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Interactive comment on "Brief communication: Rare ambient saturation during drifting snow occurrences in coastal East Antarctica" *by* Charles Amory and Christoph Kittel

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We thank the reviewer for its thorough reading of our paper, the very relevant questions and the proposed suggestions. Our responses are reported hereafter.

Response to reviewer #1

General comments This is a well-written paper with clear figures, and the topic is relevant for The Cryosphere. I do have some reservations about the need for this study to be a separate, brief communication while another study by the same author using the same dataset is presently being considered for publication in the same journal. It must

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become clearer a) why isolating this aspect of the dataset in a separate publication is warranted and b) whether the conclusions support the title adopted (see below).

Major comments RC1: Why was this paper presented separately from Amory (2019)? Is there a particular reason to do so? An obvious disadvantage is that the reader is now referred to that paper for a detailed description of the observations, which are at the core of this study.

Authors: While Amory (2019) uses single-level meteorological measurements of the full 9-year dataset to focus exclusively on drifting snow frequency and mass transport statistics and discuss applications for model evaluation, in the present paper we use a part (1 year) of this dataset and extend the analysis to 6-level meteorological profiles to discuss a different, independent topic related to the development of saturated air layers. Although the two papers do have some data in common and share a common context of drifting snow features, note that

1) the analysis proposed here relies on specific requirements that are only met during that specific period (availability of the depth sensor, no dysfunction nor burial of any of the 6 thermo-hygrometers, relative constancy in measurement height along the observation period - see the paragraph dedicated to that aspect of the study from L102 to L110), 2) the specific subject of drifting snow atmospheric interactions and sublimation involve theoretical descriptions that does not fit the objective of Amory (2019), and would have thus require additional out-of-the-scope theoretical background, 3) the meteorological profiles are not presented in Amory (2019).

For the above-mentioned reasons, and because we are 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, we believe that each of the two studies deserve separate papers. Moreover, the method section of the present paper includes a detailed description of the observations that contains the information (i.e., sensor type, range, accuracy, measurement principle and measurement height) necessary for the self-sufficiency of the paper, and relies on Amory (2019) only for additional, non-essential information about the observation campaign.

RC1: How general are the conclusions? This study only uses a single year of data for a single location. Is this sufficient to support the title of the paper, i.e. that saturation is rare in entire East Antarctica? If maintained, some more effort must go into supporting this claim. Authors: You are entirely right, Antarctica is wide and diverse. We suggest to change the title to "Rare ambient saturation during drifting snow occurrences at a coastal location of East Antarctica".

RC1: L79: ". . .in which a local balance between upward turbulent diffusion, gravitational settling and sublimation of windborne snow particles is likely to be attained." Can you be more specific, do the observations allow to assess in a quantitative way whether such a steady state is attained? Authors: This indeed may have been extrapolated a bit too far, at least in assuming steady-state drifting snow. Answering this question in a quantitative way could ideally be done through a modelling approach but observations alone hardly allows it. This part has been removed from the sentence.

RC1: L114: "Converted values in excess of 100 % were attributed to the limitations of both the instruments and the conversion method and were thus capped to 100 %." Can you provide some statistics, please? How often did this (RH values > 100%) occur? Authors: RH is the ratio of water vapour pressure to the vapour pressure for saturated air. RH values converted to be given with respect to ice (RH w.r.i.) are necessarily higher than original values given with respect to water (RH w.r.w.), since ice supports a lower saturation vapour pressure than water for a given temperature. A comparison between raw and converted RH values for the measurement level closest to 2 m is given in Fig. R1. The figure shows that converted RH values slightly exceeding 100 % (up to a maximum of 105%) occur regularly and inevitably account for most of the saturation conditions (considering that saturation is reached at RH >=100 %), as a result of the empirical character and inherent limitation of the widely used Goff-Gratch formulae. As the occurrence of supersaturation is very unlikely at our measurement site (as discussed in the paper from L115 to L118 in the original version), and the conversion of raw sensor records is needed for a matter of rigour, we maintain that Goff-Gratch conversion is a reasonable option and suggest to let the text as it appears in the original version of the manuscript but to add Fig. R1 as Fig. S1 in the supplementary materials.

RC1: Caption Fig. 2: "Frequency of saturation (RH = 100 %). . .": given the uncertainty in the RH sensors, saturation could also occur at measured values well below 100%. Have you investigated the sensitivity of your results to this definition of the observed 'saturation' threshold? Authors: Accounting for an uncertainty of 3 % in the absolute value of RH as stated by the manufacturer (Table 1 of the original version), the occurrences of a saturated air layer along the whole measurement range rise from 9.7 % to 21.3 % of the drifting snow occurrences if the saturation threshold is lowered from 100 % to 97 %. This twofold increase in the frequency of saturated conditions, also observed for the upper 4 levels (the lowest 2 levels below 1 m are less significantly affected because of initially high frequency values), still accounts for a reduced proportion of the overall drifting snow occurrences and confirm that saturation predominantly occurs within the first two meters above the surface and remains rather infrequent compared to the regular incidence of drift conditions.

We have added the following paragraph at the beginning of the discussion section, and Fig. R2 is now proposed as Fig. S2 in the supplementary materials: "Examination of the RH profiles revealed that the air is saturated with respect to ice over the entire measurement range in only 10.8 % of the drifting snow occurrences, assuming saturation is reached when RH = 100 %. Because of instrumental inaccuracy, saturation could however occur at measured RH values below 100 %. The sensitivity of the frequency of saturation can be investigated by decreasing the threshold at which saturation is considered to occur. Accounting for an uncertainty of 3 % in the absolute value of RH as stated by the instrument manufacturer (Table 1), the occurrence of a saturated air layer along the whole measurement range rises from 10.8 % to 23.2 % of the drifting snow occurrences if the threshold for saturation is lowered from 100 % to 97 %. This twofold increase in the frequency of saturation, globally observed for the upper 4 levels (the lowest 2 levels below 1 m are less significantly affected because of initially high frequency values – Fig. S2), still accounts for a reduced proportion of the overall drifting snow occurrences and confirms that saturation predominantly occurs within the first two meters above the surface and remains rather infrequent compared to the regular incidence of drift conditions.”.

The sentence in the conclusion from L214 to L216 in the original version has been completed by mentioning the threshold retained for the computation of the statistics as follows: “[. . .]. However, this is shown to be relatively rare (only 10.8 % of the drifting snow occurrences or 6.5% of the time if the RH value at which saturation is considered to be attained is taken as 100 %) at the measurement location and mainly occurs in strong drift conditions (> 0.2 kg m-2 s-1) associated with high wind speeds (U_2 > 20 m s-1).”.

Moreover, by re-investigating the dataset we have also discovered significant occurrences (7% of the data) for which the lowest humidity sensor reports raw RH (with respect to water) above 100 %, most likely due to riming on the probe and/or snow trapped in the radiation shield caused by its proximity with the surface where drift conditions are the most intense. These observations have been removed from the dataset and the (slightly modified) statistics and figures have been corrected accordingly. The following paragraph has been added to the method section: “Inspection at the dataset revealed a significant proportion of occurrences (7%) for which the lowest humidity sensor reports raw values (i.e., with respect to water) above 100 %. Such values are most likely caused by riming on the probe and/or snow occasionally trapped in the radiation shield due to its proximity with the surface where drift conditions are the most intense. After removal of these data, 16,289 six-level profiles were available for analysis.” . RC1: L123: “Before the event begins, only a thin layer near the snow surface is saturated as a result of surface sublimation.": Alternatively, near-surface air could become saturated not by sublimation but simply by cooling. See also L138. Authors: Yes it is true, a decrease in air temperature can just as likely as surface sublimation cause saturation in the vicinity of the surface. This suggestion has been added in the text as "[. . .] saturated as a result of surface sublimation and/or a decrease in air temperature". Atmospheric cooling has also been listed as a possible explanation for the persistence of saturated conditions after the cessation of drifting snow events (L138 in the original version).

Minor comments RC1: L46: "The thermodynamic effects of windborne-snow sublimation are physical limitations to accurate determination of sublimation rates from automatic weather station data. ": This is either an awkwardly formulated sentence or it contains a typo; please reformulate. Authors: We tried to resume with this first sentence the content of the underlying paragraph, but surely we did it in a clumsy way. We suggest to change the sentence to "The thermodynamic effects resulting from interactions of drifting snow particles with the atmosphere hinder accurate determination of sublimation rates from classical physical frameworks and automatic weather station data."

RC1: L52: ". . .raising instrumental accuracy as a large source of uncertainty which strongly amplifies with wind speed": please explain why instrumental inaccuracy amplifies with wind speed; one could also argue that stability corrections become less important during near-neutrality, enhancing accuracy? Authors: Turbulent mixing and drifting snow promote a weakening in the observed temperature and moisture gradients, to the extent that measurement accuracy are of the order of the existing gradients (Barral et al. 2014). In addition, increasing wind speeds are generally accompanied with an increase in relative humidity (Fig. 3 in the submitted version), resulting in a strong sensitivity of the profile method to measurement errors, particularly in the case of small gradients in conjunction with strong winds. For further details, we refer to Barral et al. (2014), in particular their section 5.2 and Fig. 12 which show that "the propagated uncertainties are amplified with wind velocity or decreasing temperature gradients”. Even if a significant influence of stability corrections in the computation of turbulent fluxes has been demonstrated for stable conditions on the Antarctic plateau (Vignon et al., 2017), a much lower influence can be expected at D17 because the near-surface atmosphere is mostly neutrally stratified throughout the year, and it is more likely that the gradient-induced uncertainty largely offsets the enhanced accuracy due to the decreasing importance of stability corrections with increasing wind speeds. To improve clarity, we suggest to reformulate the sentence L50 to L53 in the original version as “In addition, the well-known profile method commonly employed to compute turbulent heat fluxes can become hardly applicable in drifting snow because vertical moisture and temperature gradients are weakened by windborne-snow sublimation and turbulent mixing. As the observed gradients reduce, instrumental inaccuracy becomes important compared to gradients and induces comparatively large flux uncertainties which thus strongly amplify with wind speed (Barral et al., 2014).”.

RC1: L82: “. . .isothermal layer. . .”: Strictly speaking, a neutral surface layer implies that potential temperature is constant with height, not temperature. Authors: Absolutely right. This sentence has been changed to “Strong turbulent mixing due to frequent katabatic flows induces the nearly-constant presence of an isothermal layer ensuring that vertical changes in relative humidity during transport are mainly governed by particle-air moisture exchanges along the profile”.

RC1: L. 95: emerged -> exposed (?) Authors: Corrected accordingly.

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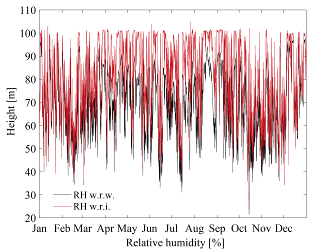

Figure R1. Timeserie of raw (given with respect to water; w.r.w.) and converted (given with respect to ice; w.r.i.) RH values at 2 m height showing the regular occurrence of RH > 100 % due to limitations of the Goff-Gratch formulae used in the conversion.

Fig. 1.

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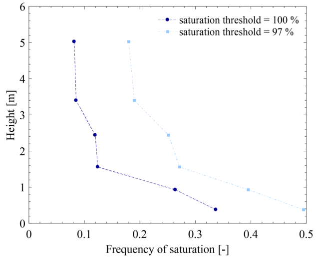

Figure R2. Frequency of saturation for in drift conditions at each measurement level assuming that saturation is reached at 100 % (dashed line) and 97 % (sold line). Yearly average instrument heights are used.

Fig. 2.

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