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Interactive comment on "How does the ice sheet surface mass balance relate to snowfall? Insights from a ground-based precipitation radar in East Antarctica" *by* Niels Souverijns et al.

Anonymous Referee #2

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Review paper from Souverijns et al. TCD (2018)

This paper presents a compilation of data from a set of instruments (Micro Rain Radar, ceilometer, Automatic Weather Station, among others) over a time period of 37 months, at the Princess Elisabeth (PE) Station, Antarctica. The authors analyse the different situations leading to accumulation and ablation at this site and conclude that "SMB records cannot be considered a good proxy for snowfall at the local scale". The authors compare the typical accumulation and ablation situations to results from a cluster analysis that describes the main weather situations at PE. Results suggest that a large part of the accumulation/ablation at the station takes place after the main precipitation

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events because "the fresh snow is easily picked up and transported in shallow drifting snow layers to more inland locations", even when wind speed is relatively low (< 7 ms−1). This latter conclusion is intriguing because it suggests that snow drift may transport snow to more inland locations (here I understand upslope), which is clearly the opposite to present knowledge. Indeed, in Antarctica, it is assumed that snow is mainly drifted downslope due to the occurrence of katabatic winds and that significant amounts of snow are even drifted away from the ice cap to the ocean (Scarchilli et al., 2010; Palm et al., 2017). If the authors' conclusion is true, this process may have large consequences on our vision of snow transport and on accumulation processes in the coast to plateau transition zone. As a consequence, a clear demonstration of this finding is key in the present study, but in the present form, I am not convinced by their demonstration and several verifications are required. In particular, the authors should refer to results from Libois et al. (2014) who performed snow height measurements at Dome C, Antarctica, and demonstrated that the motion of sastrugis during low winds may induce local snow accumulation or erosion at a decameter scale, leading to local snow height variations which seem to be very similar to the accumulation/erosion events presented in the present paper. Libois et al. (2014) suggest that "at each drift event, significant amounts of snow are deposited over approximately 20% of the total area only". This justifies that small amounts of deposited snow may move locally with low winds, and may accumulate in another location nearby, after the end of the precipitation event. If my suggestion is true, then the snow is only moved locally and not transported from (or to) remote areas.

This paper is well written and presents an interesting comparison between different sensors, but key information is lacking and new results (when compared to the previous publications) should be better highlighted: 1. The authors do not sufficiently present the methods, measurements and their uncertainty, and the reader has to refer to previous papers from the same team in order to find the information (van Tricht et al., 2014; Gorodeteskaya et al., 2013, 2015, Souverijns et al., 2017; Gossart et al., 2017). In particular, the sensor uncertainties are not presented, which impedes getting an accurate idea of the quality of the results => Please give more information on sensors and methods in the manuscript.

In particular, estimates are made in mm w.e. but there is no information on snow density at site. What is occurring with density changes caused by snow aging? What is the final uncertainty in ERds estimates? What is the representativeness of one acoustic gauge measurement at decameter or kilometer scale? Is it physically justified to compare local acoustic gauge measurements with radar data that are integrating precipitation amounts at a kilometer scale? Are there any differences between precipitation estimates from Gorodetskaya et al., 2015 (where the ceilometer and the MRR data are combined to analyse precipitation and clouds statistics) and those presented here => please clarify these points

2. Almost all the ideas and conclusions have been presented in Gorodetskaya et al. (2013, 2015). The main interests here are 1) the extent of the dataset which allows to make a statistical analysis on a long time scale, 2) the statistical approach (cluster analysis) made to retrieve the main weather situation at PE. However, this cluster analysis is different from the one presented in previous studies and it leads to a different weather atlas. Please justify the differences. Does the choice of cluster have an impact on present conclusions?

3. The main conclusion (Accumulation and ablation also occur during non-snowfall conditions) is already presented in Gorodetskaya et al. 2015, but the new idea is that snow may be transported from low lying regions => Please refer to Libois et al. (2014) paper, and try to see whether their analysis may help in understanding the snow height changes observed during low winds at the PE station.

4. Several conclusions rely on assumptions made on the blowing snow processes, but these processes are not discussed according to current knowledge based on drifting snow measurements (Li and Pomeroy, 1997; Nishimura and Nemoto 2005; Nishimura and Ishimaru 2012; Scarchilli et al., 2010 ; Libois et al., 2014; Trouviliez et al., 2014,

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Barral et al., 2014, Amory et al., 2015, 2016, 2017; Das et al., 2013). In particular, these publications present important information on the typical threshold wind speeds for snow transport. They also already discuss the impact of snow density, of snow aging, and of the sintering temperature on the threshold wind speeds. Finally, they present key information on the link between the “mean slope in the wind direction” and erosion/deposition processes. This knowledge is important to assess whether large drifting snow fluxes may be observed with low winds for fresh snow. Please refer to these publications and discuss the results accordingly.

5. The situation of the PE station is not discussed in the text, but the station is located at the lee of a rock crest. Are these results site specific? Is there any information on the distribution of accumulation at a kilometer scale around the station (with stakes and acoustic gauges). What is the snow distribution proposed by a regional scale model such as RACMO2 at that site?

To summarize, the dataset is really interesting in particular for model validation and deserves to be published. However, in its present state, this paper is not a sufficiently new contribution compared to (Gorodetskaya et al., 2013, 2015). Before publication in the Cryosphere, I suggest that the authors compare their results to the available outputs from the RACMO2 regional scale circulation model. This model includes snowdrift processes and outputs may allow “validating” their assumption. If the authors prefer the use of another model, of course, it would make sense. For instance, Stefaan Lhermite is co-author of the present paper and already published a paper using the wind transport submodel SnowTran-3D (Groot Zwaaftink et . al. 2011; Gascoin et al., 2013) => Is it possible to use this model here in order to see whether the assumption made on snow transport during low winds is physically supported or not?

As a conclusion, I suggest the authors to make major revisions on their paper. I propose:1) to describe the differences with Gorodetskaya et al. (2013, 2015) and to develop the paper around the interest of the new weather atlas presented here, 2) to present more clearly the accuracy of sensors and of their estimates, and (accounting for the uncertainty of estimations) to critically discuss the variations of the surface level measured with the acoustic gauge, 3) to discuss the potential discrepancies between signals which are describing very different spatial scales, 4) to propose a comparison with a model including drifting snow processes (for instance RACMO2) allowing to give clear evidence on the occurrence of drifting snow events transporting snow from lower altitude areas OR 5) to consider that sastrugis may move locally when the wind is not very strong (Libois et al., 2014). According to their opinion (point 4 or 5), I propose that the authors say whether processes only impact local snow redistribution or impact the regional scale snow redistribution.

Minor comments

Abstract ilne 8: please remove "an unprecedented"

Lines 9-11:The authors write: "However, snowfall events are much more common than accumulation events. During 38% of the snowfall cases observed, the freshly-fallen snow is ablated by the wind during the course of the event. Generally, snow storms of longer duration have a higher chance to attain for accumulation at the local scale, while shorter events usually attain for ablation" This conclusion is very similar to the following one given in Gorodetskaya et al., 2015: "Large accumulation events (> 10mmw.e. day-1) during the radar-measurement period of 26 months were always associated with snowfall, but at the same time other snowfall events did not always lead to accumulation.". Please clarify which are the new conclusions in the present paper.

Line 12: "As such, SMB records cannot be considered a good proxy for snowfall at the local scale." => Considering the decameter to kilometer scale variability of the SMB, or alternating of megadunes/wind glaze' areas for instance, this conclusion is trivial. Please reformulate.

Lines 13-15: "when preceding snowfall events were occurring in upstream coastal areas. This fresh snow is easily picked up and transported in shallow drifting snow layers to more inland locations, even when wind speed is relatively low (< 7 ms−1)." =>

please refer to (Libois et al., 2014) where a potential explanation of such variations without precipitation is proposed. If the authors do not concur with these explanations, please demonstrate clearly that the snow can be transported upslope.

Introduction: Page 1 - Line 20 "an important regulator of the present and future global climate and water cycle" => I don't understand this sentence. The oceans are currently a regulator since they absorb 93% of the global warming, but what do the authors mean with the word "regulator" in the case of Antarctica?

Page 1 – Line 22: what do you mean with coupled climate/surface models? Do you mean GCM? Coupled AOGCM? AOGCM coupled with dynamic changes in ice surfaces. The cited publications are referring to very different models and do not clearly indicate what kind of model the authors are referring to.

Page 2-Line 5: please refer to papers from Grazioli et al. (2017a&b) because these paper also offer new information on precipitation measurements in Antarctica.

Page 2, Line 8: the authors cite: (Vaughan et al., 1999 and Magand et al., 2007). However, the database from Vaughan et al. (1999) has been updated by Favier et al. (2013) and Wang et al. (2016).

Page 2 Line 9: "Gorodetskaya et al. (2015) were the first to quantify the different terms" => "Gorodetskaya et al. (2015) quantified the different terms"

Equation 1: I do not understand how "Sublimation of the drifting snow" and ERds may be separated? if a snow flake is sublimated while it is drifted, this means that it has been eroded first. Is the mass loss accounted twice in the calculation? i.e., is it accounted for (1) in sublimation and (2) in ERds? please explain this formulation

Page 2, line 18: stake measurements and ice cores => please refer to (Thomas et al., 2017)

Page 2, line 19: "the separate measurement of any of the components of the local SMB is considered a difficult task" => please also refer to (Eisen et al., 2008 ; Gorodetskaya

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et al., 2015; Amory et al., 2017 ; Grazioli et al., 2017)

Page 2, line 31: "Wind-induced accumulation / ablation by drifting / blowing snow over the AIS has an important impact on the local SMB" => please refer to (Palm et al., 2017)

Page 2, line 34: "using a network of snowdrift instrumentation (Leonard et al., 2011)" => please also refer to (Nishimura and Nemoto 2005; Nishimura and Ishimaru 2012; Trouvilliez et al., 2014 ; Amory et al., 2017)

Page 2, line 35: "Neglecting this term might however lead to significant errors" => How much?

Page 3, line 10: "The total SMB or snow height can be measured by an Automatic Weather Station (AWS)," => this sentence is not correct because AWS do not offer any information on snow density. More generally there is no information on snow density in the present paper. Do the authors consider the same value of density all the time? How do the authors consider variations in density related to snow aging? Please reformulate and clearly describe how the snow density is considered in calculations.

Page 3, line 19: "noted that snowfall events at the PE station do not necessarily contribute to accumulation or an increase in the height of the local SMB." => The authors write that their main conclusions have already been given in previous publications. Please describe what the new insights are? For instance, the present study may offer a more robust estimation of the frequency of different events.

Section 2.1: page 3, line 28: "In order to gain insight in the relation between snowfall and the SMB, reliable, high-frequency and long-term in situ observations are indispensable."=> repetition with the introduction. this sentence may be removed

Figure 1: the P-E station appears to be close to a blue ice area (located southward) and in the lee of a mountain ridge. Is there any impact of the location on local accumulation? For instance, when the wind is coming from the south, it is coming from the

blue ice area and the ridge, which are characterized by erosion: snow is not available for transport, but these areas are in the vicinity of the PE station. Conversely, the areas located close to the PE station but in the ENE direction are covered by snow. Erosion and transport from these areas thus occur more easily, attaining for deposition at the PE station. This may suggest that snow is transported only over small spatial scales when the wind speed is low and not necessarily from further low-lying areas, in agreement with the processes described by Libois et al. (2014). As a consequence, the impact on accumulation distribution may be only local. What is the authors' opinion?

Page 4, line 13: "snowfall rates was obtained including a constrain on its uncertainty (Souverijns et al., 2017)." => please give uncertainty values for all the sensors.

Page 4, line 16: "Furthermore, it is equipped by an acoustic height sensor, which is able to measure snow height changes with an accuracy of 1 cm on an hourly time resolution." and later, "resolution. Snowfall events are defined as exceeding the threshold of 1 mm w.e. during the continuous duration of snowfall measured by the MRR (this corresponds to 1 cm of snow when the density of fresh snow equals 100 kg m−3)." =>(1) This threshold may be too high to depict feeble precipitation: do results depend on this threshold? =>(2) In (Libois et al., 2014), they write:"we chose to set the density of fresh snow to 170 kg m−3, which corresponds to the fifth lowest percentile of the measured surface densities at Dome C during the 2012–2013 campaign.". In this study at Dome C, where diamond dust and surface hoar may occur, the fresh snow density is already 70% higher than at PE. Please justify the choice of this very low snow density value (100kg m-3). Is there any impact of this choice on the comparison between MRR precipitation and surface level variations? If higher density values are considered, then the 1cm threshold would lead to neglect events with higher accumulation?

page 5, line 4: "Recently, an algorithm for the detection of blowing snow by the use of a ceilometer was developed"=> in (Gorodetskaya et al., 2015), a combined analysis of the ceilometer and of the MRR is done to get information on clouds and precipitation, here the ceilometer is used to assess blowing snow events? Here, do the authors use the ceilometer to also analyse the clouds characteristics?

page 5, line 8: "The minimal height of the blowing snow layer to be detected by the ceilometer equals 30 m at the PE station."=> please give sensor uncertainty and the impact on the blowing snow flux estimate.

page 5, line 9: "Surface and snowdrift sublimation are quantified using the approach of Thiery et al. (2012)" => please give uncertainty values

page 5, line 18: "which is unprecedented for the Antarctic region."=> There are many other similar datasets in Antarctica available for many years but with different sensors and focus (e.g. the AWS network). Please remove or reformulate.

Page 5, line 22: "Nevertheless, since the AWS measures the total SMB directly, ERds can be calculated as the residual term after inverting Eq. 1" => this calculation indicates that ERds values include the sum of all other uncertainties. What is the accuracy of this term? Moreover, the different sensors used here refer to different spatial scales (kmˆ2 scale for the MRR and ceilometer, but a few mˆ2 for the ultrasonic gauges). Is it physically justified to directly compare these very different scales? In order to accurately compare the MRR/ceilometer data with surface height data, it may be necessary to consider snow height variations given by multiple sensors distributed over the area. In particular, referring to Libois et al. (2014) publication, is it possible that snow accumulates in depressions or along small snow barchans, which may move due to snow reptation? For instance, please observe the 3 large increases and decreases of the surface level in January, February and March 2012 (Figure 2).

Section 2.3 Page 6, Line 12: give more details on the method, and particularly on measurement uncertainty.

Page 6, line 30: "The SANDRA optimisation algorithm was selected to perform this cluster analysis," => Please explain the interest of this new cluster analysis. Why is it more interesting and robust than the one used in Gorodetskaya et al. 2013? The weather atlas presented in the two different papers looks different, please justify these differences.

Page 7, lines 3-6: "a total of nine circulation patterns was selected. [. . .] during the four types of events attaining for a change in the local SMB" => in figures, 6 patterns are presented. Please clarify. Which are the four main patterns within the 6 patterns?

Page 7, line 13: "are converted to water equivalent values"=> please indicate how density changes are considered in the first snow layers?

Page 8 Figure 2: This figure is similar to Figure 9 from (Gorodetskaya et al., 2015). I suggest the authors to present their results over the whole 37 months

Page 8 Line 5: did the authors compare Precipitation estimates from ERA-Interim with precipitation from the MRR? is it possible to consider that interpretation of the cluster and backtrajectory analyses are robust If ERA precipitations do not fit with field measurements?

Section 3.3 Page 8, line 10: "in total 50 independent snowfall episodes were detected attaining for at least 1 mm w.e. at the PE station" => Libois et al. (2014) present a statistical analysis and write that: "Under the ergodic hypothesis, we conclude that at each drift event, significant amounts of snow are deposited over approximately 20% of the total area only". This suggests that there is only a low probability to capture snow accumulation during drifting snow events with only one acoustic gauge. Do the authors believe that their results may change if they had access to the mean variations given by 3 or 5 sonic gauges separated by 10m from each other?

Page 9: Figure 3. Cases 3 and 4 look very similar. Please indicate more clearly in the text and in captions which are the Cases C1, C2, etc. . . This interpretation of the cluster should be strengthened in the text and compared with previous papers. Do the conclusions depend on the clusters analysis?

Page 11, line 12: "From the total number of snowfall events, 31 resulted in accumulation

(62 %), while 19 led to ablation (38 %)." => is there any relationship with precipitation intensities or amounts given by the MRR data and accumulation/ablation occurrences?

Page 12, line 5: please discuss this paragraph considering conclusions from Libois et al. (2014)

Page 12, line 16:"During all snowfall events, a total amount of 230 mm w.e. (approximately 230 cm in case fresh snow density equals 100 kg m−3) was registered by the MRR, which is lower than the height changes recorded by the AWS (542 cm)." => what is the accuracy of both sensors? please also discuss the impact of snow density value on results. Is it possible to consider that the AWS is located in a small overaccumulation zone (due to gravity waves at the lee of the mountain crest for instance)? I propose the authors to discuss these differences.

Page 13, line 1: "Accumulation records are therefore not advised to be used as a proxy for snowfall over East Antarctica." => please, replace "accumulation" by "punctual accumulation". Indeed, I suppose that the conclusion would be different if the authors had access to many stake/ice cores/sonic gauge data over a large area.

Section 3.4 Page 13, line 6:"snow displacement events also occur without the presence of snowfall." => what is horizontal scale here? Deca-centimeter? Kilometer? Deca-kilometer?

Page 14, Line 2:"limiting the regions that receive snowfall to coastal areas" => how do the authors observe that precipitation occur in coastal areas? Do they use ERA-interim outputs?

Page 14, line3: "to be limited to the coastal areas, not reaching the PE station" => I suppose that the MRR data are used in order to reach this conclusion. However, Gorodetskaya et al., (2015) write:"While MRR misses the most feeble precipitation (virga or snowfall) with Ze < -8 dBz, its sensitivity is sufficient to detect typical precipitation at the site. [. . .] and ice clouds or weak precipitation not detected by MRR (22%

of the total period or 63% of the overcast)." => I suppose that feeble precipitations are not always detected by the MRR. If it were the case, the MRR would not be sufficient to demonstrate that no precipitation occurred. Please clarify.

Page 14, line 16: "A fraction of 46 % of the ablation events take place within 24 hours after a snowfall event. As stated in Sect. 3.3, snowfall events are mainly characterised by a ENE flow (Fig. 4a)."=> do the authors estimate the “reptation” velocity (e. g., Nishimura et al., 2014) in order to estimate the distance over which the snow particles were transported until reaching PE station?

Page 15, line 8: "the occurrence of accumulation / ablation at very low wind speeds (Fig. 7). This points out that the time since the last snowfall event and the amount of low-density fresh snow that is available is of much higher importance than the wind speed in order to attain for blowing snow" => this conclusion is intriguing if we consider that friction velocity has to exceed a threshold friction velocity in order to allow wind drift occurrence. If the wind speed is too low, snow saltation is not possible (e. g., Nishimura and Nemoto, 2005). Please discuss this point considering current knowledge on blowing snow processes (e. g., Nishimura and Nemoto, 2005).

Page 15, lines 15-20: "A total of 1125 cm in SMB changes was measured by the AWS during the events. The ceilometer was only able to detect 274 cm, indicating that the blowing snow layer only has a limited vertical extent and that the transport of snow is restricted to shallow layers close to the ground during these type of events." => I suppose that blowing snow events with a limited vertical extent are transporting weak amounts of snow. How can this type of events explain the difference between 1125 cm and 274 cm?

Furthermore, it is noted that "ablation and accumulation can significantly compensate each other" => please discuss this paragraph considering Libois et al. (2014) results.

Page 15, line 10: please cite (Amory et al., 2017).

Page 15, line 25: please cite (Nishimura and Ishimaru 2012 ; Trouvilliez et al. 2014)

Page 15: the first paragraph of the conclusion is not really necessary (repetition of the introduction)

Page 16: "Meteorological conditions during snowfall, accumulation and ablation, were indicated, including their impact on the local SMB, which was largely unknown up to now." => Gorodetskaya et al. (2015) already proposed estimates of each SMB component. Please clarify the differences between both estimates, and justify why the present estimate is more accurate than the one proposed by Gorodetskaya et al. (2015).

In the references: (Leonard et al., 2011) => (Leonard et al., 2012) Please include missing information in (Stohl et al., 1995)

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