Reply to the Review Comments of Quentin Libois (Referee #1)

on the manuscript

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

by Alexandra M. Zuhr et al.

*Thank you for your effort and your careful and detailed review of our manuscript. We appreciate your constructive feedback that will help to improve the manuscript. Below we provide a point-by-point response to all comments. The original referee comments are set in normal font and our answers (author comment, AC) are set in blue.*

## General comments

This study presents a unique data set of digital elevation models (DEMs) of surface snow near the drilling site of the East Greenland Ice Core Project (EGRIP), acquired near-daily during a whole Summer season (May to August 2018) from a photogrammetry approach. These observations are complemented by more traditional snow height measurements and a variety of meteorological observations. The data are used to extract information on the evolution of average snow height (which increased by 11 cm along the observation period), but also on its spatial variability at the scale of the sampled area (195 m2). It highlights the complexity of spatio-temporal variations, and the poor correlation between precipitation (observed or reported in ERA5 reanalysis) and snow height variations. In particular ~60 % of the deposited snow is at some point removed. This is attributed to the significant role of post-depositional processes, such as snow erosion and subsequent transport by wind. All along Summer, this redistribution results in a reduction of the surface roughness (from 4 to 2 cm), and an overall flattening of the surface. In an extensive discussion section, the impacts of these observations on the proxies used to study climate (e.g. stable water isotopologues) are discussed.

The topic of the study perfectly suits to The Cryosphere because it both presents a novel observation methodology, a novel dataset, and interesting results regarding snow processes. The observations are robust and much care is taken to ensure that the observations are valid, and to quantify the uncertainties. The discussion points to relevant questions related to this study, which for some of them (in particular how and when erosion, transport and re-deposition occurs) could have been a bit more explored with the present dataset. The results are not particularly surprising to people familiar with snow physics in polar regions, and it'd be appreciated that more quantitative comparisons be made with previous similar studies. Besides the new technical approach, more insight about how this study complements the existing literature on the topic would be useful as well. The paper is well written and the methodology clearly described. The multiplicity of observations sometimes makes it difficult to follow, and an updated Figure 1 could certainly help the reader. I recommend this manuscript for publication after these minor issues and the technical details below are addressed, and hopefully after a slightly deeper investigation of the data for the physics of snow erosion and transport.

AC:
We are happy that the manuscript is generally seen as an important and significant work contributing to a better understanding of snow processes. We acknowledge the suggested minor points and carefully address them as outlined below. We will better discuss our results in the context of previous studies (e.g. comparing directly to Picard et al. (2019)) and define more clearly what is unique and similar to the existing literature. We will update Figure 1 for a clearer indication of individual methodologies and improve the technical details as well as the method section. Unfortunately, the temporal resolution and temporal gaps (partly caused by the fact that it was the first season in which this setup was used) limit to robustly determine the origin and timing of the erosion, transport and re-distribution. On the other hand, our dataset is designed to and sufficient to determine the statistics of deposition that are relevant for the formation of environmental and climatic records. We will extend the interpretation and discussion of this point and also adapt our title to “Local scale deposition of surface snow on the Greenland ice sheet“ to better define the focus of our study.

## Specific comments

1) The detailed quantitative results of the studies are very useful, but should be better put in the context of other studies performed in regions with similar climatic conditions. While it is clearly pointed that the methodology is original (although the differences with classic setups including only 2 cameras should be better highlighted), the novelty of the results, if any, is not sufficiently put forward. It is currently hard to say which ones among the presented results are really unique to this study.
AC: We will extend the discussion on how our study relates to other studies from regions with similar characteristics and also better define the scope of the manuscript (including its title). We will include a sentence to highlight the differences to classical ‘stereo’ camera setups.

2) Ancillary observations (AWS, snow sampling) are widely mentioned but insufficiently used. Wind speed, and possibly direction, could help interpret the variations of surface roughness (including formation of dunes which may still build up in Summer before to be flattened) or the depositional processes. Snow sampling is not detailed (except for its impact on the study area) but could probably help to stress the spatial heterogeneity displayed in Fig. 11 for instance. The comparison of some snow profiles with this figure would be very useful.
AC: During our research process, we analysed the AWS data (wind speed and direction) in detail. We agree that it would be very useful if one could establish clear links between the meteorological conditions and the development of the surface structure. However, we did not identify any clear links, potentially as our sampling resolution (every day, with some gaps in between) is too low. We will show the AWS data and a comparison to the roughness in the appendix of the revised manuscript. The snow sampling data are still in analysis and thus would be beyond the scope of this manuscript.

3) In general, the manuscript could be shortened by removing some redundancies (in discussion and conclusions), by clarifying the experiments once for all at the beginning, or by selecting the results. This would leave more room to explore the previous suggestions.

AC: In the revised version, we will remove some redundancies in the discussion and conclusion. We think that the presented choice of the results reflects the main analyses concerning the topic of the manuscript, but we will move Figure 11 to the results.

4) Although rich the discussion is a bit long and could probably be shortened. Section 4.3 could be moved to the Results Section because it still contains quantitative results not presented earlier on (e.g. Figs. 10 and 11). Section 4.4 highlights the potential impact of the research on the climatic analysis of ice cores but the conclusions are somehow general. More quantitative estimations of the potential impact would help the reader figure out to which extent the results obtained here can question the current analysis techniques.
AC: As suggested we will move 4.3 to the result sections and rewrite Section 4.4 to be more concrete.

## Technical corrections

l.6-7 : the contribution of snow re-deposition to noise in climate records from ice cores is put as a primary objective of the paper, but I'm not sure strong quantitative conclusions are reached on that question. Consider reformulating the main objective or rephrasing the conclusions.
AC: We agree that the conclusions are not answering the proposed questions. We will reformulate our conclusions to meet the stated objectives.

l.26 : detail briefly how isotopic composition can be changed
AC: Thank you for pointing out the missing information on changes in the isotopic composition. We will add a comment on this in the introduction.

l.28 : "larger" is not clear
AC: We will rephrase it to "larger scale processes".

l.31 : maybe remove "deposited"
AC: Will be changed as suggested.

l.39 : "mapping" is not clear. Do you mean in space or time ?
AC: We are referring here to both the spatial and temporal mapping of snowfall. We clarify this by adding "spatial and temporal mapping".

l.39 : why is surface roughness important here ?
AC: Surface roughness is influencing the spatial deposition of snowfall, especially during windy snowfall conditions. We will add this information in the manuscript.

l.40 : I think precipitation intermittency is completely independent of surface processes, such that accumulation intermittency and precipitation intermittency are two distinct things
AC: Precipitation intermittency is not influenced by surface processes, such as erosion or snowdrift. However, precipitation intermittency is part of accumulation intermittency, because it determines the timing and amount of snow available for re-distribution and transport. We will replace 'precipitation' with 'accumulation'.

l.49 : maybe provide the typical spatial scale of remote sensing observations. For laser altimetry for instance

AC: We will specify the spatial scale of current laser altimeter systems to provide a better understanding of the need of small scale methods.

l.51-52 : it's not clear whether SfM is a particular type of photogrammetry or something different

AC: Photogrammetry SfM is a technique for itself and is already widely used, also for similar studies in the field of glaciology (e.g., Chakra et al., 2019; Filhol et al., 2019). We will add a sentence on this technique in the manuscript.

l.53 : if laser scanners do have limitations, maybe state them here. This will support the use of SfM

AC: We will add and elaborate on the limitations of laser scanner studies at this point in the manuscript.

l.59-61 : the end of the introduction is incomplete. The objective is not clearly stated, and no outline is provided. Instead some result is provided that should not appear here.

AC: We will clarify the objectives and remove the results.

l.65 : "with a mean" is awkward. where the mean annual temperature is -29 C ?

AC: Will be changed accordingly.

l.67 : what should the reader deduce from the comparisons of accumulation rate vs annual layer thickness ? Are these numbers consistent ?

AC: We will put them all to the same unit (mm w.eq. yr-1) for easier comparisons. By showing the different accumulation rate estimates, we want to highlight the high spatial variability in local accumulation at our study site.

l.71 : are these data used in the study ? If not, this last sentence is useless

AC: We will remove this sentence.

l.74 : to achieve this ~~goal~~

AC: Will be changed as suggested.

l.75 : not clear if this is the area covered by one picture or by the whole DEM. Is it dictated by the field of view of the camera ? Clarify the link between the 390m2 and 195m2.

AC: The total area covered by all images per survey is 390m2. Due to the lack of coverage with images and the lower image quality at the rear of the area, the covered area by the DEMs is only 195m2. We will clarify this in the revised manuscript.

Figure 1 : this figure is central to understand all the measurements that are mentioned in the manuscript. Unfortunately it's not very clear. AWS is loosely positioned because the arrow should point towards the camp which is not shown. The scales are loosely defined (e.g. 90 m,

200 m, 39 m) while they could be consistent. The 10m width of the SfM method is not shown. X and y axis could be added. What are the 5 sticks above the 35 sticks in the photogrammetry area ? Add the sledge and orientation of the camera
AC: We will clarify the positions, make consistent scales and update Figure 1. We will further add the position of the sledge as shown in Figure 2 as well as the approximate field of view of the camera.

l.76 : "around" does not suggest the sticks are put on a line. Are they ?
AC: We agree that the sentence was not precise. The sticks along the x-axis are set on one line, the surrounding sticks are also positioned on line to create a rectangle. We will clarify this in the manuscript.

l.79 : why "almost"? Are the missing days due to technical issues or were they planned ?
AC: No photos were taken on very cloudy days with no visible contrast or with whiteout conditions. These conditions do not allow any snow height reconstructions with optical images only. NIR would be necessary to extract more information during these weather conditions. Further missing days are caused by (human) errors in the data acquisition. We will clarify this in the manuscript.

l.84 : how long does it take to take all pictures ? How many pictures are used for each DEM? Why is the width limited to 10 m ? How was the geometry of the study area chosen? Is it necessary to have that many images, compared to standard photogrammetry with only two or three images ?
AC: The image acquisition itself took about three to five minutes; however, including the preparation and walking time to and from the camp, the time effort was about 45 minutes. The width of 10 m was chosen based on thoughts about image quality for DEM generation. During the field period, we realised that the images are not good enough for a DEM generation up to 10 m width; thus, we restricted the analysed area to 5 m width. About 60 images were used for one DEM. If less than 50 to 60 photos were used, no DEM could be generated because the overlap between successive images was too small. This can be caused by a lack of available surface features to match the images which is a reason for using that many images, instead of two or three, as other studies did. We will add more information about the number of pictures in the methods part and extend the discussion by comparing our method to setups with e.g. only two cameras.

l.92 : y=10 m was not properly defined, hence this sentence is hard to understand
AC: We will clarify y = 10 m in Figure 1.

Figure 2 is hard to relate to Figure 1. Consider adding the footprint of the camera to help
AC: The footprint of the camera in Figure 2 will be added to Figure 1.

l.95 : does it mean that only a transect is used instead of the full 2D domain ?
AC: No, the analysed area is only restricted to the area from y=0m to y=5 m which equals a size of 195m2.

l.99 : how do you document the snow height at the glass fibers without perturbing the observed area ? Are the sticks out of the final domain ?
AC: The sticks surrounded the observed area and could thus be accessed and measured without perturbing or stepping into the studied area.

l.105 : “summarised” is unclear. Averaged ?
AC: Will be changed as suggested.

l.106 : the snow sampling was performed for all 35 glass fibers ? What was measured at this occasion? When was it performed ? Is it used in this study ?
AC: We apologise for the unclear information on the snow sampling. The sampling was performed at 30 stick positions along the x-axis every third day throughout the entire observation period. No samples were taken at the remaining five stick positions. The snow samples were measured for stable water isotopic composition and are not used in this study. We will add more details on the snow sampling procedure and the resulting surface disturbances in the manuscript.

l.110 : how is snowfall documented and how are samples collected?
AC: Snowfall was manually documented in a spreadsheet. If visual snowfall was observed and/or the snow collection tables (setup described in Steen-Larsen et al., 2014) had snow, we noted this down. We will add more information at this point in the manuscript.

l.111 : I assume snowdrift can be difficult to distinguish from snowfall in human observations as well
AC: We agree that snowfall is visually difficult to distinguish from snowdrift. We therefore analysed the DEMs with regard to manual snowfall documentation and ERA5 indications of snowfall.

Table 1 : why 30 PT sticks here and not 35 ?
AC: 30 sticks were set up along the x-axis, the remaining five sticks are mainly used to provide a reliable geo-referencing of the DEMs and not for further analyses. We will clarify this in the manuscript.

l.115 : shows
AC: Will be changed as suggested.

l.121 : not clear what peak-to-peak means, probably the difference between max and min?
AC: Peak to peak refers to the difference between the minimum and the maximum snow height. We will add a mathematical equation to the manuscript to clarify the calculation of the surface roughness.

l.131 : why cannot it be done on the main study area ?
AC: We did not want to disturb the study area by adding too many footsteps. Therefore, we set up a second area where we physically walked into the area to establish validation points within the area. We will mention this reason in the appendix.

l.133 : redundant with just a few lines above
AC: We will remove the redundant parts.

l.140 : it is not clear what additional information this section provides compared to the previous sections
AC: We tried to extensively validate our method by analysing different error terms which can arise when e.g. only using GCPs outside of the study area. We therefore included a detailed analysis of potential biases (e.g. doming effects) and mentioned the calculated uncertainties. We will restructure the method section and move most of the accuracy estimate to the appendix where an additional validation is already presented.

l.152 : why was not this sensitivity study performed directly on the study site ?
AC: The validation area has additional control points within the area, not only surrounding points as in the study area. Since we did not want to disturb the study area in addition to the snow sampling, we decided to set up a separate validation area to perform this sensitivity study. We will improve the description in the manuscript.

l.170 : sufficient accuracy
AC: Will be changed as suggested.

l.171 : here the final estimation of DEM accuracy should be mentioned. Otherwise it's used later on (1,3 cm) without relevant reference.
AC: We will add the final DEM accuracy here.

l.180 : it seems that on Panel 2 of Figure 3 the dunes have already vanished
AC: On DOP 36, the surface already became smoother compared to the beginning of our observation period. This is also shown in the reduced surface roughness towards this day (Figure 8). We will discuss this behavior in more detail in the revised manuscript.

Figure 3 : having these x and y axes in Fig. 1 would help a lot. Refer to the section where the areas in grey are used. "Snow sampling scheme" sounds awkward, remove scheme ? Clarify in the text
AC: We will remove "scheme" and clarify it in the text accordingly. We will refer to the sections where the grey areas are used and update Figure 1 by adding x- and y-axes.

(l.106) how frequently such snow sampling were performed, and make it clear whether this corresponds to the readings of snow height at the stakes or not.
AC: The snow sampling was performed every third day. Manual reading of the snow height at the stakes did not always correspond to the snow sampling dates. We will clarify this in the text.

l.201 : Reference to Libois et al. (2014) might be relevant here (Figure 2 for instance) or elsewhere

AC: We will add this reference.

l.206 : any insight/reference about the quality of ERA 5 snowfall reanalysis over Greenland?

AC: We will add a reference to Delhasse et al. (2020) showing that ERA5 provides reliable near-surface variables (2 m temperature, 10 m wind speed, energy downward fluxes) for the Greenland Ice Sheet. We find agreement in terms of timing for the ERA5 snowfall product and our documented snowfall (Fig. 4c in the manuscript); however, ERA5 shows slightly more occurrences of snowfall than we noted down. We might have missed snowfall for example when it snowed only during the night.

l.216 : not clear whether the consistency is in terms of snowfall occurrence or amount

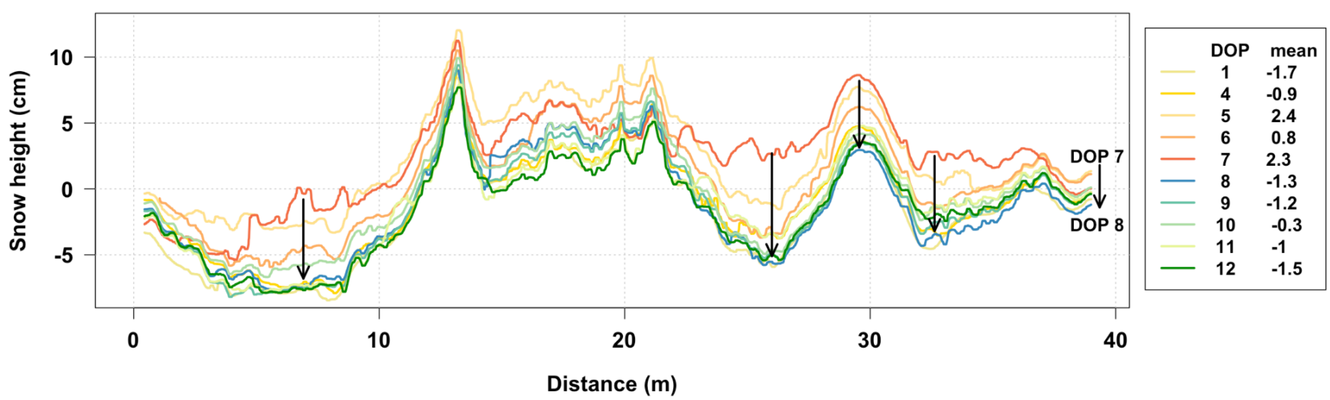
AC: With ‘timing of snowfall’ we refer to the occurrence, not the amount.

l.217 : not clear how 0,6 cm should be read in Figure 4c

AC: The amount of 0.6 cm snowfall is derived from the data presented in Figure 4c and converted from mm w.eq. to cm of snowfall (not shown in Figure 4c). We will clarify this in the text.

l.218 : it’s hard in Figure 5 to see the successive lines. Maybe consider changing color type when erosion occurs

AC: We agree that the color code in Figure 5 is not intuitive. We are currently experimenting with different color types and /or to add arrows indicating when erosion starts or occurs (see Figure 1).




Potential new Figure 1: Relative horizontal snow height profiles (20-point running median, averaged in y-direction from the 2.5m-band) along the x-axis of the study area. Different colours represent different days from DOP 1 to DOP 12 as well as respective mean snow heights in cm, both shown in the legend. Snowfall caused an overall snow height increase from DOP 1 to DOP 7, followed by an erosive event removing the new snow, and exposing the previous surface structure again. Arrows indicate the erosional decrease in the snow height from DOP 7 to DOP 8.

l.222 : a bit unclear, maybe reformulate : " . . . on one fixed day and that on any other day "
AC: Will be changed as suggested.

l.224 : the link between RMSE decrease and erosion is not strict. At least RMSE can decrease without erosion (by smoothing for instance). An interesting quantity could be the RMSE between successive DEMs, after removing mean deviation. Maybe RMSD (deviation) would be more appropriate than RMSE here.
AC: We agree that the figure is not yet as intuitive as we are aiming for. We picked the RMSE between DEM's (thus also including the mean deviation) to demonstrate that old layers seem to reappear, thus the mean value gets closer but also the shape, thus the correlation increases. We are experimenting with other metrics to make the figure more intuitive.

l.232 : not clear what this area is because it often takes a different name in the Figures and in the text. Is it the full domain or only the 0,5 m band ? Maybe give it a name, like x-transect, or "area A"
AC: It is a good suggestion to name the different surveyed areas. We will add the name '2.5m-band' for the band from y=2.5 m to y=3 m in the description of the study area and use it throughout the manuscript.

l.246 : roughness has already been defined earlier
AC: We will remove the repetition here.

l.246 : not clear where the wind parallel line is (what x ?) and whether 50 cm refers to the length of the segments, in which case why is that different than the 2,5 m used in the other direction ?
AC: The line parallel to the wind is the y-axis while the x-axis is oriented perpendicular to the main wind direction. For clarity, we will indicate the axes in Figure 1. For the surface roughness estimate parallel to the main wind direction (i.e., along the y-axis), the length of each single segment is 2.5 m (from y=1 m to y=3.5 m).

l.251 : decrease with time
AC: Will be changed accordingly.

l.258 : where does this 1,3 cm come from ?
AC: The accuracy of 1.3 cm is derived from the method validation in section 2.3 and the validation section in Appendix B and C. We will mention the final uncertainty of 1.3 cm here again to highlight this value.

l.259 : given the acquisition is probably fast, acquisition could be more frequent than daily. Maybe remove this detail
AC: Yes, it is possible to acquire images more often than once a day since the data acquisition takes only about 30 to 45 minutes, including the walking time to and from the study area. We will remove this detail.

l.261 : provide references for the 40m$^2$ and 110m$^2$
AC: The sizes of 40m$^2$ and 110m$^2$ are taken from Picard et al. (2016, 2019), respectively. We will add the references next to the numbers.

l.263 : Ok, but what's the rationale of having such a particular study area (by the way, it'd be helpful to explain earlier on how these dimensions were chosen/constrained, as a square area would be more understandable), compared to a circular area ?
AC: A circular area from a laser scanner has the disadvantage that the location of interest cannot be changed as quickly as in our setup. Furthermore, the laser itself is a high obstacle which can influence the snow re-distribution and can thus affect the natural snow accumulation conditions. We will extend the discussion here.

l.266 : the main disadvantage remains the fact that you need an operator, although this could probably be made automatic somehow. What would be the result if only 2 cameras were used in an automatic way ?
AC: Indeed, the need for an operator is a large disadvantage. Using only two cameras would probably result in a very small surveyed area since the field of view of one image does not cover an area of 20 m length with a sufficiently high resolution allowing a DEM generation. We will add the limitation, i.e. the need of a human operator, to the evaluation of our method.

l.273 : maybe clarify the human errors, which could be helpful to readers interested in deploying the same kind of instrumentation
AC: Thank you for pointing out that a more detailed description of the human errors can be helpful for readers. We will add more details on this.

l.277 : this title is not clear, maybe just remove reliable
AC: We do not agree with the reviewer here. In this section, we present an optimal measurement setup including number of sticks and their spacing to reliably determine snow accumulation, which we derive from our study area that has been surveyed at high spatial resolution. We therefore would keep the title as it is.

l.288 : please describe where the stakes are placed in these simulations(random distribution, lines etc.)
AC: We use the line at y = 2.5 m as reference and place the simulated number of stakes on this line with the given spacing (no random distribution). We then use all possible combinations with the chosen number of stakes and spacing. Depending on the number of stakes and spacing, we have a changing number of possible combinations. We will describe the approach in more detail in the manuscript.

l.295 : it seems that spacing beyond 5 m is useless in your case, which might be worth pointing. Then, consider providing suggestions, for instance how to maximize the accuracy with a minimum of stakes.
AC: Yes, based on our simulations, a spacing beyond 5 m is not improving the estimate of the snow height change. We will emphasise this in the text. Moreover, according to our experiment,

seven sticks with 5 m spacing deliver the optimal minimum setup of stakes (as mentioned in the manuscript).

Figure 9 : Is the RMSD computed on a different number of mean values for different spacings ? Maybe clarify this
AC: We will clarify our approach in the manuscript. Depending on the chosen number of sticks and spacing, we have a different number of possible combinations to put the stakes on the line. Each combination provides a mean snow height change using the chosen number of stakes. We then calculate the difference to the reference snow height change (all points along the y = 2.5 m line) resulting in the same number of differences as possible combinations. From these differences, we then calculate the RMSD.

l.307 : wind speed during the observation period could be advantageously used to explore the drift/deposition events
AC: The wind speed during the observation period is illustrated in Figure A1 in the appendix of the manuscript. During the course of the analyses for this manuscript, we also studied the relation between wind characteristics (speed and direction) and changes in the snow height. However, we did not identify any clear links between these parameters. Therefore, we did not add more details on a possible relation between wind characteristics and snow erosion/deposition in the manuscript.

l.324 : “final snow accumulation” not clear, because precipitation probably governs the final (end of season or yearly average) snow accumulation, but not high frequency variations.
AC: We will clarify this by adding "final snow accumulation during the observation period".

l.325 : 290 kg m-3 seems a bit large for fresh snow. Could you provide more details on how this value was chosen
AC: The snow density of 290 kg m-3 is derived from daily density measurements along the SSA transect. At each of the ten stick positions, the snow density of the top 2.5 cm of snow is measured in addition to the specific surface area (SSA). Snow density data are not further used in this study. We will clarify the density value in the manuscript.

l.327 : how do ERA5 data suggest that build up is very irregular in time? Not clear
AC: We refer here to the results from the DEM data (as seen in Figure 10) and the comparison of these to the ERA5 snowfall data. We will clarify it in the text.

l.329 : it’d be helpful to know what “local” means for climate studies, and how far can snow be transported in the study area
AC: We agree that the definition of "local" in this context is not straightforward. We will specify local climate signal by adding “local air temperature”. Furthermore, we analysed different resources to estimate the distance of transported snow; unfortunately, we did not find a conclusive distance for snow transport.

l.339 : consider providing the range of snow ages at the end of the observation period
AC: It is a good point to investigate the range of theoretical snow ages at the end of our observation period. Analysing the (virtual) top 5 mm of the snow surface indicates that the average day of accumulation is DOP 66.3 (with a standard deviation of 7.4 days, total range between DOP 55 and DOP 78). Real snow ages are, however, difficult to determine because we cannot distinguish between freshly precipitated and eroded or drifted snow which would already have an older age. We will add information on the (range of) snow age in the manuscript.

l.339 : does the snow sampling provide valuable information with regards to the spatial heterogeneity of the layering ?
AC: Stable water isotopes are measured in the snow samples, but the data are not analysed yet. Thus, we cannot conclude on the (isotopic) heterogeneity of the layering. Adding these data would be beyond the scope of this manuscript.

l.345 : the layering does not record each precipitation event, but when snow settles down as a single layer, it probably contains snow with different ages. Somehow there is a “snow reservoir” in between precipitation and settlement, which is fed by precipitation and at some point is incorporated to the snowpack.
AC: Many thanks for these thoughts. It is correct that a single layer, which can consist of precipitated and drifted snow, may contain snow with different ages which probably has implications for the stored climatic signal in the respective snow layer. We will include this thought in the discussion.

l.363 : this idea has already been discussed
AC: We will reword this section to avoid repetition.

l.363 : how much is strong ? Would you have references (if no measurements) regarding snow transport to compare scales ?
AC: Stable water isotopologues, density data and accumulation rates show large interannual variations on local but also larger scales of e.g. 450 km in North Greenland (e.g. Schaller et al., 2016). The authors also mention the importance of the smoothing of the snow surface. However, data regarding snow transport are difficult to obtain and we have no references about these scales for Greenland. We will extend our discussion at this point in the manuscript.

l.371 : could the images be used to identify very local re-deposition (within the same observed area) ?
AC: In our opinion, the images are not sufficient to identify local re-deposition within our study area. We cannot definitely link a re-deposited snow particle to its origin from a spot within our study area.

l.390 : are you sure that your observation of dunes vanishing in Summer is representative ? Could it be that you studied a singular year ? Were the wind statistics in agreement with longer term observations?

AC: As we only have this single season, we cannot be sure. However, a study from Summit, Greenland (Albert and Hawley, 2002), showed a similar behaviour of vanishing dunes during summer. Our observation period is comparable with regard to wind speed and direction as shown by the wind statistics from the nearby AWS for the years 2017 to 2019 (see Figure 2 and 3). We also compare the wind speed frequency during our observation period to the winter months (December, January, February) and see that the winter months generally show higher wind speeds (see Figure 4) which can enhance the formation of sastrugi. Thus the meteorological data would support the hypothesis that what we see is a representative observation.

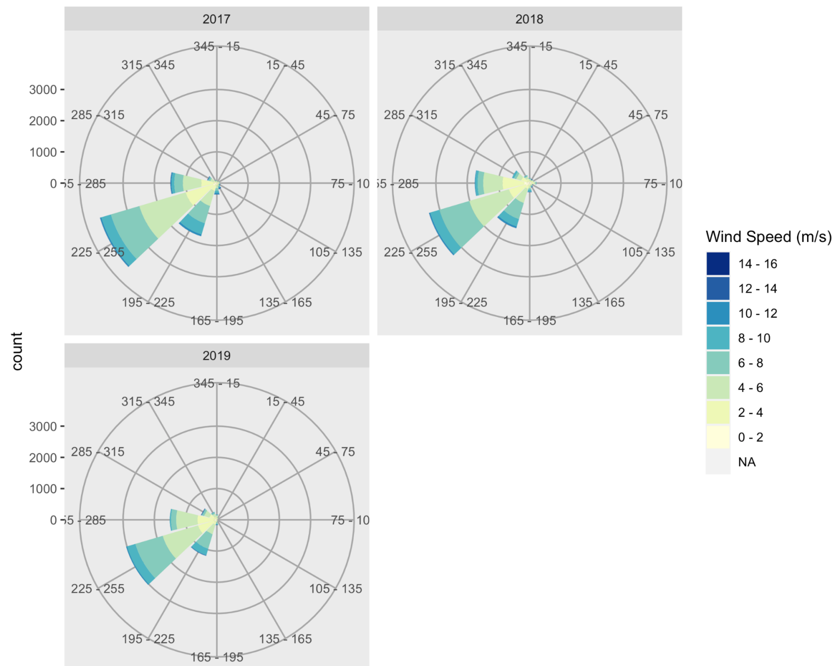



Figure 2: Hourly wind characteristics (speed and direction) for the years 2017, 2018 and 2019 recorded by the nearby PROMICE AWS.

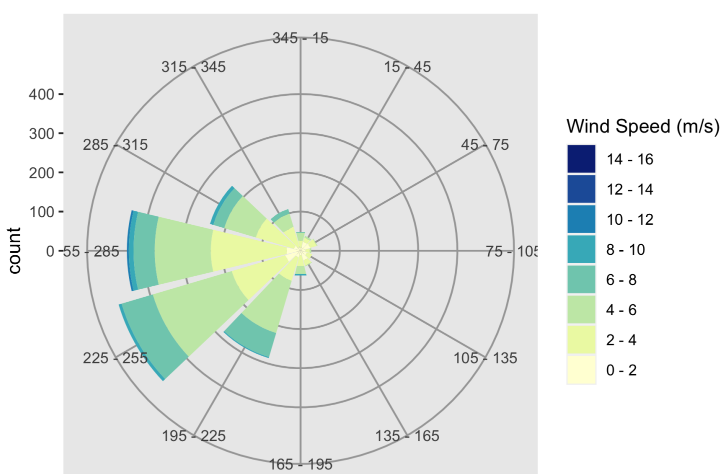



Figure 3: Hourly wind characteristics (speed and direction) for the observation period from 16th May to 1st August 2018 recorded by the nearby PROMICE AWS.

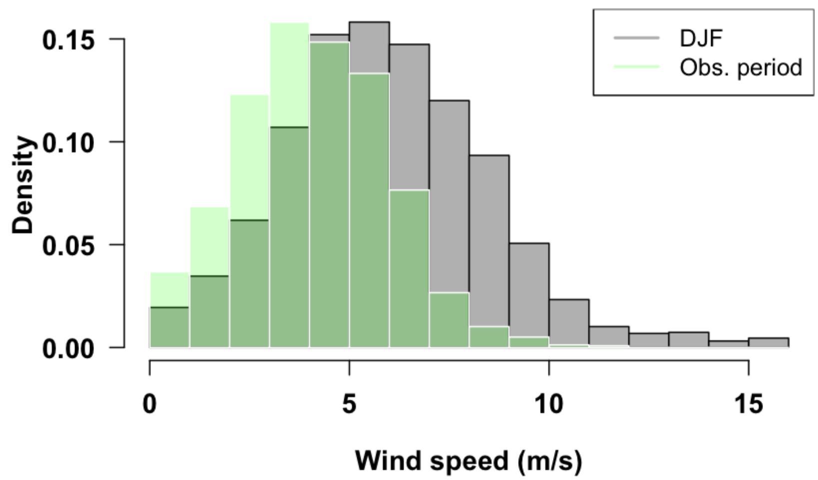



Figure 4: Hourly wind speed data from the nearby PROMICE AWS for two different time periods. Wind speeds during the winter months of 2017 to 2019 (December, January, February; DJF) are compared to wind speeds during our observation period (16.05. - 01.08.2018). The winter months are characterised by higher wind speeds with a mean of ~6 m/s while the average wind speed during our observation period was 4.1 m/s.

l.478 : missing beginning of sentence

AC: Thank you for pointing this out. We will update the acknowledgements.

References

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