

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

Anonymous Referee #1

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Review of

Brief communication: Rare ambient saturation during drifting snow occurrences in coastal East Antarctica

By C. Amory and C. Kittel

General 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 become clearer a) why isolating this aspect of the dataset in a separate publication is

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warranted and b) whether the conclusions support the title adopted (see below).

Major comments 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.

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.

I.79: “. . .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?

I. 114: “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?

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?

I. 123: “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 I. 138.

Minor comments I. 46: “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

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a typo; please reformulate.

I. 52: "...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?

I. 82: "...isothermal layer...": Strictly speaking, a neutral surface layer implies that potential temperature is constant with height, not temperature.

L. 95: emerged -> exposed (?)

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-165>, 2019.

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