Review of the paper: Aerodynamic roughness length of crevassed tidewater glaciers from UAV mapping

by Armin Dachauer, Richard Hann and Andrew J. Hodson

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.

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.

Other comments :

1. L16: replace "this heat exchange" by "the radiative heat fluxes", at least if this is what is meant here.

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 ?

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.

4. L27 (& L3): Consider using "uncrewed" instead of "unmanned". Or remotely piloted aircraft system (RPAS).

5. L28: Please clarify how UAV overcomes the spatial coverage limitation of LiDAR. Aren't UAVs also limited in the area they cover ?

6. L35: Consider referring to the recent work by Van Tiggelen et al (Cryosph Discuss 2021, https://doi.org/10.5194/tc-2020-378),. They give estimated z_0 values for very rough ice & crevassed areas in west Greenland in their Figures 9 & 10.

7. L37: Consider rephrasing "makes it hard".

8. L87: the thesis of A. Dachauer could not be found online. Consider adding a public link with DOI to this 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.

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.

11. L99: Please clarify what is meant by "since small-scale roughness elements do not represent the real topographic expression".

12. L112: "[...] all four wind directions [...]".

13. L117: Is the value for $c_d = 0.5$ from Lettau (1969) realistic for crevasses ?

14. L119: Please rephrase "turns out". What do the authors exactly mean here ?

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 ?

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.

17. L144: Which parameters are calculated row-wise in the first two raster models, besides h^* ? Possibly refer to Table 3 for clarity.

18. L157 (and L6) : It is not clear here whether z_0 varies by three or by four orders of magnitude. Please specify what is meant by "up to three (locally even four)".

19. L158: "The highest values": what are these values ?

20. L167: do the authors mean averaged over all four cardinal wind directions ? Or has the data been rotated over all 360 degrees ?

21. L171-172: refer to Table 4.

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 z_0 than the raster method. Also see comment below about L322.

23. L186: This is hard to see on Fig5 with the given color scale. Consider changing the colormap or adding annotations in Fig5.

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).

25. Fig6: Please add to the caption what is denoted by the vertical extent of the boxplots (standard deviation, or quantiles ?). Consider using a logarithmic y axis, as all the means/medians seem clustered near y = 0m.

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

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.

28. L213: The two sentences at L213-215 could be simplified in something like: "Although the deviations in z_0 are small, the lower values found in 2020 could be related to the fact ...".

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 z_0 can rapidly change in time as well.

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 z_0.

31. L274: consider removing "the theory of".

32. L278: A grain roughness of 50 m is counter-intuitive. Does form drag not occur at scales smaller than 50 m ?

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".

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

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 ?

36. L332: Please explain (in the methods) how the raster methods take into account sheltering. Around L150 would be a good place.

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.

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).

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".

40. L350 Perhaps section 4.3.3 can be made more compact. In its current form it mostly repeats previous research with generic statements.

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.

42. L366: I would argue that the raster methods are also based on the same assumptions, as they are all based on Lettau (1969).

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.

Technical corrections

1. L173: "for heavily crevassed areas."

2. L213: "This small deviation in z0 values makes ..."

3. L217 (and Fig9): "independently"

4. L284: "obtain" to "contain"

5. L292: "methods"

6. L294: "... still mostly positive ratio values, it is the glacier ..."