Review of « Local scale depositional processes of surface snow on the Greenland ice sheet », by Alexandra M. Zuhr et al.

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 m^2). 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.

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.

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.

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.

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.

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.

l.26 : detail briefly how isotopic composition can be changed

l.28 : "larger" is not clear

l.31 : maybe remove "deposited"

l.39 : "mapping" is not clear. Do you mean in space or time ?

l.39 : why is surface roughness important here ?

l.40 : I think precipitation intermittency is completely independent of surface processes, such that accumulation intermittency and precipitation intermittency are two distinct things

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

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

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

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.

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

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

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

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

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 390 m² and 195 m².

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 10 m 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 photgrammetry area ? Add the sledge and orientation of the camera

l.76 : "around" does not suggest the sticks are put on a line. Are they ?

l.79 : why "almost"? Are the missing days due to technical issues or were they planned ?

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 ?

l.92 : y=10 m was not properly defined, hence this sentence is hard to understand

Figure 2 is hard to relate to Figure 1. Consider adding the footprint of the camera to help

l.95 : does it mean that only a transect is used instead of the full 2D domain ?

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 ?

l.105 : "summarised" is unclear. Averaged ?

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 ?

l.110 : how is snowfall documented and how are samples collected?

l.111 : I assume snowdrift can be difficult to distinguish from snowfall in human observations as well

Table 1 : why 30 PT sticks here and not 35 ?

l.115 : show**s**

l.121 : not clear what peak-to-peak means, probably the difference between max and min ?

l.131 : why cannot it be done on the main study area ?

l.133 : redundant with just a few lines above

l.140 : it is not clear what additional information this section provides compared to the previous sections

l.152 : why was not this sensitivity study performed directly on the study site ?

l.170 : *sufficient* accuracy

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

l.180 : it seems that on Panel 2 of Figure 3 the dunes have already vanished

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 (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.

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

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

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

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

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

l.222 : a bit unclear, maybe reformulate : " … on one fixed day and that on any other day "

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.

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"

l.246 : roughness has already been defined earlier

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 ?

l.251 : decrease **with time**

l.258 : where does this 1,3 cm come from ?

l.259 : given the acquisition is probably fast, acquisition could be more frequent than daily. Maybe remove this detail

l.261 : provide references for the 40 m^2 and 110 m^2

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 ?

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 ?

l.273 : maybe clarify the human errors, which could be helpful to readers interested in deploying the same kind of instrumentation

l.277 : this title is not clear, maybe just remove reliable

l.288 : please describe where the stakes are placed in these simulations(random distribution, lines etc.)

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.

Figure 9 : Is the RMSD computed on a different number of mean values for different spacings ? Maybe clarify this

l.307 : wind speed during the observation period could be advantageously used to explore the drift/deposition events

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.

l.325 : 290 kg m^{-3} seems a bit large for fresh snow. Could you provide more details on how this value was chosen

l.327 : how do ERA5 data suggest that build up is very irregular in time? Not clear

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

l.339 : consider providing the range of snow ages at the end of the observation period

l.339 : does the snow sampling provide valuable information with regards to the spatial heterogeneity of the layering ?

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.

l.363 : this idea has already been discussed

l.363 : how much is strong ? Would you have references (if no measurements) regarding snow transport to compare scales ?

l.371 : could the images be used to identify very local re-deposition (within the same observed 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 ?

l.478 : missing beginning of sentence

References

Libois, Q., Picard, G., Arnaud, L., Morin, S., & Brun, E. (2014). Modeling the impact of snow drift on the decameter-scale variability of snow properties on the Antarctic Plateau. *Journal of Geophysical Research: Atmospheres*, *119*(20), 11-662.