

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2020-378

Anonymous Referee #2

Referee comment on "Mapping the aerodynamic roughness of the Greenland Ice Sheet surface using ICESat-2: evaluation over the K-transect" by Maurice van Tiggelen et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-378-RC2>, 2021

Review of "Mapping the aerodynamic roughness of the Greenland ice sheet surface using ICESat-2: Evaluation over the K-transect" by van Tiggelen et al.

General comments

This is strong manuscript that demonstrates impressive proficiency with many different sources of data (AWS, UAV, ICESat-2, modeling). The methods are generally well-described. The results section is very interesting and the development of spatially extensive aerodynamic roughness lengths for the K-Transect from ICESat-2 is commendable.

However, I do recommend some revisions. In its current form, the introduction is poor. Some of the terminology is vague, references are lacking and the overall research is poorly motivated. I encourage the authors to revise it thoroughly and have provided some ideas for doing so below.

While it is useful to know that the commonly used method for deriving z_0 from ICESat-2 (i.e. the standard deviation of ATL03 heights) tends to overestimate z_0 , the new measure is slightly unsatisfactory if it underestimates z_0 by a factor two. Without looking at the data, it is difficult to discern why. It could be due to the slightly arbitrary choice of filtering ($q_{low} = 1$ and $q_{high} = 2$) to remove photons above and below the median. It could be due to the choice gaussian covariance function, window size or assumed wavelength. Given that this is one of the first papers to investigate roughness lengths using ICESat-2 and availability of ground-truth data, it would be useful if the authors could develop a more unbiased method. I would encourage the authors to perform some sensitivity tests with these choices to see if they would reduce bias in their ICESat-2 z_0 products.

Specific comments

L16: Please consider capitalizing "ice sheet". It's the Amazon River, the Tibetan Plateau and should be the Greenland Ice Sheet. Indeed the Nature paper that you cite (Shepherd et al., 2020) has it this way.

L19: If you define an acronym, it is usually appropriate to use it here and elsewhere (e.g. L50, L146).

L18-21: Please provide some references for these two statements. A lot of work has been done on these topics and it is negligent to overlook it.

L20: "can be" is poor rationale for studying something. Please revise with something stronger, perhaps relative to radiative heat fluxes.

L22-26: Again, please provide references to backup these statements. A paragraph in the introduction without any references indicates that the research is poorly motivated or that the authors have a complete lack of respect for previous research on this topic. Please revise.

L37: What do you mean by "confined accessible areas"? Please provide some examples.

L39: Consider replacing "unmanned" with an ungendered term.

L40: What do you mean by "limited". Please be more specific.

L41: I am not aware of a satellite altimeter that maps the surface roughness of entire glaciers. The ground sampling distance is not small enough. This sentence also makes it sound like UAVs are completely unnecessary. Please revise and be more specific.

L42-44: This sentence about sea ice does not fit here in a paragraph about glaciers and ice sheets, please move somewhere else.

L99-100: Presumably Fig. 1b could be referenced here?

L145: missing an "of" between transect and AWS.

L226: I thought you just said that this approach did not require interpolation to 1 m profile?

L252-259: This text would be more useful in the introduction.

L260-274: Some more references to Fig. 6 in this paragraph would be useful to the reader.

L285: Please clarify what is mean by "satellite backscatter". I presume you are referring to a satellite radar instrument since ICESat-2 does not measure backscatter.

L288: Fig. 6? This figure does not show an elevation profile.

Consider swapping Sections 4.1 to 4.2 and Fig. 5 and Fig. 6. I think it would make more logical to move from small to large scale.

L396-397: It would be useful to briefly state again why Lettau (1969) is not recommended. Some people may only read the abstract and conclusions.

L399-402: I'm not sure I follow this logic. How do you know that ICESat-2 does not capture snow sastrugi or ice hummocks > 1000 m a.s.l. when your UAV surveys are constrained to < 600 m a.s.l.?

L405: It's a bit of stretch to say ICESat-2 cannot map z0m above 1000 m when this study presents no UAV surveys above > 1000 m.

Figure 1. Most of panel (a) is irrelevant, given that data from S9 are not used in this study. It makes it difficult to see how the ICESat-2 tracks intersect the UAV survey grids

(A and B). Please consider removing the picture of S9 and providing a zoomed version of the UAV survey grids around the margins of the ice sheet. In the caption please specify if these are the ICESat-2 reference ground tracks or from an actual ICESat-2 beam (e.g. 1r).

Figure 2: What is the rationale for these wind directions? Prevailing wind direction from AWS? Please clarify.

Figure 6: There is no reason for such large x and y axis limits on this figure which makes it difficult to determine the correspondence between the SEC and VPEC dots and modeled lines. Please provide a zoomed version of this figure.