

Review of: Temporal and spatial variability in surface roughness and accumulation rate around 88 S from repeat airborne geophysical surveys” by Michael Studinger et al. (MS No.: tc-2020-51)

This manuscript provides a detailed and in-depth report of surface roughness and accumulation over the Antarctic Plateau. The analysis uses the airborne NASA/Operation IceBridge ICESat-2 calibration surveys around the 88° S latitudinal limit of ICESat-2 and CryoSat-2. The authors’ results demonstrate the complexity and variability of surface roughness and accumulation rates over wide areas, and the manuscript makes a substantive and important contribution to the discipline and study region.

The manuscript is very well-written, datasets and methods are comprehensively reported, and the data are presented effectively. The manuscript is full of useful and important scientific content, but I am not fully convinced that the key messages and impact of the paper are maximised, or as accessible, as they could be (see ‘major’ issues below). The primary finding of the manuscript is a negative result (i.e. “our analysis shows that there is no simple relationship between surface slope, wind direction and snow accumulation rates for the entire survey area”), but that is not an issue in any way. *It is imperative that such negative results are reported and published.* The main finding here has important implications for our understanding of the relationships between these factors at local (correlation) and regional (no correlation) scales.

Broad comments:

There are 5 issues that I believe are worth highlighting about the manuscript in its current form. Were these to be addressed, the manuscript would be much improved:

1. A lack of discussion about the wider implications of the study. I would encourage the authors to consider the broader implications of this work (e.g. implications for satellite-derived accumulation rates, implications for ice core research at South Pole, Hercules Dome etc.) and incorporate these aspects into the abstract, discussion and conclusions sections.
2. A lack of information about the geographical setting of the area of investigation (e.g. which Antarctic drainage basins and ice stream catchments does the 88°S survey line intersect and survey?). A ‘study area’ section to the manuscript may help in this regard.
3. A somewhat awkward structure to the paper, with inter-mixing of study area, methods, results, and background information. There is no clear delineation between description of results and discussion/interpretation of those results. To me the current structure is not that logical, but this may reflect my training/background discipline. However, a decision on what section 3 is needs to be made. Is it a methods section, or something else? Currently it is an amalgamation of background, methods and results. Section 4 also seems to be predominantly methods, with sections 5-7 results/discussion intermingled. My recommendation is to restructure the manuscript to more clearly delineate between background/methods/results/discussion & interpretation. Without this restructuring it will not be clear whether the manuscript is a ‘methods’ paper or a ‘results’ paper.
4. A lack of citation to previous, potentially relevant, surface accumulation/roughness investigations in study area (e.g. results from the South Pole-Queen Maud Land traverses of 1964-65 and 1965-66), and potentially to Antarctic-wide surface accumulation literature (e.g. Arthern et al. 2006 <https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2004JD005667>).

5. Statistics are not my strongest suite, so I am not terribly well qualified to comment on the validity of those aspects. The statistics used seem simple and relatively unsophisticated however (e.g. lines 391-409), and I do wonder if a more sophisticated and rigorous statistical analysis has the potential to tease out more insights from the data set. Perhaps the current statistical analysis does the job however (i.e. 'reviewer 2' is simply scaremongering unnecessarily), and there is no requirement for introducing more complexity in this regard. I was a little unsure as to why it was so important to assess the correlation of the standard deviation (lines 391-409) however. Perhaps this could be explained a little more?

Specific comments:

Line 23: "overall" instead of "entire"?

Lines 26-28: Implications of this study for developing elevation bias corrections is stated, but I'd encourage the authors to include other broader implications (e.g. for quantification of surface mass balance and for ice core studies) here.

Line 46: "...ice surface, volume...?"

Line 47: "Radio-wave signal detection below the noise floor...?"

Line 50: "...azimuth dependent elevation"?

Line 52: "...we specifically studied...."?

Line 62: Change title to "Data sets and methods"?

Line 85: no comma after "both"?

Line 86: "...a ground speed..."

Line 105-108: Change to: "The difference in geolocation between distinct elongated topographic snow surface features (sastrugi) between overlapping orthorectified images is on the order of several metres. The DMS images....."?

Lines 119-123: More detail on the survey flights would be helpful. For example, what was terrain clearance, and was it consistent each year?

Line 127: Section "3.1 Background": It is unclear to me why this section is titled background, and why it is positioned here. If the section is background, then it should probably come before datasets/methods. This section seems to be a mix of background (lines 128-137 & 150-157) and surface roughness results (137-144).

Line 134: "Elongated....sastrugi" is repetition of line 105-108.

Line 155: Use of Gow reference is really effective. Engagement with more literature of this age could benefit the paper, e.g. publications from the South Pole-Queen Maud Land traverses of 1964-65 and 1965-66. Lots of the results of these traverses are published in "Antarctic Snow and Ice Studies II" <https://agupubs.onlinelibrary.wiley.com/doi/book/10.1029/AR016>

Line 158: Section "3.2. Relevance of surface roughness.....": This seems to be background material rather than results.

Line 170: No need for an acronym for bidirectional reflectance distribution function. It is only used once after this in the entire manuscript.

Line 172: “the relationships”

Line 180: Section “3.3. Surface roughness estimates”: The entirety of section 3.3 seems to be methods, or assessment of methods (lines 207-216) rather than results or discussion. An overall decision needs to be made about what section 3 is (see ‘broad comments’ above).

Line 234: year of Mouginot reference (2019?) is missing.

Line 239-249: Description of the longitudinal ranges of features of interest. I did find it tricky when reading the text to think about compass bearings in both westerly and easterly compass directions in a single sentence. I understand why the authors have done this (i.e. to describe what is shown in figures 3 and 4), but it is a little jarring and non-intuitive when reading a single sentence. For example, for me it is much easier to comprehend “between 175° W and 60 °E” when written as “between 60-185°E”. As currently written, it is also not clear without reference to the figure whether the smooth area is clockwise or anti-clockwise between 175° W and 60 °E. It may also be worth considering annotating the area described in figure 3 (&4)?

Line 250: I am not sure what “...appears to have less of a roughness anomaly....” means. Why not just “..the surface of this dunefield is less rough in 2017 compared to 2014 and 2016”?

Line 253: “beyond” rather than “out of”?

Lines 254-257: this paragraph seems a little ‘bolted-on’ to this section and is a little perfunctory. Does it need a few more sentences to describe the data presented in figure 4e a little more fully? This section is entitled “3.4 Surface roughness, slope and elevation...” but is very much dominated by roughness.

Line 254: Why “...slope of the ATM ICESN nadir platelets...”? Why not just “ATM-derived surface slope”?

Line 262: A, B & C are labelled in 4b, rather than 4a.

Lines 265: “simultaneously” or “concurrently” rather than “at the same time”?

Line 267: no comma after “Both”

Line 268: “...seems to be even slightly lower...” – recommend rewording to either quantify this statement, or to make it more certain. Perhaps “...are slightly lower...”?

Lines 282-318: “Section 4” – this all seems to be method description here, rather than description of data or results? Perhaps a re-structuring of the paper is required to make it clear to the reader which sections are methods, results and interpretation? If the manuscript is a methods development paper then that’s fine, but that’s not the impression currently given by the abstract.

Lines 320-340: “Section 5” – the first part of this section is a mix of background information (i.e. lines 320-329) and further description of methods (i.e. lines 329-340). It does not describe “Spatial variability in snow accumulation rates”. I would suggest that the opening line of section 5 is not the best place to state “Accumulation of snow on the Antarctic ice sheet is primarily the result of precipitation of snow”. Such a sentence should be on the 1st page of a manuscript. The entirety of Lines 320-324 should be much earlier in the manuscript.

Line 347: rather than “several”, can the authors provide a range (e.g. 0-3 cm w.e. yr⁻¹)?

Line 349: again, here it would be good to quantify the statement made (e.g. “the highest accumulation rates (xx cm w.e. yr⁻¹) near....”

Line 352-353: Good to cite original paper locating the bedrock low (i.e. Studinger et al. 2006). Authors could also add an up-to-date reference here to reflect new bed data acquisition in this area. Either Paxman et al., 2019 <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018GC008126> or Morlighem et al., 2019 <https://www.nature.com/articles/s41561-019-0510-8> ? Perhaps also move the reference to figure 3a to earlier in the sentence as it only shows surface depression, rather than the subglacial topographic low?

Line 354-360: this is an extensive description of previous work that is not directly linked into the data description/interpretation here. Could it be moved to a ‘study area’ section earlier in the manuscript? It might be more effective there, and can then simply be referred to at its current location?

Line 362: suggest insert an r² value after “correlates”.

Line 365: “highly variable” – requires some quantification in the text (i.e. range of values should be quoted).

Line 369: Reword to “However, several peaks in snow accumulation rate still correlate...”?

Line 370: again, insert an r² value after “correlates”?

Line 372: “topographic” rather than “topography”?

Line 372-373: change to “..lowest part of the depression where it reaches it’s highest point.”? Perhaps quantify the “highest point” too? How high was it? Such statements should be quantified in the text.

Lines 373-374: change to “...with lows in topography results in an overall negative correlation coefficient of....”

Lines 393-394: insert an r² value after “The correlation is strongest”? I note that in-text quantification of data description is much better in the following section 6.

Line 432: is there really a requirement to say “ESA’s CryoSat-2”? Why not just CryoSat-2?

Line 466: change to “....MERRA-2, which have low spatial resolutions.”?

Lines 467-471: this is a very important finding.

Figures:

Figure 1: Cite source of rock outcrop polygons (Antarctic Digital Database?)

Figure 2: I found it difficult to orient myself between figures 2 and 3. Where is figure 2 located on figure 3?

Table 1: This is table is really useful.

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