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

Review by M. Lehning

The paper presents an observational study on moisture dynamics at one site in Antarctica with frequent events of strong katabatic winds and associated snow transport. The paper is nicely formulated and timely as the role of sublimation during snow transport is a currently debated problem in meteorology and snow science.

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Discussion paper

I asked myself whether this rather limited dataset is worth a separate publication but concluded that it helps to shape our understanding of what blowing snow sublimation may look like in these katabatic wind zones. However, I suggest that more complete context and discussion is provided. There have been recent LES simulations on drifting snow sublimation (e.g. Sharma et al., 2018; Huang et al., 2016), which claim that previous efforts in modelling sublimation may have started from wrong assumptions. It would add value and impact to the current paper if the authors could discuss whether their observations are consistent with the new model findings or not.

One major comment I have is on the overall limitation of sublimation in snow transport clouds. The authors revisit the argument that with stronger wind and snow transport, total snow sublimation may be limited because saturation occurs. This argument has been formulated by Bintanja (2001) based on a model study, in which the author considers a model depth of 10 m to look of blowing snow sublimation. I have always been very skeptical about the conclusion of limited sublimation because I expect in these situations the level of maximum sublimation to be simply lifted to higher elevations such that it is not seen in the first 10 m. If I understand the Bintanja model study correctly, then he only sums up sublimation occurring in the lowest 10 m, which is of course only part of the total sublimation if high winds cause deep clouds of blowing snow. The authors are therefore encouraged to either present clear evidence of total sublimation reduction in stronger winds, or not conclude about this aspect.

Minor comments:

In practical meteorological applications, drifting snow is below 2 m and blowing snow above. Snow scientists, however, would rather define drifting snow as saltation and blowing snow as suspended snow.

l. 30 (and first sentence in the abstract): I think that it is not proven yet or generally accepted that snow transport and sublimation is the main ablation process over the entire Antarctic ice sheet.

l. 50: Suggest to replace “verified” by “met”.

l. 75 vs. l. 80: You cannot say that D17 is in an accumulation zone and then that you achieve equilibrium horizontal mass flux. This is contradictory.

l. 107: Whether or not the vapor pressure really increases towards the surface (or only most often) depends on the air temperature gradient (and the one in the surface snow to a lesser degree).

l. 161 ff: Mark this as hypothesis/discussion and mention (again) temperature gradients, which are also able to produce moisture gradients.

l. 175: See major comment above on moving the elevation of maximum sublimation upwards in higher winds. This is what I expect to occur.

l. 212: Check wording “preferably”.

References:

Bintanja, R.: Modelling snowdrift sublimation and its effect on the moisture budget of the atmospheric boundary layer, Tellus A, 53(2), 215–232, doi:10.1034/j.1600-0870.2001.00173.x, 2001.

Sharma, V., Comola, F., and Lehning, M.: On the suitability of the Thorpe-Mason model for Calculating Sublimation of Saltating Snow, The Cryosphere, 12(11). 3499-3509, https://doi.org/10.5194/tc-12-3499-2018, 2018.

Huang, N., X. Dai, and J. Zhang (2016), The impacts of moisture transport on drifting snow sublimation in the saltation layer, Atmospheric Chemistry & Physics, 16(12), 7523-7529.

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