Comment on tc-2021-36

Simon Filhol (Referee)

Referee comment on "Local-scale deposition of surface snow on the Greenland ice sheet" by Alexandra M. Zuhr et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-36-RC2, 2021

General comment

The manuscript entitled "Local scale depositional processes of surface snow on the Greenland ice sheet" by Zuhr et al. present a technique based on ground-based photogrammetry to survey daily the geometry of the snow surface. The experiment was conducted over the course of 78 days, covering an area of 195m^2.

Overall the manuscript is well written and present an interesting novel dataset, which to my knowledge only one study in Antarctica had realized. The data show at 1cm scale the deposition and erosion of snow in a dry cold environment, in which wind is a driving force to the overall landscape. The snow surface is marked by bedforms which are assumed to play a crucial role in the deposition of snow to the ice sheet. Bedforms would be responsible for spatial variability which consequently should be looked at attentively if one is to reconstruct seasonal accumulation rate of precipitation or chemical from past and deeper ice cores. The relevance of the study is therefore justified by the advent of high-resolution description of ice core compositions.

The intent of the manuscript is in its current form unclear. While the title suggests a study on processes concerning snow deposition on the ice sheet, the content corresponds more to a method paper. There is little to no background on snow deposition processes, the description of depositional/erosional events is scattered around various parts focusing on validating a method. In that sense, I would either suggest to change the title to clarify that the content has to do with the validation of a method to measure snow deposition, or the content of the paper should be deeply revised to fit the title meaning and focus on a description of processes. Moreover, this study repeats very closely work done by Picard et al. presented in two prior papers from which very little has been used to inform this study, or even perform a comparative analysis in between Antarctica and Greenland.

If the intent of the manuscript is to validate the method, then the analysis presented seems robust and demonstrating the capability of this simple setup. Though, a thorough and unbiased discussion on advantages and limitations is necessary, including the entire pipeline, from instrumentation design to the final processing of the data, all taking place in the lab, the field and the office (post processing). However, while the setup has some contextual particularity, using photogrammetry for snow is by now a well-established technique.

Overall, this manuscript presents an interesting dataset from which the authors fall short to extract information describing accumulation processes, informed from the recent literature investigating snow surface morphology shaped by wind. For instance, figure 11 of the manuscript holds interesting data about the snowpack internal structure which is barely exploited. Finally, the main conclusion of the authors is arguable given the data presented. The authors claim that the snow surface become smoother which would corroborate past studies, but the data do not show such trend. Figure 8 shows a surface roughness oscillating twice over the coarse of the study from 4 to 2cm. Little is discussed about it and how this oscillation happens.

Simon Filhol

Detailed comments:

The introduction presents well the techniques available to accomplish the goal of the research question, but little attention is given to providing background on the geomorphological parameters of interest. Such addition would help refining the research question. For instance, the recent work by Picard et al. could be used further in grounding this study and using the data presented here to expand our understanding of snow surface morphology evolution in dry, cold, and windy environment of high ice sheet plateau.

In section 3, the authors chose to first present the signal of interest and explain posteriorly the reason of these changes. I would suggest doing the opposite; first describe the meteorological events relevant to snow accumulation/erosion, and then present the actual change occurring at the snow surfaces as a consequence to the weather events. The connection in between the two can then be drawn and interpreted. Also, a careful description of the wind conditions (speed, direction, frequency) for the period of the study but also at this season in other years would also help contextualizing the relevance of this study. Figure 4 and appendix A1 hint to this direction, the information could be reorganized more effectively to the reader's advantage.

In a second part, you may provide a qualitative description of the surface geomorphology accompanied by one or more photos. The description of surface bedforms in the current second paragraph could be refined. Such description can consider an area with a larger extent than the mapping area itself. It would add context to the maps (that are smaller than many of the typical snow bedforms and deliberately across the main wind direction).

The discussion reads like an in-depth analysis of the method rather than an in-depth discussion of the processes generating the local scale deposition variability. Here the authors argue that the snow surface is being smoothed, which in itself is arguable given the data presented in Figure 8, nevertheless we find no discussion of why and how a rough surface would become smoother (which implies an uneven snow deposition). Therefore, linking the results presented as is (snow height change and surface roughness) to climatic proxies is not clear and missing key connections.

In the discussion, there tends to be a bias towards the method used with little of the disadvantages discussed (e.g. need of an operator, possibility of interreferences with the snowpack microstructure, post-processing effort in terms of computational power and manual work). Also, little discussion is done on how it could be improved in terms of processing, or experimental setup (to the exception of NIR). Is it credible to expand this protocol for an entire year? Is this credible given the resources required to accomplish data acquisition to deliverable DEMs?

Line specific comments:

- L22: "accumulation rates", accumulation rates of what?

- L33-35: the self-organization of snow in dunes, ripples and sastrugi results in a heterogenous snow surface and spatial variability of snow depth deposition. There are now more relevant sources describing processes shaping the snow surface in windy environment. I invite the authors reading more carefully papers by Filhol and Sturm, Kochansky et al. and Picard et al.

- L43: the terms photogrammetry and structure from motion are often mistaken for being two independent methods, which in the field glaciology most often refer to the same technique. Also, what is meant by "remote sensing products"? photogrammetry and lidar are remote sensing techniques themselves

- L46: are sonic ranger newer than laser scanner or the recent rise of user-friendly photogrammetric software?

- L56: There is a number of studies demonstrating time-lapse photogrammetry, and some actually applied to snow (See Eltner et al 2017, Filhol et al 2019 and Chakra et al 2019 to cite a few). Despite the illumination challenge to expose snow properly and the logistical challenge to perform measurement in the cold, there is little inherent differences applying such technique to snow than any other type of surfaces.

- L66-69: Can you provide the precipitation estimate in the same units (either in SWE or ice layer thickness)

- L86: How many images per survey on average?

- L94: 32 to 35 sticks per image or total? How many were you able to obtain GCPs per image?

- L116-117: The sentences are unclear as the terms have not been clearly defined prior. What do the authors mean by snowdrift? This can either be interpreted as snow accumulation behind an obstacle or the action of snow being mobilized by wind. "snow drift, erosion and re-distribution" are not three independent processes. If the snow height changes in negative (e.g. the snow surface subsides) this would indicate either erosion or compaction.

- L120: There exist a variety of ways to describe surface roughness, can you provide the mathematical expression of the surface roughness of choice?

- L136: variance is fine, but standard deviation has the advantage to be expressed in the same unit as mean and RMSE. a choice to be considered throughout the manuscript

- Figure 3:

        - Is the colorscale choice of the first panels a linear gradient (e.g. viridis)? If not the colorscale can introduce representational artefacts. Or why not choosing a divergent colorscale centered over the median value of the first map?

        - I would suggest overlaying a hillshade to the elevation colormap to highlight the surface texture of the three upper panels.

- Indicate the main wind direction with an arrow to ease reading of the graph, as wind direction is a prevalent variable to surface texture development and anysotropy. A wind rose showing the period prior and during the time of the study would be even more interesting.

- You choose the zero-level to be the bottom left corner, but why not using the median elevation of your first day? Over very large area we can expect the mean and median surface height to converge, but given the scale of the study area and the size of dunes and sastrugi, the median will be less prone to bias. Then the DOP36 and DOP37 maps (if using the same colorscale as it is) would show areas where snow accumulated or was eroded in relation to a reference plane closer to where the average physical "zero plane" is.

- I found nowhere a plot showing time series of snow height for each pixel (or a substantial subsample) as in Picard et al. (2019). Figure 4 panel b) shows an aggregate of this value, but why not showing in the background the entire area of interest.

- L179-180: This statement is not obvious to me. first the amplitude of snow height seems to be reduced greatly in between DOP1 and DOP36, and second,

- L181: "show" -> showing

- L187: remove "further"

- L186-188: Given the small amount of accumulation and the spatial interdependency of snow accumulation in relation the wind field, could the human footsteps have created depression in which the snow would be trapped and therefore affecting the general accumulation/deposition pattern of the snow, or even the snow microstructure (local compaction). Could the authors provide a rough estimate of the volume of snow that this could represent in relation to the volume accumulated in the rest of the area. Also, what about the footsteps along the transect created while pulling the sled? Providing the average footstep depth would be a great indicator of the underlying snow hardness as well.

- L206-212: Is there a precipitation gauge or distrometer in the area?

- L214: Why do you not use all the DEMs and only 10 out of the 34?

- Figure 4: the legends do not indicate if the graph show mean or median values. Also, points from the DEMs, bamboo forest, and SSA stick could include error bars.

- Figure 6: The intent of this figure is unclear as it shows similar information than 4b

- Section 3.4: Why not including the entire DEM region in the plot of figure 7c and then focus on the three areas of choice (top of dune, troughs)

- Section 3.5:

    - Why the choice of 2.5m peak amplitude?

    - While the surface roughness decrease is more pronounced in the direction perpendicular to the wind, it is very interesting to observe that the roughness had reached 2cm by DOP 36, and then rose back to 4cm to then sharply drop back to 2cm. Would you have further insight as to why? It would actually indicate that the surface roughness magnitude can change sharply and does not necessarily converge to a given value. At least I see no such evidence happening in the data presented here.

- Section 4.1: The authors bring many advantages of the method but too few limitations. For instance, how intrusive is this method? Have you considered the effort (post processing time) and the resources (computational, and data storage) required to process photogrammetric data in comparison to other methods? The method proposed has many advantages, but a fair assessment including the entire workflow (and not simply the fieldwork) is necessary.

- Section 4.2: prior you used the term RMSE and not RMSD. But overall, I do not see the use of this sub-analysis in respect to the title of the manuscript, as it refers to local scale processes rather than method to estimate local-scale snow deposition.

- L300: providing your own wind analysis would be more convincing, as explained in the comments above.

- L301: Are the spatial scale of the two studies (this one and van der Veen) comparable? And are we actually convinced that this surface roughness decrease is a trend and not a coincidence to the period of experimentation as the intermittent points of figure 8 would indicate?

- L305: The link between lower wind speed and smoother surface is unclear as stated. As long as there is snow transport by wind, we know that surface bedforms are generated. Is the link between wind speed and surface roughness this simple? Snow erodibility is another important variable contributing greatly to the processes at play.

- L319: The dataset presented contains no information of micro-scale properties to the exception of the snow surface geometry.

- L317 & 331: "surface snow" -> snow surface. (there are multiple instances throughout the manuscript, and the title itself)

- Figure 11: This figure is fascinating! Throughout the paper you focus on the snow surface geometry. In this graph you show the internal structure of the snowpack derived from your surface measurements. This internal structure is a lot more relevant to ice core data than the surface itself, isn't it? Why not focusing the study on the geometrical properties of these internal layers rather than focusing on net snow height changes and snow surface roughness only? If one was to plan a study to retrieve past seasonal precipitation rates, how many ice cores would be needed, or how large of a sample would be needed to overcome the 2D variability of the snow internal layering? Those are simple example questions that could bring relevance to this study linking snow deposition at the surface and the internal structure of the snowpack.

- L365: The statement is unclear. Can wind scouring be defined in relation to previous terminology (aka erosion)? Why are those data not suited for this? They seem quite appropriate given that multiple wind drifting events had occurred during the period of interest.

- L366: Was estimating the amount of climatic signal mixing even a stated goal of this study?

- L387: misuse of "further" like in few other instances in the manuscript

- L387-389: The statement is not evident given data presented in Figure 8 and 11. Both show a variable internal structure of the snowpack that must originate from some processes. If not snow bedforms, then what? In a way this paragraph and the previous one are contradicting each other.

- Figure A1: Can you indicate when snow transport occurred?

Eltner, A., Kaiser, A., Abellan, A., & Schindewolf, M. (2017). Time lapse structure□□from□□motion photogrammetry for continuous geomorphic monitoring. _Earth Surface Processes and Landforms, 42(14), 2240-2253.

Filhol, S., Perret, A., Girod, L., Sutter, G., Schuler, T. V., & Burkhart, J. F. (2019). Time□□Lapse Photogrammetry of Distributed Snow Depth During Snowmelt. _Water Resources Research, 55(9), 7916-7926.

Chakra, C. A., Gascoin, S., Somma, J., Fanise, P., & Drapeau, L. (2019). Monitoring the snowpack volume in a sinkhole on Mount Lebanon using time lapse photogrammetry. Sensors, 1(18), 3890.

Picard, G., Arnaud, L., Caneill, R., Lefebvre, E., & Lamare, M. (2019). Observation of the process of snow accumulation on the Antarctic Plateau by time lapse laser scanning. *The Cryosphere*, *13*(7), 1983-1999.