to the caption: "Whiskers are visualizing the variability outside the upper and lower quartiles up to 1.5 times the interquartile range.". Furthermore, the y-axis was changed to logarithmic.

26. L201: *Consider rephrasing "all the observed patterns recognized in the investigations of this study" by "all the patterns found in this study".*
Reply: Suggestion implemented.

27. L207: *Please clarify here that the resampling is only done in the following part. Otherwise the very different values for 2019 and 2020 in Table 4 do not make sense.*
Reply: To clarify this issue, the following sentence was added: "Furthermore, for this particular comparison [...]".

28. L213: *The two sentences at L213-215 could be simplified in something like: "Although the deviations in z0 are small, the lower values found in 2020 could be related to the fact ...".*
Reply: In consultation of the second referee's comment the authors decided to remove the whole sentence since the deviations were too small to give reason for a meaningful explanation.

29. L215: *Would it be possible to check this in the true-color UAV images? If so, this statement would be a very interesting example of how z0 can rapidly change in time as well.*
Reply: Potential snow-bridges could not be determined in the true-color UAV images of both years since it's hard to distinguish ice and snow especially in the deep crevasses. Nevertheless, there might still be some remaining snow-bridges deep in the crevasses. However, since the deviations are so small and the impact of snow-bridges deep inside the crevasses might be minor we decided to not further question the rationale of a 10 % deviation.

30. L245: *A link could be made here with the assumptions of the Lettau (1969) model that does not account for the displacement height (or penetration depth). Underestimating crevasse depth using UAV could have a compensating effect on the modelled z0.*
Reply: The following text was added to address this issue: "Equation 1 of Lettau (1969) and the transect methods are not defining any penetration depth limit. The raster method however, assumes that effective roughness only depends on the roughness elements above the detrended plane level which indicates how far the effective turbulent mixing advances into the crevasses."

31. L274: *consider removing "the theory of".*
Reply: Suggestion implemented.

32. L278: *A grain roughness of 50 m is counter-intuitive. Does form drag not occur at scales smaller than 50 m?*
Reply: For clarification, the following sentence was added/rephrased: "Accordingly, given the large roughness elements investigated in our study, the 'grain' roughness is assumed to belong to the texture on the crevasses. "

33. L280: *What is a "considerable spatial resolution "? Consider rephrasing in something like "50 m was chosen as it is the highest resolution that still includes the size of an average obstacle".*
Reply: The sentence was rephrased to: "50 m was chosen, since it is the smallest resolution possible to still include the size of an average obstacle."

34. L282: *consider renaming section "Model outputs of aerodynamic roughness length estimation" to "Estimated aerodynamic roughness length".*
Reply: Suggestion implemented.

35. Figure 10: *How do the authors know that parallel winds are more likely to occur or not than perpendicular winds? Could it be that there is some confusion here in the interpretation of Ω?*
Reply: Several studies (e.g. Fitzpatrick et al., 2019; Karner et al., 2013) found the winds to be more likely to blow parallel, due to katabatically forced down-slope winds. However, this is not related directly to the results of Figure 10. Figure 10 only compares the z_0 values of parallel vs. perpendicular flowing winds showing that z_0 values are strongly anisotropic and parallel winds are dominant.

36. L332: *Please explain (in the methods) how the raster methods take into account sheltering. Around L150 would be a good place.*
Reply: In the methods part (line 141) the following sentence was added: "In the raster method all areas below the detrended plane were neglected, assuming that they would be effectively sheltered."

37. L322-L332: *these statements mostly repeat previous statements, so they could be removed. Subsection 4.3.1 could then be removed if lines L332-335 are added after L178.*
Reply: suggestion implemented: L332-335 were added after L178 and L322-L332 were deleted. Additionally, the following sentence: "This indicates that the z_0 values are correlated, but does not provide any conclusion about the quality of the individual models." was added after L169.

38. L343: *While it is true that Macdonald et al (1998) state that inter-obstacle sheltering becomes important at roughness densities above 20-30 %, they also show that the displacement height is non-negligible at roughness densities below 20 % (see their Fig.4). The latter is not taken into account in Lettau (1969).*
Reply: The authors agree that Lettau (1969) and accordingly the two transect method models, are not taking into account the displacement height. However, the three raster method models are assuming the displacement height to correspond with the detrended plane. Therefore, a comparison between the different models gives insights in the impact of a considered displacement height.

39. L347: *A roughness density of less than 0.10 - 0.15 is not the only criteria required for the equation by Lettau (1969) to be valid. Consider also replacing "this study shows" by "this study assumes".*
Reply: For clarification, the words "with respect to the sheltering effect" were added. Additionally, "this study assumes" was implemented.

40. L350: *Perhaps section 4.3.3 can be made more compact. In its current form it mostly repeats previous research with generic statements.*
Reply: Section 4.3.3 was rephrased into a more compact version. Additionally, the subdivision into subsections was removed.

41. L364-371: *I propose to remove this subsection. Without a direct comparison with wind profiles or turbulent fluxes, the statement that one model performs better over another is difficult, if not impossible, to make. Instead a discussion, or possibly a sensitivity analysis to quantify model uncertainty (relating to the equation from Lettau) would be beneficial.*
Reply: the mentioned section was removed. Additionally, to further evaluate the model uncertainty the impact of h^*, s and c_d was discussed in the prior subsection.

42. L366: *I would argue that the raster methods are also based on the same assumptions, as they are all based on Lettau (1969).*
Reply: The raster method is not based on these assumptions since the raster methods which are based on Lettau (1969) were further modified to exactly prevent these shortcomings (see e.g. Smith et al., 2016).

43. L389: *The statement "leading to a better representation of turbulent heat fluxes" was not proven in this preprint. I suggest rephrasing this in something like "potentially leading to a better representation of turbulent heat fluxes". This prevents any confusion when only reading the conclusion.*
Reply: Suggestion implemented.

Technical corrections

1. L173: *"for heavily crevassed areas."*
Reply: Suggestion implemented and "up to" added.

2. L213: *"This small deviation in z0 values makes ..."*
Reply: Sentence was rephrased according to Editor's comment 28 in section "Specific comments".

3. L217 (and Fig9): *"independently"*
Reply: Suggestion implemented.

4. L284: *"obtain" to "contain"*
Reply: Suggestion implemented.

5. L292: *"methods"*
   Reply: Suggestion implemented.

6. L294: *"... still mostly positive ratio values, it is the glacier ..."*
   Reply: Suggestion implemented.

References:

Fitzpatrick, N., Radi ́c, V., and Menounos, B.: 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, 2019.

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

Quincey, D., Smith, M., Rounce, D., Ross, A., King, O., and Watson, C.: 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, 2017.

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