

Interactive comment on “Drifting snow statistics from multiple-year autonomous measurements in Adelie Land, eastern Antarctica” by Charles Amory

Anonymous Referee #1

Received and published: 19 November 2019

The manuscript describes a unique long dataset of blowing snow observations in Adelie Land, Antarctica, using weather stations that contained FlowCapt sensors for detecting blowing and drifting snow particles. Given that the surface mass balance in Antarctica is dominated by wind erosion and deposition (and thus blowing snow), in-situ measurements are very valuable. It's important that the manuscript is published, describing the setup, and providing some first analysis of frequency of blowing snow events, to increase the value of the dataset. The dataset may become useful for a broad community, including remote sensing and ice sheet (climate) modelling. I think the paper is well suited for TC. I have some minor comments which may help to improve the paper. A major concern I have is that the data is only available from the author on request,

whereas I think that the Copernicus Data Policy strongly discourages this. Using for example zenodo and doi versioning, the current dataset could be deposited there, and updated with newer data as a newer version of the data.

General comments

1) A better discussion of the accuracy of FlowCapt is required. In section 2.3, L140, the sensor is described as being accurate, and the only reference is Cierco et al. 2017). However, they write: "As a consequence, and even if the sensor provides good information in operational use, a regrettable inaccuracy in the collected data prevents the use of such measurements for research purposes. Nevertheless, a correction algorithm based on a statistical calibration of the sensor is proposed, which should make it possible to use the recorded data for preliminary approximations." In L151-153, it is discussed qualitatively, but can quantitative error margins be established? In any case, this section needs to be expanded, with a more detailed accuracy assessment of FlowCapt sensors.

2) It is not clear what the measurement heights were, particularly for D17. Also, it should be made clear throughout the manuscript which measurement level is used for wind speed, temperature and RH, at D17 when plotting or analyzing.

3) Fig. 2 shows that drifting snow occurs with lower wind speeds in D17 than in D47. Is this because the surface snow density, or bond strength is lower at this site? Lower average wind speeds can be associated with lower surface density. Or are the two measurement heights not comparable? It would make sense to also discuss the occurrence of drifting snow as a function of friction velocity, as mentioned in L238-239 which is often the variable of interest, determining whether or not snow erosion from the surface occurs. For D17, the profile measurements easily allow for a determination of friction velocity from the 6 measurement heights of wind speed. Similarly, L288, surface roughness could also be quantified from the wind speed profile, which could be used to demonstrate this relationship.

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4) Eq. 2 confuses me, because the summation symbol lacks what it is summing over. At first, I thought that this was just how to compute half-hourly transported mass, but it seems to be for event transported mass and that the summation is over all the time steps constituting an event. In that case, L276 should introduce the definition of drifting snow event, if it is not each half-hourly data interval. Is the first half hourly interval that drops below the limit considered the end of the event? Nowhere in L262-271 is it introduced that there is a switch to the event based analysis.

5) Cierco et al. (2007) mentions that FlowCapt sensors can saturate. Could that be an explanation for the apparent upper bound on snow transport mass (L291-292)?

6) I'm not sure if the Online Supplement is necessary. It seems that the manuscript would benefit from inclusion of most of the materials in the main text. In particular, I think that the map with location of the stations should be part of the main text.

Minor comments:

L7: "hydrological"? Maybe "surface mass balance" is better (see L21-22)?

L8: "punctual" doesn't seem to be the right word here. I suggest: "model and satellite based products".

L13: "The data provided nearly continuously so far constitutes". Maybe add commas for clarification: "The data, provided nearly continuously so far, constitutes"

L25: It's confusing: wind confluence for me is the *convergence* of wind, unless there is compensating acceleration (which doesn't seem to be the case given that D47 has higher wind speeds than D17), so how does this relate to the horizontal *divergence* of snow?

L26-28: "Sublimation of snow particles ..." Please add citations. There are several good papers on drifting snow sublimation out there which deserve citation here.

L35: "are to be found" should that not be "are found"?

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L36: "in the absolute values attributed to the relative contribution of these various mechanisms." Sounds vague.

L40: "3 times fewer" seems to refer to "mass fluxes". But either "3 times fewer" refers to occurrence, or it should be "3 times less".

L43: "The degree of plausibility of model-dependant features". I assume "model-dependent", but the sentence is a little vague.

L80-82: It reads as if the experiments were only in January 2010, and it should be highlighted here that the dataset is much longer. I would recommend writing something like: "For long term data acquisition following the measurement campaign, two distinct locations ..."

Section 2.1: How is the power supply organized? Can there by a bias in data availability depending on power source? (For example, if the battery tends to be drained towards the end of the winter season).

Section 2.1: It would be good to mention here explicitly that D17 is still operative.

Section 2.1: Fig. S2 in the supplement should show the dates on which the photos were taken.

L112: "the drainage of the sinking near-surface air" sounds vague to me.

L113: "over an unobstructed"

L118: "higher incidence of drifting snow", maybe add a reference to Fig. S4?

L119: "combined with"

L118-119: I suggest to reference the accompanying Brief Communication here. "Brief communication: Rare ambient saturation during drifting snow occurrences in coastal East Antarctica"

L160-161: This seems like an appropriate location to introduce Equation 2, instead

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of Section 3.4. At least, I assume that the same correction is made when part of the FlowCapt was buried as done in Equation 2?

L171: Again, I don't think punctual is appropriate here.

L194-195: This statement deserves citations.

Fig. 1, as well as Fig. 2: The figure caption should also mention what the gray shaded areas denote (currently it's only explained in the main text).

L268: Maybe add: "h_ref = 2 m, which is the sum of two 1 m long FlowCapt sensors."

L290: This is confusing. Fig 4, right panel should be a non-log (i.e., linear) scale in order for it to show a linear increase of QT and event duration.

L296-297: Could this be substantiated by showing the two levels separately?

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-164>, 2019.

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