Response to reviewer RC2

I thank the reviewer for his thorough reading of the paper, the comments and the proposed suggestions. My responses are reported hereafter in red.

This paper presents the analysis of 8 yr field observation of drifting snow at two sites D17 and D47 on Terre Adelie Land (east Antarctica). The main tools used in this study are FlowCapt acoustic sensor and associated Automatic Weather Station. The paper contributes to knowledge concerning the measurement of negative term of surface mass balance driven by wind.

The manuscript subject is appropriate for Cryosphere Journal, well written, data and analysis are very important. The data are partially already presented and analysed in previous paper (Trouvilliez et al, 2014 and 2015) and a paper under review on the Cryosphere (Amory & Kittel, submitted) presents the same data under the aspect of the sublimation that is the main issue not discussed in this manuscript. The interpretations of data acquired are supported by the result and the amount of the good data, but the statistic analysis present in the manuscript are not relevant to support the publication on the high quality Journal such as "The Cryosphere" and the Surface Mass Balance condition from the previous studies are not taken adequately in account. The previous paper on SMB survey at the two sites (D17 and D47) and model on the Adelie Coast are not adequately discussed and reported (example: Pettrè et al., 1986; Bintanja, 1998; Pourchet et al., 1997; Frezzotti el ta., 2004, Genthon et al., 2007; Agosta et al., 2011, Favier et al., 2013, Barral et al., 2014 in the reference but is not taken in account in the manuscript; Goursaud et al., 2017). The manuscript does not analysed the Surface Mass Balance and in particular the extensive presence of the blue ice area in the Coastal Terra Adélie (Favier et al., 2011) and their implication on the drifting snow.

The author does not distinguish drift from blowing snow phenomena and the threshold of snow sublimation, and their implication on the mass transport/sublimation and the difference between the two sites. Blowing and drifting snow are not redistribution process, a significant part of blowing snow sublimate as pointed out by snow radar survey (see Frezzotti et al., 2007; Eisen et al., 2008) or satellite survey (Scarchilli et al., 2010, Palm et al, 2011, 2017; Scambos et al., 2012). The AWS and FlowCapt sensor provide single measurement point for limited number of years and must be analysed in the contest of Surface Mass Balance study derived from other field measurements as stakes, firn cores, snow radar profile and satellite studies.

Although rather concise, many issues are raised in this report and I'll try to address all of them. Note that some of the elements of response provided here are redundant with the information provided in the manuscript but are reported here since they constitute key elements of the argumentation.

One major issue raised by the reviewer is the lacking character of the publication concerning surface mass balance aspects, and the discrepancy between the reviewer's expectations and the content of the paper. If the subject of the paper were to investigate the relations between erosion and variability in the surface mass balance in Adelie Land, I should have indeed considered those points raised by the reviewer, I agree, although one would face some serious complications by doing so, as I will discuss it in the following paragraph. But the objective of the paper in the proposed version is different: I aim here at publishing and presenting the drifting snow database while providing some examples of use of the data through a first statistical (temporal and spatial) analysis of drifting snow mass transport and frequency (made possible by the high sampling frequency and the continuous character over the respective measurement periods) and in which emphasis is placed on aspects relevant to the modelling of drifting snow. The interest of the paper partly relies on the quality and the open-access character of the drifting snow database (which has been deposited on zenodo and can be now downloaded at https://zenodo.org/record/3630497; see my response to reviewer RC1), that compiles new observations which are almost inexistent in the extreme and remote Antarctic environment. By making them freely available to the scientific community without condition, the paper is an opening to a larger field of applications, such as evaluation of climate models, simultaneous analysis of ground-based and remotely sensed data, investigations on polar boundary-layer physics or accumulation/ablation processes, each of them belonging to a specific area of expertise and individually warranting a careful, detailed, equally interesting attention.

It is of crucial importance to understand the possibilities offered by the drifting snow data. In particular, the relation between the snow mass flux and ablation at a given area is far from being direct. Snow mass fluxes do not constitute an estimation of local erosion; rather they are the integrated result of all the mechanisms that contribute to the presence, amount and time residence of snow particles in the air, including notably precipitation and advection from upwind areas. Unfortunately the FlowCapt™ sensor does not distinguish neither the source or the geographical origin of the particles impacting the tube, and how much precipitation contribute to the snow mass transport in Antarctica is still an open research question. Therefore these observations must not be perceived as measurements of "wind-driven negative term of the surface mass balance" and cannot be used to quantify local ablation rates without the use of a complementary approach such as numerical modelling. This is the main reason why all the references mentioned by the reviewer are indeed not discussed in the paper since they would lie beyond the scope of the paper. Some of other major concerns that would arise from the multidisciplinary and quite ambitious approach suggested by the reviewer result from the fact that:

- the transect along which stake measurements are indeed performed in Adelie Land is not aligned with the main slope in the wind direction, so any spatial variability depicted in the local SMB signal might not directly correlate to the magnitude of the snow mass flux.

- the fine spatial resolution of the stake networks in Adelie Land demonstrates a high, sub-kilometre spatial variability that couldn't be supported by analysis of mass fluxes performed at only two distant locations, and whose spatial representativeness cannot be assessed in the absence of other comparable measurements in the area.

- the observations presented in this paper are, as you mentioned, "*single measurements point for limited number of years*", moreover performed over the most recent years. A mismatch in timing of several decades can thus be expected with the information contained in deep snow/ice layers sampled through ice cores and radar stratigraphy.

Moreover, temporal and spatial variability of the SMB in Adelie Land has already been quite well investigated (as demonstrated by the long, non-exhaustive list of references mentioned in your report), while drifting snow mass transport has received much less attention. Documentation of spatial et temporal variability in snow mass transport over Antarctica almost exclusively relies on models whose ability to represent drifting snow processes has been extremely limited owing notably to the current extreme scarcity of drifting snow observations. This justifies, in my humble opinion, an initial and independent documentation of the entire database before exploiting further possible connections with other processes and synergetic uses with other products. For the above-mentioned reasons, and because I'm also deeply convinced that keeping the scientific message of a paper as onefold improves clarity, readability and efficiency and thus prevent the paper from being too long with various scientific messages and disconnected sections, I believe that the various applications mentioned by the reviewer are all interesting subjects for separate papers. Finally, note that SMB-related aspects are discussed in a manuscript that I've submitted to Geoscientific Model Development, in which the data presented in this paper have been used in conjunction with the SMB observations in Adelie Land to evaluate the drifting snow scheme of the regional climate model MAR and its ability to represent the variability in accumulation along the transect.

The following comments gather some elements of responses to the remaining comments of the reviewer:

- The driting snow observations are performed far from the blue ice area (i.e. respectively 10 and 100 km away from the ice margin), which covers the very first hundreds of metres of the ice-sheet margin, and relate to different local (topographical) conditions irrelevant to the objective of the paper in its current form.

- As the sensors are installed at both measurement sites so as to cover the first two meters above ground, which is the height conventionally used to distinguish drifting from blowing snow, they do not enable a distinction between drifting and blowing snow.

- Sublimation of windborne snow is determined by the temperature and humidity gradients across the boundary layer between each snow particle and its environment and is proportional to the undersaturation of the atmosphere (Schmidt, 1982). For a given wind speed, threshold values at which airborne snow sublimation becomes significant can thus be expected to vary significantly depending, among others, on the snow particle concentration and thermodynamic properties and structure of the atmosphere, or the dynamical origin of the boundary-layer flow. From this perspective, as a *"threshold of snow sublimation"* sounds quite vague to me, I also assumed that the reviewer possibly meant "wind speed thresholds for snow transport". A comparison between both measurements sites in terms of occurrence of drifting snow as a function of wind speed is already proposed and discussed in the paper (see Figs. 2 and 3 in the former and revised version of the manuscript, respectively). Moreover, the actual quantity involved in the triggering of drifting snow is the friction velocity, which is only dependent on surface snow properties (Gallée et al., 2001). In the absence of measurements of surface snow properties, and knowing that atmospheric flow conditions would also influence the results, accurate determination of such thresholds can be achieved through turbulence measurements (not available either at D17 or D47) or wind speed profiles using the similarity theory by computing the friction velocity at the onset of drifting snow (e.g., Trouvilliez et al. 2014). However, such an alternative involve a thorough determination protocole (Amory et al., 2017) and selection criteria that are not continuously and homogeneously met at D17 and would result in a discontinuous time series, additionally subject to impeding factors (see my response to general comment #3 of reviewer RC1). For instance, accuracy issues arising from various variable numbers of available anemometers, absence of knowledge of measurement heights, choice of stability correction functions, or the validity of the similarity theory in drifting snow conditions, as well as inclusion of drag effects to the shear stress estimates (e.g., Amory et al., 2016) and artificial roughness created during maintenance operations, are all arguments that would, again, certainly deserve an entire sensitivity study in the form of another publication. Finally, the single measurement level at D47 preclude such determinations, and therefore spatial comparison with D17.

- Limitations in computing sublimation rates from AWS data and drifting snow mass fluxes have already been discussed in a recently published paper (Amory and Kittel 2019). The authors made use of one year of this dataset at site D17 in complement to relative humidity profiles to investigate the development of a near-saturated surface air layer in relation to the occurrence of drifting snow. As also mentioned in that paper, such an exercise involves specific requirements that are only met during a reduced period of time at site D17, justifying its treatment in a separate publication and precluding its application to D47 and outside of the period of study (year 2013). Similarly, strong limitations in the use of the thermo-hygrometers and in the applicability of the Monin-Obukhov similarity theory for retrieving latent heat fluxes at D17 (from which drifting snow sublimation rates could be inferred) have also been discussed in Barral et al. (2014).

- Sublimation of windborne snow has been inferred from accumulation measurements (e.g. Frezzotti et al., 2007; Scambos et al., 2012) but still remained to be confirmed and quantified by measurements of the latent heat flux within the atmosphere and drifting/blowing snow layers, accounting for the physical constraints mentioned above. The usual alternative is the use of gridded model products. Attempts using also satellite data have been made (Palm et al., 2017), but they involve the use of (i) parameterizations for snow particles properties, (ii) snapshots of the atmospheric conditions that are representative of instantaneous conditions only, and (iii) reanalysis produced from model that do not take into account interactions of snow particles with the atmosphere, particularly the negative feedback of windborne snow sublimation thus leading to a dry bias that can result in strong overestimation of sublimation rates and give the role of an infinite mass sink to the atmosphere. Moreover, as discussed in the introduction, strong discrepancy currently remain between the available model products (~100 vs 400 Gt/an), to the extent that the difference between each estimate is one order of magnitude higher than any other ablation term of the surface mass balance as determined from regional models (e.g., Agosta et al., 2019; Mottram et al., 2020). Different model-based approaches have also been proposed in which drifting snow mass transport is believed to be the first-order process with respect to sublimation, because of the low capacity of the atmosphere to hold moisture in the cold environment where accumulation measurements have been performed (Agosta et al., 2019). The role of sublimation during snow transport, particularly as a negative ablation term in Antarctica, is a currently debated problem in meteorology and snow science. I kindly refer to Amory and Kittel (2019) for a more detailed discussion on that matter from D17 data.

- Note that in Trouvilliez et al. (2014) only the initial results of the drifting snow observation campaign (the first 2 years) are presented under different processing criteria relative to a less complete knowledge on the FlowCapt™ capabilities at the time of redaction, and with no accessibility of the data and much less emphasis on drifting snow mass transport.

- Trouvilliez et al. (2015) focus on data collected in the French Alps and their work is not connected to the observations in Adelie Land.

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