Reply to the Review Comments of Simon Filhol (Referee #2)

on the manuscript

TC-2021-36: Local scale depositional processes of surface snow on the Greenland ice sheet

by Alexandra M. Zuhr et al.

We are very thankful for the detailed and thorough review of our manuscript and for the many constructive comments. Below is a point-by-point response to the general, detailed and line specific comments. The original referee comments are set in normal font and our answers (author comment, AC) are set in blue.

General comments

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 195m2.

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 1 cm 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

AC: We thank the reviewer for the detailed evaluation of our manuscript and the in-depth comments on the objectives of our study. We will adjust the title to “Local scale deposition of surface snow on the Greenland ice sheet“ and focus the manuscript more on the investigation of the processes at the snow surface, and less on the method and the validation thereof.
We will shorten and focus the method part on the general photogrammetry SfM method and our specific setup. We will keep only a short accuracy estimate in the method section and move most of the detailed validation (section 2.3) to the appendix where we will summarise the already presented additional validation of the method and associated uncertainties of the data.
We agree with the reviewer that our study is similar to the laser scanner study performed by Picard et al. (2016, 2019) at Dome C, East Antarctic Plateau. Even though our data are collected with a slightly different method, covering a much shorter time span and having a study site with different accumulation conditions, our data show comparable behaviors between ERA5 snowfall, wind speed and accumulation characteristics (see Figure 1) as shown in Picard et al. (2019). However, our snow accumulation occurs differently than in East Antarctica. More information on this behavior is presented below and will be added in the manuscript.
Overall, we provide a temporal and spatial data set from a study site in Greenland which is relevant for snow accumulation and deposition statistics on a local scale (tens of metres). The data further allow the analysis of (accumulation) noise in ice cores and contribute thereby to a better understanding of proxy data.

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.

AC: Thank you for the detailed feedback on the introduction. Comparing our study to Picard et al. (2019) and referencing our results to his work will be added in more detail in the manuscript. However, our data set is shorter and not as detailed as the one used by Picard and due to data gaps, a detailed comparison with wind characteristics (wind speed, direction and frequency) does not provide reliable conclusions on snow erosion and transport. Nevertheless, our data provide valuable information on local accumulation and deposition statistics for a new study site in northeast Greenland. Figure 1 shows similar patterns as presented in Figure 5 by Picard et al. (2019). In contrast to the data from the East Antarctic Plateau (EAP), our study site receives much more snowfall and we therefore expect different accumulation conditions. According to Picard, accumulation on the EAP is more patchy, whereas our data are more indicative of layer-by-layer accumulation. The similarity of Figure 1 here and Figure 5 in Picard et al. (2019) might indicate similar erosional patterns. We will add this figure as well as a discussion on the comparison to our manuscript. However, we cannot conclusively determine what processes are driving snow accumulation at our study site and how they differ from other areas. More high-resolution data sets from a variety of (polar and alpine) study sites would be needed to quantify the driving processes.

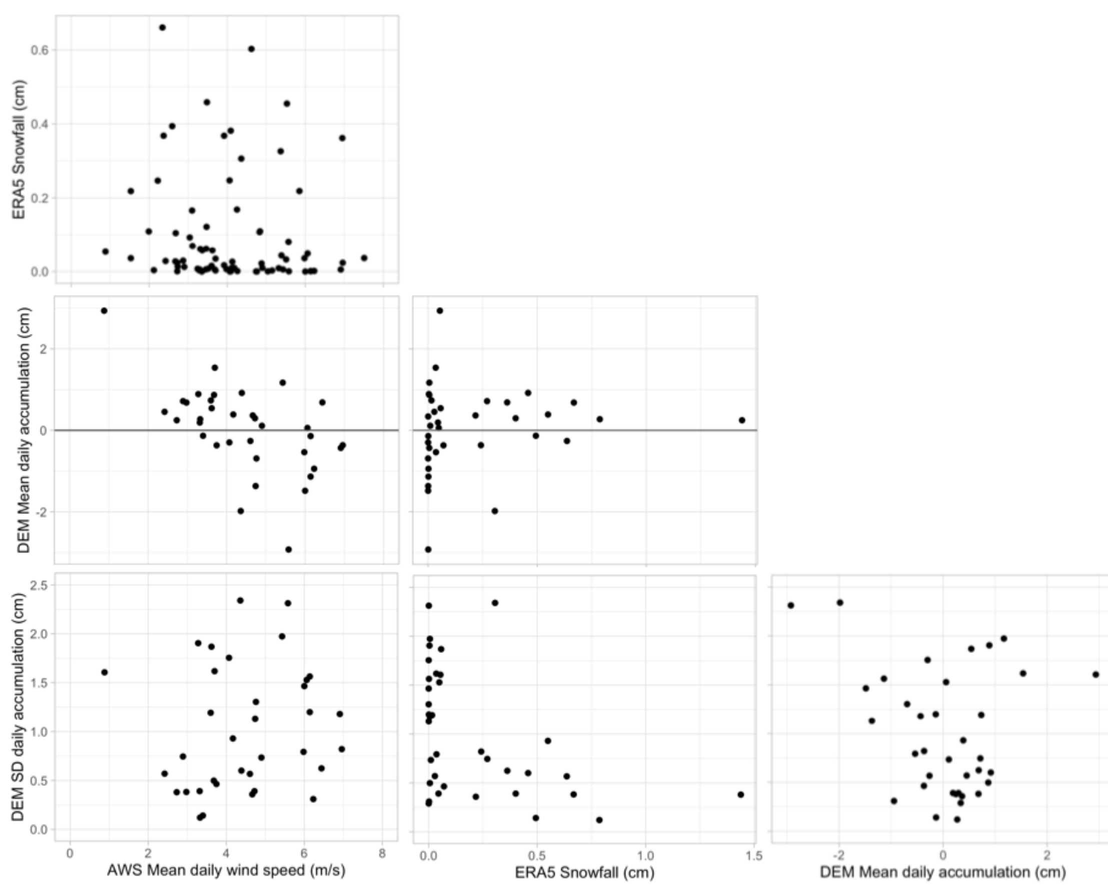

Figure 1: Comparison of different environmental parameters and snowfall specific characteristics. Following Picard et al. (2019), we compare DEM-derived mean daily accumulation as well as the standard deviation of daily accumulation to daily snowfall from

ERA5 and mean daily wind speeds from the nearby PROMICE automatic weather station for the observation period (16.05. - 01.08.2018).

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.
AC: Thank you for the detailed thoughts. The starting point of our research was indeed to compare the meteorological conditions (e.g. wind speed and direction) with our dataset. We agree that it would be very useful if we would be able to predict the snow surface evolution based on the meteorological conditions. However, similar to Picard et al. (2019), we did not find any clear relationships which might be partly due to the shortness of our dataset.
Our aim is thus to provide a statistical description of the snow accumulation and redistribution at this site; and for this aim, we think that the current structure is more useful.
We agree with your point to add a more detailed description of the wind characteristics and we will include a wind rose for our observation period as well as for the years 2017 to 2019 (see Figures 2 and 3).

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).
AC: A description of the wider surface geomorphology is not the aim of our study. Nevertheless, we will include a short description of our area in section 3.1 complementing our DEMs. In general, the main focus of our manuscript is on the statistics of snow deposition and accumulation. We deliberately do not focus on single morphological structures and, thus, have not described and discussed them in more detail in the manuscript.

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.
AC: We see the evolution of the surface roughness together with the negative relation between initial snow height and amount of accumulated snow as indicators for a smoothing of the snow surface during our observation period. We agree that the discussion can be improved by adding details on how the snow surface can smooth (e.g. the contribution of wind). Nevertheless, we

think that our results and especially Figure 11 (in the manuscript) allow a discussion of the effects of snow accumulation intermittency on climatic proxies.

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?
AC: We agree that the discussion of the method is highlighting the advantages of it and is lacking limitations and disadvantages, e.g. the need for an operator and the difficulty to extend our approach to year-round surveys. We will assess and include disadvantages of the method and present a more profound analysis on the future use and possible extensions of our method.

Line specific comments

- L22: "accumulation rates", accumulation rates of what?
AC: We will change the wording to "snow accumulation rates".

- 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.
AC: We agree that adding more references to the formation of surface snow structures will improve this section. We are thankful for the references and will add these to the manuscript.

- 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
AC: We agree that our current list of methods is not very consistent. We will rephrase this part and better differentiate between the individual methods.

- L46: are sonic ranger newer than laser scanner or the recent rise of user-friendly photogrammetric software?
AC: Sonic rangers are frequently used at automatic weather stations (AWS) and are easy to operate at remote locations with little maintenance (e.g. Cohen and Dean 2013, Castellani et al. 2015); however, they are not new. We will change ‘A newer technique’ to ‘Another technique’ in the manuscript. At the EGRIP camp site, a Campbell Scientific SR50A instrument has been used to record the snow height at the nearby AWS since 2016 as part of the PROMICE network (https://www.promice.dk/WeatherStations.html).

- 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.
AC: Thank you for these suggestions. We will extend this part and add more references.

- L66-69: Can you provide the precipitation estimate in the same units (either in SWE or ice layer thickness)
AC: Yes, we will provide all accumulation estimates in the same unit (mm w.eq. yr-1).

- L86: How many images per survey on average?
AC: We used on average 60 images per survey/day to generate one DEM. We will add the number of used images to the manuscript.

- L94: 32 to 35 sticks per image or total? How many were you able to obtain GCPs per image?
AC: Since our sledge was dragged 1.5-2 m apart from the row of sticks, only two to four sticks (dependent on the spacing between sticks) are visible per image. We will add the field of view of the camera to Figure 1 for a better illustration of the coverage on the photos. After arranging all ~60 images, we were able to detect between 32 and 35 sticks (depending on the image quality and light conditions), all are used as GCPs. This is mentioned in the manuscript as well.

- 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.
AC: Thank you for this observation. We will add this to clarify that we refer to snowdrift as snow being mobilised by wind. We will further indicate that negative snow height changes can be 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?
AC: We will provide a mathematical definition of the used surface roughness.

- 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
AC: We agree that mentioning the standard deviation is more useful. We will change this 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?

AC: The used color scale was a default topographic color scale. Thank you for the suggestion to use the viridis color scale. We will adapt the color scale to this color palette to reduce representational artefacts.

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

AC: We avoid the use of a hillshade to keep the illustration simple and clear.

- 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.

AC: Thank you for the very helpful comments. We will add an arrow indicating the main wind direction as well as wind roses showing the wind conditions during our observation period (Figure 2) and for the years 2017, 2018 and 2019 for comparison (Figure 3).

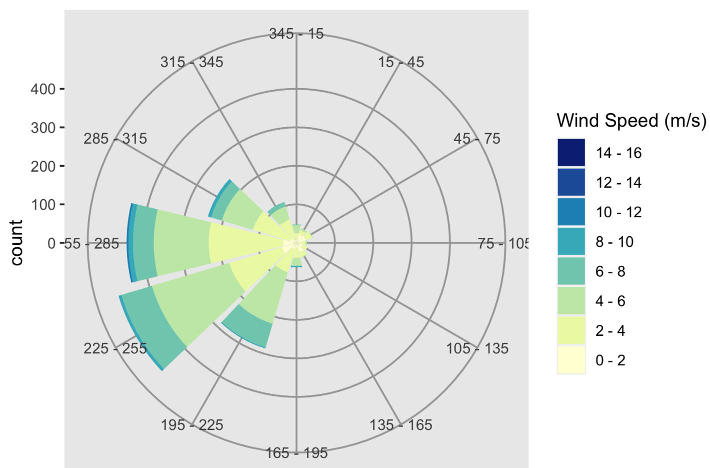

Figure 2: Hourly wind characteristics (speed and direction) for the observation period from 16.05. - 01.08.2018 recorded by the nearby PROMICE AWS.

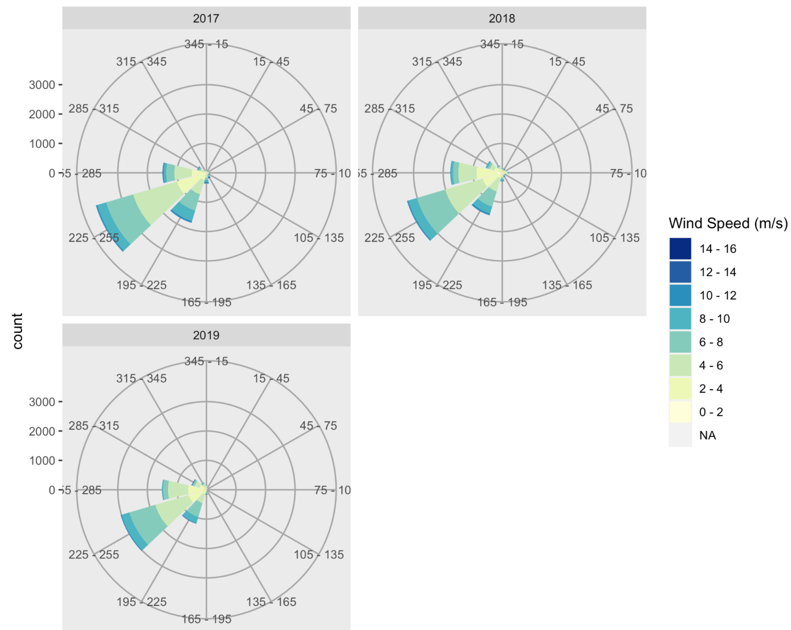

Figure 3: Hourly wind characteristics (speed and direction) for the years 2017, 2018 and 2019 recorded by the nearby PROMICE AWS.

- 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.

AC: The zero-level is chosen arbitrarily but could be changed of course. However, since we are more interested in the relative changes of the snow height and the statistics thereof, and less in the absolute snow height increase, the absolute zero-level does not change our analysis. Nevertheless, we will change the color scale to the viridis color palette.

- 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.

AC: This is a good point. We will add a plot (see Figures 4 and 5) showing the snow height evolution for each pixel of our study area in the appendix of the manuscript. The number of pixels with a value decreases during the observation period due to increasing missing data caused by footsteps and sampling positions. To avoid a bias by missing data, the aggregate in Figure 4b is the average of individual pixels along the entire x-axis (39 m) and from y=2.5-3 m. This area is not affected by either missing data from the snow sampling or by bad image quality (which is another limiting factor in the DEMs).

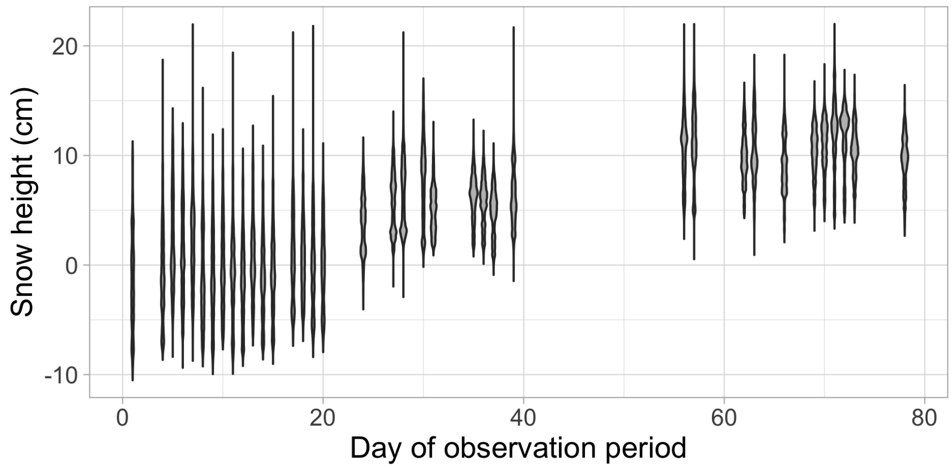

Figure 4: Variability of snow height for each day throughout the observation period. Each density represents the snow height distribution for each individual pixel in the study area per day. The spatial resolution of the DEM is 1 x 1 cm. Due to increasingly missing data in the area (footsteps and snow sampling positions), the number of available points per day decreases to about 70 % of the initial number of data points (1,885,506 on DOP 1 to 1,319,095 on DOP 78) towards the end of the observation period.

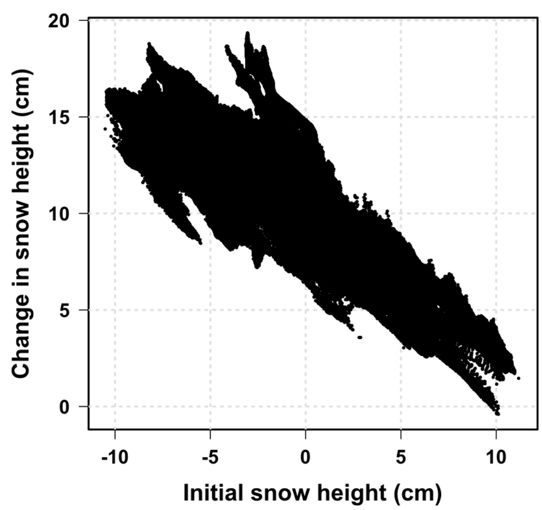

Figure 5: Relationship between the initial snow height on DOP 1 (16.05.2018) and the change in snow height to DOP 78 (01.08.2018) for each pixel in the study area (same area as in Figure 4).

- 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,

AC: The amplitude decreased between DOP 1 and DOP 36 from 21.8 to 12 cm. The amplitude on DOP 78 accounts to 13.8 cm, which is similar to DOP 36 and less than in the beginning of the observation period. In the manuscript, we will elaborate more on different timespands during our observation period and present different phases of snow erosion and accumulation.

- L181: "show" -> showing
AC: Will be changed as suggested.

- L187: remove "further"
AC: We will 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.
AC: Thank you for your comments on the effect of the footsteps on accumulation and deposition patterns. These are valid thoughts about snow re-distribution possibly caused by our actions close to the study area. We carefully closed the snow sampling positions and footsteps and tried to smoothen/flatten the surface as much as possible to avoid disturbances. The sampling positions are downwind; thus, we expect the influence of these positions to be minimal since the wind was blowing quite constantly from the same direction (see Figure 2). We will add a wind rose to the manuscript showing wind speed and directions during the observation period. The footsteps along the transect while pulling the sledge were not very deep and the snow surface was not very soft. We therefore do not expect effects on the snow accumulation. We will add information on the footsteps in the manuscript.

- L206-212: Is there a precipitation gauge or distrometer in the area?
AC: There is unfortunately no precipitation gauge or distrometer in the area or in the vicinity of the EGRIP camp.

- L214: Why do you not use all the DEMs and only 10 out of the 34?
AC: We use only the first 10 DEMs and not all 37 because we want to highlight the increase and subsequent decrease of the snow surface. Including all 37 DEMs into one line-plot reduces the visibility of single lines and events and the focus would rather be on the overall snow height increase instead of the day-to-day variability. We will add a graph showing all 37 DEMs, separated in three panels, in the appendix (see Figure 6).

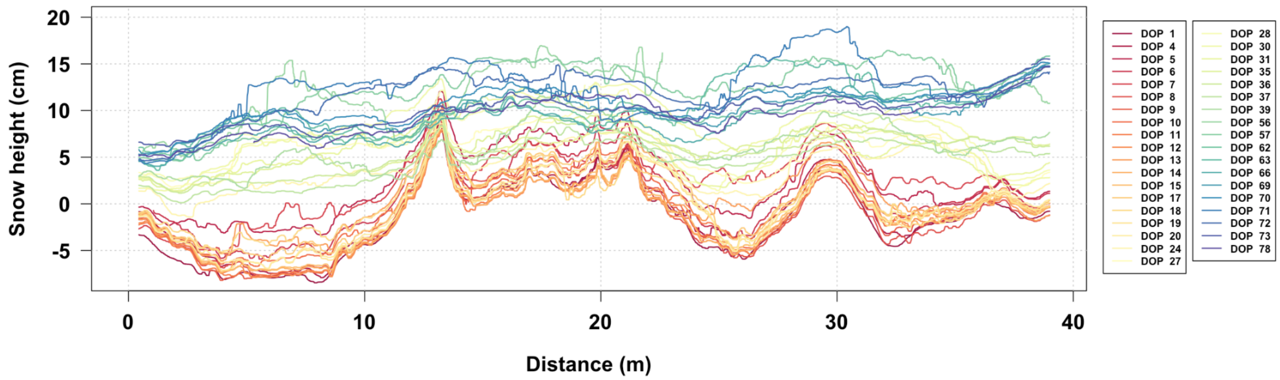

Figure 6: Relative horizontal snow height profiles (20-point running median, averaged in y-direction from the 2.5m-band). Different colors represent different days throughout the entire observation period from DOP 1 to DOP 78. Overall, snowfall caused an increase in the snow height at each point in the study area.

- 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.

AC: The graph in Fig. 4b shows mean values. We will add this information to the legend. We will also elaborate on error terms (e.g. finite number of averaged data points) concerning the DEMs, the bamboo forest, the SSA stick as well as the PT stick estimates.

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

AC: The intention of Figure 6 is to illustrate the similarity of surface snow structures from one day to other days, thereby indicating changes in the overall surface structure due to specific events. Figure 6 contains only data from the DEM data set while Figure 4b also includes the overall evolution of individual snow height estimates from other methods, however, no information on the surface structure within the study area. We will add exemplary arrows to the plot indicating when the surface structure changed due to deposition (green) or erosion (red) events.

- 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)

AC: We did not include the entire DEM in Figure 7 and 7c because the area under consideration would decrease throughout the season with increasing missing data caused by the snow sampling and foot steps. We therefore focus on four sub-areas representing the different snow surface height conditions at the beginning of our observation period in our study area. Nevertheless, we added here a plot showing figure 7c from the manuscript for the entire DEM region (see Figure 5). We will add the scatter plot in the appendix in the manuscript.

- Section 3.5:
- Why the choice of 2.5m peak amplitude?

AC: We chose the 2.5 m peak amplitude for the surface roughness calculation in order to provide results comparable to the study by Albert and Hawley (2002) from Summit, Greenland. Their study site has similar environmental conditions as the EGRIP camp site and is therefore suitable for a comparison of surface roughness estimates. We will add a mathematical equation describing the surface roughness calculation. We will further show two different surface roughness estimates; one describing the large scale undulations (i.e. the standard deviation across the entire DEM) and a second one using the 2.5m peak amplitude focusing on smaller scale variation in the surface.

- 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.
AC: The surface roughness indeed decreases to 2 cm already at DOP 36 with a subsequent increase. We also investigated the variability of single 2.5m-peak amplitudes (Figure 7) and saw a decrease in the spread of values from DOP 36 to DOP 78 which indicates an overall decrease in the surface roughness towards the end of our observation period. We therefore concluded that the surface roughness reached a more stable state on DOP 78 than on DOP 36 with the roughness staying around ~2 cm for about two weeks (DOP 62 to DOP 78). We will add more information on the evolution of the surface roughness at this point in the manuscript.

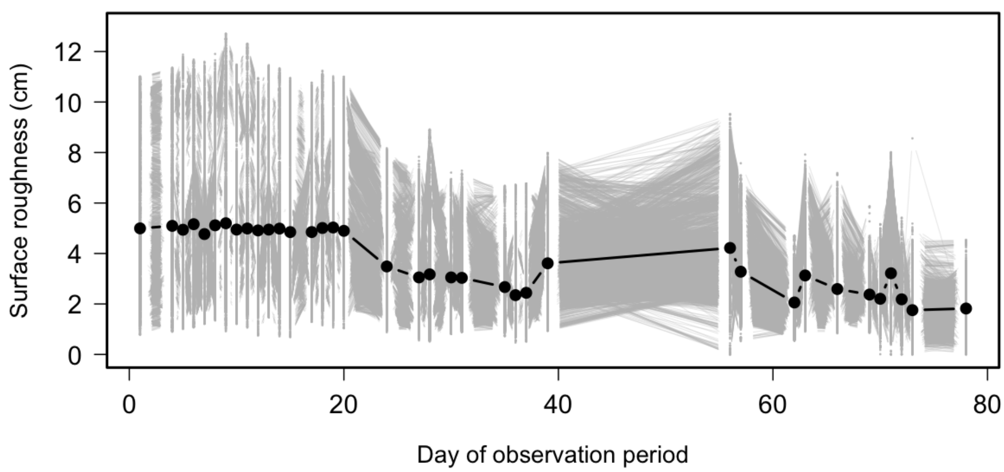

Figure 7: Surface roughness estimates throughout the observation period based on 2.5m-peak amplitudes perpendicular to the main wind directions. The individual estimates (grey) show a decreasing spread towards the end of the observation period. The mean across all estimates is shown in black.

- 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.

AC: We agree that section 4.1 is addressing the advantages of our method rather than presenting an independent evaluation and assessment of our method. We will add and discuss limitations and disadvantages of our method in this section.

- 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.

AC: We do not agree that this section does not fit the title of the manuscript. The small-scale depositional processes, which we visualize and analyse with our photogrammetry method, cause strong spatial variability in local snow accumulation, which affects the representativity of point measurements (snow stakes) in terms of the average regional snow accumulation. Here we explicitly utilise our data to evaluate the optimal setting of snow stakes to obtain representative snow accumulation measurements. We will also use the term RMSE here.

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

AC: During our data analysis prior to this manuscript, we investigated the relation between wind characteristics (speed and direction) and the changes in the snow height, as for example performed by Picard et al. (2019), but the results did not provide clear results linking the parameters to each other. We therefore did not add more details on the relation between wind conditions and surface snow changes in the manuscript. We will attach a wind rose for the observation period as well as for the years 2017, 2018 and 2019 for comparison.

- 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?

AC: van der Veen et al. (2009) studied the northern part of the Greenland Ice Sheet; thus, a much larger area than covered by our study. They also used a different mathematical expression to calculate surface roughness. Additionally, van der Veen et al. (1998) use airborne laser data from May 1995 to assess the surface roughness for central Greenland including the drilling site GRIP and the Summit location. Their result suggests that no reduction in surface roughness occurs during the summer season in central Greenland. However, the results from Albert and Hawley (2002) who studied local surface roughness on scales similar to our scale, found a similar behavior as we do. The different results of surface roughness highlight the natural complexity of this parameter and the lack of clear information. Nevertheless, we contribute to the increasing understanding of surface roughness by providing our local-scale estimate which suggests a decrease of the surface roughness to about 2 cm in the end of our observation period as an indication of a smoothing of the surface. We will add the mentioned discussion on the different spatial scales to the manuscript. The shortness of our measurement period of only one season does not allow us to clearly distinguish between variability (= the trend is a coincidence) or a seasonal trend;

- 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.
AC: We agree that lower wind speeds in summer are not the only parameter which can lead to a reduction in surface roughness. Thus, the link between wind speed and smoother surface is not as straightforward as it is stated in our manuscript. We will remove the sentence and clarify that more parameters (e.g. snowdrift, wind conditions, temperature, humidity, metamorphism, and others) are influencing the conditions and erodibility of snow surfaces and may therefore contribute to changes in the surface roughness over the course of a year. Nevertheless, the formation of sastrugi during the winter time with higher wind speeds can still be a significant contribution to the surface structures at our study site (see Figure 8).

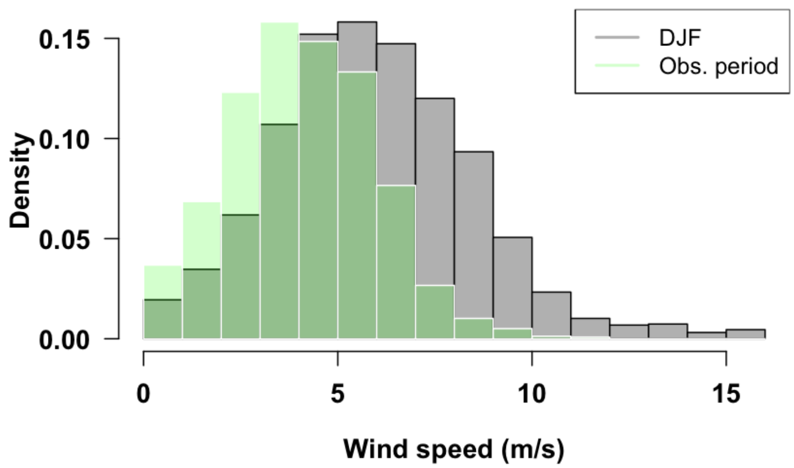

Figure 8: Hourly wind speed data from the nearby PROMICE AWS for two different time periods. Wind speeds during the winter months of 2017 to 2019 (December, January, February; DJF; grey) are compared to wind speeds during our observation period (16.05. - 01.08.2018, green). The winter months are characterised by higher wind speeds with a mean of ~6 m/s while the average wind speed during our observation period was 4.1 m/s.

- L319: The dataset presented contains no information of micro-scale properties to the exception of the snow surface geometry.
AC: We agree that our data set does not provide information of micro-scale properties and we therefore did not include any analysis about this scale in the manuscript.

- L317 & 331: "surface snow" -> snow surface. (there are multiple instances throughout the manuscript, and the title itself)
AC: We will standardise this throughout the document.

- 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.
AC: Thank you very much for these thoughts. We agree that this figure is a key result of the manuscript. It highlights the main message of our study, namely the heterogeneous internal structure of the upper snowpack, and provides valuable insights in the deposition structure. We also agree that it should be presented earlier in the manuscript and discussed in more detail. We will move this part to the results section and analyse the implications in more detail.

- 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.
AC: Wind scouring describes a process where winds persistently carry away snow from the surface by erosion and sublimation, especially at steep surface slopes. This special and complex (deposition) process is most important in low accumulation areas and can be mapped with e.g. radar stratigraphy which detects metamorphosed layers. A fraction of the eroded snow can also fill topographic troughs downwind (Das et al., 2013). With our data set, we are not able to detect whether normal snow erosion and re-distribution or wind scouring occurred because we cannot resolve the micro-properties of the snow surface. We will add more information in the manuscript on wind scouring and the lack of detailed snow properties in our data to detect this process.

- L366: Was estimating the amount of climatic signal mixing even a stated goal of this study?
AC: One aim of our study is to provide an outlook on the implications of snow erosion and on the interpretation of reconstructed climatic signals from ice cores. We agree that the objectives are not clearly outlined. We will improve the outline of our objectives.

- L387: misuse of "further" like in few other instances in the manuscript
AC: We will remove “further” and go thoroughly throughout 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.
AC: We will rephrase this paragraph.

- Figure A1: Can you indicate when snow transport occurred?
AC: We will indicate the wind speed threshold of 4 m/s which enables snow transport, in Figure A1. However, we cannot confirm that snow transport (only) occurred during these periods.

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